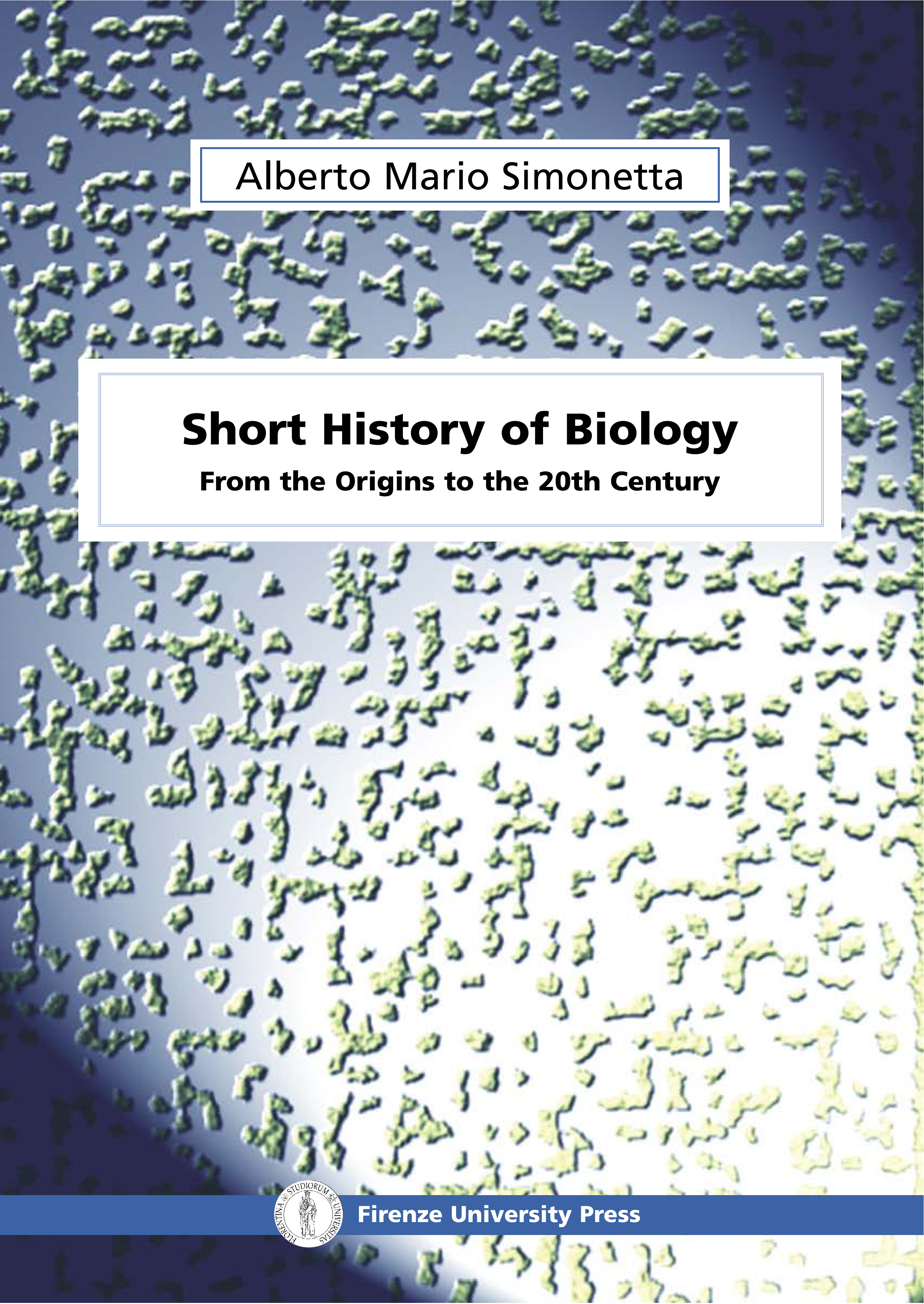
Alberto Mario Simonetta

Short History of Biology

From the Origins to the 20th Century



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2003

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Preface

During the last three centuries Natural Sciences have become, crucial to the development of civilisation, and their impact on our lives is daily growing. However, in spite of the equally increasing concern for environmental issues, endangered animals and plants, not to speak of medical issues (as indeed these are marginal to this book), and of the proliferation of books on the various aspects of biology, the story of the development of biological sciences has been by and large ignored.

Apart from such treatment of the history of Biology and of the biographical informations that may be found in the general histories of Sciences and in the general biographical dictionaries of scientists, whereas there are good books on the history of Physics, Mathematics and Cosmology, and though there are many books (admittedly mostly bad) on Alchemy as the forerunner of modern Chemistry, the books, such as Nordeskjöld's, as were written on the story of biological thought, have been long out of print and the history of biology has been confined to, at most, a brief introductory chapter in general textbooks on biological sciences, and they are often poor summaries of a type of traditional lore, which the reader may well ignore as it is nowhere apparent why their content may be relevant for the modern scholar.

Indeed a few excellent books have been published during the last 20 years or so, on the development of some topics of biological thought, such as Mayr's book "A history of biological thought", which however, is almost entirely concerned with the growth of evolutionary ideas, Stevens' book "The development of biological systematics" (1994), dealing with the development of botanical systematics, or in Italy Barsanti's *La scala, la mappa, l'albero* (1992) on the evolution of classifications between 1600 and the middle 1800s and their connections with graphic conventions, or Omodeo's (1984, 2000, 2001) and a few others, yet there a there is no comprehensive work currently available in English, and even the French transaltion of de Wit (1994) is practically unobtainable, so that, to my knowledge, the only text pratically available is Duris and Gohan's *Histoire des sciences de la vie* (1997).

Such a dearth of information has been the main reason which prompted me to prepare this summary, but, by itself, it may not have been sufficient reason for this work. Indeed one may legitimately ask why the knowledge of long obsolete ideas may be relevant for the modern scholar or for the cultivated layman. I do not know if my argument for it may sound convincing, but here it is.

The reader who will have the patience to go through the following chapters, will notice that the development of biological sciences was notably continuous: there were, indeed momentous periods of acceleration, periods of stasis and even, because of lack of facilities in communication of scholarly ideas and of technical problems, even periods of widespread cultural regress, but even such momentous episodes as the publication of Vesalius' "*Fabrica*", of Harvey's "*Exercitatio*" or of Darwin's "Origin of Species" were the culmination of periods of preparation and maturation of the scientific environment, that, without detracting from the greatness of these scholars, made their ideas readily acceptable within the scientific media. Nor do recent developments in biology escape this pattern, for good and bad, as we shall easily point out how important aspects of present scientific debates have a neat pedigree of Augustinian or even Platonic or Aristotelian origin.

Just as in any other aspect of cultural or practical life, we live (and are adapted to live) in an environment that has been forged by past events, so that, to the student of biology, the history of this branch of sciences should be as significant as the knowledge of his country's history to the citizen of any state.

Again, we might be asked why the history of biology offered here to the reader is a history of Western biology, with just a brief chapter of the Medieval development of medicine and biology in the Muslim world. As a matter of fact, while every culture has organised the body of its biological knowledge within the framework of comprehensive theories, the non-European cultures had theories which were wiped away by the impact of Western biology. They had sound practices, excellent remedies, and both of these are increasingly received currently all over the world, but their theoretical framework was at such variance with our Western methods of scientific research and philosophical thinking that they had no impact whatsoever on the development of our ideas and are, for us Westerners, just erudite curiosities. However, the reader has, at least, to be cautioned: a good deal of the past scientific thinking of the non-European peoples is still poorly studied and even unavailable for the Western scholar. Such translations as actually exist are the work of philologists and thus often unreliable as far as the interpretation of technical terms is concerned. Much work is still needed on the history of non-Western sciences before they may be fairly assessed. The overwhelming triumph of our sciences may well have obliterated deserving discoveries; let us hope that Oriental, Amerindian and African biologists will investigate their own scientific traditions with sound methods and unbiased attitudes.

However, whether or not future research will change our outlook, there is no doubt that in the present world, the history of biology that matters to the scientist is that of Western biology.

Within the practical limits set by the size of this book, I have endeavoured to frame my account of the development of biological sciences within the general historical, social and cultural environment which surrounded both researchers and teachers. After all no one, least of all the scientists, live and work insulated from society and, as my past experience has taught me that young people are very adept to forget the history learned in high school, thence I have prefaced each chapter with a selection of dated historical references in the hope that these may help the reader to place both the personalities of the scholars and their discoveries.

This volume is not a simple translation of my Italian book: "Breve storia della Biologia dalle origini all'inizio del XX secolo": all chapters have been revised and some have been entirely re-written and I hope that all changes are improvements.

CHAPTER I

The beginnings of Greek scientific thinking

SYNOPSIS OF MAIN HISTORICAL EVENTS AND OF CONTEMPORARY THINKERS

Thales of Miletus c. 620-c. 550 BC, Anaximander c. 610-c. 540 BC
612 BC Niniveh is conquered by a coalition of Medes, Babylonians and Persians.
610 BC Ciassarres of Media destroys the last Assyrian king Asur-Uballit of Harran.
585 BC war between the Medians and the Lydians, battle on the river Halys, supposed eclipse announced by Thales and peace between the Medians and Lydians. Solon dictates the new Athenian constitution.
Anaximenes c.580-520 BC; Pythagoras of Samos c. 570-c. 500 BC
561-528/27 BC Pisistratus rules Athens. Final editing of Homeric poems.
550 BC Cyrus the elder creates the Persian empire and conquers Media.
547 BC battle of Pteria and end of the Lydian kingdom, Miletus is allied with the Persians.
Heraclitus (c. 540-c. 475 BC), Parmenides (c. 520-c. 430 BC)
539 BC Cyrus conquers Babylonia and in 538 BC authorizes the reconstruction of the Temple of Jerusalem. Ezra and Nehemiah begin the collection of the Biblical texts.
530 BC Cyrus dies while campaigning against the Sacae (Scythians).
530-522 BC Cambyses king of kings; in 525 BC defeats at Pelusium the Pharaoh Psammeticus III allied with the Athenians and unites Egypt to the Persian empire; however, in the next two centuries Egypt repeatedly regains temporary independence.
522-521 BC Persian civil war, Darius I becomes king of kings.
511 BC the Crotoniates, led by the Pythagorean sect, attack and destroy Sybaris.
Alcmeon of Croton (c. 510-c. 440 BC)
510 BC Hippias is expelled from Athens by Athenian exiles supported by Spartan troops and with the political support of the Oracle of Delphi.
507 BC Athens allies herself with the Persian satrap Artaphernes against Sparta.
500/499-497 BC the Ionian towns revolt against the Persians, only Athens and Eretria send help, but only unitl 498 BC.
495 BC battle and destruction of Miletus.
491 BC the Persian ambassadors who were asking for the submission of these towns are massacred in Athens and Sparta, marking the beginning of the first Persian war.
490 BC the Athenians and Plateians defeat the Persian army led by Hippias, Datis and Artaphernes at Marathon, the Spartan army arrives a few days later.
Anaxagoras of Clazomenae (c. 500-c. 425 BC), Aeschilus (525-456 BC), Sophocles (496-406 BC), Euripides (480-406 BC), Zeno the sophist (c. 495-c. 440 BC), Protagoras (490-420 BC), Gorgias of Lentini (c. 490-c. 420 BC), Empedocles of Agrigentum (c. 480-430 BC)
481 BC beginning of the second Persian war.
480 BC at Salamis the Greeks defeat the Persian fleet, simultaneously the Syracusans defeat at Hymera the Carthaginians, allies of the Persians.
479 BC decisive victories of the Greeks on the Persians at Plathaia and Mycale.

465 BC Xerxes is killed, civil war in Persia.
467-428 BC Age of Pericles.
449/48 BC peace of Callias, the Persian empire acknowledges Greek supremacy in the Mediterranean, Delian league and pre-eminence of Athens.
Leucippus (c. 450 BC), Socrates (470-399 BC), Democritus of Abdera (c.455 BC).
431-421, 413-404 BC Peloponnesian wars.
412 BC Sparta allies herself with Persia and, supported by Persian gold, finally beats Athens, which surrenders in 404.
399 BC trial and death of Socrates.
386 BC "Peace of the King": the Greek states submit to the arbitration of the Persian king.
Architas of Taras (a. 388-p. 360 BC), Philolaos the Pythagorean (c. 495-c. 395 BC), Plato (429-356 BC)
359-336 BC Philip II is king of Macedonia.
338 BC battle of Chaironeia and Macedonian overlordship of Greece.

Factors which allowed for the development of speculative thought

Before we begin the actual study of the development of Greek thinking, we must first note that, to our present knowledge, this conceptual approach is the only one in antiquity to develop a precise interest in logically rigorous abstract generalisations and for an argumentative treatment of problems.

Scholars who studied the origins of philosophical speculation have often underlined the significance that – in the process – may have had both linguistic and political factors.

It is impossible here properly to discuss either of them, but we may well briefly mention some considerations.

Such scholars who maintain the significance of linguistic factors, have remarked that while other ancient languages, such as classic Hebrew, have both an extremely simple grammar and syntax, in Greek the precise meaning of names in a sentence is defined by both article and declension and that verbs are especially complex. Thus, whenever a common name is united with an article, the meaning is automatically restricted to one or a few, precisely identified, individual objects or beings, whenever no article is used the same word signifies the whole category or class of objects which may be called by that name. At the same time the niceties of verbal flection, such as the use of dual or of aorist, allow for an extreme precision of speech. Obviously this does not mean that the interpretation of a text never poses problems, especially when, as it often happens with philosophical works, they survive as isolated quotations. The significant thing is that, as a language is the work of a whole people, the Greek language testifies, as such, to a generalised interest in clear, unambiguous speech and for the possibility of abstract thought.

As far as politics are concerned, the socio-political organisation of the Greeks is characterised by a more or less early, general evolution towards democratic assemblies. Even the Homeric poems show us chiefs who have to account for their actions at popular meetings or have to argue and persuade numbers of people in order to win their support for their plans.

We do not know whether this already occurred in Mycenean times, but the arts of persuasive oratory and of clear argument were already vital in archaic Greece. This implied logical argument as a necessary tool in debates in the assembly, and this is a natural premise to an argued philosophy and science. Indeed, traditions indicate that most of the older philosophers were leading political figures in their towns: were they eminent politicians because they were natural philosophers or was it the habit of political debates that led them to debate natural truths as well?

To all this we must add a peculiarity of Greek religion: the Greeks lived in a number of entirely independent communities; this clearly favoured the development in each community of local varieties of even the most widespread myths, whereas the lack of a "sacred book" and of an organised and hierarchical clergy made it difficult to charge people with heresy. Such a charge was posed in a few instances, but, as a general rule, thinkers felt free to propose new interpretations and elaboration of traditional myths (as is amply proved by dramatists and comedians) or to propose entirely new myths. In fact when objectively considered, the so-called "scientific theories" of the early philosophers are nothing but myths, as we shall see further on.

Finally, and we shall return to this point as well, the special significance that the "impassible Gods" had in Greek religion must be minded as well. These Deities maintain the laws of the "cosmos", of men and destiny. They watch the order of the universe, can not be prayed to and to them even Zeus must bow.

Greek philosophy and biology before the times of Aristotle; the archaic Greek world

We do not know when Greek thinking began, but it undoubtedly has developed a first precise character and organisation in the Homeric poems.

We now have a precise *terminus ante quem* for the final redaction of a Homeric text in the quotation of some verses in an inscription from Ischia dated 720 BC, almost 200 years before the traditional date of the collation of the texts during the rule of Peisistratus in Athens (around 550 BC), and the fact that it comes from an island off the coast of Southern Italy proves that their knowledge was widespread.

Indeed we can not be certain of how much Homer testifies to the Helladic tradition and how much it portrays the Greek world of the IX-VIII centuries BC. Most modern scholars believe that Homer did in fact know very little of the life and times of the Mycenean lords. However the Mycenean texts, though mere administrative documents as they are, show that he did indeed know something of it, and, most significant, they bear witness of a clearly Greek people. The texts of Pylos etc. are in fact written in an exceedingly archaic Greek, but nevertheless in an unquestionably Greek language. Besides administrative matters, they relate the names of many Gods, including many of the Olympians and even Dionysos, the god of knowledge alternative to that of Apollo (and that has, incidentally disposed of a time-honoured theory that, as Homer does not mention this God must have entered Greece at a comparatively late date, after the compilation of the Homeric poems).

In recent years some scholars have maintained with good arguments and on the evidence of the decoration of some Mycenean jewels, that the Myceneans must have been familiar with some theorems on the circle and on the hexagon all somewhat more advanced than those known to contemporary Babylonian mathematicians.

However it is significant that what is usually defined as philosophic-scientific thinking, only very gradually distinguishes itself from mythologic tradition (in the literal meaning of a tale or argument about Myths), the two being completely separated in but a very few thinkers before the Hellenistic times.

The first schools of Greek philosophy

Though it is usual to preface the study of the Greek contributions to the sciences by a consideration of what in the various disciplines had been achieved by Egyptians, Sumerians, Assyrians and the other peoples of the Near East, I shall not follow this habit.

From the beginning of history and long before it, all peoples, during their long history, collected a considerable amount of empirical knowledge, and even the oldest surviving texts often mention different animals and give us an account of various medical practices which require a precise knowledge of the pharmaceutical properties of different plants and minerals, of anatomical and physiological data etc. However, all this knowledge, albeit codified and, occasionally, generalised to some extent, is always devoid of any speculative content, as, vice versa, is usually the case with Greek culture. The Greeks eventually derived such information from their neighbours (it was, indeed, an established tradition that the first great Greek thinkers, such as Thales, Pythagoras, etc., had learnt much of their knowledge during their true or supposed voyages in the lands of the Barbarians). However the Greeks were able to reshape it in the guise of theoretical generalisations, which can only be considered the forerunners of proto-philosophic and scientific thinking. At all events, there is very little that can be considered as 'biology' in what we know of the thoughts of the Greek philosopher-scientists preceding Aristotle.

First of all, it is clear that to all presocratic philosophers the distinction between the world of living organisms and that of inorganic, non-living, matter was either obscure or has definitely to be ruled out.

The obvious character which allows these philosophers to separate living from non-living things was the fact that living things are 'self-moving' whereas non-living objects and corpses have to be moved by something outside them. Now such a criterion was equivocal (to Thales, among others, the lodestone was a living being as it was able to move itself towards an iron object) and left an ambiguity of somewhat intermediate objects, such as eggs or seeds. On the other hand, as they could not explain the apparently spontaneous movements of inorganic bodies, such as earth, water, and especially wind, they naturally tended to attribute them to the whims of 'personalities', perhaps different from those responsible for such 'rational' movements as those of the celestial bodies. As a result we must deem that Thales' statement "The world is full of Gods" is a perfectly rational one.

We must here stress an observation that has had a very lasting significance in the biology and physics of the Greeks: death can easily be identified with the ceasing of breathing, and winds may well look as the breath of the world, on the other hand there is no motion apparently more spontaneous than wind, and its motion moves the seas, the clouds and any other sufficiently light body.

The Greek word 'Pneuma' (πνεύμα) (and of 'pneuma' we shall have much to say) does not mean breath as the act of breathing or the blowing wind, it means 'the breath', independently from what is actually breathing. Thus the concept of 'pneuma' will slowly evolve through the centuries, but it will always remain an important concept in all biological and physical Greek theories.

In order to understand Greek science, philosophy and religion, two other concepts are significant: namely that of 'Noús' (Nooζ) and of 'Nomos' (Nόμοζ). Taken together they characterise all that is rational both in men and in the cosmos: This, in the end, was the basis of the progressive identification with Gods of many celestial bodies, who, with their unchangeable and mathematically perfect movements, tell the times of terrestrial events.

All the points raised in the previous sentences, are already implicit in Homeric poems, which are the oldest surviving documents of archaic Greek thought.

Indeed in Homer the word 'soma' (Σώμα), body, is used only for corpses. Living beings are always described by means of their 'composing parts', such as legs, arms, head etc. and of their, so to say 'active parts': Thymos (Θύμοζ), Nous and Psyché (Ψυχή). Thymos is that something which is responsible for emotions, while Nous is what is rational and conscious. Lastly Psyché (literally 'breath, puff', but also 'butterfly') is that which makes an individual alive and, in men, their only immortal part. Thus we often find sentences of the type "he was willing to do that, but his thymos paralysed his legs". On the other side, while 'thymos' is shared by men and animals, 'Nous' is common to men and Gods. We shall see how these ideas were significant in later discussion of the "vegetative soul, the appetitive soul and the rational soul", which had a great influence on the development of systematics, embryology etc. and that are still implicit in many extant legislations.

The fact that 'Psyché' and 'Pneuma' are to some extent synonyms led to a line of thought, which began with a fragment of Anaximenes written around 546 BC and which literally reads "Just as soul (psyché) is our air (pneuma) and keeps us together (but one may also translate "controls us") by that, so air and breath keep together (or "control") the whole world". It then passed through stoic and neoplatonic philosophers, and had a considerable impact on the development of the ideas of the relationship between Macro- and Microcosmos and went into vitalism down into the 20th century.

Obviously the Greeks could not overlook the significance of the relationship between the 'soma' (material body) of living beings and non-living bodies. So we shall shortly begin to consider the evolution of concepts concerning the nature of what is 'material' in the world, and more properly the increasing credit of the theory of the four 'stoicheia' (singular Στοιχεΐον), commonly translated as 'elements', but more properly 'material principles'.

According to the essentials of the theory; all objects including the bodies of living beings, are composed of matter, and in this we can identify a certain amount of dry substance, that is of 'earth', mixed with a certain amount of 'water'. The breath or vital pneuma ('air') gives them life, and as they are moderately hot, they must contain also some 'fire'.

In fact the theory of the four elements: earth, water, air and fire and of the four qualities, opposed two to two, heat and cold, dampness and dryness, was expounded by Empedocles, but has almost certainly much older roots. It was finally developed fully by Aristotle (and, as we shall see, while it was accepted by many, it was questioned even by Aristotle's friend Theophrastus). Furthermore, the theory had a great importance in the whole development of sciences until modern times.

All the hypotheses advanced by ancient peoples in order to explain the origin and nature of things are myths, and it is fascinating to follow how the ancient cosmogonies of purely religious pattern (at least in the sense we currently give to these terms), such as those of Hesiod, gradually change because of the unstated growing requirement of empirical plausibility, and eventually become what we may term as scientific hypotheses or theories. The Gods, not only for what concerns the first origin of things, but as rulers of the present course of phenomena, change from somewhat whimsical players with things and men into the rational guardians of a universal Nous.

Greek religious attitude was particularly apt for this change as, even since our earliest testimonies, the Impassive Deities: Ananke, the Moirae, Dyke, Themis, who all may be subsumed under the Latin term of 'Fatum' (=that which must be and cannot be otherwise), must be obeyed even by Zeus.

There is no doubt that since the earliest times the Greeks were quite convinced of an ambivalence in the relationship between the individual man and the events which befell him: man may well make his choices, but this only within the limits of what has been decreed by the impassive Deities, first of these Ananke, and the Moirae, and by the 'laws': Themis and Nomos. A choice different from the one so expected was indeed possible, but it was the supreme offence (hybris) upon the Gods and the implacable Nemesis was there to punish it. In this context the reply of Achilles to Thetis, who is urging him to avoid his destiny leaving alone Hector and Troy, is typical: "Should I do it, I would no more be Achilles!".

This attitude almost naturally led to the belief in the existence of immutable laws in the universe, a concept that is the very core of all scientific thinking as we conceive it. The alternative is occasionalism, which was, indeed, advocated by quite a few Christian and by many Islamic thinkers. They maintained that everything that happens is directly the doing of God, who plays with men and things as puppets and that God's laws are not really laws, but simple sequels of events that might be changed at any time by God's will[1].

Starting from the religious beliefs that we have summarised, Greek thought developed and, not surprisingly, reached its greatest achievements in Mathematics and Astronomy, fields where, because of the extreme regularity and comparative simplicity of phenomena, the implementation of a rigorous conceptual framework could better succeed.

Chemistry and Biology approached themselves to the ideal models of science only later and to a limited extent because of the complexity of biological phenomena, and, in the case of chemistry, because of the difficulty of quantitative controls in the absence of sufficiently precise instrumentation.

Aristotle is quite clear in his distinction between science and empiricism; he maintains that science (or philosophy) is the asking and answering the questions of how and why the observed phenomena happen, while empiricism merely observes the phenomena and possibly cares for the practical utilisation of the observations.

We must here remind the reader of a special difficulty in the understanding of the early Greek philosophers. This is that, in order to explain their ideas, they usually use comparisons with familiar phenomena, and it is not clear whether they thought of these comparisons as real analogies or as rough approximations.

So, for instance, by the statement of Empedocles that sounds are moving air that hits inside our ear onto a membrane hanging "like a rattle", it is not clear whether he did in fact know of the tympanum and had made a shrewd guess at its working, or whether his was a fantastic idea such as the kind of connections that he believed to obtain between the eye, fire and vision.

The oldest Greek philosophers called themselves 'physiologists', from the Greek words 'physis'[2], that is Nature and 'logos', discourse, meaning that they were arguers

[1] On this we have a curious *Quaestio quodlibetalis* by St. Thomas Aquinas: "Can God restore virginity to a girl who has lost it?", The answer being that though he may indeed restore the physical features of it, even God can not cancel the fact that such loss had happened

[2] φὺσιζ is a term that, in the oldest authors, like Hesiod, derives from the verb φυναι that means to give birth, to generate, thus it is used literally with the meaning of 'birth of things'.

about Nature. And, indeed, their main problem is the origin and nature of things. It was only much later - and when it had already reached a high complexity - that philosophy became interested in other problems, such as that of the nature of the human mind, of the principles underlying knowledge, and of morality. Anyway, the problem of Nature, including biology, is always the foundation of Greek philosophy, even in those schools that left it rather in the background.

Ionic philosophers

A time-honoured and amply justified tradition rooted in Aristotle's writings considers that philosophy began with the teachings of three Milesian thinkers: Thales, Anaximander and Anaximenes.

In Thales' times many Greek towns flourished along the coast of Asia Minor. These were most, but not exclusively of Ionian origin, and originated by the wave of settlements which occurred in the wake of the 'return of the Heraklids', that is the Doric invasions, which apparently caused the collapse of the Mycenean civilisation. Such colonisation had been further enlarged and strengthened by new settlements during the great age of colonial foundations in the 7th century BC.

A sort of symbiosis had developed between the Greek cities of Asia Minor and the kings of Lydia. Miletos was probably the richest and more powerful of them.

However, though in 585 BC the Lydians had succeeded in throwing back a first onslaught by the Medians (battle of Halys); forty years later, in 545 BC, Croesus, king of Lydia, was attacked by the Persians, a new power who, after co-operating with the Medians and Babylonians in the destruction of the Assyrian kingdom, had turned against their former allies, had crushed them and had embarked on a course of unlimited imperialism. The tradition relates that Thales, in his time, had advised the Greek towns to support the Lydians; nevertheless the Greeks either remained neutral or actively supported the Persians, and, when the Persians crushed the Lydians (battle of Pteria), they discovered that they had exchanged a peaceful neighbour with an oppressive power who would, at most, leave them limited autonomy.

At this point the Greek towns rose in arms, gained some limited support from Athens and a few other towns from the motherland, but were equally beaten and in 494 BC Miletos was temporarily destroyed.

This is the historical framework in which the early Ionic philosophers operated. As with many other Greek philosophers, we do not know the dates of their birth and death: the Greek historians did not care about such things and they tell us, instead, when was the "Acme", that is the culmination of the activities of the person they quote.

Thus the Acme of Thales, son of Praxiades, was around 580 BC, and we, therefore, presume that he may have been born around 620 and died around 545 BC The

Greeks themselves did not know of any writing of his, but they traditionally credited him with some geometrical theorems and said that he maintained that the Cosmos is an ordered and intelligible system.

Apparently Thales was the first to think that everything was the embodiment of a single material principle or 'Arché', and he supposed that this was water. His idea brilliantly developed an old tradition, which is clearly expounded in the Homeric poems, where it is stated that the Earth is surrounded by Oceanus and that its movements, including earthquakes, are due to the aquatic God 'par exellence', Poseidon.

Apparently Thales conceived all matter as potentially animated and he especially considered the lodestone (natural magnetite) as fundamentally a living thing, because it was capable of self-movement towards iron objects. It is typical of this kind of trend in thought both to study the rationale in Cosmos, as the foundation of all scientific research, and to cry "the world is full of Gods!"

Anaximander, a junior fellow citizen of Thales (born perhaps in 610 and died around 540 BC) held the same basic feeling, later termed ilozoism or ilopsychism.

Anaximander held that the basic substance of which the universe was built could not be defined. He thought that whatever substance you choose it implies the exclusion of 'something'. Therefore he calls the basic substance 'Apeiron', that literally means 'without limits', a universal substance which is the substratum of everything. According to Anaximander things become identifiable by the opening of spaces or by the emerging of quantities inside the Apeiron. It was starting from this hypothesis that he imagined a complex cosmogony and from this he developed a chain of hypotheses which explained every phenomenon.

As to the origin of living beings, as far as we can gather from the quotations of later scholars and especially of Aristotle, he had some precise ideas. Anaximander believed that there was a progressive desiccation of Earth. Living beings came from a primaeval mud which originally covered the whole Earth. First animals and plants were formed, then mankind. Both men and animals originally lived in water and were sheathed by a scaly cover. When they left the water, the terrestrial animals lost the protective shell. Clearly this hypothesis was needed considering that had the first terrestrial animals and especially man been born from earth as they are now born from their mothers, they could not possibly have survived by themselves, therefore they must have first emerged from water as adults. We do not know whether Anaximander derived this idea from the observation of the metamorphosis of tadpoles into frogs.

Some scholars have argued that the ideas of Anaximander foreshadow some evolutionary ideas. Neither the surviving fragments of Anaximander, nor the accounts that later authors give of his idea provide any support to this interpretation. In fact the name of Anaximander creeps up in some discussions on transformism in the 18th and early 19th century, but as a gratuitous assumption.

Anaximenes was also a Milesian and a pupil of Anaximander, and his Acme is around 550 BC. He considered Air as the Arché, but this was in the sense that he saw in air both the limitless Apeiron and the principle of life and movement. So his is the first formalisation of the pneumatic theory. We have no idea of what Anaximenes thought about living beings.

Now that Miletos was conquered and destroyed by the Persians, and the cultural centre that had been there did not survive. Yet the Ionic school had some late followers. Among them we must mention Diogenes of Apollonia, a Cretan physician, who lived around 430 BC (to be distinguished from Diogenes of Sinope, the famous cynic philosopher). Diogenes of Apollonia is said to have made both anatomical and embryological researches. He described the ramifications of the vascular system in Man (or, more probably, in some mammals) and his description survives. He also studied the development of the embryo in the uterus. Diogenes is also known as 'the eclectic', as he attempted a synthesis of the various Ionic theories, mainly following Anaximenes, with the Eleatic theory of an immutable cosmos; he is a convinced pneumatist. For him the principle of everything is Air, an increate substance, unlimited and rational. Thinning air gives rise to fire, whereas by condensation it changes both into water and earth. The air is also the soul and as such the principle of life and movement. Warm air, not as hot as the sun, but warmer than atmospheric air, flows in the vessels and heats the body. All living things, Man included, originated from mud under the influence of the sun's heat. All differences among things result from minor changes in the basically immutable air, by the action of different qualities; these are relative to each other and to the observer. So, for instance hot is relative to cold, and, anyway different persons will judge differently about how hot or cold a thing may be.

Another physician, contemporary of Diogenes is Hippo, and we know that he made embryological observations and that, as Thales, he considered water, or rather dampness, to be the principle of life.

Pythagoras and the Pythagoreans

The Pythagorean school was begun by Pythagoras of Samos (who migrated to Croton in Calabria and died around 500 BC).

While Pythagorism is extremely important in physics and mathematics, its contribution to biology is a minor one. However, we must still mention the Pythagorean theory of numbers, of harmony and of opposing qualities, as they were relevant to the medical Hippocratic school and also to later medical schools.

Since it was the habit of the Pythagoreans to credit all their discoveries and ideas to Pythagoras, it is impossible to tell apart the contributions of the various members of the sect.

The influx of the Pythagorean ideas on numbers is complex. The Pythagoreans believed that the unit had an objective reality, they thought of it as a sort of numeric 'atom' and that all reality was made up of such atoms. They had remarked that by an orderly arrangement of points (equated with units), one could build all the regular figures and the combinations of these did produce solid (three dimensional) figures. Among these only four, the so-called 'Pythagorean solids' (the fifth was discovered only much later) were characterised by all equal faces. Thus they thought that these figures, apart from the circle and the sphere, must have a special significance. Most thinkers, therefore, assumed that the elementary particles were either made in the shape of the elementary flat figures (and this was the opinion of atomists such as Democritus), 'things' being made by the assemblage of flat atoms. The believers in the four elements naturally identified Air, Fire, Earth and Water with the four Pythagoric solids.

Quite naturally these guesses, like those on the circle and the sphere in astronomy, had a lasting influence on the evolution of scientific ideas.

As the Pythagoreans studied the laws of consonance of sounds, they evolved the theory of the 'harmony of celestial spheres' (which had such a great significance in directing the work of Kepler towards the discovery of his basic astronomic 'laws'), and this, in turn was a powerful factor in the development of 'humoral' theories in medicine and biology. The four basic 'humours' being yellow bile, black bile, phlegm and blood) were supposed to be the equivalents in living beings of the four elements. Their balance or unbalance determined whether an individual was healthy or sick.

We may conclude that, while the Pythagoreans contributed almost nothing to biology, their physico-mathematical ideas had an indirect lasting influence on medicine and hence on biology. Their other beliefs on the transmigration of souls, their magic prescriptions for living, so dear to the Acusmatic sect of the Pythagorean school, are practically irrelevant in the history of biology.

The school of Elea

Xenophanes of Colophon (who, after journeying through many countries, came to settle in Elea, in Magna Grecia) was both a poet and a philosopher, and is considered as founder of the Eleatic school. Among the philosophers of this school it is Xenophanes who is worth remembering in a history of biology. To support his thesis of the marine origin of all things and of dry lands having gradually emerged from the seas, he quotes some examples of clearly marine fossils found well inland. Apparently he was the first to give a correct interpretation of these finds, which became the subject of lively debate for centuries.

As late as in the times of Steno and Leibniz scholars considered two alternative hypotheses: a) that these were true remains of animals which had once been alive (it does not matter whether they were marine or terrestrial, even though the commonest fossils in Europe are marine). They had been changed into stone by some local power, which was usually called in the Latin texts a *vis* or *virtus petrefaciens*; or b) if one assumed the possibility of spontaneous generation of organisms from mud, fossils were organisms which had not succeeded in completing their development and had thus remained in a mineral state.

Nothing significant for biology was said by the other Eleatic philosophers, whose chief contributions are in the field of logic.

Other philosophers and scientists

Some other thinkers of relevance, who cannot be grouped under any school's label, deserve some attention.

The earliest is Alcmeon of Croton, who is usually quoted in histories of medicine and of biology and is generally labelled a Pythagorean, because he was a Crotoniate and lived at approximately the time when Pythagoras was active in Croton. Actually we know very little of him. There is no doubt that, as a physician, he was among the earliest students of many anatomical and biological problems, but, although we know which they were, we do not know what he actually thought of them.

Another extremely important philosopher was Heraclitus of Ephesus (born c. 540 BC). He was famous as the advocate of general and perpetual motion and change. His 'Arché' was fire. It seems that he must also have written on problems of biology, but nothing survives of these writings.

We are, however, reasonably well informed on the biological opinions of Empedocles of Agrigentum.

Empedocles maintains the reality of change against the Eleatic philosophers who hold that change is basically an illusion. He also thinks that there are just four roots of things ('stoicheia'): earth, water, air and fire. There are two basic forces at work in the world: the one which mixes and unites and the one that separates and destroys. Both plants and animals were born from earth in a sort of gradual way: first their various parts originated, later these, by the virtue of 'philia' (this is commonly translated as 'love', but its proper translation is rather 'friendship, concord, uniting power') joined between themselves at random. The result was there appeared a multitude of different individuals, many of them monstrous: Most of these individuals were incapable of surviving and vanished, only those which happened to have a well balanced structure could survive, reproduce and now their progeny prospers.

Just as with Anaximander, some scholars claimed that we have here an embryonic evolutionary theory, including the survival of the fittest. Now a true evolutionary concept is impossible for Empedocles, who believed in a series of cycles repeating themselves, where Philia first prevails until the perfectly homogeneous 'Sphairos', the sphere of unity, is achieved. At this point Neikos (Neikos is hostility, quarrel, opposition) gradually gets the upper hand, until everything is again plunged into complete Chaos and from this a new cycle begins. Whether these ideas of Empedocles had any influence on Charles Lyell's early theories on geological cycles (see chapter X) is debatable.

We do not know much about the anatomical and naturalistic knowledge of Empedocles, but all sources agree in stressing his great interest in the study of living beings.

According to later quotations from his writings he maintained that respiration took place not only through the lungs, but also through the pores of the skin. He argued that during embryonic development the foetus receives some of its parts from the male sperm and other from female's sperm and the two unite as the two parts of a broken ring; growth in young animals depends on the increase in bodily heat, while the weakness of old people stems from on their low temperature. Empedocles maintains that sensations depend on extremely minute particles which become detached from the object and must join with the same kind of particles contained in sensors; he maintains, that each minute particle of in the image that travels from the perceived object to the observer must be perceived by the corresponding particle occurring in the sense organ of the observer; consequently the earthly part of the perceived object is sensed by the earthly parts of the sensory organs of the percipient, the fiery by the fiery parts etc. (this last interpretation is however doubtful, if we rely on a sentence on the nature and functioning of the eye, which is quoted by both Plato and Theophrastus as being by Empedocles).

Heraclitus also believed that thought is a function of the body and that it is located in the blood, as this is the part of the body which is richest in all the different elements.

It is clear that Empedocles' ideas were pure guesses, but though guesses, they testify to a genuine interest in the mechanisms of life. I must, however, stress that many modern historians of philosophy have falsified the true attitude of Empedocles, describing him as a materialist. We have enough of his fragments concerning the Gods to show that while, in true Greek style, Empedocles considered them as parts of the Cosmos, nevertheless he mentions them with veneration, and especially Aphrodite, who, rather than Philia, is often recalled as the cause of union and harmony.

Traditionally the last philosopher of the Ionic trend was Anaxagoras of Clazomenae (c. 500-428 BC), who lived and worked mainly in Athens in close association with Pericles. It appears that the political enemies of Pericles charged him of impiety just because of their friendship in the same political campaign which saw Pheidias charged with theft. To avoid prosecution at a delicate political moment, Anaxagoras fled Athens and went to Lampsacus, where he died shortly afterwards. It is said that when he was dying the town's magistrates asked him how they could best honour his memory, and he replied that he desired that on the anniversaries of his death, schoolchildren should get a holiday, so that they could joyfully remember him.

Anaxagoras was undoubtedly a true naturalist in the widest sense. Thus he extended the ideas of the Milesians in astronomy, and maintained that the Sun was a burning stone larger than the Peloponnesus and that it was further away than the Moon, but nearer than the stars. He held that meteorites were fragments of celestial bodies which had been detached from their originating planetary body by some earthquakes and that light was generated during their flight by the heat of the vortex of air they were crossing (an idea probably suggested by the familiar heating of a wheel or disc rotating on a spike).

Anaxagoras developed some ideas concerning biology in order to answer to some of the logical difficulties which had been raised by the Eleatics. Anaxagoras assumed that, instead there being just one or a few 'Arché', substances were infinite and immutable. However they were composed by an infinite number of infinitely small particles, which he called 'sperms' (literally 'seeds'). The visible changes in things were simply due to the disaggregation and re-aggregation of sperms. So, for instance, when we eat, our organism chooses among all the innumerable sperms which are in the food, in Anaxagoras' example bread, the sperms of meat, of hairs or of bones and assimilated them in their proper place. To us the interest of this hypothesis is double: on one hand it introduces for the first time the idea of a particulate universe, which was later developed by the atomist Democritus of Abdera, on the other it is a first approach to the concept of Homoiomery, which was developed by Aristotle and which brought the Stagirite pretty close to the concept of tissue, such as was envisaged by later biologists between the 18th and the 19th century (see chapter X).

Another important step made by Anaxagoras, developing previous ideas, was his concept of the Nous as a principle of movement provided with a natural rationality and that, as it occurs everywhere in the cosmos, explains its natural order. This last concept gained him the nickname 'Nous', and is the original core of the concept of Universal Pneuma of the Stoics.

As for the other biological views of Anaxagoras which are quoted in our sources, there is little that is new: he follows the common opinion that all living beings originated from mud which had been fertilised by appropriate sperms coming from the air or from the ether.

The atomists

As we have seen, to say that the Greek philosophers were sanguine in suggesting their explanations of the basic natural history problems and on the past and future story of the Cosmos is certainly, by modern standards, a blatant understatement. Indeed they had absolutely no way of verifying their ideas. However, great is our debt to their unflagging optimism, as the credit they won in the minds of later scholars led these to reinvestigate with much more adequate techniques their daring hypotheses and find that a number of them happened to have almost hit the target. Among such pioneer theories the atomic one of Leucippus and Democritus is unquestionably among the most historically significant.

Leucippus may be practically dismissed as we do not know anything of him except that he was the master and inspirer of Democritus. Unfortunately exceedingly little survives also of the vast production of Democritus of Abdera (c. 460-360 BC), but both his critics, his Epicurean admirers and the doxographers (= writers who collected and recorded the opinions of the ancient Greek philosophers) of Roman and Byzantine times relate most of his ideas, except, unfortunately, for those on biology.

By tradition he was a pupil both of Leucippus and of Anaxagoras.

Democritus held that the universe was made of atoms and vacuum. Atoms are extremely small, but yet they have a precise size and shape and the number of these shapes is limited. It seems that he conceived this number as corresponding to that of the Pythagorean solids and the sphere or, rather, of the flat figures which made up these solids. So they came to correspond in number to the traditional 'stoicheia'. If he conceived of flat atoms, than the various kinds of matter would result by their assemblage into regular and irregular solids. Just as Anaxagoras with his spermata, in order to meet the requirements of Eleatic logic, Democritus assumed that the atoms were unchangeable, eternal and indivisible and the substance forming each one of them is homogeneous in that it is the basic 'undefined matter', only their shape and size are different. They move spontaneously at random in an infinite vacuum (an idea probably suggested by the sight of the fine dust particles dancing in a sun's ray). Matter is neither created nor can it be destroyed and nothing exists but atoms. All properties and changes in visible things depend on the movements and chance aggregations of various kinds of atoms.

In addition actions at a distance, like the influx of the lodestone, are due to atoms and our sensations are also due to them, Soul itself is made up of round and smooth atoms, like those of fire.

The Democritean cosmos is both rigidly mechanistic and stochastic, and it even forecasts the continuous formation and disintegration of other worlds. Most subsequent philosophers, first and foremost Plato (who wished the total destruction of Democritus' writings; which did in fact occur probably as a result of the merging of Christian and Neoplatonic trends in late antiquity) hated Democritus. However Aristotle, though basically dissenting from Democritus, had a great respect for the Abderite.

For anti-Democritean philosophers, who were in the majority until the 18th century, the cosmos appeared as a basically harmonious construction, where everything had a precise meaning and purpose, something which looked incredible, should the world have a basically stochastic origin. Anyway, just in the field of biology, well into our century, scholars like Rosa, Father Teilhard de Chardin, etc., advocated 'programmed' models of evolution and even the evolution by regular dichotomies of Willy Hennig is basically anti-Democritean.

Our sources all state that Democritus paid much attention to the study of human and animal nature, but we only know in some detail, as related by doxographers, his theory of human cultural evolution, which is very well argued and factual. Otherwise it seems that he maintained that also the smallest of living beings must have had a well-developed structure and organs, though not visible to the human eye; he paid attention to embryological development and to the problem of the sterility of mules. Democritus maintained that the brain was the seat of thought (Aristotle, instead thought that it was an organ which function was to cool the blood). Finally he may have been the first to suggest the division of animals into 'Enaima' (with blood, vertebrates) and 'Anaima' (without blood, invertebrates), and to argue that all animals were capable of some reasoning.

The Sophists

In every observation or experience there is a subjective factor. This was one reason why the Eleatic philosophers denied all possible change in the 'Being' and maintained that all such change as we experience was deceitful. Anaxagoras and Democritus were very clear about it as they distinguished between the essential qualities of their sperms or atoms and of their aggregations on one side, and our perceptions, which, so to say, 'read' them as colours, smells and so on.

This problem was central to the Sophist's school. They posed as the cornerstone of their theories that the individual man is the measure of all things, and concluded that there is no absolute truth, but only the individual's truth and, therefore, that real knowledge of absolute truth is impossible. Protagoras of Abdera (485-415 BC), a compatriot and contemporary of Democritus, was the first and foremost advocate of this thesis.

While Sophists were basically concerned with pure logic and gave no contribution to empirical sciences like biology, they were feared by people like Socrates, who thought the relativism and subjectivism of the Sophists a danger for morality.

Many later naturalists, including a number of present day scientists, maintain that that relativism and emphasis on the subjective side of knowledge either implicitly or explicitly denies the possibility of a science of nature. This position was typical of positivist philosophers and of not a few idealists some fifty or a hundred years ago, but it is still debated by philosophers of science.

Socrates and Plato

While Socrates (470-399 BC) may solely be mentioned as the master who outlined the philosophical principles which were fully developed by Plato (428-347 BC), the latter, though he was not interested in biology as such and gave no positive contribution to it, had such a pervasive influence also on biologists during the following centuries, even though his teachings were distorted almost beyond recognition, so that we must give him some attention.

Plato was mainly interested in purely rational approaches to problems, rather than in empirical observations, but he was sure that philosophy was a single, coherent system covering at one time mankind and all its problems as well as all natural phenomena.

Moreover the Academy, Plato's school, was the environment where Aristotle developed his genius, and to him we owe the tradition of the prominent place that the study of nature must have in philosophy.

Almost all Plato's ideas in the field of natural history are expounded in the Timaeus, possibly the worst of his 'dialogues'. This is extremely long and tedious, but, nonetheless, had a great importance, as its Latin translation by Calcidius was the only Platonic dialogue known in the West during early medieval times.

This dialogue, also because of the continuous influence of Neoplatonic tradition on scientists until the 17th century, had a far greater influence than it deserves.

All in all, if we consider biology properly, Plato could effectively be disregarded, as he never made any observation on animals and plants and barely mentions biological problems. However, since the idealistic approach of Plato had a great influence on the subsequent development of biology and more generally on the sciences and caused a considerable change in outlook on its problems as well.

Plato, in order to refute the Sophists, takes his start from their gnoseologic doubts and their taking man as the yardstick by which all things shall be measured; but he then created an anthropocentric system where the paramount values are spiritual ones. If we limit ourselves to natural sciences, and we ignore his ethics and his theories of knowledge, Plato's anthropocentric philosophy had a damaging influence on the development of sciences, though neither he nor Socrates, in true Greek fashion, ever supposed that the universe had been created for the benefit of mankind, as was believed by not a few thinkers of monotheistic faith.

Also the Platonic concept of 'Eidos', which is that the Archetype of anything, its idea, pre-exists to the thing itself played a negative role in sciences, in spite of the prompt criticism by Aristotle.

On the whole it is difficult to estimate the precise influence of Platonism on biology which was, nevertheless, considerable. In a sense, even if it may look like a paradox, Plato might be considered as the founder of systematics or, at least of that type of systematics where the concept of 'archetype' is more or less presumed in the formal description of a taxon. Linnean systematics are often quoted as an example of this type of systematics, but, as we shall see, this is a complete misunderstanding of Linnaeus' ideas.

Plato maintains that a horse, for instance, meaning any particular horse we see, is just a more or less accurate material expression of an ideal 'horse' which exists and is, in itself perfect and eternal. Therefore the naturalist should, according Plato, strive to understand and know that pre-existing and eternal idea or form of horse by studying as many individual horses as he can (and Chrysippus commented: "Oh, Plato, I can see the horses, but not the horseness!"

There is no question that Plato succeeded admirably in showing the imperfection of the knowledge that we can get from our sensations. But from that he derived a gratuitous corollary: that true knowledge can be reached only by pure reasoning. He therefore gave the naturalist the task of acquiring, starting from observable things, the knowledge of ideas and of laws, which are both unchangeable and eternal. This little devil lingered in the practice of biology and is at the root of what is erroneously called the typological concept of taxa, which should more correctly be termed 'the idealistic concept'.

Some general remarks

In the next chapters we shall see how the balance between observation and theoretical developments evolved through classical times, but we must here point to a technical problem and to its consequences. We shall see that after Aristotle, while sciences such as mathematics and astronomy with the highest rational content and less need for detailed empirical observations made considerable progress, the natural sciences had an increasing tendency to become subservient to medical 'praxis'.

The lack of optical instruments in classical times had a paralysing effect on biology. As a matter of fact the Romans had some knowledge of lenses and of their properties; but magnifying instruments were not employed until spectacles came into use in the 13th century and the first to use lenses as an aid to biological observation was apparently Gesner, well into the 16th century.

It is equally true that where the Greek astronomers made their worst mistakes, this was not the result of a faulty method, but a consequence of wrong measurements due to the lack of sufficiently accurate instruments. When these became available, the revision and refutation of old theories was immediate.

Early Greek medicine

The close connections between biology and medicine through all the period covered by this book, compel us to sometimes consider studies and events that, though more significant to the student of the history of medicine, can not be conveniently ignored by the student of the history of biology.

We must, therefore, pay some attention to the early development of Greek medical science.

If we turn again to the Homeric texts, we find that, although in the 8th century BC, both health and disease (and particularly epidemics) were bestowed by the Gods, there already was a tradition of medical practitioners independent, to some extent, from temples and religious ceremonies. Though the Gods might give some help in aiming the spear or the arrow, this was accessory to a man-made wound which men could cure by merely practical means. So Asklepios (in Latin Aesculapius) in the Homeric poems is still a mere hero and a Thessalian prince, as purely human as are his sons Podalirius and Machaon, both renowned surgeons. It was much later that Asklepios became a God and the son of Apollo. His cult was introduced in Athens only in 429 BC and in Rome in 292 BC.

We owe the traditional emblem of apothecaries to the cult of Asklepios, the snake coiled around a staff. The snake was his sacred animal, and occasionally was even considered his epiphany, that is his earthly manifestation (the use of two snakes coiling around the winged caduceus, presently often used as a symbol with the same meaning is a gross mistake, as this is the symbol of Hermes in his function of 'psychopompus', the guide of souls to the underworld!).

Around the cult of Asklepios grew many sanctuaries, and some of them became famous as healing places. There, as we know from a variety of documents, including votary gifts and tablets relating cures and healings, both religious practices and medical care were administered and, though we have no evidence of a direct connection between the two, some medical schools existed within the precints of some of the most important sanctuaries. Moreover, several famous families of physicians are known to have been known as Asklepiads, that is descendants of Asklepios. Both Hippocrates of Cos and Aristotle were Asklepiads.

Beside the religious, moral, psychological and medical cures that were practised at the sanctuaries, the Askepieia, we know that in Greece there were a number of lay physicians; these were free men who, in order to get a licence to open a consulting room (called 'iatreia') had to prove that they had followed the teachings of a qualified physician for some years. We also have some records of medical officers who derived a regular salary from the community. There were also wandering medical practitioners, the 'periodeutae', but they were commonly held to be hardly better than quack-doctors, though they often practised 'Lithotomy', that is the removal of bladder stones, a kind of surgery which the celebrated 'Hippocratic Oath' forbids to medical practitioners, as there was a real danger of damaging the spermatic ducts, and thus causing sterility.

Among the most celebrated early medical schools was that of Croton, whose most famous master was Alcmeon of Croton, whom we have already mentioned, and who is quoted as the first to dissect the human corpse, or, at least, some part of it. His book *On Nature* is lost, but some scholars consider it likely that some of the earliest texts of the *Hippocratic corpus* may actually derive from his teachings. Tradition credits Alcmeon with the statement that the brain and not the blood was the seat of mind and with the distinction between veins, which are full of blood, and arteries, which contain air; finally he may have maintained that when the blood concentrated in the heart, sleep would begin and that death had a somewhat similar mechanism. Some other anatomical discoveries are credited to Alcmeon by extremely dubious traditions.

Again to Alcmeon is credited the idea, of Pythagorean origin, that health depends on the correct balance of all the substances in the body, so that the medical practitioner must aim, in order to heal, to rebuild that balance, which is upset in diseased conditions.

To the school of Alcmeon belonged Philolaus of Taras, who lived in the 5th century BC. He seems to have restricted the concept of disease to some inbalance of the four basic humors of the body (thus pioneering one basic concept of the Hippocratic school) and that the balance was ordinarily kept by the soul.

Similar concepts in pathology were advocated by Empedocles of Agrigentum. There are also stories about this philosopher which describe him as fighting epidemics by reclamation of marshes and public fumigations, but as with most of the stories concerning Empedocles, they are probably groundless.

Other famous schools of medicine of the 5th century BC were at Cyrene, Rhodss, Cnidus and Cos. The oldest was probably the North African school of Cyrene, and we know the names of some of the Cnidan masters, but, by far the most important school is that of the island of Cos.

The fame of Cos is linked with that of its most renowned master: Hippocrates, son of Heraclides (c.460-c.375 BC). We do not know which connections existed between the famous temple of Aesculapius in Coos and its medical school, but it is at least certain that the great number of pilgrims who visited the sanctuary to seek healing must have offered ample opportunity for observations, and the fact that Hippocrates belonged to the sacred Asclepiad family and that his father was a physician gave this most remarkable man the best opportunities. A large *corpus* of some 70 treatises credited to Hippocrates have survived. They are very different in nature and style and, as the earliest commentators of early Alexandrian age knew, only of some thirty, there is a good chance that only about one third of them really belongs to Hippocrates, some being earlier (probably including the famous *Oath*), while others were later. However, it seems that the Hippocratic corpus was consolidated by the end of the century following Hippocrates' death, when the various writings were collected and to some extent edited to be copied for the Library of Alexandria. Ancient scholars were well aware of the fact that not all 'Hippocratic' treatises were genuine and tried to sort them out. It is a great pity that the work that Galen dedicated to this problem is lost (it survived into the 8th century AD as we know that it was translated first in Syriac and from that into Arabic. Neither of these translations has, so far, been recovered).

While all students of the Hippocratic corpus, both ancient and modern, have hailed it as an invaluable source of information, wise advice and sound practice, we shall only consider such items in it that concern biology, and exclude both diagnostics and medical treatments.

We must first praise, as everyone did, the emphasis there is through the books both on accurate observation and exact reporting. The corpus is notable also for the factual analysis of observed symptoms and for the fact that it does not concern itself with any magical or religious practice, though we know from other sources that these were both familiar to sick Greeks.

Hippocratic pathology and, by consequence, therapeutics, were based on the theory of complexions, that is of the kind of basic humour that was assumed to predominate in the patient's complexion. Thus we owe to Hippocratic medicine terms familiar to everyone, such as the Sanguine, Phlegmatic, Melancholic and Bilious complexions, just as for a number of medical terms such as 'crasis', 'discrasia', 'crisis', 'prognosis', etc.

As for the scientific knowledge of the Hippocratic school, it was not great. They had some knowledge of the anatomy of bones, but their anatomy was still rudimentary: nerves, vessels and tendons are not clearly distinguished; both the trachea and the bronchi were called arteries and likewise, true arteries were considered to be pneumatic vessels and air was supposed to pass from the bronchi to the heart by an arterial vessel and there, mixing with blood, it created heat, which was the cause of life. They also supposed that from the left side of the heart, where the blood was heated, thus acquiring its vital powers, blood reached the liver. As far as reproduction was concerned, while the Hippocratics considered the uterus of women to be bicornuate like that of many mammals, they thought, alternatively, either that sex was determined by the development of the embryo either in the right or in the left horn of the uterus, or that it depended on sperm coming either from the right or from the left testicle. They also thought that the embryo derived from the union of both parents' sperms (as they considered vaginal and vulvar secretions to be a sort of feminine sperm). Male sperm accumulated in the testicles, but, as maintained, for instance, by Anaxagoras, it was made of innumerable, infinitesimal particles coming from the various parts of the body.

Indeed, while, the therapeutic practices of the Hippocratic school were sound, their observational principles good and their ideal of an empirical medicine equally good, they contributed little to the advancement of biology. However, just because of their principles, they are at the root of that splendid age of biology that opens with Aristotle and practically closes with Galen in the 2nd century AD. Naturally medical practice did not get frozen with Galen, and we shall see, throughout the late classical times and even in the early medieval times, here and there new techniques were perfected, new drugs were added to the existing lists, some new knowledge was added. However a truly scientific approach to life studies had to wait exactly 1,000 years before truly scientific enquiries in life sciences were resumed, such being the span separating Galen from Saint Albert the Great (Albertus Magnus).

CHAPTER II

Aristotle and Hellenistic biology

SYNOPSIS OF MAIN HISTORICAL EVENTS AND OF CONTEMPORARY SCHOLARS

359-336 BC Philip II rules Macedonia.
338 BC battle of Chaironeia and Macedonian overlordship on Greece.
Aristotle 384-322 BC in 335 begins his teaching in the Lyceum, Heraclides Ponticus c. 350-300 BC, Theophrastus c. 380-326 BC
336-323 BC between 334 and 323 Alexander the Great conquers the Persian Empire.
c. 310 BC Ptolemey I Soter begins the building of the Museum of Alexandria.
Zeno the stoic c. 310 BC, Epicurus c. 300 BC; Herophilos c. 290 BC ; Aristarchos of Samos c. 280 BC, Euclides c. 280 BC, Erasitratos c. 275 BC, Apollonius of Pergamus c. 260-200 BC, Strato c. 287 BC, Archimedes 287-212 BC, Eratosthenes 273-192 BC
264-210 BC first and second Punic wars, Rome becomes the foremost Mediterranean power.
197 BC the Roman victory in the battle of the Cynocephalae, ends the Macedonian supremacy in Greece.
190 BC Roman victory at Magnesia, Rome gains supremacy in Asia and the Seleucid kingdom begins to disintegrate.
168 BC Rome annexes Macedonia after the battle of Pydna.
146 BC destruction of Corinth and end of the anti-Roman revolt led by the Corinthian league, Rome annexes Greece, but leaves intact local self-government in the main towns.
Ipparchos 180-100 BC, Seleucus of Babylon c. 150 BC, Phylo of Byzantium sometimes between 150 BC and 150 AD, Hero of Alexandria 1st century BC

The zenith of Greek biology

ARISTOTLE

When we meet Aristotle we may properly speak for the first time of a true science of biology. We have seen that since the very beginning of Greek speculation, several thinkers had considered all aspects of physical world and some had actually titled their writings ‘perì physeos’. We should also remember that for most of them and for Greeks in general the understanding of Nature had a strong religious significance (as shown, for instance by the Orphic hymn to Nature).

While Greek scholarship had made great advances in mathematics and especially in geometry, and had provided elaborate calculations and advanced theories in astronomy and cosmology, the situation was entirely different in the other branches of science. Especially in the field of 'natural sciences' daring hypotheses had been advanced, including that of the possible mathematisation of the whole universe and the geometric-atomic hypothesis of Democritus, but all these hypotheses, being untestable, were suggested rather as reasonable guesses, than as what we would call scientific theories.

Most of the pre-Socratic philosophers, and indeed of the later thinkers, were concerned with 'saving phenomena', that is to provide a coherent logical framework within which all known facts could be framed. Even the Sophists, Socrates (to a very limited extent) and Plato, though mainly concerned with man as an individual and as a citizen, felt the need to complete their teachings by general hypotheses on the nature of the world and on its laws.

Thus a rich set of ideas had been born, which needed sorting and verifying. This was largely the responsibility of Aristotle.

Aristotle was born in 384 BC in Stageira, a small town almost on the border of Greece proper, at least his contemporaries would have thought of it as such. He was the son of a well-known physician who often practised at the Macedonian court. Some sources say that Aristotle was an Asklepiad, that is that he belonged to an acknowledged dynasty of physicians. It seems highly probable that Aristotle learnt from his father the Hippocratic tradition of careful and methodical observation of facts.

When eighteen years old Aristotle entered the Academy, where he remained until the death of Plato, a master who certainly for some time had a great influence on him. When Plato died he bequeathed the authority of 'Scholarch', the headmaster, for his sister's son Speusippos, an interesting and remarkable philosopher by his own right, at least judging from the few fragments of his that survive. Aristotle, possibly also taking into account the latest political events left for Assos, in Asia minor, with some other pupils. He had probably been invited there by Hermias, lord of Atarnaeus, a eunuch who had himself been a pupil of Plato some time before.

It was apparently during his stay in Assos, from where he probably paid several visits to neighbouring islands, that Aristotle made most of his remarkable studies on marine animals.

Aristotle married a niece of Hermias, and when Hermias was killed by order of the Persian king, he first fled to Mytilene and then to the court of Philip II of Macedonia, where he was appointed tutor to Alexander (III or 'the Great').

Although Alexander did not seem to heed his master as far as politics were concerned (Aristotle was a moderate conservative and a supporter of moderate democracy), he apparently had a deep feeling for his master.

In politics Alexander was thoroughly imbued with the dreams of his mother Olympias, a Molossid princess who claimed descent from Achilles. While for his father the war against Persia was politically expedient, to Alexander it was a mission: to unite the civilised world into a Koiné patterned by Greek culture.

The considerable number of 'scientists' that Alexander included in is staff and charged with the collection of all sorts of information concerning the Barbarians and their lands, is certainly the result of Aristotle's teachings; moreover some ancient authors say that Alexander kept Aristotle well supplied with money, specimens and information.

When Alexander began to have an active role in his father's government and shortly after became king, Aristotle returned to Athens (around 334 BC), where he developed his teaching in the Lyceum (though the formal establishment of the peripatetic 'school' was the work of Theophrastos, who formally instituted it four years after the death of the master). Like other Greek 'schools', the 'Lyceum' was not a school in the modern sense. It was rather both a brotherhood and a research centre, where, under the guidance of the 'scholarch', the pupils developed their own researches and personality within what we can call a 'study group'.

When news of the death of Alexander reached Athens in 323 BC (we should always remember that for the good Athenians the Macedonians were Barbarians), Aristotle, as a leading figure in the pro-Macedonian party, felt insecure and left for Euboea, where he died shortly afterwards (322) at the age of 63.

Aristotle was a prolific writer and the subsequent story of his writings is rather curious. During his junior years at the Academy he had written poems and dialogues on the platonic model, but as far as we can judge from the few surviving fragments, rather independent of the beliefs of his master. Of Aristotle's poems we have just a short one *in memoriam* of Hermias and a few other lines.

In Aristotle's times the equivalent of modern 'publication' was the final copying of the text and making it available for public reading. The cost of books was such that people usually did not read them themselves, but assembled in small groups in private houses where someone (usually a slave) read the book to the audience.

The 'finished' writings of Aristotle have practically all been lost, but we still retain their titles. As most, if not all of them, were still available in the 5th century AD to scholars such as Simplicius, I suspect that they ceased to be copied under pressure from religious preoccupations. Besides the lost 'finished' books, either at his death, or at the time of his hurried departure from Athens, a large number of Aristotle's writings were left to his pupil and friend Theophrastos (whom we shall consider further on for his botanical works), who was the first official scholarch of the Lyceum.

Theophrastos, who had somehow also acquired the library of Speusippus, at his deathbequeathed all his books to his own nephew Nelaeus of Scepsis, rather than to the next scholarch, and so the Aristotle books went to Asia. Either Nelaeus himself or his heirs sold some of the manuscripts to Ptolemy II Philadelphos for the Library of Alexandria, and hid the rest.

Many years afterwards the peripatetic Apellicon of Teos purchased this remainder and brought it back to Athens. But Apellicon, besides being a rich and passionate aristotelic, was one of the leaders of the party that in Athens favoured Mithridates VI

Eupator of Pontus against the Romans; and when Sulla, during his campaign against Mithridates, conquered Athens (84 BC), Apellicon was killed (or he may have died shortly before), and Sulla, as an intelligent and cultivated man, among his share of the plunder, took the library of Apellicon. Thus the manuscripts ended in Rome. Shortly before that, other Aristotelean books may well have reached Rome through Rhodes, where we know that there was an active Aristotelean school, by the agency of Lucullus, another brilliant Roman general and a cultivated man.

Around 72 BC all the manuscripts were entrusted for editing to a former prisoner of Lucullus, the grammarian Tyrannion. However, his work did not proceed further than to the production of a catalogue of the manuscripts, although, as his friend Cicero relates, Tyrannion made them known among the Roman elite, which included Pomponius Atticus, who was the protector and 'editor' of all the great Roman writers of the Augustan period.

It thus happened that, apart from the *Athenian constitution* (a book which was part of a comparative study of the constitutional history of 158 Greek towns and which came to us through a papyrus discovered in 1891 at the British Museum), what we have is the result of the work of Andronicus of Rhodes who, around 50 BC undertook the reordering and 'editing' of all the material.

Actually he grouped the various texts so as to make them in some fashion into organic treatises. Although they are all in Greek, as they were edited in Rome, they were then generally known (and are familiar to us now) by their Latin titles.

It is certain that not all of Aristotle's biological work survived. We know, *e.g.*, of a treatise of his on plants (see further on) and the Stagyrite himself refers in his surviving works to a *Zoika* and to an illustrated *Anatomai*, which have been lost as such, though we indirectly know part of their contents.

There are also several works of biological content that were traditionally included in the Aristotelean *corpus* and which have been dismissed by critics as either entirely spurious or, at least, not Aristotelean in their present form.

Aristotle mentions in his writings on natural history that he had prepared drawings or diagrams, apparently to clarify his lecture notes. These diagrams are lost and so we may also have lost some of his general conclusions.

In order to understand really Aristotle's biological works, it is necessary to summarise the philosophical-logical framework of all his scientific work.

We may consider that Aristotle posed the problem of the essential character of the logical problem in the *De interpretatione*. To this premise there follows, in the *Analytici I*, the analysis of the syllogistic argument and the clarification of the causal relations that are the premise of any demonstration. After that, in the *Analytici II*, he studies the demonstration and the conclusion. Finally, as there are statements which cannot be shown to be either absolutely true or absolutely false, he examines in the *Topici* the judgements of probability.

To sum up the whole discussion, Aristoteles holds that science has some precise limits: as far as its 'first principles', that science can not prove its very basic principles, but, that by clearly identifying them, it can falsify deceptive probabilities and thus identify the real difficulties or 'Apories'.

The terms of logical discourse are propositions and these can be either syllogistic, that is that they can be proved or falsified by this type of logical procedure, or non-syllogistic, which cannot be either proved or disproved by syllogistic analysis. Of these last, he does not provide a full discussion, but lists over a hundred examples (these had the greatest importance both in the development of medieval logic and of modern modal logic).

Within a proposition Aristotle distinguishes a subject and the 'predicates' of the proposition.

The 'categories', which may be identified with the predicates, are that which can be said (predicated) of a subject. Thus, of a given animal you can say (= predicate) that it is a mammal. The 'categories' are the object of a special treatise of the same name, which had the greatest significance in the general development of human thought. The categories are: substance or essence (for instance man, horse)[1], quantity, quality, relationship (*e.g.*, double, half. etc.), where, quantity, quality, relationship (*e.g.*, double, half, etc.), where, when, position (*e.g.*, standing), situation (*e.g.*, booted, loved), action and passion (this in the sense of being subject to an action, *e.g.* to cut is an action, to be cut is a passion).

The subject to which categories apply, that is the 'bearer' of the attributes, is the substance. Moreover Aristotle identifies quite clearly a critical point even in today's debates and this is a point where he has often been misunderstood: Aristotle says that species and genus are secondary substances, while the primary substance is the essence and the essence may well be entirely fictitious (or imaginary).

It is necessary here to point out that, unless it was discussed in his lost writings, both Aristotelean physics and metaphysics deal with transcendent problems in a strictly rational way, and it is not at all clear whether, apart from 'God' who thinks himself, and 'Nomos', a natural law which rules Nature's matters in the best possible way, there is a room in his world for the Olympian Gods.

The fact that a good deal of Aristotle's writings vanished shortly after the closing of the Academy by Justinian I on religious grounds, as he thought the Academy to be a dangerous stronghold of Paganism, suggests that such a possibility was there, at least in the views of later scholars.

As we shall see, during Roman times, philosophers spent a good deal of ingenuity in an effort to reach a synthesis of the main philosophical schools and paid much attention to Aristotle (so much that the late antiquity handled down to medieval thinkers some spurious Aristotelean treatises with a strong Platonic or neoplatonic tinge, that our western scholars took at face value until well into the early renaissance) and the loss of certain Aristotelean writings may well have been planned.

[1] Substance was however defined as a special kind of category, see appendix to Chapter IV.

Coming back to Aristotle's scientific works, I shall not consider his works in mathematics, physics and astronomy, as they are usually adequately dealt with in high-school textbooks (I want, however, to recall that his considerations on the infinite and on potential infinitesimals opened the road to the calculus of Archimedes and of Leibniz, and equally important is his discussion on the possibility of non-Euclidean geometries). Nevertheless there are some general points worth mentioning.

The first thing to notice is that, though Aristotle was the first to deal systematically with logic and of logic as the foundation of true knowledge, he not only developed syllogistic logics, but was well aware of the need of instruments to deal with non-syllogistic statements, which he did not have the time to study; moreover, he always stressed the need for empirical observations. his observations on anatomy, physiology and behaviour of animals unquestionably prove him to have been a first class observer and a scholar of literary sources, the more remarkable if we consider that, as he says himself, he had no examples to follow, nor had he magnifying glasses or any other optical instrument.

A second point: he is constantly preoccupied with the 'utility' of the structures or of the behaviours that he describes and holds that everything exists for a purpose (the so-called, and criticised, 'teleological view' of the phenomena). However, he separates that which happens 'by necessity' or, better, 'that must be', from that which is merely useful (and we shall see that his pupil and successor Theophrastus raised serious objections to this interpretation. It is to the credit of Aristotle that he chose as a friend and successor a man who was critical of his ideas, but it must be said in fairness that also a dogmatic character such as Plato, choose for his successor his nephew Speusippos, a man who did not see eye to eye with him).

Finally Aristotle tried to frame all the basic phenomena within general physical theories (unfortunately mostly wrong). More precisely he tried to explain also biological phenomena in the framework of the following theories of his: (i) theory of motions, (ii) theory of substance, (iii) theory of rationality. Of this last we shall have to say something later, on the first two let us consider their basic tenets.

Concerning movement, Aristotle distinguishes two basic kinds of motions: natural and violent. Natural motions are those that the objects possess spontaneously in order to get to their 'natural place', that is the natural place of their main constituent elements. So as fire and air are 'light', they tend to move upwards and objects mainly made of air or fire, such as smoke, move naturally upwards. Vice versa, earth and water being heavy move downwards and so will move basically watery or earthly items. 'Violent motions' are such motions that are imposed on an object by an external agent. However, on theoretical considerations Aristotle discusses the possibility of vacuum or 'emptiness' and, notably, he remarks that if vacuum existed, objects moving in vacuum would have a tendency to move forever, a remarkable anticipation of the principle of inertia which, as we shall see also on other matters, brought Aristotle very close to some of the greatest achievements of science, and that he refused for curious verbal illusions: 'Emptiness' he says, is 'nothing' and 'Nothing' can not be an existing thing. But, having denied the existence of a vacuum and not having the concept of energy, Aristotle thinks that, in order to continue moving, an object moved by a violent motion, it must continue to get pushed by it throughout its trajectory, otherwise it would instantly stop. Moreover to Aristotle any change in shape, including growth, is a kind of motion.

As for matter, his theory is both complex and confused. Matter consists in the traditional four 'elements' (Stoicheia): Earth, Water, Air and Fire, but there is a fifth element which characterises the celestial spheres (which spontaneously move in rational movements), and is the same as the 'forces' which keep the living beings alive and moving. So Aristotle calls as 'primary substances' these elements, and distinguishes them from 'secondary substances', the two pairs of basic qualities: heat and cold, dryness and humidity. It is the various mixtures of the 'secondary substances', acting on the undifferentiated Arché, which make it into the four elements, which mixture, in turn, made the real, observable things (and this whole theory was criticised by Theophrastus). It must be remembered that, as Aristotle rejected the atomic model and saw matter as a continuum, he did not conceive of the mixture of the 'secondary substances' or of the 'elements' as a mixing, but rather as a sort of alloy or of an algebraic addition of the various characteristics.

We shall see later how this Aristotelean theory became the basis of much alchemical research on transformation, especially of metals. Indeed, if one assumes that any known object potentially contains something of every substance, it was reasonable to assume that by appropriate treatment one could either enrich it with some qualities or vice versa and thus that one could transform things (which are not substances in the Aristotelean sense). It may also be added that to the Greeks and for many centuries afterwards, such transformations were a daily experience: the production of pottery, of metal alloys, dyeing of objects, fermentations, digestion itself (and on that we shall have much to say) were all examples of such transformations.

It must be added that the concept of 'matter' by Aristotle is complicated as his idea of substance (his 'ousìa') is basically correspondent, in the physical field at least, with the older 'stoicheia', while the 'form' (Eidos, translated also as 'species' and, in Plato, with 'idea') is the actual cause by which the indeterminate substance gets its own characters (attributes).

Given these premises, we may now turn to Aristotle's biological works. Sparse, and sometimes important remarks are scattered in several of his books, and we have a series of short treatises (*Parva Naturalia*) of biological subject, but at least some of them are certainly spurious. The main biological treatises are the *Historia animalium* (10 books), *De partibus animalium*, *De incessu animalium*, *De generatione et corruptione* (5 books), *De anima* (3 books), moreover, as we have already said, we know the titles of other biological treatises now lost. Of his botanical treatise in 7 books only a few quotations survive, but we know that he was the first to compare a plant with an animal with its head buried in the soil and which roots were functionally equivalent to mouths.

As a whole, in his works, Aristotle deals with some 540 kinds of animals, and he assembled much evidence on their aspect and structure. Aristotle must have been indebted for many such evidence to some older and lost works (probably including those by Democritus), or have relied on second-hand information by supposedly reliable informers, but most of the evidences related were certainly personal observations.

As he constantly recommended following in investigations the still prevailing practice of: (i) to proceed to the statement of the problem, (ii) to quote older literature and discuss it, (iii) to perform and describe personal observations, (iv) to reach conclusions; we may be sure that he must have tried to verify the reliability of his sources whenever possible.

We shall now list, as examples from his gigantic work, some of his most remarkable achievements.

However, a necessary premise to the description of his achievements is the consideration of his interpretation of the animal's structures. Though he is to some extent rather ambiguous, Aristotle does often consider the concepts of analogy and identity. Obviously he could not provide either a clear definition of them or avoid some serious mistakes with this which is the very first discussion of problems that were sufficiently clarified only in the second half of the 19th century on the basis of evolutionary theories. It is, however, surprising how clearly the Stagirite stated the problem. He writes: "Groups that are different only by the type or number of identical features are grouped into one single class, while groups whose attribute are analogous but not identical, must be separated. Thus the different birds differ by the type of their feathers, sometimes long and sometimes short, but all of them have feathers. Fishes and birds, instead, are distant as they have only analogous organs: birds having feathers and fishes scales: these analogies are scarcely useful for the grouping of the animals as almost all of them show analogies in their corresponding parts".

Let us begin with terrestrial Arthropods (Entoma), as they clearly illustrate the range of Aristotle's curiosities concerning even animals apparently devoid of practical interest.

Aristotle defines these animals as follows: "Animals without blood, with more than four legs, some winged, They are neither osseous nor fleshy and their body is rigid both internally and externally", a rather good definition except for this last, rather obscure statement (does it refer to the existence of tentoria?).

Though Aristotle deals rather vaguely with some groups such as butterflies or grasshoppers, he maintained that Insects should be grouped by the characters of their wings and of their mouth-parts, precisely the basic criteria followed since the end of the 1700s and, to some extent, still used.

Aristotle suggests the following alternative groupings:

On the evidence of the wing's structure:

1) Winged (Pterota)
   A) With helitrae (*e.g.* .Beetles, Coleoptera)
   B) Without helitrae
      b1) with four wings (*e.g.* Bees)
      b2) with two wings (*e.g.* Flies)
2) Without wings (Ptilota)

On the evidence of mouth-parts:
1) With teeth, eat everything (*e.g.* Beetles)
2) Without teeth and with a proboscis
   a) consume any liquid (*e.g.* Flies)
   b) consume only blood (*e.g.* Cowflies)
   c) consume only sweet liquids (*e.g.* Bees)

Generally Aristotle describes in detail only the exterior aspect of insects and discusses their metamorphosis. He justifies himself for not doing their anatomy, saying that they are too small to see in detail their anatomy, but even in this field he made some remarkable discoveries.

Unquestionably Aristotle had considerable difficulties with dealing with the developmental stages of insects. So he uses the term 'skolex' both for insect larvae and for worms; he uses 'Kampe' for caterpillars, for the triungulins of the Cantarids, and for the campodeiform larvae of fireflies. 'Chrysalis' is usually the term for pupa, but speaking of the Bombyx which was bred in Cos to produce a kind of silk, Aristotle calls 'Kampe' the first developmental stages, and 'Bombylios' the advanced stages, while the pupa is called 'Nekadylos'. 'Skolex' and 'Nymphe' are respectively the larvae and the pupae of Coleoptera, Diptera and Hymenoptera, while 'Kones' are both the eggs and larvae of lice, fleas and of cockroaches.

Aristotle describes the moults of Arthropods, but completely misunderstands their metamorphosis, and has some important observations on the reproduction, feeding, care of the eggs and of the larvae and on the production of sounds.

Though he made only sparse observations concerning the anatomy of insects, nevertheless he says that the heart is between the head and the abdomen and that some insects have only one heart, while others had many of them, so that, if they are cut into two pieces, they can still live for a while. Surprisingly he maintains that some insects have a trunk, but others (which we would call 'mandibulates') have a similar organ (the labium in our nomenclature), between the teeth; that in Cicadas the mouth and the tongue are fused and that they feed through this organ as through a root. Aristotle says that the gut of insects can be either straight or convoluted and that the big ones have a stomach to the fore of it.

Aristotle thinks that the insects eat little 'because they are cold', adding 'because heat requires and digests food rapidly'. We shall come back on his remarks on insect reproduction, a subject on which he gave very accurate descriptions and a wrong interpretation.

All in all I think that his observations just listed qualify Aristotle as a most acute observer.

Aristotle made a number of studies on marine invertebrates, which is most notable as these animals tend to look as of small interest. Apart from his studies on the anatomy of Sea-urchins, he described the peculiar reproduction of some Cephalopods, where one arm of the male becomes modified into a storage place for semen and works as the copulatory organ, being introduced into the syphon of the female where it may even detach itself. Although it is often credited to him in histories of zoology, his description of the ectocotile tentacle is based on *Octopus vulgaris,* and he, apparently, never examined *Argonauta*, where the ectocotyle arm of the male becomes detached and swims by itself into the female. Aristotle's description was discarded for centuries as fantastic, and generally overlooked, until in 1827 Delle Chiaie, discovered the ectocotyle arm of *Argonauta* attached to a female, and misidentified it as a parasitic worm, describing it as a Nematode, a diagnosis which none other than Georges Cuvier changed into the equally wrong identification as a Trematode and proposed for it the name *Hectocotylus octopodis* (Delle Chiaie had ranged it into the genus *Trichocephalus*). The first doubts occurred independently to Oronzo Gabriele Costa and to Defilippi around 1841, but it was only in 1852 that J. Müller finally showed that Aristotle had been absolutely right.

Still on Cephalopods, we owe to Aristotle the description of the development of the eggs of the Octopus and the Cuttlefish and of the peculiar relationship that the yolk has with the mouth of the embryo.

Aristotle says that usually dogfishes reproduce by eggs, but that there is a species where the embryo is fed inside the mother by a placenta like that of Mammals, which he proceeds to describe. Again this was not believed by later scholars, until Steno made the same observations in 1673; but it was only in 1840 that again J. Müller proved that in the Mediterranean species *Mustelus laevis* and in a few others, the development was precisely as described by Aristotle.

Again, Aristotle gave an accurate description of the Angler-fish and of how it captures its preys, but, because of its poorly ossified skeleton, grouped it with the Sharks.

We shall come back to the studies of Aristotle on reproduction; here we just mention two things as related by him, one correct and one wrong, and consider the reasons that explain he error.

Aristotle describes the reproductive behaviour of the catfish living in the river Achelous, how the male remains at the nest and attacks possible predators of the eggs, and, finally, how fishermen take advantage of this habit. This piece was generally labelled as fantasy, as such behaviour was not known in any other European fish. Then, around 1850 Louis Agassiz described exactly the same behaviour in an American catfish. The absolute reliability of Aristotle's account was thence verified precisely in the Achelous catfish (*Parasilurus aristotelis*) and it is presently known also in other species, such as the Danubian catfish, *Silurus glanis*.

In another passage Aristotle relates how Crocodiles, alone among the quadrupeds, in order to open the mouth, elevates the maxilla instead of lowering the lower jaw. Obviously crocodiles must depress the mandible, just as any other tetrapod and, moreover, as their skull is completely akinetic, the upper jaw can not move even slightly, with respect to the braincase, as it is possible with the upper jaw of vertebrates with a kinetic skull. Nevertheless the statement of Aristotle (who, for that and a few other items has been charged as being quite gullible by phantastic tales), is fully justified as (i) the braincase of crocodiles is very short in comparison with that of the snout and the jaw is articulated at the back end of the skull, and (ii) the legs are short. Thus, especially when basking in the sun, the mandible rests on the ground, so in order to open the mouth (as a thermoregulatory device by ventilation), crocodiles are forced to rotate the skull on a vertebral hinge, thus giving exactly the impression of merely rotating the upper jaw.

Naturally Aristotle also made some, apparently inexplicable blunders, such as maintaining that both the Lion and the Wolf have a single bone in the neck (yet in some small mammal which Aristotle did not know and in Cetaceans, the cervical vertebrae actually fuse together, apart for the Atlas).

If we are to evaluate Aristotle's work both in the context of Greek science and in comparison with later developments, we must acknowledge that he was the first to employ consistently comparative methods in order to study the correlations between organs and their functions as well as between different organisms and to evaluate their significance in terms of affinities, and thus that he was also the first to consider the possibility of systematics.

It is clear that he was not interested in merely cataloguing and describing animals. Thus a number of animals very common in the areas where he lived, and that he must have known quite well, are not mentioned at all in his writings. In his surviving books (his *Zoika* may have been different) the animals quoted are referred to just because they have some characters which are significant to the discussion of some general problems.

It is quite clear from the texts that Aristotle's studies imply criteria for affinities and distinctions (that is grouping the animals at the same time by 'genus' and by 'species', more or less inclusive), but it is equally clear that, unless this was formalised either in the lost tables or in the lost treatises, while he was establishing the basic criteria by which a formal classification is possible, Aristotle refrained from proposing one.

As a whole Aristotle considered some 540 species or groups of species of animals and, starting with the consideration of a number of correlations such as "all horned quadrupeds lack the upper incisors and have a multi-chambered stomach" (which he describes), he reached some general conclusions. In principle, Aristotle was sceptical of dichotomic classifications, and wrote a vitriolic criticism of them. Nevertheless he recognised a basic distinction between 'Enaiman' animals, that is 'with red blood', *i.e.* Vertebrates, and 'Anaiman' animals, without blood or more precisely without red blood, the invertebrates (actually, on this point Aristotle makes an ambiguous reference to Democritus, so that it is not clear whether this distinction was originally proposed by the Abderite).

Within each of these major groups, Aristotle distinguishes various 'genera', each one inclusive of a number of 'species' (eidos). The reader must remember that in translations this same word 'eidos' is given as 'idea' when used by Plato, and 'species' when used by Aristotle. In fact the correct translation in both cases would be 'model' or 'archetype', of which the different individuals are but the empirical manifestation. We shall come back to that when we consider Aristotle's ideas on reproduction. Anyway, and this is made quite clear by Aristotle's books on logic, 'genos' and 'eidos' are relative categories: when one considers the totality of animals, the Anaima and Enaima are genera, while each of the included, comprehensive categories are 'Eida', for instance 'Cetae', the Cetaceans. But if we consider a subordinate category only, again the 'Cetae', then 'Cetae' becomes the 'Genos', and each kind of dolphin is an 'eidos'.

With this proviso, the 'genera' of vertebrates (defined as 'with blood, viviparous or oviparous') are 1– man, 2– Viviparous quadrupeds (which include as subordinate groupings "non anphodont" (ruminating, with a clowen hoof and incisors only in the lower jaw); Monycha (without clowen hoof, horses); other viviparous quadrupeds; 3– Cetae (viviparous, with mammae, without scales and with double respiration: Aristotle believed that Cetaceans could breath both air and water, as shown by the puff of vapour, which he considered to be water, that they emit on surfacing); 4– Birds, which are further subdivided into Gampsonycha (= raptors), Steganopods (birds with webbed feet), Peristeroeida (Pigeons), Apodes (Swallows, House Martins and Swifts), other birds; 5– Oviparous quadrupeds (Amphibians and the majority of reptiles) 6– Ophioda (snakes and some limb-less lizards; though Aristotle remarks that Vipers do not lay eggs and are ovoviviparous); 7– fishes, which he subdivides into osseous fishes and selachians or cartilagineous fishes, among these last he includes the Angler fish, a mistake justified by its poorly ossified skeleton.

Turning to the Anaima (invertebrates), the subdivision implied by Aristotle's texts is the following one: 1– with imperfect egg: Malacia (today's Cephalopods); 2– Malacostraca (the Crustaceans still known by this name); 3– With scolex (that is with worm-like larva): Entoma (Insects, spiders, scorpions, etc.); 4– With generative mucus, budding, or with spontaneous generation: Ostracoderma (shelled molluscs, sea-urchins, ascidians); 5– reproducing only by spontaneous generation: organisms intermediate between plants and animals, they include: Acalephae (jelly fishes), Tethya (corals), Holothuria (holothurians, but probably also other animals).

Although, as we said, Aristotle has not left us a formal classification, it is apparent that he recognised several groups which are still deemed to be perfectly natural and valid groups. Moreover his arrangement was influenced by concepts that he expounded in the *De anima*, the first treatise on psychology. Aristotle thought the soul to be a sort of complex 'gadget', which could to a large extent be identified with the 'form' or with an 'efficient cause', that is a causative agent, which not only is the cause of life and growth, but which also produces and develops the own peculiar characters of each organism.

In the Homeric tradition, Aristotle holds that there is an elementary 'vegetative soul' which exists in all living beings and which appears from the earliest embryonic stages. Later in all animals appears (or becomes) a 'sensitive soul' which allows them to feel and react to sensations. Higher animals have an 'appetitive and locomotive souls', *i.e.* is that they have desires and can move in a planned way. Finally man has a rational soul.

This stance is qualified by a passage in which Aristotle emphasised that there is no basic difference between man and animals, but only a difference in the degree of development of intellectual powers, which must also exist in dogs and horses. Moreover in several passages Aristotle maintains that all organisms form a continuous series in which the qualities of one kind merge and vanish gradually into those of another.

This is the basic principle underlying the *scala naturae* which had a great influence on Islamic scholars, was enthusiastically supported by St. Albert the Great, and through him continued to influence biologists even after Linnaeus.

Even today average learned people are liable to ask the zoologist questions that subsume the *scala* with its lower and upper steps.

This chapter naturally leads to the consideration of Aristotle's observations and theories on reproduction.

We have seen that Aristotle considered that the ways in which animals reproduced were highly significant for the assessment of their affinities. So he paid great attention both to the collection of data and to their theoretical interpretation.

Aristotle distinguishes various kinds of reproduction: spontaneous reproduction, reproduction without coupling, budding and reproduction by copulation. In the times of Aristotle, spontaneous reproduction (in Latin *generatio aequivoca*) was generally considered quite common, even if Redi, for instance, writes that the idea of his classic experiments which disproved spontaneous generation in insects occurred to him when considering a passage in the Iliad where Achilles asks Thetis to keep the flies away from the corpse of Patroclus so that worms will not develop in it.

Aristotle thought that spontaneous generation occurred only in some plants, in many, but not all insects and in most of the animals that he grouped into the Testacea (Ostracodermata), and in those that he considered to be intermediate between plants and animals (Zoophyta). Aristotle thought that, when it occurred, spontaneous generation was something like a fermentation or leavening, which are spontaneous (obviously Aristotele had no idea of bacteria or leavens).

A *Vis*, a virtue or force, as it was called by later medieval scholars, which existed almost everywhere in the environment, by acting on appropriate earthly-watery substrates, could start this process, which was even easier when it was acting on decomposing organic materials, and, according to the kind of matter on which it was acting, would produce some minute germs, which then developed into visible organisms.

Indeed Aristotle was well aware that many insects, including flies, copulated; but he gave the most curious interpretation of the metamorphoses. While insects with gradual development, such as grasshoppers, were no problem to him, holometabolic insects, *i.e.* those with complete metamorphosis, he thought that copulation produced a scolex, a worm (apparently either he did not notice or recognise the eggs), but thought that the worm was a peculiar kind of animal and believed that the pupa was the true egg. Because of some of his theoretical assumptions he thus developed the idea that the larvae were peculiar, imperfect organisms, which remained such until death, as he considered the pupal moult as a death. At their death, inside the larvae an egg (the pupa) was produced by spontaneous generation, and from such an 'egg' the insect was born. The theoretical background for the wrong interpretation of facts correctly observed was this: according to Aristotle, if from an organism there arose a different organism, itself capable of reproduction, it could in turn produce something different again, and so on *ad infinitum*, but as nature cannot admit the unlimited, the scolex must be unable to reproduce!

Among vertebrates Aristotle believed that spontaneous generation occurred only in the eels, which is a mistake that further substantiates the greatness of Aristotle: indeed eels are catadromic fishes, which when adults, reach the sea and swim to breed in the middle of the Atlantic ocean, the Sargassum sea, whence their larvae return to swim upriver to complete their development. The difficulty of tracing the whole story of the eel's development was such that it was finally unveiled only by Grassi and Calandruccio when, about a hundred years ago, they recognised that the larvae already described under the name *Leptocephalus* were just eels.

Aristotle considered reproduction without copulation as normal for some fishes (but he does not specify which ones), for the bees and in plants (he obviously thought of the higher plants), and identified the seeds with eggs.

As for the bees 'pseudo-Aristotle' (in fact the whole IX book which deals with the bees was not written by Aristotle and we do not know the real author) provides a curious account: the queen is believed by pseudo-Aristotle, as by everyone in his time, to be a 'king', it procreates a small number of individuals like itself and a large swarm of workers; the workers generate the drones, which are sterile.

Reproduction by budding is quoted only in a few cases and is apparently considered as an auxiliary possibility for organisms which reproduce also 'more normally'; it is quoted for some plants and some shellfish and clams such as *Buccinum* and the Purple clam (*Murex*); in this last instance Aristotle, apparently, had got the idea by the frequent occurrence of small clams growing on large individuals. Aristotle considered sexual reproduction the normal reproductive way in most animals he studied and, as he was always very careful to properly define phenomena, defined sexes thus: "we call male the animal which generates into another animal, female that which generates within herself". This is a definition unquestionably correct for all animals with internal fertilisation (external fertilisation was proved only at the end of the 18th century).

Quite naturally Aristotle studied the anatomy of the reproductive organs of mammals. He described their blood vessels and the deferentia, the testis, the epidydimus and the external genitalia. Here, again, Aristotle made a mistake, but a reasonable one: he thought that the semen was formed in the first genital tract, and not in the testis; in fact while the sperms are obviously formed in the testis, the bulk of the seminal liquid is secreted by the seminiferous tubules and by the glands of the deferentia and the prostate, so that, as far as the liquid was concerned he was reasonably right. Again, he denied that fishes have a true testis because he judged that long and thin testes of these animals were deferentia. Having thus considered the deferentia as the source of sperm, there arose the problem of the function of the testis. Here Aristotle, who cannot conceive of useless structures, thought that their function was to slow down the flux of the sperm, thus aiding in its maturation ('coction' in the language of alchemists-physiologists).

In fact Aristotle correctly considers that digestion, especially in its early stages, is like the cooking of foods (hence 'coction'). According to Aristotle, food, once ingested, is first 'cooked' in the stomach, thence it is refined in the gut until it is made quite liquid and useless materials are eliminated, and thus absorbed through the gut's walls. Thence it is 'cooked' again in the veins and in the liver, where it is transformed into an impure blood (ichor). From the liver the ichor is passed to the heart (which he considers as being both the seat of life and of intellect), and in the heart it is further refined and enriched with vital spirits, so that it becomes true blood, with the power to regenerate the tissues and make them grow. He holds that, even in the refined blood, two fractions may be distinguished, one sour, that nourishes the less noble parts (= tissues) of the body, such as nails or bones, and a sweet one, which nourishes the noble parts: muscles, sense organs, etc.

The sperm is an extremely refined part of the sweet portion of the blood, which is produced in the spermatic ducts, where it is enriched with a *vis spermatogena*. It will be noticed that, given the facts that could be known in his times, the physiology of Aristotle is remarkably reasonable and matter of fact.

In the animals provided with semen, this transmits the 'eidos', the form or, to use the medieval term, the *vis informativa*. Even when there is no sperm (which Aristotle believed for insects, in which he could not possibly observe it), the male is nevertheless capable of transmitting the *vis informativa*, the 'power which induces the form'.

Aristotle is, indeed an extreme, 'macho', the female only supplies the menstrual blood, which is just an inert substrate, in which may develop the processes which produce the new individual only if the semen brings its *vis informativa*. Aristotle sometimes compares the action of the semen to that of the artist who carves the statue, but more often his comparison is the yeast which causes the coagulation of milk.

Aristotle deals at length with the problem of the semen and of its function and some of his conclusions are right and others are wrong. He is right in criticising the idea, which seems to have been commonly held in his times, that the semen forms in all the organs and then accumulates in the genitalia, so that each organ contributed a little of itself to build the new organism (an idea which died hard, as it is still traceable in Darwin's hypotheses on the transmission of hereditary characters).

He is also absolutely critical of the idea of Empedocles that, like the two halves of a broken ring, the male semen produces one half of the body and the female semen (actually the vaginal and vulvar secretions) produced the other half.

So far his arguments are reasonably sound (and we shall see that the problem of the female semen was discussed anew by St. Albert the Great with interesting results); but his argument that, if the female semen existed, we should have for each pregnancy a male and a female twin, looks definitely strange.

Aristotle holds that in almost all animals the female produces eggs. For him these do not however, exist in mammals or in many 'Entomata' (insects, etc.).

It is remarkable that Aristotle noticed the difference between the eggs of Reptiles and of Birds (which he calls 'complete eggs'), which do not grow after laying, and those of fishes, which grow ('incomplete eggs'); in fact, the eggs of fishes, as they do not have a calcified shell can absorb a certain amount of water during development, and thus grow somewhat in size.

It is interesting to see how Aristotle, when discussing reproductive phenomena, combines data and hypotheses of different origins. His argument for the thesis that the 'form' is induced only by the male semen is based on the 'evidence' that as semen and menstrual blood are the corresponding secretions of the male and the female, and as the menstrual blood is still blood, and therefore, is clearly not sufficiently enriched with 'pneuma' to cause the development of the 'program' of movements that is the 'transmissible form', it follows that this 'program' belongs to the semen only. One may notice that here, as in other sections of the Stagirite's physiology, the 'pneuma' plays an important role. Now 'pneuma' is an ambiguous entity in his writings: in some passages, Aristotle speaks of it as just an ordinary material entity, while in others it appears just as a purely immaterial power.

Aristotle deems copulation to be necessary for reproduction, and that in Mammals, which do not lay eggs, menstrual blood has the function of the egg. Nevertheless he also notices that in fishes fertilisation is external as he describes how fishes, instead of copulating, swim side by side, occasionally hitting each other.

The description by Aristotle of the copulation of lobsters is famous for his precision.

It is worth remembering that Aristotle believes that the first result of fertilisation is to bring to the egg a 'principle of motion', the stimulus to begin growth: obviously Aristotle could not have any idea of the contraction wave of eggs, following penetration by the spermatozoon.

In his investigation on embryological development Aristotle, quite naturally, used bird's eggs at various developmental stages, and his brief remarks are about the best that one could make without the help of any magnifying instrument. We do not know whether the Stagirite also examined mammalian embryos, but he states that after the first 'coagulation' of the embryo, the first organ formed is the heart, followed by the main vessels (in fact the haemal node and the first vessels are about the first thing that can be clearly distinguished in a chicken embryo).

He assumes that in mammals and in some sharks these vessels will reach the uterine wall and there form the placenta. The second organ to appear, he says, is the brain, and the eyes will later bud from the brain.

Aristotle believes that the various apparatuses are made by 'omoiomerous' elements ('omoiomerous' literally means 'made by identical parcels'), which roughly correspond with our concept of 'tissue', and the Stagirite thinks that there are five basic kinds of them. He thinks that during development first the 'noble' parts are formed: meat and sense organs, later bones, tendons, nails etc. The theoretical interest of Aristotle's distinction between omoiomerous and anomoiomerous parts has recently been investigated by the famous mathematician and theoretical morphologist René Thom, who holds that in fact Aristotle's concepts are more complex than is usually explained in textbooks and that they still have a precise interest in theoretical biology.

The development of birds, according to Aristotle, follows the same pattern: he thinks that the embryo is formed from the albumen (probably as it actually forms on the surface of the yolk mass, which later remains attached to the embryo by the yolk sac) and describes how the early blood-vessels develop until they reach the yolk mass, and how later and gradually develop the different structures.

Aristotle was thus a staunch believer in the gradual development of embryos, that is an 'epigenist' in the sense that this term had from the 17th to the 19th centuries, and that his ideas, in some way, imply the first hint of the genetic pool as an information program. It is also necessary to stress that for the Stagirite the 'soul', though its nature is close to that of the higher celestial spheres, still has a peculiar material nature and is not immortal in the sense of salvationist religions, as with death every soul loses its individuality.

We might easily continue listing the many correct observations made by Aristotle and his shrewd deductions, just as it would be easy to list several serious mistakes.

Among these we find instances of unjustified belief in ancient traditions, such as that goats and some horses might be fertilised by the wind. More serious is for instance his denial that the brain is the centre for consciousness, as it was already currently believed, and to held, instead, that its function is basically that to cool the blood coming from the heart. This mistake was the consequence of an argument partly based on correct observations. As we said, Aristotle had noticed that the cardiac node is the first visible structure in the embryo; as he presumed that there could be only 'one' soul which is both the principle of life and of conscience, it appeared that from this premise would follow the conclusion that if the soul had a precise location this should be where the main organising centre appeared to be. Moreover, given the limited technical means available, he had rightly observes that the meninges, which are rich in blood vessels, are not part of the brain. The latter appeared as an almost bloodless organ, and thus, being poor in blood, the brain must be a cold body (it is odd to remember that in fact in some Mammals the blood vessels at the basis of the midbrain, in the horns or in the nasal mucoses, have a peculiar structure and arrangement so that they really function as heat-exchangers keeping the temperature of the brain tissues constant and comparatively low).

Another seriously weak Aristotelean theory concerns motion. Equally confused and partly wrong is his description of the anatomy and function of the circulatory system.

Vice versa I shall quote, as an instance of his understanding and objectivity, Aristotle's discussion of inter- and intraspecific competition for the limited resources of what we would now call the 'ecologic niche', and he goes so far as to ask Darwin's fundamental question: could it be that the perfect adaptation of every organism (that looks so well designed for a precise purpose), is the simple result of the extinction of the less adapted animals? Aristotle wonders: some fish produce a large number of offspring, but that is because many of them die before they have completely grown up. Finally Aristotle argues that it is not extinction of the less fit, which leaves the world to the fittest. His conclusion is accurately argued, but on premises we now reject. His conclusions are wrong, but who else could ask the right question based on the only evidence that he had personally collected, twenty-two centuries before it was again correctly posed?

Aristotle was also greatly interested in problems of behaviour and physiology. For instance he accurately describes the function of the filament of the Angler-fish and the way this fish 'sucks' its prey, and the electric shock of the Torpedo.

In the *Problemata*, a text of which the authorship is disputed, there is a study of problems of acoustics and of the anatomy of the ear, which much later had a great significance in the development of morphology and physiology. We may bypass the valuable considerations of physical acoustics, but we must recall that 'Aristotle' was convinced that sound could be transmitted only by air. He thus studies the morphology of the ear, and provides a summary description of the external ear and of the acoustic meatus, then maintains that there is a closed cavity filled with air (*aer innatus*) which, in a difficult passage, appears to function as a resonator and a site at which sounds are perceived. In fact Plutarch of Chaironeia credits Empedocles, Diogenes of Apollonia and Alcmeon with the same opinions and a cursory description of the ear-drum occurs in the *Corpus hippocraticum*. Many later anatomists argued that 'Aristotle' thought of the inner ear as being filled with air. I think, instead, that he was referring to the tympanic cavity, that, not having seen the *tuba Eustachii*, he thought it to be closed, and I do not think that 'Aristotle' had any knowledge of the labyrinth. Indeed the labyrinth is almost the same in terrestrial and aquatic animals, while the author of the *Problemata* appears to consider as a possibility only aerial transmission of sounds. Anyway there is no doubt that the opinions advanced in the *Problemata* were a source of difficulty and confusion to the Renaissance anatomists.

Before we leave Aristotle, we must come back to some basic informative and directing concepts of the whole of Aristotlean biology, which are: the conformity of all structures for the purposes for which it is used, and the 'necessity' of nature, 'necessity' being a traditional, but unfortunate and approximate translation of the Greek 'anagke', that exactly means that which cannot be otherwise (which in his Divine aspect, Ananke, is the goddess to whom one cannot pray to, as she is what she is and can not be moved or be made to change her rulings; in her temple in Corinth men were not admitted so that they could not be tempted to pray).

It is precisely Aristotle's emphasis on the conformity with or tendency towards a foreordained end that made many sections of his natural philosophy so adaptable to Christian thought, and was thus instrumental in furthering the unparalleled longevity of his theories.

As regards the necessity (= law) of Nature, this is not understood by the Stagirite in a Democritean material sense (what we would nowadays call deterministic), but also both causal and chance-determined. Aristotle envisaged a finalised necessity (to him the 'final cause' is that which tends and pushes towards a given goal and only towards that). In other words Nature has a purpose of her own, and for its fulfilment it must follow a certain way or, at most, choose between a few opportunities, and Nature always chooses the way best fitted for her purpose and thence cannot depart from the set track. It follows that each organ, every organism, even the smallest detail necessarily occur for a given purpose.

In order to understand Aristotle's ideas on life it is also necessary to bear in mind his theory of the four causes: the final cause or the end envisaged, the logical reason or formal cause, the material cause, and the efficient cause or principle of movement.

The first two causes work practically as one and in biology are the organism itself and its soul; the material cause is just the passive matter of which the organism is built, which in reproduction is supplied by the female. The efficient cause, again in the reproductive processes (particularly suitable in order to understand his concepts) is the active, male semen.

Yet another concept to remember is the distinction between first and second causes. Again we can clarify it by considering reproduction: Aristotle is well aware that organs appear in succession in the embryo, and does not believe that they pre-exist in the semen as a sort of miniature animal (this was a common idea in the 18th century); he was thus an epigenist. But he does not believe that the various organs must form one as the consequence of the other. So the heart does not cause the liver, but simply the liver follows it like night follows the day or the man follows the boy. However, as it is necessary in nature just as in art that that which is being generated must be the result of the action of something actually being (entelecheia) acting on something potentially existing (dynamis), the philosopher must try to distinguish what is entelecheia and what is dynamis. So the heart does not produce the liver because, not only would we then be faced by the question of what causes the heart itself, but also if the heart were to produce the liver it should contain the qualities of the liver, which Aristotle deems incredible. Therefore there must be something else, something existing in fact (*in actum*) before the heart and the liver which causes both. This entelecheia is the soul, which is essentially the same as the logos, the rational and formal plan of the organism, which is like the Nous of Anaxagoras, and which exists *in actu*, while the dynamis 'actually' exists in the matter. The semen, which in a sense acts like a link between the soul-eidos of the parent and that of the son is the 'efficient cause', the cause of movement.

The semen, this entity 'superior and more divine', has a much higher dignity than the feminine semen, the menstruum, and because of that it is capable of organogenesis.

In order to appreciate correctly the value of this complex mechanism imagined by Aristotle, and not dismiss it in a supercilious and superficial attitude, as happens even in recent books, we must consider the technical possibilities for scientific research into these matters until quite recent times. We must also remember that science is basically a great unitarian building which must pool the evidence and theories of other branches of science into the development of each individual sector.

The difficulty scientists had in avoiding some degree of teleology will be evident when one considers that even in 1918 an extremely learned zoologist like Daniele Rosa was advocating, as alternative to Darwinism, his theory of hologenesis, which won considerable support in France and which still had supporters in the '40s and '50s; a theory where, having eliminated all traces of optimistic finalism 'necessity' ruled. With Rosa's theory all phylogeny was, in a sense, planned since the first organisms appeared, just as the whole development of the individual is programmed in the zygote, just to employ a comparison dear to Rosa *like the adult in the egg*, and the environmental conditions merely caused the organisms to react by evolving, and selection merely eliminated the unfit organisms, just as advocated by Empedocles or Epicurus. It is notable that some important aspects of the hologenist evolutionary model are still quite alive and active, embedded in the beliefs of Hennigian biologists. In the 30s, Aristotelean teleology was rather prominent in the writings of Teilhard de Chardin for quite different philosophical reasons.

Nineteenth century biologists of the positivistic school, though aware of the gigantic work done by our philosopher, have criticised Aristotle both on account of his finalism as well as for his tendency to allow his metaphysical theories to impinge on his biological work. I think that this is a gross mistake due to a complete lack of historical perspective: we should never judge the work of a past scholar by the yardstick of later theoretical advances. We should rather appreciate how much the work of a particular scholar has advanced knowledge beyond existing ideas and appreciate how far he has been able to pursue the fundamental scientific ideal of the search for a unified science. Under both these accounts the best appreciation of Aristotle may be found in a letter by one of the giants of the history of biology;

Charles Darwin, when writing to Ogle, who had sent him a new translation of Aristotle *De partibus animalium* writes:

> "My Dear Dr. Ogle:
> you must let me thank you for the pleasure which the introduction to the Aristotle book has given me. I have rarely read anything which has interested me more, though I have not read as yet more than a quarter of the book proper.
> From quotations that I had seen, I had a high notion of Aristotle's merits, but I had not the most remote notion what a wonderful man he was: Linnaeus and Cuvier have been my two gods, though in very different ways, but they were mere schoolboys to old Aristotle. How very curious, also, his ignorance on some points, as on muscles as the means of movement.
> I am glad that you have explained in so probable a manner some of the grossest mistakes attributed to him. I never realised, before reading your book, to what an enormous summation of labour we owe even our common knowledge. I wish old Aristotle could know what a great Defensor of the Faith he had found in you.
> Believe me, my dear Dr. Ogle,
> Yours very sincerely
> Ch.Darwin"

and before leaving old Aristotle, let us listen to his own words:

> "Some of the works produced by Nature are ungenerated and incorruptible, others instead, participate in becoming and in corruption. Of the high and divine things we can know very little, as they are scarcely accessible to our senses. If we take these (few things) as starting points, we may still enquire on them and on that part thereof which we wish to know. Instead we can know well mortal things, such as plants and animals, that are near and familiar to us, and on which we have more ample sources of knowledge. (On them) we may verify a lot of facts on every genus, if we are only sufficiently committed. Both fields of research have their own fascination. Even if we may learn only to a limited extent of those incorruptible things, yet, because of their very height, they are dearer to us than all the things of our world, just as it is sweeter to us to grasp even the smallest fragment of things dear to us, than to observe with the utmost precision many other things, albeit they may be by themselves important.
> However, corruptible things are the most important in science, as of them we may achieve a comprehensive and multiple knowledge.
> As they are closer to us and to our nature, they largely compensate for the incomplete knowledge of divine things.
> After I have explained my thoughts on this, I must still speak of the animal nature, and I shall not leave anything out, as far as I am able, both if it appears of the humblest quality, or of the highest.
> .... Indeed, even in these parts that are less pleasant to our senses, nature supplies not mean joys to whom is able to understand the causes, and has a mind open to philosophy. It would be absurd and unreasonable if we were to enjoy more by the contempla-

tion of images produced by painting or sculpture, than by that of those made by nature, just as we are able to understand at the same time the creative force of the artist – as this is indeed the case with paintings and sculptures – and, instead we were not to sense an even greater joy when considering the works of nature herself, and even more if we can have a glimpse of their structure. And we should not despise, like little children, the lower animals, as in all the works of nature there is something marvellous. And just as it is said that Heraclitus said to some who had come to visit him, and yet they hesitated, as they were seeing that he was drying himself close to the oven, that they could freely enter as even there some Gods were present. So we must, without disgust, begin the study of animals, as in everyone of them there appears the beauty of Nature, built as they are by nature itself so that nothing is at random, but everything is for a purpose, and the purpose for which they are made takes the place that beauty has in a work of art. I say "beauty" as in the works of nature, and especially in them, purpose dominates and not blind chance. But the ultimate purpose for which a thing exists or has been born has taken the place of beauty. If someone thinks that the study of other living beings is something inferior, he should, logically, think the same of his own person, as we cannot consider without disgust the different constituent parts of a man. It must also be absolutely clear that when we speak of given organs, or of given vessels, we are not merely considering matter, nor do we organise our search for this mere purpose, but we do it in order to understand the complete form: we deal with the house, not with the bricks, the clay, or the timber. So the naturalist deals with the structure and with the complete being of a thing and not with its mere parts, which detached from the unity to which they pertain, have no real existence".

Hellenistic biology

At the same time that the Academy continued to develop along Platonic lines for a while, Theophrastus, as the scholarch of the Lyceum, continued the work of Aristotle. Theophrastus was born in Eresos, on the island of Lesbos, about in 371 BC in poor family. His real name was Tirtamus and it was Aristotle who changed it into Θεοφραστοζ (= divine speaker). He had co-operated in Aristotle's researches since the days in Assos or shortly afterwards, and led the Lyceum for as long as 36 years, beginning in 322 BC. The list of his writings is as long as their subjects are varied; but they are almost all lost. He wrote on logic, rhetoric, ethics, politics, religion, metaphysics, physics, including books on soils and rocks and his celebrated botanical books; moreover he produced several historical studies on older philosophers, in all about 200 titles. Now, apart from fragments, we still have an important study on metaphysics, a considerable part of his history of psychology, some short and incomplete treatises on physics, his famous *On characters*, his book on rocks and minerals and the two basic studies *A study of plants* and *The causes of plants*, a total of 15 books. Several scholars credit Theophrastus of some texts which are usually considered to have been written by Aristotle. Because of the importance of Theophrastus in the history of botany, and in order to correctly appreciate his personality, it is useful to consider also his contributions in the other fields of Natural History.

In all the works of Theophrastus the influence of Aristotle's thought is evident, but the pupil is always prepared to criticise the master. The most critical point, for the basic framework of Aristotelean theories, is that Theophrastus does not agree with his master on some physical theories, nor does he agree with the Stagirite on first and final causes.

We have already mentioned the essentials of Aristotle's ideas on final causes and how he is still somewhat influenced by Platonic ideas. Theophrastus does not reject the theory outright, but he drastically qualifies it. First he maintains that to identify the final causes is much more difficult than it looks; second he flatly refuses the principle, so dear to Aristotle, that everything has a purpose; he remarks: what is the purpose of tides? What of the breasts (mammae) in males? Some of these things, as for instance excessively developed antlers in deers, may actually damage their owners: Theophrastus concludes with a moral: "We must think of limits to the final cause and to the tendency towards that which is best, and we should not expect this to obtain always ... Indeed, even if that was the wish of Nature, there is much that does not obey it or that does not accrue any benefit".

Theophrastus is equally critical of those philosophers who undervalued final causes, he merely insists that it is sometimes a mistake to try at all costs to find a final cause for anything.

In truth even Aristotle had conceded something of the sort where he had distinguished between that which happens 'for love of the better' and that which happens 'by mere necessity'.

Theophrastus holds that, in principle, the cosmos is well-ordered and that especially celestial bodies show order at its best, but thinks that in the sub-lunar world much happens both to elemental substances and to animals, which is just the result of chance and has no purpose.

Another field where Theophrastus is a good critic of his master is the basic theory of elemental bodies. In his short work *On fire*, Theophrastus remarks how fire is basically different from the other elements; true: all elements are capable of changing from one into another, but fire is the only one capable to generate itself; second: fire is the only element that both naturally and artificially requires a force to be produced; third: while we cannot create the other elements, we can create fire, and we can do it in several ways. The last and basic difference between fire and the other elements is that while the others can exist by themselves, fire can exist only where other substances occur, so he concludes "Everything that is burning is like it was existing and self generating at the same time and the fire is a sort of movement; it vanishes while it is being created, and, as soon as the fuel is gone, so itself perishes. Therefore it seems absurd to consider it as a primary element as if it was a principle (stoicheion), as it cannot exist without other materials". Nevertheless Theophrastus, typically, does not propose a new theory of elements, nor does he decide what Fire actually is.

Again, in his metaphysics, Theophrastus suggests some basic criticisms of Aristotle's theory of the 'immovable mover', but does not suggest any alternative idea.

The same occurs with his criticisms of the theory on the nature of heat and cold, which we have seen to be primary qualities for Aristotle, but which Theophrastus holds to be simple attributes. In his treatise on fire, Theophrastus, amongst a series of other arguments on problems and facts of varying relevance, remarks that he considers that the whole theory of the simple substances or elements needs a radical revision, but does not proceed with it.

The major surviving contributions by Theophrastus are his short treatise *On the stones* and his botanical works.

Theophrastus in his mineralogical work divides the substances which may be found in the ground in two classes: those, like the metals, which are mainly 'water' and those like earths and rocks, which are mainly made of 'earth': Within these two classes Theophrastus suggests a classification based on colour, toughness, grain, specific weight, solidity and – and here he is especially interesting – on the evidence of their reactions towards other 'substances', fire and heat being especially significant. Discussing this point Theophrastus remarks that while certain minerals fuse, others break and fly to pieces, while yet others, like marble, burn and change into something else, like lime, and still others, at last, neither burn nor change.

In his discussion Theophrastus provides many technical and geographical details. At a certain passage he mentions the digging of something, which is probably lignite and this is the first time that a 'mineral' is recorded to be used as fuel. In another passage he provides a lengthy discussion on the touchstone, which, again, is the first mention of the possibility of establishing the amount of certain substances in an alloy. In the section on pigments Theophrastus records the first account of the preparation of lead-white and, among the recent discoveries, he records the preparation of red ochre and the extraction of cinnabar. At the end of the book Theophrastus states that art may imitate nature and create its own substances, some for their practical use, some for their look and some for both purposes, 'like quicksilver', and describes its production and concludes: "perhaps many other like discoveries might be made".

While most surviving Greek texts on stones give ample room to their supposed medical or magical virtues, Theophrastus has only a few cursory references to these items, and usually doubts the traditions that he quotes.

His typical conservative approach to theorising, his care in ordering data and his keen eye as an observer excel in his two famous botanical works.

These were patterned on the model of the zoological works of the master and include both careful descriptions of individual plants and general theoretical discussions.

It is highly probable that many reports quote facts which were common knowledge to gardeners, horticulturists and so on. Here Theophrastus' work consisted of collecting, evaluating, comparing and organising his information, but in most instances, all or part of the evidence is first hand.

Theophrastus, as was his style, did not advance a true formal classification. Plants are empirically divided into trees, shrubs, bushes (phrygana) and herbs, but Theophrastus emphasises that these divisions must be taken in a very general and broad sense, as some plants have an intermediate character between two groups and, when they are cultivated, change their aspect and diverge from their essential nature. He remarks "Nature does not seem to obey clear laws (literally "possesses necessity"): our study and distinction of the plants in general must be taken in a broad sense and as such must be understood".

A good part of the two treatises consists of the methodical description of the individual species, and here he has sometimes been criticised as quoting nonsensical legends and traditions. However when the original text is read, we find that Theophrastus almost always clearly distinguishes what was known to him from personal experience, what he reports and considers to be reliable and what he was quoting for the sake of completeness, but on which he had his doubts. But for a few instances, the legends for which Theophrastus is reproached by his critics are quoted with substantial reservations.

Theophrastus also deals with some general problems and identifies different natural groups, such as Leguminosae, Graminaceae, Conifers and Palms. He is also the author of a uniform nomenclature for the different parts of plants and for the different types of fruits. Theophrastus, also paid special attention to the different modes of reproduction of plants. He tells us how they can reproduce by budding, from seeds, from a root, by suckers, from branches, from the main stem and so on.

He then discusses the problem of sexual reproduction in plants. Several authors, probably following popular tradition, had already mentioned the possibility of sex in plants. We do not know what Aristotle thought of it, as his botanical books are lost, but in most instances, those that Theophrastus identifies as the male and female of one species belong in fact to different species.

In one instance, that of the typically dioic Date-Palm, Theophrastus is right, as its feminine flowers, in the date orchards, were traditionally fecundated by shaking the male flowers above them. Equally correct is Theophrastus' description of the ancient practice of caprification. We should remember that serious advances in the field of sex in plants had to wait for the contribution of Camerarius, almost at the end of the 17th century.

In his discussion on seeds, Theophrastus comments on some important differences between monocotyledon and dicotyledon plants and he remarks that while monocotyledon plants have bushy roots and often several caules, in dicotyledon plants there is a main root and stem, and this usually branches repeatedly.

When Theophrastus died his appointment as scholarch was taken by Strato of Lampsachus, but only a few fragments of his writings survive.

What we know is that the pupils of the Lyceum continued to add to Aristotle's *Zoika* (whether for the better or for worse we cannot tell) and this *augmented Zoica* was one of the main sources used by Pliny.

The school of Alexandria

The contributions to biology by the scholars of Alexandria pertain almost entirely to human biology and medicine.

The two main authors are Herophilus of Chalcedon in Bithynia, a pupil of Prassagoras of Chos and of Chrysippus of Cnydus (both of whom should not be confused for their homonyms both philosophers and theoreticians of knowledge), and Erasistratus of Cheos, a pupil of Metrodorus, who was a good friend of Aristotle (some late authors say that Erasistratus was a relative of Aristotle, but that is probably legend). Both were active in Alexandria around 300 BC, that is in the times of Ptolemy I and Ptolemy II.

But for a few lines, none of their writings survive, but, as their discoveries and theories are exhaustively discussed by Roman authors such as Soranus, Celsus, Rufus and Galenus, we are fairly well acquainted with their work.

Herophilus followed the Hippocratic school and was a keen anatomist. He is credited by Galen with being the first to practise the dissection of human and animal corpses systematically and to compare them and Celsus says that both he and Erasistratus practised dissections also on living criminals from the king's jails (a practice which was revived for a short time in Tuscany and perhaps elsewhere in Italy in the late Renaissance).

Herophilus made a special study of the anatomy of the brain and surrounding tissues: he described the ventricles, the *calamus scriptorius* in the floor of the fourth ventricle, the *Torcular Herophili*, the *plexi chorioidaei*, several of the vessels of the braincase and maintained, contrary to Aristotle, that the brain was the seat of mind and soul. He was also the first to clarify the distinction between nerves, arteries and veins. He studied the anatomy of the heart and eye (and proposed the term which has come to us in its Latin translation *retina*. His study of the human genitalia led to the discovery of the ovaries and he compared them with the testes. Again we owe to his description of the gut the term *dodekadactylon* (= 12 fingers in length), which the Latin anatomists translated as *duodenum*. From a quotation in Galen we know that, studying the liver, he remarked the different lobation of this organ in different animals, though he, apparently, did not say which ones he had studied.

Most of Herophilus' anatomy is good, but there are, naturally, mistakes. One such alleged mistake, however, may not be a mistake after all: Herophilus and other Greek anatomists maintained that the optic nerve was hollow, which is generally charged to be false; however, they may have noticed the retinic artery, which for a brief tract runs inside the optic nerve and would obviously be found empty upon dissection. We do not have the original description and, thus, we shall never know for sure.

Obviously much of his work is on purely medical problems, and does not concern us here, but it must be mentioned that his book on the arterial pulse is largely quoted by the Roman writers and is surprisingly advanced.

Erasistratus is chiefly remembered for his physiology, which is indeed the first systematic discussion of animal functions. Luckily, besides the quotations we have a substantial fragment of him in the so called *Menon's papyrus*, a text of the 1st century AD. Rather than for his conclusions, often quite wrong, Erasistratus is interesting for his methods and principles.

He clearly distinguished between arteries and veins, and assumed that the two systems were in communication and gave a good description of the heart's valves and understood their function; however, he was convinced that the arteries contained 'pneuma' and that, when the arteries were cut, the pneuma escaped, so that blood was sucked into them from the veins. He was thus to some extent less advanced than many of his contemporaries, who held that in the arteries circulated a mixture of blood and pneuma.

Erasistratus' physiology of digestion (like that on excretion and circulation) is based on some sound observations of the chilifer vessels and the general assumption that all processes are the result of purely mechanical factors related to the selective function of vessels in proportion to their size.

Galen, himself a stoic of sorts, thinks that Erasistratus' preoccupation with 'pneuma' is a development of the dogmatic school, which held that the universal basic principle was Anaximenes 'pneuma', which, in its common aspect is just air, and became more and more 'refined' as it went through the smaller vessels, thus becoming 'vital pneuma' and in its most refined form actually runs through the nerves as a sort of nervous fluid (and see further on how the concept of a nervous fluid continued to hold sway through the 18th century). Indeed Erasistratus holds that all organs are actually a thick meshwork of extremely thin veins, arteries and nerves.

Here we must make a brief digression on the barely mentioned fact of human dissections. That these were practised in Alexandria was probably a side effect of the traditional Egyptian practice of opening the corpses and removing the viscera for the preparation of mummies, so that the local society was not, in principle, opposed to the dissection of human bodies, moreover the opportunity of properly examining pathological conditions was obvious. More controversial was, even in antiquity the attitude to vivisection, particularly of humans.

Physicians of the 'dogmatic' school were definitely favourable (though in fact they seldom practiced it) and one of them writes: "indeed it is not cruel, as most people say, to search for cures for multitudes of men of future ages, by the sacrifice of a few criminals".

On the other side Celsus is probably expounding the advice of most learned men in the Roman empire when he writes: "To open the bodies of living men is both cruel and superfluous, to open corpses is necessary for medical students, as they must know the position and disposition of the different parts, that a corpse will show better than a wounded man. As for that which may be learnt only from the living being, experience will teach, albeit more slowly, but also more leniently, while treating wounded people".

A problem which challenged Greek scientists since Empedocles, was that of vision, and it had a manifold influence on many aspects of scientific research. The Greeks ignored the black chamber, which was discovered by the Arabs. So since remote times, they grappled with two basic theories: either the objects continually produce some sort of film-like 'eidola' which fly from the object into the eye, or it is some power of the eye itself, which goes out from it and, so to say, explores the environment, somewhat like the blind-man's stick or a sort of invisible hand. Indeed this last idea is still with us in our common language, as, when we say that we fix something when intently staring at it, this actually means that our stare 'fixes', it is actually holding the object so that it can not move.

A final remark concerns the wealth of information about plants and animals which was assembled by the Hellenistic geographers, and, among them, a special mention deserves Strabo, who, among other things, was the first to precisely mention the Egyptian fossiliferous localities.

CHAPTER III

Roman times

SYNOPSIS OF MAIN HISTORICAL EVENTS AND OF THE CHIEF SCIENTIFIC SCHOLARS

Before Christ
102-48 Caius Julius Caesar.
Titus Lucretius Carus (dies in 55 BC), C. Terentius Varro, c. 50, Vitruvius c. 25
42 Battle of Philippi, de facto end of the Roman Republic.
32 battle of Actium, Egypt is annexed by Rome, Octavian acclaimed Emperor.
Strabo 64 BC-20 AD

After the beginning of the Christian Era
Galenus 129-199, Cl.Ptolomaeus c.150, Dioscorides 1st or 2nd century
180 death of emperor Marcus Aurelius after having defeated the first German invasions of the Empire.
Diophantus c. 150-250, Pappus c. 300
235-285 military anarchy.
313 the edict of Constantinus and Licinius reinstate the tolerance towards the Christians which had obtained in Severan times and again after the Law of Gallienus (253-268).
348 Christian Goths are the first Germans allowed to settle some districts in the Balkans.
376 the Visigoths, under pressure from the Huns, are allowed to settle South of the Danube as foedi = allied, but soon they rebel and in 378 destroy the Roman army at Adrianople. Theodosius I, emperor since 379, begins systematically to settle German 'foedi' in the provinces; he dies in 395.
382 the Roman legions leave Britannia to support their leader Magns Maximus in his bid for the Empire; Maximus is beaten and killed by Theodosius I in 388, only local auxiliaries remain in Britain.
389 or 391 a mob of Christians led by the Patriach Theophilus burn part of the Library of Alexandria
393 last Olympic games.
406 The Roman Limes along the Rhine collapses on a wide front under pressure from many German peoples, at the time it was garrisoned only by Frankish foedi, as the Roman Army had been recalled to Italy to face a Visigothic invasion.
410 Stilicho, commander in chief of the Western imperial armies, to face German invasions recalls the last levies from Britain; in 429 invaders, probably from Scotland are beaten off Oxford. Tradition holds that the British leader Vortigern recruits Saxon mercenaries, who soon rebel and in 449 the invasions of the German tribes begin, first the Jutes, then the Anglo-Saxons.
415 the Christians murder Hypatia, practical end of the Alexandrian school, except for medicine.
476 Romulus Augustulus is deposed, end of the Western Roman Empire
481 the Merovingian Clovis, successor of Childeric, becomes king of the Franks and in 498/99 is baptized with his army and becomes a Catholic. He is practically the founder of the Frankish kingdom; Clovis rules the Franks as king and, as most other German rulers, the Romanized Gauls formally as Patricius for the Byzantine emperor; dies in 507.
490 the Ostrogoth Theoderic conquers Italy.

Rome and Hellenistic Greece

We do not know much about Roman intellectual life before the second Punic war (218-201 BC); however it appears that the prevailing attitudes were influenced by Etruscan beliefs, and the Etruscans were, as far as we know, scarcely interested in philosophic-scientific speculation. This still rather mysterious people, enjoyed a great repute as diviners by different techniques (haruspicine, based on the examination of the internal organs of sacrificial victims; augural, based on the observation of certain birds, etc.). All these practices were duly observed by the Romans both in the private decisions and cults as in the public matters, and the various 'collegia' in charge of their public practice operated down to Theodosian times, and they carried a great social prestige, so that people like Cicero were greatly gratified when elected to the haruspicine collegium.

The first clear impact of Greek beliefs on the Roman mind was probably the introduction of Dionysian Mysteries and it elicited a strong reaction by the Senate, who issued the famous "*senatus consultus de Bacchanalibus*" which allowed the practice only as a private cult, that is by groups of no more than six people (the Roman tradition was always completely respectful of family cults, but aimed to strict control over any 'public' cult), that is a cult involving large organisations.

There is no question that Dionysus is himself an expression of a way to knowledge (let us remember the tradition of the seasonal occupation by Dionysos of Apollo's temple of Delphi and the still ongoing debate started by Nietzsche on the anthitesis-complementarity of Apollo and Dionysos), Dionysean 'knowledge' is undoubtedly the "other" knowledge, both alternative and complementary to that of Apollo to which philosophy belongs and is sacred.

The Etruscan influence encouraged the inspection of the viscera of sacrificial victims as this allowed some particular future events (a practice usual also for the Greeks: Xenophon for one was particularly proud of his abilities in this field). Etruscan models of livers with its divisions necessary for the haruspicium, are still preserved.

The early Roman madical practice was well described by Cato the censor in his book, and was a mixture of empirical and religious practices. The special traits of Roman religious attitude were largely implemented in everything pertaining with health preservation and recuperation, insuring the regular development of vital functions, as well as in anything pertaining to agriculture, forestry, animal husbandry and game. However, apart from the names of many gods and Indigitamenta (an almost impossible word to translate), we have but a fragmentary knowledge of the actual practices of cults. The best evidence comes from Cato major's book *De re rustica*, and it is difficult to judge how much in practice was purely ritual and how much was a ritualized implementation of empirical experience.

In 291 BC Rome suffered from a dramatic epidemic. As usual under the circumstances, the Senate consulted the Sybilline books of prophecies and, as advised, sent an embassy to Epidaurus to summon Asclepios = Aesculapius. The God, in the guise of a snake, willingly followed the envoys and boarded the ship which was to take him to Rome. When the boat, sailing upstream the Tiber reached by the Tiberine Island (henceforth called *Insula sacra*), the snake slid in the water, reached the island and settled there. As the epidemic ceased, the Romans built there a temple and a sort of hospital. So the still extant hospital on the Tiberine island happens to be the only medical establishment in the world to have been operational for well over 2,000 years.

It is possible that the first Greek practitioners came to Rome in the wake of the cult of Aesculapius.

The first such medicine recorded is a certain Arcagathos, who may have arrived in 219 BC, but who, apparently, was hardly better than a quack, so that, as a surgeon, he was nicknamed *carnifex* (= butcher, executioner). Anyway the cultural prestige of everything Greek did certainly help the settlement of Greek doctors, who were almost the first foreigneirs to be granted Roman citizenship. Nevertheless, the collection and preparation of medicines continued to be practiced by empiricists.

The first really famous physician in Rome was Asklepiades of Prusa, in Bithynia, who arrived in Rome in 91 BC and lived to a ripe old age. He was the doctor and friend to Cicero and to several other famous Romans. He had lived in Athens and had studied medicine and philosophy at several famous schools, including Alexandria. As a scientist he may be rated as the founder in Rome of the, so called, 'methodic school', which basic theories were, however, codified by Themison of Laodicea (c. 50 BC). Asclepiades was also the first to teach in Rome the basic ideas of Democritean atomism. We do not know, however, whether he taught them in their Epicurean version.

Different and differing schools were active in Rome:

The 'pneumatic school' was greatly influenced by Stoic theories, which we have already mentioned. The pneumatic school considered the pneuma as the true basis of life and thought that air is but a gross or, rather, contaminated kind of pneuma. The 'pneumatics' adopted the 'humoral' theories of Hippocrates and we shall see a little further on the impact of these theories on such a great scientist as Galen. Moreover we may easily trace their influence on biology through centuries of development.

Another very influential school was the 'eclectic' one, who aimed to collect and organize what was proved to be of practical use, no matter by which school a theory was advanced.

We shall not follow here the many and notable advances of medical practice during the five centuries of the Roman empire, but we shall rather record such advances that the medicines of the imperial age brought to biology.

During the 1st century AD, there lived in Rome some outstanding physicians. Such was Soranus of Ephesus, who was active in Trajan's and Hadrian's times (98-138): he wrote excellent books on obstetrics, gynaecology and child's care. These were obviously the results of a great practical experience and his influence was great until well into the Renaissance. Soranus, though his interests were more restricted than those of Galen, yet is both a notable observer and theorician.

As far as pure science is concerned the Romans did not add anyting in Latin, but for a few details and interesting hints during the late Empire, while they gave a number of important technical contributions. Indeed until after the final partition of the Empire into West and East after Theodosius, any citizen of the empire, including the emperors themselves (think of Hadrian, Marcus Aurelius or Julian II apostate) when they had to write anything concerning philosophy, wrote in Greek, just as western Europeans up to the 18th century wrote Latin and now we write in English.

There is an established tradition that Lucretius' poem *De Rerum Natura* is an important scientific contribution. As a matter of fact, whatever its merit as poetry, and there is a unanimous consensus that they are very great, its content is but a faithful account of the standard Epicurean doctrines. It just shows how well cultivated Romans had assimilated Greek philosophy.

Titus Lucretius Caro lived in the 1st century BC, the tragic times of the crisis of the Republic: the civil wars, the social war, economic troubles, but we do not know anything about his life but for a possibly unreliable tradition, that he died young, poisoned by a love potion.

The real importance of Lucretius' poem is that its beauty moved all its readers. Ovid, Tacitus, Statius mention him as a great poet; the Christian authors admired him in spite of the radical opposition of their views. During Medieval times his original was unknown until Poggio Bracciolini discovered its first codex in 1417 in Germany, but the fact that all the oldest codices now known are copies made during Carolingian times, show that it was widely read up to the IXth century. Lucretius was equally popular with both scientists and literati of the Renaissance. Giordano Bruno, while he remains basically faithful to the Florentine neoplatonic school of Marsilio Ficino, was clearly influenced by Lucretius; Pierre Gassendi, when he advanced the first modern hipotheses about the corpuscular nature of matter had Lucretius in mind: thus the influence of Lucretius in the origin of modern atomic theories is certainly considerable.

From the biologist's standpoint the best part of Lucretius' poem is his account of the origin of human society and civilization, which is as good (and similar) to the better ones advanced up to the end of the 19th century.

Personally I wonder whether most of the scientists who, over the last couple of centuries, have maintained that Lucretius was a great scientist, even if they had really read his poem, had an adequate knowledge of Greek sciences of his times, while they were keen on building an ancient and respectable pedigree among the ancient Epicureans for the positivists of the late Victorian times.

Didactic literature

While Lucretius, as a poet, was able to make a book on popular science into a great poem, during the Roman empire a number of writers, both Latin (who are better preserved, and Greek, were busy producing texbooks synthesising the 'status of art' in the different fields of science and technology.

Such books often include not only the best of available knowledge, but also some original material. Such production of texbooks and encyclopaedias was the natural result of the fact that the original sources were generally available only in a few great libraries and copies of them were so costly that only the very rich could afford most of them. Therefore it was essential to provide good and updated handbooks. For us the capital faults of such books are two: the minor one is that they usually fail to mention the type and purpose of their books, which is natural as their contemporaries had no problem in understanding it, the second and more serious is that, usually, they fail to mention their sources, so that, quite often we can not decide whether a given opinion or account is the thought or experience of the author of the book or of someone else. This, again, is an understandable fault as, while their users were not much interested in knowing the names of who had first said a given thing, to burden the book with precise sources and quotations would have resulted in more work for the copyist and a parallel increase in the cost of the book itself.

As far as biology is concerned the authors worth mentioning are Cato major, Varro, Columella, Celsus, Pliny the elder and the Greeks Dioscorides and Galen.

Marcus Porcius Cato, nicknamed 'the Censor' because as 'censor' he had inflexibly and occasionally stupidly striven to defend the pure Roman tradition, was born in Tusculum (present Frascati) in 234 BC and died in Rome in 149 BC. A little of interest may be found in his treatise *De re rustica,* which is preserved complete.This is a sort of country life book where one can find cooking recipes, rules for the cultivation of crops or the administration of farms, how to deal with slaves, medical recipes, magic formulae and any other kind of information supposedly useful in farming. An interesting book where one can find snippets of information of natural sciences if one searches carefully.

Agriculture was the mainstay of the Roman economy, yet, for a number of reasons, it was always verging on crisis, and this prompted a number of authors to suggest all sorts of remedies and thus to write books.

Marcus Terentius Varro, from Rieti, was born to a noble family in 116 BC, died in Rome in 27 BC and, after having served as a general under Pompey in Spain, apparently spent his whole life writing books: according to our sources he wrote 600 books on all conceivable subjects, but his only book which has come to us complete are the three volumes of his *Rerum rusticarum*. Again, apparently, it was the importance of agriculture which prompted a multiplying of copies, so that some were saved. He provides rules for breeding stock, for the management of grasslands, and so on. In his text he even sets rules for the best hygienic building of the *villae* which, were, in fact big farm buildig complexes, which included all services, from the granaries to the potter's owen and also a residence for the owner.

In modern times Varro has been hailed as a precursor because he thinks that in swamp areas there could develop animals so small as to be invisible, which could enter the bodies through the mouth and nose, and produce serious diseases. In fact, as the hypothesis that diseases could be produced by microorganisms could not be verified, it was sporadically advanced (see, for instance, in the Renaissance by Fracastoro), but was not taken seriously until the microscope did show that such invisible organisms not only existed, but were, in fact, ubiquitous.

Varro's hypothesis, is therefore just a *curiosum*.

Also Junius Moderatus Columella, who lived at the beginning of the Christian era, wrote on agriculture. He, like his contemporay Seneca, was born in Spain. We have of hin 12 'books' *De re rustica* and a *De arboribus*. They are all very well and carefully written by a wholly competent author, so that he was naturally both a source and an inspiration for several later authors, especially in Medieval times.

Celsus is important almost only for the medicine. He was probably born in Gaul (precisely in the Gallia narbonensis) and we only know that he was active in Rome between 18 and 39 AD under Tiberius. As already said his work is extremely important for the history of medicine, but hardly for the biologist. He was an excellent writer of encyclopedias. Actually he wrote a big tratise calles *Artes* devided in four sections: *De Agricoltura, De Medicina, De Rhetorica, De Re militari.* Only the eight books on medicine survive, and they make a very complete medical handbook of eclectic pattern.

Celsus has been unfortunate: the Latin sources mention him rarely just as a learned scholar, his work was practically ignored during the Middle Ages, though we still have two copies done, as usual, in Carolingian times, and a third codex was copied by Niccolò Niccoli in the first years of the 15th century, and is now lost. Pope Nicholas V is also considered his rediscoverer and Celsus' treaty has the distinction of being the first printed medical book (Florence 1478). The physicians of the Renaissance did regard him as a most authoriative author and indeed, all modern scholars praise him as an exceptionally competent and well balanced author.

By far the two most important authors of the Roman world were Pliny the elder and Galen.

Caius Plinius Secundus, or Pliny the elder to distinguish him from his nephew Pliny junior, was born either in 23 or 24 AD in Como, in Northern Italy, and when very young went to Rome. There he entered the administration and was an excellent public servant, so that he held high responsibilities both in the civil and in the military administration. Thus he had an opportunity to travel into a number of provinces in Gaul, Germany and Africa. His last appointment was as admiral of the fleet based in Misenum. There, in 79 AD he watched the great eruption of the Vesuvius which destroyed the towns of Herculaneum, Pompeii and Stabia. He immediately moved with the fleet to help the endangered populations and, while his sailors and marines were busy evacuating the local populations, he decided to investigate what was going on at the volcano and thus died suffocated. Of his death we have a touching account in a letter by Pliny junior, then a boy, who was then in Pompeii.

Pliny the elder wrote an encyclopedia of natural history, *Naturalis Historia* which was of paramount significance through the whole of the Middle Ages and is still an important source for historical, geographical and philological matters, beside, obviously, the purely natural history subjects.

Pliny hiself tells us how he used, at any free time, be it at home, in camp or travelling, to have someone reading him and taking notes under his direction. As a matter of fact, even if Seneca tells us something about the magnifying power of a flask filled with water, the Romans had no spectacles, while books were written without separation of the words and without punctuation (full stops appear in late Roman times, commas and other punctuation gradually through medieval times). So reading was painful and slow and the rich Romans usually employed *lectores* who studied the text before reading it aloud, and also used to dictate their notes.

According Pliny himself, his book is based on the writings of 873 authors, 546 Romans and 327 foreigners. His natural history is a sort of encyclopedia covering cosmology, geography, ethnography, zoology and botany, but includes also informations concerning the medical use of plant and animal products, the qualities of minerals and information on metallurgy and stonework!

As Pliny was not a trained scientist, but an efficient administrator, it was as an administrator that he went about his job. So his main preoccupation is for completeness of information: nothing, apparently interesting was to be omitted. Moreover he was apparently attracted by the unusual, so that he relates a number of incredible stories (though some accounts on animal behaviour, such as those on the social behaviour of dolphins and their relations to men, which have been dismissed as fanciful lore, may, in the light of modern advances in ethology, have a good core of truth). Anyway he certainly did not miss any opportunity to get some moral lessons from the account he read.

Thus his stories were always a favourite and, during the Middle Ages, they were often the source for the accounts in the *Bestiarii.*

This habit of attributing human habits or feelings to the other animals is still with us and it is often a source of trouble for the scientists, while we meet with opposite problems when we try to explain some human behaviour in terms of evolutionary ethology.

The average attitude in classic times (not however, of Neoplatonists) was a very sensible one and neither did it completely separate man from other animals, nor did entirely separate men from the Gods. This attitude was almost completely obliterated by the triumph of Christianity untill the great scientific explosion in the 13th century. The complete separation between Man and the other living beings is still a deep feeling also among cultivated people, while “Animal rights” fans are equally wrong in the other way when they feel that other animals, especially mammals, are our immature little brothers.

It is obviously easy to criticize the merely erudite and uncritical work of Pliny, the fact that he takes at face value several incredible stories, his complete lack of interest for any scentific theorizing about the facts he relates, and this is surprizining indeed when compared with enthusiasm and the painstaking accuracy which are clear throughout his work. Thus he, naturally, has consulted Aristotle, but he uses Aristotle in a peculiar way: practically all that he takes from Aristotle apparently comes from the *Zoika* which through the successive editing by Aristotle's continuators appears to have become an encyclopaedic catalogue of all known animals. Truly it seems that he was using the critical edition of this book prepared by Aristophanes of Byzantium (a phylologist of Alexandrian times, who had been at pains to identify the interpolated passages in a number of famous authors such as Homer, Plato, etc.) but who had not expunged the questionable passages, but had merely marked them off. As it is incredible that Pliny might not have had the opportunity of consulting the other books of Aristotle on natural history, the fact that he simply ignored all the more scientifically relevant works of Aristotle, must be a purposeful choice. It appears clearly that Pliny did not plan a philosophic-scientific treatise, but simply as a sort of big popular handbook where any educated man could look in order to gain the credit of being a learned man in his social life.

It is thus fitting that the animals in his book are listed merely according size, without pretence to any rational order: ordering by size is very practical when you have to find an animal by leafing through a book

Pliny's system was closely followed through the Middle Ages and is still the basic one in Gesner's great work (see chapter 7).

In spite of its faults and limitations the *Historia Naturalis* had an immense influence through the Middle Ages and well into the 16th century and still is a basic source for our understanding of life and culture in Roman times.

More or less contemporary with Pliny, was the Greek Pedanios Dioscorides or Dioscurides from Anazarba, near Tarsus of Cilicia, a famous physician, who was probably attached as a surgeon to the Roman Army. It is said that he was an excellent practitioner, but his name is linked with a basic book, which had an exceptional fortune in the history of scientific literature. The book is titled *De Materia medica,* which until early this century was the name of the medical curricular subject now known as Pharmacology, which, indeed is the subject of the book.

Until about a hundred years ago most of the recipes used the active principles that can be obtained from plants, it was, therefore necessary to identify them and to know how to prepare the active principles they contained. Moreover the medical organisation of the empire both for the army and for the civil services required practical handbooks for the purpose. Thus Dioscorides, both sifting through written sources and from his personal experience, wrote exactly such a big treatise as was required, and that earned him, together with Theophrastus, the nickname of "Father of Botany". His book was apparently illustrated since its first publication. It seems that the habit of figuring plants in medical books goes back to Crataeus or Crateva, who is generally identified with the physician and apothecary of Mithridates VI Eupator of Pontus, which dates him at about 90 BC. There is, however a chance that the Crateva of the botanical figures was actually a grandson or great-grandson of the first Crateva by the same name and that, according a strange novel in the forms of letters and known as *Pseudo-Hippocratic letters*, was also a physician and a botanist approximately contemporary with Augustus.

Hellenistic and Roman novels were very popular, and, though usually developing very fantastic plots, are usually rich with references to contemporary events or habits. Some of them survive and, perhaps, in spite of not being masterpieces of the language, their reading could do something to revive among students interest in the Greek and Latin languages.

It is fairly certain tthat the best copies of Dioscorides faithfully reproduced, with additions, the accurate figures of the lost book by Crateva, as is clearly stated in the beautiful *codex Vindoboniensis* copied around 512 AD for a certain Anicia Juliana, obviously a dame of the noble family to whom both Boethius and Symmachus belonged as well as the ephemeral Emperor Olybrius. It is also possible that Crateva's writings had some influence on the much less known Sextius Niger.

In fact Dioscorides, as a physician, deals with all medicaments which can be obtained from any natural product. The book is devided in five sections: the first deals with oils, ointments and trees, the second with animals, honey, milk, fats and of various species of grains and fodder, the third and fourth with herbs and roots, the fifth with wines, other drinks and minerals. The description of the preparation of the recipes is extremely accurate and so generally is the description of the about 600 species of plants mentioned. So much so that in the 18th century the French botanist Tournefort says that he used with much success Dioscorides's book to identify plants met with while travelling in Turkey.

Given the purposes of his book, Dioscorides employs for the description of plants only external characters, some of them of little systematic significance, but quite useful to the herbalist searching for his medicinal plants.

Some other books are credited to Dioscorides: a treatise on poisons, one on poisonous animals and their bites, one on 'simples' (that is medicine made by a single product) and one *De herbis femininis*, some are assuredly spurious, but the one on simples is almost certainly by him.

The immediate practical use of the book of Dioscorides made it immensely popular. As we said we still have copies from the 6th century: the already mentioned codex of Vienna, the slightly later codex of Naples; but it was still copied much later as testified by the *Dioscorides Longobardorum*, copied in the 8th century. For centuries it was a standard book in the medical curricula and thus it was continuously reproduced both in excellent copies and in economic ones, in which figures, both by their cavalier execution and because of the ignorance of copyists, are hardly recognizable sketches.

As soon as the print was invented, Dioscorides was a must, and the first printed Greek edition, by Aldus Manutius, is dated 1478. Of the many early edition the most famous and many times reprinted, is that edited by the Sienese physician Pier Andrea Mattioli, with extensive comments and additions by Mattioli himself.

Another physician of note for the biologist is Rufus of Ephesus. Apart from his medical contributions, which were highly considered by Galen and by the Arab scholars, so that he was influential in Western schools even before fragments of his original works were discovered in the 15th century, he was a most notable anatomist. He gave good descriptions of the anatomy of the eye, discovered the optic chiasma, was the first to decidedly mainain the the sperm was produced by the testis and described the spermatic ducts. We have his complete text: *De appellationibus partium corporis humani*.

The last notable anatomist before Galen is Marinus (beginnings of the 2nd century) who, according Galen wrote a treatise of anatomy in 20 books, entirely based on personal dissections. Unfortunately only the index survives.

Galen

The greatest biologist of Antiquity, apart from Aristotle, was unquestionably Galen of Pergamon, whose life is well known from the accounts that Galen himself gives us in his writings.

Galen was born in Pergamon in 129 AD, the son of architect Nichon, in his youth he studied philosophy and was especially influenced by the peripatetic school, and an Aristotelean influence is apparent in all his writings. His Aristotelianism, however, was strongly tinged by Stoic physics and by a religious attitude partly Stoic and partly oriental.

Old scholars thought that his first name may have been Claudius because of a 'Cl.' preceding his name in some of the oldest surviving manuscripts. In fact it is now certain that the Cl. stands for *Clarissimus*, a title that, by Adrian's legislation was granted to the *Equites* (= knights), while the Senators were to be called "*Viri excellentissimi*" (by late Flavian times the habit had been spreading to call the Senators as *Viri clarissimi*, but, by Adrian times also the knights were given the title, so, wisely, the emperor regulated the matter in order that the fashion should not spread, with time, to anyone). It is difficult to tell whether it was because they wanted to be assimilated to Galen, or, rather, as a result of the successful struggle of the Medieval Doctors to be entered into the Collegia (fraternities or guilds) of the Nobles and Jurists or in the corresponding medical Collegia, but the *Clarissimus* decreed by Adrian is still with us in Italy when adressing a letter to a university Full Professor!

Galen began his medical studies at seventeen in his native town, which was a famous cultural centre, thence he went to Smyrna, where he studied for two years, and then moving to Alexandria, where he stayed for five years. He tells us that he learnt little in Alexandria, as the masters were poor things. While he thought little of most of his tutors, he greatly enjoied the teaching of mathematics and geometry, subjects that, given the dates, were probably taught by the great Ptolemy. If the teachers were poor, the library was excellent and the young Galen must have spent a lot of time there, so that, annoyed by the lack of precision in his master's lectures, he wrote his first treaty (now lost) on medical-scientific nomenclature.

He thence went home and for four years he was appointed as surgeon of the gladiators. This was a coveted appointment as gladiators were then as popular as football champions are nowadays (Martial dedicated one of his most famous epigrams to the lady-fans of a famous gladiator), and offered to a young practitioner ample opportunity to study traumatic wounds, Galen thus became an excellent sugeon and was able to use his surgical abilities in his classic experiments on animals which deservedly make him the true founder to experimental physiology. Thus it was at this time that he was able to show how, by cutting the recurrent nerve one could paralyze the larynx of a pig, thus definitely falsifying Aristotle's ideas on the respective functions of heart and brain.

In 164 he decided to try his fortune in Rome.

Once in Rome he became acquainted with Emperor Marcus Aurelius. The Emperor rightly appreciated the qualities of character and the scientific merits of Galen, who quickly became both famous and successful. Galen's first stay in Rome was brief: in 166 or 167 an epidemic broke out in Rome and Galen returned to Pergamon, but the Emperor called him back and he was again in Rome in 169. After the death of Marcus (180) Galen was court doctor to his successors, Commodus and Septimius Severus. Finally he retired to Pergamon, but often travelled around and it appears that he actually died in Sicily in 205.

Galen's lectures and public experiments were true social events.

While he was undoubtedly both an excellent medical practitioner, a first class anatomist and the founder of experimental physiology, yet he made some serious errors. The trouble was that, taken as a whole his work was so good that its immense authority actually prevented anyone from challenging his statements, so that his teaching turned, in the long run, into a hindrance to any further development of both anatomy and physiology.

His writings are notably clear in all his many books, which covered not only medicine and related subjects, but also mathematics, philosophy and laws.

According to Galen himself, his works numbered not less than 125, all in Greek. Some of them were already lost while he was still alive when the only copies were burnt in the fire which destroied the Temple of Peace. Moreover Galen himself tells us that some spurious books were circulating under his name when he was still in Rome. We still have 83 of his genuine books complete, another 45 credited to him are certainly spurious, 19 more are of dubious attribution: they may or may noy have been written by him. Finally we have 15 commentaries to some of his lost works, so that we indirecly know their contents, and for some 80 more we have only fragments. Some of his works, however, survive only in Arab translations.

Galen himself subdevided his medical-scientific treatises in seven groups: anatomical, on pathology, therapeutics, diagnostics and prognostics, commentaries of the Hippocratic books, philosophical, grammatical.

Among his books the most famous two, at least until the 17th century, were the *Methodus medendi* a tratise on therapeutics, which is also known by the names of *Ars magna* or *Macrotechné* and the *Ars medica* or *Microtechné*, which the Italian medieval scholars call "Articella". This last is a sort of summary of all his medical books and thus was for centuries a standard textbook in medical faculties. Other famous titles among Galen's books are the *glossary of Hippocrates*, the *On the function of the parts of the human body*, *On the preservation of health*, *On the dogmas of Hippocrates and Plato*, *On temperaments*, *On the qualities of simple medicines*, *On antidotes*.

As far as anatomy and physiology are concerned, the subject that interest us, we do not know whether Galen had an opportunity to dissect human corpses while in Alexandria as a student. Later on, though deploring it, he had no opportunity to examine human corpses, but on two chance occasions. Thus he systematically studied the anatomy of all sorts of animals (pigs, goats, monkeys, even an elephant). His descriptions are truly wonderful for both accuracy and clarity, but his mistake was to think that his findings could be safely extrapolated to man.

As we said Galen is also the father of experimental biology. Indeed he made some most notable experiments: he practiced different kinds of cerebral lesions on various animals and studied their consequences. From this he passed onto the study of cerebral nerves and was thus able to distiguish sensory and motor nerves. He did show that the pulsing of an artery where he had introduced a quill to register its movement ceased if the artery was choked by tightly binding it proximally to the ligature. He showed that if the cervical nerves are cut, then the heart ceases its beat, and thus he confirmed that nerves do not depart from the heart, as claimed by some previous authors, but came from the brain. Such are just a few examples of his many experimental researches.

The problem was that Galen's morpho-physiology, was built into a complete theoretical framework and was by far the best that could be achieved, given the times. He is indeed particularly good in his accounts of the bones, muscles and nervous system. Thus he made great advances on the Alexandrian anatomists on the nervous system: his account of the cerebral nerves is notable, though he uses an entirely different notation form our traditional one (and with some reason). He recognized only seven cerebral nerves as he rightly, considered the olfactory bulb as part of the brain and so did not count the olfactory nerve. The optic nerve he counted as the first nerve, but he rigtly comments that, again, it should be considered as part of the brain and not as a typical nerve. He did not see the small troclear and abducens nerves and, curiously, he did not recognize the optic chiasma (which, however, does not occur in some species). He counted as two separate nerves the first two branches of the trigeminus and the *ramus mandibularis*, again he counted as one the acoustic and the facialis, though he correctly remarked that they have separate roots and that, after, running together, one ends in the labyrinth, while the other (*facialis*) has a complicate course, which he superbly describes, up to the stylomastoid opening. Finally he thought that the Glossopharyngeal, the Vagus and the Accessory nerves, were to be counted for one.

Equally notable are his contributions to myology and osteology, though there he may have been a little dishonest, as he is sometimes ambiguous as to whether he is relating his own discoveries, or simply confirming those of other anatomists.

As far as his physiology is concerned, and this is the most often decried part of his work, we may well begin with his theory of the blood circulation, which was the first part of it to be shown to be wrong and which, in its ruin, carried along the very image of Galen.

Galen thought that air (more precisely pneuma) arrives to the left auricle of the heart through the pulmonary vein. In the left part of the heart, the air was supposed to mix with blood and thence to pass through pores into the righ half of the heart. Galen had two reasons, both generally overlooked by critics, to propose this wrong idea: first, as he did clearly describe the *Foramen ovale* or interatrial foramen, and the *ductus Botalli,* so he had seen that during the fetal life the separation of the left and right halfs of the heart is incomplete and a particular (and temporary) connection obtains between the pulmonary and aortic circulations; the second was that he was fully convinced of the essential truth of the pneumatic theories. Therefore putting together, the two things: observation and theory, it did not appear irrational to suppose that even in the adult some connections continued to exist between the two parts of the heart. In Galen's model of blood circulation, digested food, through branches of the *vena porta* reaches from the intestine to the liver. In the liver it is transformed into blood. From the liver part of the blood may go directly into the rest of the body, much of it goes to the heart thorough the *Vena cava*. Meantime blood mixed with pneuma inspired into the lungs reaches the left heart by the pulmonary artery. Through the pores supposed to exist between the left and right halves of the heart, blood and pneuma are exchanged. Thence through the *Aorta* pneumatized blood is carried to all the body, where it nourishes the organs; at the same time other blood, through the *vena arteriosa* (actually the pulmonary artery) reaches and nourishes the lungs and the blood reaches the brain through the internal carotid (correct), having been previously enriched in the heart of universal pneuma (air); in the brain it is further purified: a part of it becomes 'vital pneuma' and, passing along the nerves, reaches the various organs and keeps them working, the residue is transformed in the pituitary (= hypophysis) and is eliminated through the cribrose lamina of the ethmoid and the olfactory nerves as nasal mucus.

It must be noted that Galen, contrary to Erasistratus, holds that in the arteries passes not air, but aereated blood and he plainly states that it is possible for some blood to pass from arteries to veins and thence to the heart. Harvey, in his fundamental work on blood circulation (see chapter 9) comments that it is surprising that Galen did not notice the contradictory features of his system and did not discover the blood circulation himself. Basically the mistakes of Galen stem from the fact that he conceived of the heart as a pump both sucking and pressing, while it is a purely pressing pump. He thus concluded that, though blood could possibly circulate in the system, basically both in the arteries and in the veins there occurred an alternating flux towards the heart and thence toward the perifery. One can conceive of Galen system of something working to some extent as that of the lower vertebrates, with an incomplete double circulation (indeed up to the Reptiles a small amount of venous and arterial blood actually mix in the heart and aortic bulb) and partly working as that of some invertebrates with an open circulatory system. His ideas, though quite wrong, were not absurd.

Galen' concept of circulation was accepted for many centuries and we shall see how gradually ideas improved until William Harvey was able to propose the true and complete interpretation in 1628 (but at the beginning of the 18th century there were still some schools who followed Galen!).

Galen proved his value in his study of excretion: by binding the ureters, he was able to give experimental proof that urine is formed in the kidneys and he provided a sensible and basically right theory of the function of kidneys.

In the field of general physiology, Galen adopted a compromise between three schools of thought: the Hyppocratic, the Aristotelean and the Stoic. The pneuma is the principle of life and is part of the universal pneuma, omnipervasive and all-ruling.; however in the organisms there are three kinds of pneuma: he takes his leads from the very ancient doctrines mentioned in chapter 1, of Thymos, Nous and Psyche: the phsychic pneuma or animal spirit, which is purified in the brain, is responsible for sensations and movements, the zotic pneuma or vital spirit, which is produced in the heart, is responsible for animal heath and for circulation, and, finally the physic pneuma or natural spirit, which is produced in the liver, rules nutrition and exchanges.

We have said that Galen's view of the world is rigidly teleologic: everything occurs or exists for a purpose and such purpose is the will of God. Galen repeatedly says that he believes in only one God; however, he enphasizes that his God is not that of Moses (it is pretty clear that he could not tell apart the Jews and the Christians) as his God acts only within the framework of the natural laws that He himself has decreed and, by His own will, He can not arbitrarily work miracles.

We shall see that such an attitude is recurring in the history of scientific and particularly biological thought as, for instance with Cuvier, the British theists and so on.

Galen thus holds that the body is a most perfect instrument at the service of the soul. Galen is much more rigid and dogmatic than Aristotle: Nature never does anything useless and functions for a precise purpose, thus every organ is built so as to be perfectly fit for its functions. Galen's writings are often punctuated by exclamations on the wonders of Providence and on how the strict connection between structures and final causes proves the goodnes and the omniscience of God. Galen the anatomist is Greek, but his religious attitude is Asiatic and is remarkably different from that of his master and protector Marcus Aurelius. Western Stoicism, indeed, was closely linked with the pagan tradition and was slowly evolving towards a cult of the celestial bodies.

In fact Galen did not completely rule out the possibility of astral influences, but linked it to purely physical facts, he thus thinks that they may be relevant in the occurrence of 'critical days' in the course of diseases, with the choice of the best days for the collection of plants or for the preparation of medicines. We shall meet again with the same attitude in many medieval physicians.

Let us deal with but some aspects of Galen's medical work.

Galen correctly holds (contrary to Hippocrates) that all malfunctioning function must be related to some alteration in some organ and conversely, that any alteration in an organ will necessarily result in some sort of disease. Again he does not trust entirely in the complete power of nature for healing: for him the physician must not simply help nature, but must actively strive to restore health in the diseased organ, and thus restore the regularity of functions.

Galen was certainly a great and complex man. His abilities allowed him to be free from any rigid following of given schools, but rather to operate a new synthesis. He had an excellent command of the previous literature, which he verified and increased by accurate observations and experiments, so that his synthesis was almost the best possible in his times. However his renown and his high opinion of himself, which is quite clearly stated in his writings, made the man quite dogmatic. It is often repeated that he was an Aristotelian, which is certainly not true, though he naturally had Aristotle in the highest esteem. His thoughts are more akin to the average of the second Stoa, which produced a number of outstanding thinkers, as, for instance, Posidonius. The second Stoa had been, since Cicero's times, particularly popular with the Roman upper classes. It gave an organic picture of the world where many aspects of the best Aristotelianism, both in logics and in sciences were developed and produced a view of the world where the Man-citizen was expected to serve his country and the world at the same time, with tha spirit of immense and detached dedication wich is so clearly stated in the writings of Marcus Aurelius, and especially in that sentence: "As a man I am a citizen of the world, but as Antoninus I am a Roman."

It is plain how Galen's moral attitude and his faith in the perfection of Nature as the expression of the perfections of God, made him quite acceptable in the Christian community, as well as, to a considerable extent, in the Jewish and later, in the Muslim communities.

Because of later developments a short digression on Christianity and the Roman upper classes is not out of place here.

Sporadic conversions to Christianity of memebrs of the upper classes obtained since Neronian or Flavians times, including members of the imperial family, but were quite rare up to the 4th century. On the other side actual persecution of the Christians were generally both local and shortlived outbursts. The general attitude of the authorities, which was made official by Trajan, was that while Christianity was legally a *superstitio illicita*, Christians should be punished only when individually the subject of a non anonymous denounciation and that, if the charge was not proved the denouncer should be himself punished for slander. Some Christian authors certify that the Emperor Philipp I, the Arab (244-249), was himself a Christian and his killing was followed by the first true (and bland) persecution by Trajan Decius (249-251). Valerian (253-260) after a period of toleration, resumed a systematic persecution, but Gallienus (253-268) formally cancelled all anti-Christian laws and ordered that all properties confiscated from Christians should be returned to the Church. There followed a period in which true persecutions alternated with fairly long periods of peace. The famous Edict of Constantine practically just reestablished a situation that had been already prevailing for over a century. Its true significance is that, while before it, Christianity was tolerated, after Constantine, but for brief spells under Julianus II the Apostate (361-363), Magnus Maximus (383-388) and Flavius Eugenius (392-394), the emperors actively supported the church and, beginning with Theodosius I, actively persecuted the Pagans.

Coming back to our story, none of the later physicians possessed the qualities of Galen. Some were quite good doctors and made some original contributions, especially for practical purposes, but for centuries, unfortunately, no one followed the Master along that path of experiment that he had so brilliantly opened.

Many took Galen's theories as dogmas and Galen's influence on medicine was much more stifling than that of Aristotle on philosophy: simply many of the basic theories of Aristotles on physics and philosophy were so incompatible with Christianity that it was unavoidable to criticize them.

Unfortunately it is not uncommon that the influence of a great man actually hampers the development of the very sciences that he has much advanced; such was, for instance, the pernicious influence that the genius of Cuvier had on the development of French biology in the 50 years after his death.

Basic as the study of Galen's writings was for medical students for centuries, yet, as we shall see, some Medieval doctors, on the evidence of their own experience, decided to take Galen *cum granu salis*. We shall anyway see how, when some of Galen's basic tenets were challenged in the 16th century, even the best scientists were in trouble and upset as to how to fit the new evidence into some theory alternative to the traditional ones.

When the late renaissance released itself from Galen's influence, the repute of this great man plainly suffered from the fact that throughout his work he was a physician and nothing but a physician. He was indeed a great physician and an excellent anatomist, but his anatomical studies, which ought to be his major claim to glory from the standpoint of the biologist, were not adequately valued, just because even his best descriptions and most brilliant experiments were strictly limited to what he considered as possibly useful to the understanding of human anatomy, physiology and pathology. He was totally devoid of that inquisitive spirit, of that love for nature in itself, that interest on comparison, that are so conspiquous with Aristotle. Thus, when his mistakes were discovered, the ruin of his authority entailed the almost complete oblivion of those discoveries on animal anatomy that he had so cleverly made.

Biology after Pliny

Biology after Pliny is a poor thing.

While in medicine several older knowledge continued to be re-elaborated and enriched by practical experience and new techniques, as far as animals are concerned writers deal with them almost solely as symbols for ethycal values.

While Pliny was fascinated by Nature itself, he could well be mistaken in his evaluation of the credibility of some stories, yet his is the spirit of an amateur naturalist, Claudius Aelianus goes about his business in a quite different attitude. Aelianus was a rhetor who lived in the 2nd century. Though born in Prenestae (Palestrina) and living in Rome, he wrote Attic Greek. Several of his works survive. Amongst them a *On the nature of Animals* in 17 books. It is a vast compilation somewhat similar to that by Pliny in using a good many sources and was commonly read in Byzantine times. Unfortunately the author's only aim is to tell a good assortment of moral stories (some may indeed be true). Several, such as that of Androcles and the lion, became quite popular and were often repeated or elaborated.

Aelianus is a scientific nonentity, but he provided a model for many story-tellers through centuries and well until the 19th century children's books were indebted to his stories (Androcles and the Lion is his, Bernard Shaw was still able to make a good play of it).

The Greeks had invented scientific research as the ultimate goal for man. The 'bios theoreticus' the philosopher was the model for the perfect man, but having stipulated that study was the best way to occupy the *otia* for the free man, had undervalued the practical potentialities of science. True some progresses in sciences were implemented for practical purposes (mainly for weaponry), but these were of sufficiently minor impact in practice that they did not, by themselves, recommend the teaching of natural sciences except as niceties good to round off an education which basically aimed to produce citizens and administrators (later Christians and administrators). Under the circumstances sciences were bound to come a good thrird or fourth after ethics, laws and military training.

The Romans were always primarily concerned with practical issues and for them sciences were primarily techniques, and in these fields they were unsurpassed. On the other side the Empire was practically bilingual: all through the lands bordering on the eastern Mediterranean, Greek was a recognized official language, equal to Latin in the official documents. So, for instance almost all the imperial coinage issued by the Eastern mints bears only Greek inscriptions. Latin was the language of the West, but when they had to write on philosophy or science also Westerners (including the Emperors, such as Hadrian, Marcus Aurelius, Julianus II) wrote in Greek, just as we now write our papers in English.

Thus it is not surprising that in the absence of official support and devoid of optical instruments, natural sciences, though had obtained an impressive amount of good informations, had a growing tendency to become subservient to medicine, and there quacks and more or less crazy people apparently had a growing influence, while, instead, logics, mathematics and physics continued to progress throughout the imperial age.

Beginning with the 1st century AD, it is easy to notice a general growing interest for pure logics and for religious problems.

Classical Paganism had developed with strong local connotations and thus, since the time of Alexander the Great, it had considerable difficulties in adapting to the situation of great, transnational, states. Philosophers tried, indeed, to deal with the problem, but, devoid as they were of our moderns techniques for the study of these problems, either they chose a more or less strong Euhemerism (Euhemerus, c. 340-260 BC, thought that the gods were, like the heroes protectors of the cities, historical figures divinized by tradition), or they adopted some kind of popular sycretism, identifying such deities who had common traits (such as Zeus-Ammon, Herakles-Melkart, Aphrodite-Astarte). As a final alternative they could go back to the old tradition of the divinity of celestial bodies, as their movements, which could be rationally, mathematically predicted, were the embodiement of rationality itself and thus lent them to a true cult.

The naturally conservative attitude of the upper classes remained at least formally faithful to the traditional gods, possibly within the framework of some philosophical doctrine. Such an attitude is that of great men like Plutarch of Chaeronea, Marcus Aurelius, Julian the apostate, and, as we have said, only a few of the patricians joined the salvationist religions coming from the East, Christianity included. It was the zeal (and the persecutions) of the post-Theodosian emperors which shifted the balance. The edict of Arcadius forced Paganism into clandestinity and Justinian I killed off its cultural residual life.

Anyway, among the late classic writers on medicine who enjoyed a good fame, we may remember Asclepiodotus of Alexandria (around 490), who was celebrated for his eurdition and who wrote books, now lost, following Hippocrates and Soranus. Severus Jatrosophystes and John of Alexandria (both 7th century) were commentators on Galen.

A very important compiler is Oribasios (326-403), who was physician to the Emperor Julianus II apostate. He, ordered by the Emperor, wrote a treatise in 70 books synthetizing all available medical knowledge, mainly following Galen, Soranus and Rufus. The work was conceived as supplying a basic text for the imperial schools. Still about one third of this work survives and, moreover, we have by the same Oribasius a digest in 9 books that he wrote for his son. It is interesting to remember that of both the fragmentary treatise and of the Synopsis, we have Byzantine copies annotated in Carolingian writing and fragments of Latin translations, also of Carolingian times, which testify of the continuing inflence that Greek medicine had in the West.

Finally, one more physician from Byzantine times deserves to be mentioned, as he added a little to zoology. Alexander of Tralles, who lived in Rome in the 6th century and was acclaimed as both a good physiscian and an excellent master. He wrote, amongst others, a book on the intestinal worms providing the first description of the Ascaris, the Oxyurids, the tape worms and suggesting various medicaments for them. We also have from him a *therapeutics* in 12 books, which was popular in Medieval universities.

Gnosis, the irrational and Hellenistic sciences and the reasons of the stasis and subsequent decadence of culture

Many factors may be listed as causes of the decadence of classical Paganism, some are clear, but some are still the objects of debate. Some relavant significance must have had the intrinsic difficultly of Paganism. Indeed Greek religious attitude, which we know much better than the Roman one, tend to pose to man the hard problem of accepting a destiny that he is framing all along by his own choices. Man is thus conceived to be at the same time free to choose his destiny, while morally bound to follow such destiny as the impassive Goddeses propose for him. And this is without hope for compensation either in this world or after death. True there are myths where the good lives after death in the delightfull Elysium and the bad are punished in Tartarus, miths in which someone is punished during his life either for his crimes or for having defied his destiny, but it is only on this last point that Greek mythology is clear, for anything else there is no certainty: 'Hybris', to decide to defy one own's destiny, is the ultimate sin.

Anyway there is no doubt that through all the imperial times, there grew a popular expectation of some sort of 'end of the Times' did grow and for the beginning of a new era, perhaps as part of a cycle. At the same time grew the expectation of some sort of 'salvation' and salvationist religions, the foremost being Mithraism and Christianity, promised just what the Olympians denied and that philosophy too denied (apart from Neoplatonism, which is anyway more a religion than a philosophy).

Given these premises, to know an 'absolute truth' was of paramount importance and its search was to spread among growing numbers of people.

Such was the psychological environment in which 'scientific mysticism' was reborn, just while the Classical world approached its ruin.

While the best brains succeeded in realizing an harmonious symbiosis of their religious attitude with philosophy, producing different trends that we will, partly, meet again in Medieval philosophy; for common people and especially for what concerns medicine, mysticism degenerated into base magic practices, esoterism and there merge with the ancient tradition of theurgic and sacral practices which had never vanished. Indeed such trends had been invigorated, among more educated people by semi-philosophycal schools (Neopythagorean, Neoplatonism) and by the Eastern traditions of High Magics.

In the Middle East, and especially in that sort of human and intellectual crucible which was Roman Egypt, where, until the Muslim conquest, international contacts reaching as far as India, were common, Alexandria housed a number of well organized ethnic groups with their different religions. There, throughout the imperial times, the most varied religions and semi-philosopycal sects were continuously burgeoning.

So there, for instance, was produced the first Greek translation of the Biblical books (the, so-called, 'Translation of the Septuagints', with its many minor and not so minor differences from the Masoretic version in classical Hebrew (the *Septuaginta* was to have a major importance in the development of Christianity). There too Philo of Alexandria tried, in a very amateurish way, to join Platonism with the Bible. And it is in Egypt that, while several of the main 'Fathers of the Church', such as Origen, produced some of the most important contribution to the development of Christian orthodox doctrine, just at the time when were created a number of texts of those crazy schools known as Pagan and Christian gnostics (these lasts responsible also for the compilation of some apocryphal gospels).

These sects remind us, *mutatis mutandis* of some recent salvationist sects. As a whole one might argue that the Hellenized East basically rejected both the religious and philosophical essence of Greece, but a discussion on this point would take us too far from our subject.

Besides purely religious schools, a mixture of empirical lore, magics and mysteriosophies, all factors which had never been entirely lacking either in the Greco-Roman or in the Asiatic media, produced an important literature relevant for both medical and magic-alchemistic tradition, especially since the third century AD. As we shall see this material was to exerce a significant influence on the development of later biology.

Usually the authors of these books tried to pass them as the works either of famous Greek thinkers or of Jewish personalities. Some of the authors may have written parts of the books under their own name. Such is the case for Arpocration, who was certainly the author of a *Perì physion dynamaion*, but not of other books credited to him (one of them was certainly written by a certain Thessalos of Tralles, who wrote in the times of Nero). Another possibly real author is Bolos Democritus, whom many later writers confused with the great Abderite philosopher. Most of these writers, anyway are clearly fictitious personalities; such are Pseudo-Ptolemy, Alexander, Evax, Damigeron, Moses, his sister Miriam and the most celebrated of all Hermes Trismegistus.

This literature includes several books on plants and minerals, but these are mainly concerned with their use in the concoction of magic philtres or talismans.

Some of these books have strange or unintelligible names, such are *Kirannides, Capsulae eburneae, Pikatrix* or the most famous of all: *Tabula smaragdina.*

All this material is not entirely rubbish: it details quite a few techniques for the production of alloys, for the staining of different materials and so on, and is of great importance both in history of alchemy and chemistry, history of technologies, history of religions or psychology, but is irrelevant for our purposes.

We shall discuss here another factor which was quite relevant in the subsequent developments of all aspects of science and philosophy.

We have noted that the development of philosophical and scientific studies in Antiquity was always difficult because of the exceedingly small number of scholars and of the crippling lack of really appropriate instruments for the gathering of new or more accurate evidence. However there is no doubt that scientific research underwent a crisis between the second and third centuries AD, well before the edict of Constantine (315). At the same time the attitude of almost all the early Christian writers was one of hostility towards all aspects of philosophy and science. This was only parly linked with their social background, as it rather stemmed from their prevalent expectation of the second coming of Jesus within a short time. It may be somewhat simplistic, but the attitude of most of them may be summarised as such: To live as a good Christian one does not need philosophy or sciences. Moreover all truth is found in the sacred books and in the writings of the saints inspired by the power of God. The tendency to doubt that is inherent in any attempt towards a purely rational explanation of the world or in the exploration of the powers of logics, may well distract the Christian from the right path of faith.

Obviously we have no space here for analyzing the many nuances of this attitude in the early Fathers and to follow their influence through the Middle Ages. The early Christians were quite ready to use any bit of classic knowledge which was of practical use. However, soon after Christianity got the upper hand and became a support for the Empire, a new school of thought arose, which we may well call 'the imperials', these included some of the greatest saints of the 4th-5th centuries, such a Jerome, Ambrose, Augustine. They were impeccably orthodox and, on occasion, quite intolerant, but, contrary to those who daily expected the coming of Christ, aimed to a synthesis which would blend the traditional cultural inheritance with the firm framework of Christianity.

The quarrels between the two schools were sometimes pretty nasty.

As an example of the anti- or a-scientific attitude we may take Cosma Indicopleustes (an Alexandrian of the 6th century), who is also one of the few people of this age to add a little to zoology, by providing some interesting accounts of African and Indian animals that he had seen in his voyages, but who, in the name of the Bible, rejected all of Greeks astronomy, and believed in a flat Earth, covered by a sky similar to the canopy which covered the Ark od Covenant!

On the other side, St Augustine has something important to say on Creation: he holds "As within the seed all parts of the future tree are contained, albeit invisible, so we must believe that the World, when God suddenly created everything, when the day itself was created, not only the Sky with the Sun, the Moon and the stars, but also everything that Earth and water produced, were created *potentialiter et causaliter*, well before that, at the right time and after a long time, these appeared in the world and now we know of them; and such are the works of God also in the present" and further on he says "If He (God) so created from earth man and the beasts, what has man which is better that what the beast have, other than the fact that he was created in the image of God. But truly not in his body, but in his intelligence and mind."

The immense authority of Augustine gave special weight to these statements through all of the Middle Ages and for much longer, as they are at the root of the beliefs of such people as Bonnet and Cuvier! In a way we may well argue that Haeckel's trees or Rosa's Hologenesis have Augustinian roots (though both would have been sorely shocked if told so).

It may be argued that the prevailing philosophical schools of the Empire: the Stoics and the Epicureans, and even more, because of their strong mystic tinge, Neoplatonists and Neopythagorists, because of their prevailing interest for Man as a moral being, did not promote empirical studies such as are prerequisite in biological matters, which, moreover, looked as thoroughly useless. Some scholars undeline how in Stoic phylosophy there is an increasing interest in the 'Logos' as against empirical research. However, throughout the Imperial period there was always a flux of ideas and influences between the different schools and, even if the stoics may be charged with having such an holistic view of Nature as to paralyze its analytical and empirical study, and that much the same effect may be charged to their deep interest in the verbal meaning and etymology of words, is seems that even more responsible were the Neoplatonists and the Neopythagorists with their mystic views, their passion for the magic of numbers and for astrology.

But also the organization of schools is to be blamed: more and more learning became a rethorical exercise on given texts, rather than an original investigation of new facts. While lay culture became more and more linked to the knowledge of the authoritative texts, religion, also because of Asiatic influxes, was equally based on the exegesis of revealed texts (let us remember that Classical paganism had no sacred books, oracles and for the Romans the collection of prophecies known as the *Libri Sybillini,* are a quite different thing).

Thus biology begun a long slunber, but it was not dead, as we shall see when we shall come to deal with the early Medieval times

CHAPTER IV

Early high schools and their relationship with the development of sciences and philosophy

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MAIN HISTORICAL EVENTS OF THIS PERIOD

Before Chirst.
102-48 BC Cajus Julius Caesar.
42 BC battle of Philippi, end of the Roman Republic.
32 BC battle of Actium; Egypt is annexed by the Romans, Augustus emperor.

After Christ.
69-76 Vespasian emperor.
180 death of Marcus Aurelius after having defeated the first Germanic invasions of the empire.
235-285 military anarchy.
313 tolerance edict of Constantine I and Licinius.
361-363 Julian II apostate emperor.
389 Christians burn at least part of the Library of Alexandria.
394 edict of Theodosius I, Christianity becomes the state religion, persecution of the Pagans.
415 Christians murder Hypatia, end of the Alexandrian school, perhaps but for medicine.
476 Romulus Augustulus is deposed, end of the Western Empire.
490 the Ostrogoth Theoderic conquers Italy.
529 Justinian I closes the School of Athens. Its teachers fly to Gundishapur, under the protection of the Sassanid king.
568 the Longobards invade Italy.
622 the Hejira: Muhammad flies from Mecca to Medina.
640 the Arabs conquer Alexandria, supposed destruction of its Library.
642 battle of Nihavand: the Arabs destroy the Sassanian empire.
717-718 Leo II Isaurian finally crushes the Arab onslaught on Constantinople.
752 battle of Poitiers: Charles Martel blocks the Arabs in France.
763-809 Caliphate of Haarun al Rashid: apogee of the Abassid Caliphate.
800 Charlemagne is crowned Holy Roman Emperor.
888 Charles the fat is deposed, practical end of the Carolingian dinasty.
961 Otto I Emperor.
1066 William of Normandy conquers most of England.
1073 beginning of the strife between the Pope and the Emperor for the investitures.
1085 the Spaniards capture Toledo from the Muslims.
1095 proclamation of the 1st Crusade (1096-1099).
1130 alcohol is first distillated in Germany (it was already known to the Arabs).
1145 paper is produced in Europe for the first time.
1158 diete of Roncaglia, Frederick Redbeard (Barbarossa) grants priviliges to the School of Bologna and generally to students and teachers. Universities are founded: Bologna (1189), Paris (1194-1200), Oxford (before 1208), Montpellier (1220), Padova (1222), Naples (1224), Cambridge (1229) (its School is,

however sporadically, documented since 630, like Pavia), etc.
1176 battle of Legnano: the league of several North Italian cities defeats the Emperor Frederick I Redbeard. Petrus Valdus begins preaching his reformed Christianity.
1180 coal begins to be used in Europe.
1205 beginning of the rule of Gengis Khan.
1208-09 crusade against the Albigenses in Southern France.
1215 King John (Lackland) grants the first Magna Charta Libertatum, the Pope grants a charter to the University of Paris.
1258 the Mongols destroy Baghdad and the Abassid Chaliphate.
1340-1440 the Hundred years war.
1389 the Turks conquer Serbia.
1397 Michael Chrysolora is teaching Greek in Florence
1400-1434 Hussite wars, schism of the Wetern Church, Councils of Constance and Basel.
1453 the Turks capture Constantinople, end of the Byzantine empire.
1462-1500 Ivan I is czar of Russia.
1492 Columbus discovers America.

The significance of schools and teaching

Albeit this is a subject usually barely mentioned in books on History of Sciences, the story of the organization of studies, the kind of culture prevailing in the different social classes, and especially in the upper classes, the links which always existed between 'higher education' and both the political organisation and the development of technologies are matters of the greatest significance for the understanding of the development of sciences.

A comparison between their developments in Europe and in non-European countries largely explains the ascendancy of European predominance in the world and its consequence: the triumph of Western models of science and of scientific philosophy.

While some mention of essential facts shall be done at their proper places, here I shall summarize its earliest development during Classical and Medieval times.

Greek and Roman schools and schooling

In Greece, before the Roman conquest, as later it followed the evolution schooling had through the Empire, teaching was a strictly private matter. It was learnt as any other craft and it developed mainly in the main commercial towns as the learning of elementary writing and arithmetic. However the fact that at least in Athens already before the first Persian war exile (ostracism) was voted in a ballot where all citizens had to write the name of the persons they wanted to exile on a sherd (ostracon) appear to show that the majority of adult males had some knowledge of writing.

The, admittedly partly legendary, biographies of the pre-socratic philosophers, such as Thales, Empedocles, Pythagoras, shows how 'philosophers' could gain even a major political influence.

In Athens we read how there was a magistrate charged with verifying that whoever wanted to become a teacher had the necessary training and listed him in an official roll. We are told that Pericles thought of some sort of public school, but we do not know whether anything came out of it.

Shortly afterwards the social influence of philosophers is well documented: the leading Sophists were charged with the drafting of whole legislations for several towns, while the events of the lifes of Anaxagoras and Socrates show how even thinkes who did not meddle in the local politics were well known, so much so that, as Aristophanes did with Socrates, they could be taken as more or less funny characters in comedies, and how anyone who could afford it, was glad to charge them with the education of their children.

Greek 'schools', however, while active centres for debate and research, were not established for teaching purposes. They are, instead a typical by-product of the traditional system of tribes and brotherhoods (Phratries), which characterized the archaic social system.

Usualy fellows of a 'school' donated to the 'school' all or part of their wealth and, by managing the estate that had been thus formed, the 'School' met all its own needs and the essential of those of its memebers. Quite often, as in the Pythagorean schools or in the Platonic Academy, besides its essoteric teachings, which were of public knowledge, the 'school' had an esoteric doctrine, strictly reserved for its members. These 'schools' had much in common with all such brotherhoods that practised the many 'Mysteries' of the Greek cult and which were generally rooted into extremely ancient traditions. There is little doubt that Orphic features entered into Pythagorism and mysteric features appear in the Platonism of the 'First Academy'.

Unfortunately for us, even when 'Mysteries' (such as the Eleusinian ones) had thousands of adepts, the secrets of their beliefs and rituals were very carefully guarded for centuries (the Eleusinian Mysteries were clebrated well into the latest times of the Roman empire. And so they were guarded even by the most famous and educated peoples, so that we are almost totally ingorant of their contents. For instance, of such an important tradition as Orphism we only have a collection of prayers and a few formulae.

While Alexander had no time to do anything about teaching, the Hellenistic kings dealt with it on the basis of clear and practical ideas.

The early Ptolemies and Seleucid kings pursued an active policy of colonisation of their domains.

While the 'colonies' founded by the Greeks during their first Mediterranean expansion were usually scions of single towns, were entirely free towns themselves, who entertained with the motherland only sentimental and religious bonds, the Hellenistic colonies were military establishments. They were planned in order to establish a network of strategic settlements. They were expected to be strong enough to carry on by themselves the control of local security and to function as fortified pivots of manouvre for the royal armies. Their political structure was indeed (in Asia, but not in Egypt) that typical of the free towns of Greece, albeit framed in a more standardized pattern, with an elected 'town council' (the 'Boulé'), but were, anyway, under the political control of the Satrap or of the 'Strategos' of the province.

These transplants of Greeks settlers and institutions would function only provided that the continuity of Greek education and tradition were made certain. Thus all the Hellenistic kings did, within the limits of their power and economic resources, actively pursue a policy of cultural institutions.

Our knowledge of the developments within the Seleucid Empire is fragmentary, also because of the extremely troubled history of their dinasty, of the progressive breaking up of their domain and of the fact that a large share of their lands were conquered by 'barbarian' dynasties (Parthians, Galatians, etc). Two small, but specatcular, pieces of it have, however, come to us (and have been lost in the destruction of the Kabul Museum during the Russian occupation and the civil war): The French archaeologists discovered in the the Greek city which stood at Ai-Khanum (close to the former Soviet and Chinese borders of Afghanistan), a fragment of a papyrus of an Aristotelean treatise and a Greek inscription by the philosopher Clearchos of Solis, who was known to be a pupil of Aristotle, who had there taught the Delphic maxim: "When a boy learn a balanced education, when a joung man be master of yourself, in maturity be just, as an old man give wise counsel, die without regrets" and stating how he was there as a teacher. This shows that well known Greek philosophers travelled into these distant regions, where, for instance, Greek mathematics and philosophy may have interacted with their Indian counterparts.

Likewise significant is the enormous number of cuneiform astronomical tablets of Greek and Parthian times recovered during the excavation in Iraq, which show how the traditional activities of astronomical observations continued in the Babylonian temples and the nature of the records that astronomers, both local Greeks, such as Seleucus of Babylon, or native Babylonians, such as Kidinnu, could use and how Babylonian experience could merge with Greek geometrical science.

Well known is, on the other hand, the extremely centralised system implemented by the Egyptian Ptolemies: the Library and the Museum of Alexandria.

The Ptolemies adopted entirely the status of the ancient pharaons and Egypt itself (not its dependencies) was considered as being entirely their private propriety, to be managed by a Greek bureaucracy and army and with a middle class of marchants and craftsmen which was at least half Greek or Grecisised. This machinery needed agood cultural center and the wealth of Egypt, which was then the main cereal producer of the ancient world, allowed the Ptolemies to pay themselves both a good army and the best cultural center in the world.

It is said that, at the times of Caesar, when it was damaged during the war between Celopatra VII, supported by Cesar and her brother, the Library had some 700,000 volumes!

We are not well informed as to how the bureaucrats were trained, but it is clear that, by their behaviour resembling that of our renaisssance princes, the Ptolomies succeded in assembling in Alexandria a great many of the best brains of the Greek world. This is especially true for the scientists and technicians, probably because of the better facilities available there.

It is indeed notable that, as far as we know, most of the attempts to the practical implementation of scientific principles were made in Alexandria (such as the hydraulic apparatuses of Heron).

Anatomical studies were considerably advanced by the Alexandrian physicians,whose task was made easier by the local habit of opening corpses to remove their viscera during the process of mummyfication. Moreover we know that, that at least occasionally, live criminals were supplied by the kings for purposes of anatomical studies.

There is no doubt that some teaching activities were current at the Museum, and this went on until its final destruction.

Galen (2nd century AD) is positive that physicians should study at Alexandria, as the only place where they could learn by the direct study of human corpses or, at least on their skeletons. Galen himself was in Alexandria for a while and tells us that, otherwise, he had but two occasions to study human corpses, or rather their skeletons. He goes on to complain as to how an opportunity was wasted by the incompetence of the surgeons, when emperor Marcus Aurelius made available for dissection the corpse of a German during his wars against these invaders.

The continuing work of the Alexandrian school is also proved by the fact that, while either in 389 or in 391 the library was severy damaged by a mob of Christians led by Bishop Theophilus (apparently all the section of the Library housed in the Serapeum was destroyed), later the saint bishop Cyril, excited his flock to the massacre of Hypatia (415 AD), as he believed her teachings of mathematics and physics dangerous for Christianity.

Finally we know how, even into the 7th century, some Byzantine physicians, whose writings survive, had studied in Alexandria.

Greek city-states of the motherland were largely autonomous through the Hellenistic age, though more or less vassals either to the Macedonian kings or to someones of the other major kings.

Thus the Athenian 'schools' continued to function. First were the Academy and the Lyceum, later also the Stoa and the Garden. Still later, after Athens was sacked by the Roman army led by Sulla and probably for budgetary reasons, the Academy, the Lyceum and the Stoa merged together and the appointment of the scholarch came under the control of the city's magistrates. This new Athenian school lasted until it was closed by Justinian I in 529 as he considered it a dangerous stronghold of Paganism. While the Athenian schools still produced some significant scientific contribution, their main activities were concentrated in the fields of logic and ethics.

So, while the Hellenistic world saw the first establishment of organized centres of scientific research, yet there does not seem to be an equally well planned development of public education; this was left to the Romans.

Schooling in the Roman Empire

The Romans followed the pattern of Greek teachings well into imperial times. Elementary schooling was, for the rich, provided by private tutors, quite often Greek slaves (it was fashionable just as it was usual for the 19th century upper classes to have foreign teachers at home to help children to learn languages), otherwise people opened little schools: a headmaster with a couple of helpers and the pupils were fee-paying. Somehow the system was reasonably efficient as it appears that both in towns and in the castra, the majority of citizens and all of the military both of the legions and of the provincial cohorts knew how to write and read.

Anyway the imperial administration became soon aware of the need to train professional administrators and army officers. Emperor Vespasiam (69-79) established the first chairs paid for by the state treasury and to help poor citizens to raise and educate their children established the first *Institutiones alimentariae*, sort of charities that, funded by the Imperial treasury, provided loans for raising and educating young people at a nominal interest. These were to develop later into true scholarships for deserving students.

From the 2nd century onwards all the schools were, in principle, under imperial control and true imperial schools were created in the main towns.

It was just at this time that the traditional *Volumen* or roll (be it of papyrus or parchement) is rapidly substituted by the more practical *codex* made of separate sheets bound together, exactly as our books. Codices continued to be written either on papyrus or on *vellum* (= parchment), well into Byzantine times, and, obviously by reason of cost, papyrus vanished from the West around 700 A.D, as it is proved by Merovingian documents.

The emperor's policy was always to support the centres of learning of Greek origin and to help in the opening of public libraries, though many were donated by private, wealthy, citizens.

A special funcion was performed by the *Schola palatina* in Rome. This was housed in the imperial palace and was aimed both at the training of such youth who could be expected to become high military or administrative officers and to 'Romanize' prominent foreign hostages. At the same time athletic and military training was promoted by the *Collegia juventutis*, sort of youth brotherhoods (who were also often accused of behaving like hooligans).

However the teaching was mostly concerned with literaty and legal subjects and was given in the many schools paid for by the local communities and by an increasing nunber of state funded ones. Its final organisation was achieved by Julian II the apostate in 362, by a legislation aimed also to counteract the growing influence of Christianity. How much the government of that age cared for higher education is shown, for instance, by the order given by the same Julian to his physician Oribasius (see chapter 3) to produce a summary of all medical knowledge.

By this time the whole empire is being wholly geared up for war, as, since Diocletian (284-305) the Empire was regarded as a besieged fortress, within which every citizen had precise duties to fullfil, all ordered from above. The oft repeated story of an effete decadence, which is still found in some texbooks is untrue. Even so, the dramatic scarcity of manpower which followed the economic crisis of the 3rd century and the many epidemics of that age, had obliged the government, in order to keep the economy running, to exempt an ever growing number of plebeians from military service and to compensate for the loss by recruiting barbarians into the army, while political expediency had barred the Senators from holding high commands, but the aristocracy was ever more obliged to supply the officer ranks and to train romanized barbarians who were necessary to command the auxiliary troops.

Our available sources testify that schools were flourishing in the provinces well into the 5th century and our academic titles of Doctor and Professor were first used just during the 4th century. Thus we have a papyrus codex probably of the 4th century with a series of laudatory poems commemorating a professor at a high school in Beirut (Berytus) and in approximately in 386 Ausonius wrote a poem for the 'Professores' of the High School of Bordeaux.

Returning to the late empire and thence during Gothic times, we know that among the 'Counts' (*comites*) or 'companions' responsible for the high direction of the different departments of the administration (they actually were either something in between a modern minister and his permanent secretary, or governors of the 'Dioceses' which had substituted for the 'Provinces') there was a *comes archiatorum* helped by a special *collegium*, who was charged to organize and control the teaching of medicine and the qualification of young physicians for actual practice.

A new *Schola palatina* of university level had been established by Constantine I in Constantinople in 330 as one of the moves to change the old town of Byzantium into the new capital of the Empire. Similar schools existed in the East not only in Alexandria, but also at Antioch, Beirut and Gaza, cities which were to have a great importance in the origin of Arab sciences, while in the West we know, besides of Rome, of schools in Milan (where St. Augustine was teaching for a while) and other towns.

The school of Constantinoples had a checkered story: it was strenghthened by Theodosius II in 425, but it later underwent a pronounced decline, changing under Justinian I in 535 into a *Pandidacterion* with a declared church-orientated aim; between the 7th and the 9th century all teaching was strictly under the control of the Orthodox Church. Public teaching was reinstated by the Emperor Theophilus and its 'restoration' was the work of Bardas, who appointed as its director Leo the mathematician. The school was closed apparently by Basil II, but Constantine IX reopened it in 1045 as a school of laws and philosophy, and appointed as director Michael Psellus, the erudite who was responsible for the Neoplatonic renaissance, a trend which emigrated into Italy four centuries later by the activities of Georgios Gemisthus Pletho[1], just a few years before the fall of the Byzantine empire.

The three phases of flourishing of the Byzantine school approximately correspond first with the period when lower case letters, separation of the words and the usage of accents and spirits were adopted, and all these changes in the practice of writing resulted in an all out effort in copying the old manuscripts, and in two more periods of intensive search and study of the surviving ancient books.

The Crusader's conquest of Constantinople (1225) and the establishment of the Latin empire brought about the closing of the Constantinople university, but it was reestablished upon the Greek recapture of the city and Andronicos II entrusted its direction to the Great Logothetes. A last effort was made by Manuel II (1391-1425), who reorganized it a few dozens years before the fall of Constantinoples (1453) and enhanced the medical studies.

In the West the scholarly curriculum already begun to evolve unto its typical later pattern towards the end of the Roman Republic, but it became standardized only during the great crisis of the 3rd century, when the Empire was often fragmented. It finally took the traditional pattern of the *Trivium* and *Quadrivium*, later typical of Medieval studies, by the activity of Cassiodorus.

To understand subsequent events we must remember that it was common practice in the Barbarian kingdoms of the early Middle Ages, to reserve military service for the ethnic Germans, so that there the Romans were practically demoted to a rank of 'half-freemen', but that did not prevent them from holding the highest offices at court, just as it was still happening into the Carolingian empire. Actually the decay of both the Roman administrative framework and culture was a slow process until the end of the 6th century and possibly until the Muslim conquest of North Africa disrupted all the more or less formal links that still held together the old Roman world. The true catastrophe was the wave of internicine wars that plagued most European countries, of the Muslim raids and conquests and of the crisis of the Byzantine empire, hard pressed both by the Arabs in the south and by the Slaws in the North.

The Roman Cassiodorus (c.480-p.540), who had been a minister of the Ostrogoth king Theoderic (who ruled Italy from 490 to 536), had thought to create a school in Rome similar to that that, guided by Origenes, had briefly functioned in Alexandria as an anthitesis to that linked with the Museum. Indeed in Rome normal lay schools of laws and philosopy were still functioning and teaching the traditional classics. Cassiodorus thus wrote about this idea to Pope Agapithus in 534. However, his project

[1] Pletho, in Florence, preached the return to classic Paganism and was quite influential on many great Florentine Humanists.

did not materialize because of the outbreak of the war between the Goths and the Byzantines in 535. Cassiodorus thence retired to Vivarium in Calabria, where he founded a monastery which was also committed to teaching.

As we said, Cassiodorus gave final formal definition to the curriculum based on the *Trivium* (grammar, rhetorics and dialectics), that is the ensemble of linguistic and logical studies, and the *Quadrivium* (arithmetics, geometry, astronomy and music), that is the mathematical subjects. All these were to be the groundwork on which the study of theology could eventually be built.

We not only have the complete catalogue of the books owned by the library at Vivarium, but the library itself later went to the Monastery of Bobbio (founded by the Irish St. Columba) and from there several of its codices went, during the 14th century, to other libraries (Vatican, Ambrosian in Milan, Vienna, Turin, Naples, Wolfenbütel, Nancy and Paris), where they are still preserved. We have thus a very good idea of what was considered basic knowledge at the end of the Western Roman Empire. There, while classic literature is quite well represented, in the field of biology we have only books of medical interest: Latin translations of Hippocratic texts, of Galen, Dioscorides, Celius Aurelianus.

Some additional evidence on the cultural interests of this age is provided by a handful of codices, personally copied by Roman aristocrats during the 6th-7th centuries from copies going back to the 4th-5th centuries. Chroniclers and diplomatic correspondence tell us the same story of a basic survival of the late Roman culture well into the 7th century.

All this is easily understandable considering that all the Latin curricula had been aimed to for centuries was to prepare politicians, administrators, judges and lawyers, and, indeed when a Roman (taking the term to mean any native of the Western provinces of the Empire) had to write on sciences and philosophy, always wrote Greek. Thus in the standard curricula sciences were conspicuously absent and also the *Quadrivium* had a tendency to be considered merely as a proper complement to the education of a learned gentleman. This also explains the increasing preoccupation of Latin writers: providing summaries.

We have seen that already the work of Lucretius or the philosophical books of Cicero were very much a sort of high quality popularizing textbooks. The cost of books was exceedingly high, so that it was indeed necessary to provide students with adequate handbooks and encyclopaedias (such as the works of Pliny the elder). Again it is just natural that, even among such summaries, those on applied sciences and technologies, such as medical and pharmacological books, outnumbered those on pure sciences.

It is precisely for these purposes that Boethius begun his work. Manlius Anicius Severinus Boethius was born in 475 into one of the most famous senatorial families in Rome. His family was related with the ephemeral emperor Olybrius and had but lately been converted to Christianity, though there is little doubt that Boethius himself was a sincere, if somewhat unortodox, believer. He was related by marriage with Symmachus junior, whose ancestor Symmachus senior is famous for his passionate speech (which is luckily preserved) in defence of the statue of Victory which Emperor Gratian wanted to remove from the house of the senate as a pagan symbol. Boethius was to become a minister of Theoderic as his brother in law Symmachus junior and both were to be executed on order of Theoderic in 524. Boethius' works was basically an attempt to summarize all kinds of scientific knowledge available in the Latin world within the framework of a peripatetic system tinged with neoplatonism.

Boethius was an excellent compiler and his critical remarks are well founded, even if he did not contribute anything really original. However his importance in the history of Western thought is immense and his neglect in textbooks is just stupid. In fact almost all that Western thinkers knew of Greek and Roman philosophy up to the late 12th century was what was related by Boethius. Moreover it concerns also biology, as we shall see how Boethius' discussion on the concepts of genus and species is especially relevant for all subsequent developments.

Early Medieval times

While throughout the earliest stages of the Medieval times Latin historical and literary texts, at least in Italy and Southern France, continued to be copied and, possibly, most of the vanished texts were actually lost during the chaotic times following the disruption of the Carolingian empire, yet, apart from that concerning medicine, there was little interest for technical and scientific literature.

We have seen how during the late Roman times culture became increasingly bookish: the authoritative text, whether sacred or not, was the foundation of knowledge and its correct interpretation, be it logical, mystical or even some sort of esoteric 'gnosis' was 'the Truth'; in the meantime there vanished almost all interest in natural sciences, with their apparently purely theoretical contents.

The barbarian invasions were undoubtedly often quite destructive, even if we must remember that not in a few instances the barbarians came as peaceful settlers to fill the space vacated by the dwindling Roman populations. For instance, while the raids of the Huns or the invasion of the Longobards, were tremendously destructive, the earliest settlement of the Franks was comparatively peaceful. On the other side probably no one cared about the pensioning of the last Western emperor, Romulus Augustulus: formally, in fact, the empire had been unified under the Eastern Emperor, Odovacar was ruling Italy and the few remaining districts of the Western Empire as king of the Heruli and *Patricius* of the Romans in the name of the Eastern emperor, just as, for instance, the Frank Clovis was ruling Northern France as King of the Franks and *Patricius* of the Romanized Gauls. This formal arrangement had been usual for about a century, since the first *Foedi* had been more or less peacefully settled in the empire and was punctiliously observed for over a century: such gold coinage as was struck by the *Foedi* had the types, name and titles of the Eastern Emperor, while copper was struck indifferently either in the name of Rome or in the name of the German ruler!

Still the Roman administrative framework, schools included, was extremely strong and the breed of professors very vital as, in Italy at least, the Gothic wars, the Longobard invasion, those of the Franks and the post-Carolingian chaos were not sufficient to completely extinguish either of them. Sporadically at least, it appears that schools were functioning also in England and Southern France.

Throughout the early middle ages we hear of a curious debate: the strictly orthodox, we would call them 'the fundamentalists', who clamour for the substitution of classical models in the schools, philosophers included, by Christian texts, and the conservative masters who, undeterred, went on exercising their pupils on Virgil and a few other favourite pagan authors.

In Italy lay schools of laws and medicine continued to operate in Pavia through the Longobard rule and most probably in Rome and Ravenna. Medical texts were copied or imported, and some still survive: *e.g.* the *Dioscorides longobardorum*, preserved at Montecassino, and which includes some original drawings added to the original series, or the two Byzantine superb codices: the *Dioscorides vindoboniensis,* an incredibly well illustrated copy prepared around 512 for an Anicia Juliana, who, judging by the name, must have been a relative of Boethius, and the somewhat later and almost as beautiful *Dioscorides neapolitanus*.

But, as we said, the century beginning around 650 was one of increasing chaos and poverty almost all over Europe and was the real beginning of the so called 'Dark Ages'.

Thus the Church begun to worry about schools and the Rispacense Council of 798 recommended the creation of school at all bishop's sees.

Shortly afterwards and following this example, Charlemagne, as part of his program of restoration and recovery, repeatedly recommended, especially by the 'Capitular of Thionville' (805), the opening at all monasteries of *scholae exteriores* where lay people could study.

Charles also resurrected the *Schola palatina* and charged of it Alcuin of York, who recruited most of his masters from Rome. Alcuin also started a systematic search and copying of surviving Roman texts, an activity which flourished through the times of the Carolingian dinasty. Indeed almost all the earliest Latin manuscripts surviving date from Carolingian times and derive from copies of the 5th-6th century.

Shortly after the death of Charles, his son Lothar I granted permission, by the Capitular of Olona, for establishing high schools for lay people in Pavia, Ivrea, Cremona, Florence, Fermo, Verona, Vicenza and Cividale, though we do not know if any action was actually taken.

Though the Carolingian attempts achieved little because of the rapid crumbling of the empire into a new period of chaos, yet its organisation set a pattern, which they had largely inherited from the late Roman empire, and was to evolve into the full fledged feudal systen on one side, but was also influential in shaping the future organisation of the universities.

At least in principle, the Carolingian system aimed to restore the organisation of the early Merovingian times. The significant difference was that the early Merovingians were ruling their Roman subjects formally as Patricii for the Byzantine emperor, the Carolingians held themselves to be the Western Roman Emperors, ruling of their own right on a, by now, unified people. At the top was the Emperor, who ruled by means of two parallel hierarchies: a *comitatus* of noble warriors (*milites)*, freemen in their own right and mostly of barbarian origins, and a *comitatus sacri palatii* whose palatine counts were instead either slaves of the Emperor or, anyway, bound men, and who were generally recruited either among the Roman nobility or among the secular clergy (until about 700 in most countries mainly from the first, in Carolingian times almost exclusevly from the second). Usually the jurisdiction of the military counts overlapped with that of bishops and roughly corresponded with the ancient 'dioceses' (which were changing their name into 'counties'. However usually while the authority of the military counts was prevalent in peripherical areas of the empire, the authority of the palatine counts and of their subordinate officers (*Missi dominici*, curial notaries (*Tabelliones*) was prevalent within the central administration. Moreover, just because of their servile or semi servile condition, the palatines were considered as 'parts' of the Emperor himself who 'owned' them and, therefore, they could fully represent him and thus could inspect and control the military counts.

Locally the central system was duplicated on a minor scale: the local military count acted through his vassals (military) and his *tabelliones* (civil servants).

At the same time the ecclesiastical authority, stemming directly from the Roman organization, was parallel to the civil one: the Pope was parallel to the Emperor, the Bishops to the counts, and the clergy was just equivalent to the vassals, that is to the lesser nobility and enjoyed the same privileges.

Thus were laid the premises for the parallel development of Medieval chivalry and of the scholastic system.

In the early Medieval times and for several centuries schooling as was preliminary to legal and medical studies was centered in the monasteries. There we notice two different possibilities: some monasteries had both *scholae interiores* where the young oblates studied (an oblate was a child who had been given as an obol, that is donated, to the monastery, usually at about ten years of age, but, though meant for growing up in the Church, usually took his final vows much later) and *scholae exteriores* open to lay students. However most monasteries were not big enough to support both types of schools and then the arrangements varied. Children could be entrusted to the monastery formally as 'oblates', but on the understanding that, when of age, would not take the vows, or they could attend the school as *extramoeniales*, that is as 'from out of the walls'.

As throughout the early Middle Ages fiefs were not hereditary (in principle if not in practice) and marriage was not forbidden for the secular clergy and was fairly common at least in the lesser clergy (but we know of married cardinals and even a Pope, Adrian II 867-872, whose wife and doughter were kidnapped and murdered by the son of the bishop of Orte), there was a strong tendency to send to monastery's schools the cadets of both military and curial noble families, thus fostering their gradual merging.

The obligations of Christian religion and especially those of the Benedictine 'rule' made assistance to pilgrims and sick mandatory. Thus monasteries had to have physicians among the monks and where they were learning their trade also lay people could learn.

So in the monasteries books were copied, but usually they were only either literary or medical ones. Thus we still have a number of 'herbals' or *hortuli*, often copies more or less complete and correct of Dioscorides and of Pseudo-Apuleius, but sometimes quite original, such as the little poem of Walfrid Strabo (808/9-849).

After the 11th century the Church begun to worry because of the excessive interest of several monks for medical practice ouside their monasteries (*medicina exterior*); thus the practice was gradually restricted until finally banished by the 4th Lateran Council (1215) and by the Decretals of Pope Honorius III (about 1220), and only clerics holding the minor orders and who had no other means of subsistence were exempted. However, by that time universities were already born and flourishing and the medical faculty was about to gain formal recognition.

Indeed the equation Vassals = Ecclesiastics and the many matters in which secular clergymen and curiales were indifferently mixed up was to prepare for the recognition of the equivalence of clergymen and university graduates.

This was first officially recognized by Emperor Frederick Redbeard with his decree *Authentica habita* of 1180 aimed to repay the Bolognese doctors for their support.

Higher education in Islamic countries

When Arab armies first advanced beyond the borders of Arabia proper to try to conquer the world, they met with poor resistance. The Byzantine hold on Egypt and Syria was shaky because of the conflict between the Orthodox church, which had the Emperor's support and the local Christian churches, especially Nestorians, who, at least to begin with, found their Muslim conquerors much more tolerant than their Orthodox brothers. Moreover all the Byzantine provinces were crushed under the burden of taxes, which the imperial administration was obliged to enforce in order to pay for the perennial wars in the Balkans, in Italy and along the Sasanian borders: seemingly the Sasanians were exhausted by and almost ininterrupted series of civil wars and by the long border's struggle with the Byzantines, the Hephtalites etc.

Egypt, Syria and Mesopotamia were captured after a few battles and Iran soon followed, all between 634 and 650. Such a swift collapse left the existing cultural organisation intact. There is a tradition that the great Library of Alexandria was destroyed by order of either the Arab general 'Uqbah (641) or of 'Amr ibn al-As (642), but this is probably a legend and, anyway not much could remain after the attentions paid to it by Christian mobs (see above).

Soon after the Arab conquests in all the main centres of Islamic power were established some high schools (*Madrasah*, plural *Madaris*), perhaps the first of importance being Cairo in 1005. These rapidly became extremely active cultural centers where Greek sources were both studied and translated into Arabic either from the original Greek or from Syriac.

Soon, however, quarrels exploded between the strict orthodox (today we would call them 'fundamentalists') and the other trends: 'Sufis', who tended to a considerably free, symbolic and mystic interpretation of the Quran and the 'falaisifa' who were more closely linked with the classical rational thinking. Anyway the organisation of the Arab schools had certainly some influence on the development of the organisation and curricula of our Universities.

As for the history of the Islamic institutions little needs to be told: parallel with the development of the empirical and theoretical studies of the Muslim scholars, grew the resistance of the fundamentalists, who finally won the day, approximately on the lines of the teachings of Al Ghazzali and thence succeeded in the complete mummification of all teaching.

We shall come back to the significance for the West of the enthusiasm of the early Muslim scholars for translating Greek authors.

The birth of European universities

As we have often stressed, the 'events' of culture have no precise dates for their beginnings or their end. However, as far as higher education is concerned there is a number of historically significant facts crowded around the end of the 12th century.

Around year 1000, first in Italy, where the municipal Roman organization had partly survived and was overlapping with powerful 'consorterie' (that is groups of rich families, their servants and satellites), municipalities were fighting the rule of the bishops and developing into full fledged communes, and there begun a powerful economic development, with rapid growth of both towns and trade, and consequently grew the requirement for adequate numbers of educated people. At the same time there were often marked signs of decadence in the monastic and cathedral schools, a decadence partly fostered by such reformers of monasteries as St. Pier Damiani, who deprecated teaching of laymen, and especially teaching them the lore of the always resurgent pagan culture.

The urban development and the new economy prompted the proliferation of trade associations that, though often quoted in textbooks as 'guilds' or 'arts', in the official and Latin documents are usually called *Universitates*.

The Medieval universities were often the development of cathedral schools, but they were also often simply private schools where a more or less renowned master was teaching what he thought fit. Often, in major towns, like Paris, masters and teachers were housed and taught in specific quarters of the town. In Paris, which is, perhaps, the most famous and studied example, besides the cathedral school, a number of independent masters were apparently active since the 11th century and the concentration of schools and students on the 'Rive Gauche' where they subsequently stayed, was begun by Pierre Abelard, one of the earliest Medieval logicians, just in order to escape the authority of the chancellor of Notre Dame.

The first famous Doctor of Bologna was Pepo (about 1075), followed by the even more famous Irnerius (early years of the 12th century). We know of 'Doctors' from Bologna connected with the election of Pope Gelasius II (1118), which implies that there were there schools of laws. The Bolognese Doctors were, again, to supply the Emperor Frederick I Redbeard with legal arguments in his quarrel with the Pope and the Welf Communes, and received from him the already mentioned charter *Authentica habita* of 1189, which is often considered as the birth charter of the University. By secession from Bologna were born the Universities of Padova (1220) and of Siena (1321). Pisa is mentioned as a school of laws already in 1193, but was granted a charter by the Pope only in 1343, when dozens of Universities in Italy had already been born or were about to be and some had already vanished. Naples was created by order of the Emperor Frederick II as a by-product in his struggle against the Pope, as Bologna had now turned Guelf.

As for the other European countries, in Spain Salamanca was established in 1258, Lerida and Huesca sometimes in the 14th century (Lerida in 1391 had a school of medicine). Palencia had already vanished in 1263.

In France the University of Paris got its first privileges under a charter of Philip Augustus in 1200 and may be considered as wholly formalized by the papal bulla *Parens Scientiarum* of 1231. Secessions of students from Paris in 1229-1231 practically turned other schools of Northern France into universities. Schools existed in Montpellier in the 12th century, in 1181 Guillaume VIII octroyed a charter allowing every qualified person to teach medicine there (and there were some Jews) and approximately at the same time masters and students begun organizing themselves, receiving the papal charter in 1220.

In England schools had existed in Oxford for a long time, and it was the murderous conflict between students and burghers that prompted the King to grant a charter to the University in 1208. Cambridge, born around refugee students from Oxford in 1208, was chartered in 1318.

In Germany universities, first in the Rhine valley, appeared shortly after the French universities.

In general the development of the local autonomous institutions met with more resistance from the feudal clergy, whose power was centered in the towns and was thus directly challenged, than from the feudal lords, whose power was basically linked to their estates. Thus, like the guilds of the craftsmen and traders, so grew the *Universitates* of doctors and of scholars (sometimes separately and sometimes as mixed guilds) and they, as all other guilds, strove to get from the authorities self government and privileges. What we now call Universities, when they were chartered, were generally called either a *Studium* or a *Studium generale* which was for long subdivided into several *universitates* either according the ethnic origin of the different groups of students, or according to the different 'licences' that they could grant.

Each such university had its rectors, bailiffs etc. and only gradually and slowly they fused to become what we now call 'faculties'. The two main problems on which pivoted the tug-of-war between the local authorities and the 'Universities' were on one side the need for some public recognition of the academic degrees awarded, the other to escape control by the local authorities, especially in matters of taxes and fees and of penal law. These two goals prompted first the quest for Charters, possibly by the Pope or by the Emperor, as these two, being deemed to be, albeit theoretically, universal authorities, were deemed to be the ones who could authorise the grant of degrees equally valid everywhere (the *Licencia* or *jus ubique docendi:* the right to teach anywhere). The second problem was solved by claiming assimilation with the lower clergy (the simple tonsure as *Clericus* did not entail any perennial vote), so that both students and teachers were free from the local courts, could not be arrested by the police, could be judged only by the Bishop's courts and could appeal to the Pope, and, last but not least, were exempted from some taxes.

A major factor in the cultural renaissance of late Medieval times was that the same economic growth which prompted the flourishing of new schools, also prompted a generalized onslaught on both Islam and Byzantium, which paved the way for a flood of Greeks texts into our schools. As a matter of fact (and luckily as otherwise the wholesale destruction of ancient literature following Turkish conquests would have utterly destroyed our Greek heritage) many Greek scientific books personally annotated by famous Byzantine personalities now survive just in our Western libraries where they arrived either through Venice or through Sicily. Another large group of texts became known at least as Arab translations either through Sicily or, even more, through Spain. There translations from Arabic into Latin were already undertaken before 1000, but most work was done in Toledo after the Spanish reconquest in 1085.

Western Europeans were apparently craving for books: for the next one hundered years or so anything either in Greek or Arabic concerning philosophy, science or technology was good for the mill of our newborn universities: it has been estimated that between 1150 and 1250 not less than 3,000 and possibly as many of 5,000 books were translated into Latin, while many Arabic textes were translated into Hebraic and these too were used by Christians scholars as well as by Jews. The success of the Medieval schools in digesting this flood of information and ideas is truly surprising.

The result was, however that debates went on everywhere and everything was debated, especially as *quaestiones quodlibetales*, and this soon worried the Church. Indeed the Church was then facing a proliferation of heretic sects, some noble, such as the Valdesians, some serious, as the Cathars, some crazy, as the Luciferians.

Thus in 1277 the bishop of Paris hastily organized a committee to investigate the situation in the University, and the committee condemned as heretic 219 theses which had been debated in the schoolrooms of the Sorbonne. Some of these theses were openly anti-Christian, but some had been originally proposed by none less than St. Thomas Aquinas! Shortly afterwards the same occurred in Oxford, but it must be stressed that these condemnations and others which followed were of merely local implementation and neither the masters nor the pupils were punished in any way: scholars who abjured their theses had no further trouble, those who did not abjure simply moved to another University, where the condamnation had no effect.

The fact that the Universities of the doctors and scholars were much like guilds came to influence even some aspect of teaching.

On one side the fact that the schools aimed to provide degrees having a trans-national validity required a fair standardization of curricula in the different schools and this, in turn, required the standardization of the basic texts. On the other side, once bound to a standard text it was (and is) natural that the teacher tries by his comments, to teach the pupils just how he thought that the text was to be understood and used.

In order to standardize the texts, in each University a committee of Doctors chose for each text in the curriculum the best possible copy (*exemplar*), this was then disassembled into small packages of pages *(peciae)* and each *pecia* was given for copying to a different copyist who, by copying always the same pages, could attain a reasonable speed. Then the copies produced were checked for accuracy and bound.

Teaching went by alternating *lectiones* when the teacher read and commented (technically 'glossed', that is 'spoke on') the text, and *disputationes*, when the teacher answered the questions and debated the points raised by the students. From time to time, usually once a year, the teacher exercized himself in the *quaestiones* and in the *quaestiones quodlibetales* debating either some set problem or problems of his choice. The *quaestiones quodlibetales* were an especially important obligation for the holders of the 'licence' of the faculty of Arts aiming to a doctorate in either of the higher faculties of Laws, Theology or Medicine.

However, the *questiones quodlibetales* were also used by Doctors to prove themselves by discussing some very complex issue or arguing it from a novel perspective. The result is that almost everything new, important or personal of the work of the main scholars is to be found in the texts prepared for the *quaestiones quodlibetales*, while those corresponding with the ordinary *lectiones* are just more or less brilliant comments on other, preferably ancient, people's ideas.

An additional stimulus for the debate came from the common practice of charging two different teachersa of different affiliations to teach the same course, so as to prompt both the commitment of the teachers and the habit for meditation into the students.

A basic feature in Medieval schools were the *Summae* or *Summulae*: larger or smaller textbooks for the common student prepared by authors with access to important libraries. Indeed the average student, because of the cost of books, when graduated could not be expected to leave the *alma mater* with more than three or four books in his bag.

Several things in this description of the Medieval high schools may well sound familiar to today students. Such critics of Medieval thinking and schooling who have decried it as 'flat scholasticism' have often been just repeating the criticism of the 18th century' enlightenment, partly based on faulty information, partly of a biased anti-clerical attitude (however justified it may, at times, have been). They should have thought of what would have been their opinion of the great and thriving sciences of the 18th and 19th centuries, if these were to be judged only by textbooks!

As far as Greek-Arab sciences are concerned, in France and England (other countries, apart from Italy, were of minor importance at this time) they were assimilated and developed mainly within the faculty of Arts, which was preparatory to the higher faculties of Theology, Laws and Medicine. In Italy the doctorate in arts was normally present, but was of minor importance, except in universities such as Padua, where it simply merged into that of Medicine, which became a faculty of 'Arts and Medicine'. So there sciences and philosopy became a province of Medicine. The difference between Italy and the other countries was significant for future developments also in another matter: While in the French, English and German universities surgery was long ruled out of the medical curriculum and was oftern considered undignified for a physician to indulge in surgery, which was the domain of the 'barber-surgeon', in Italy surgery was a regular subject in the curricula of the medical faculties since their inception, and it entailed the teaching of anatomy, a fact which goes far to explain the splendid achievements of the Italian Renaissance anatomical school.

Again in the rest of Europe the degree of 'Bachelor' was very important and came to correspond with the *licencia* of Arts and the term is the same as that used in knighthood to qualify the junior warrior who followed the banner of a full knight while training for knighthood himself (the Doctors always claiming same rank as knights). In Italy, instead, where feudality was always somewhat shaky and there was soon a preponderant communal nobility of *milites* or *patricii* almost or entirely autonomous in respect to the feudal nobility, the Bachelor's degree is practically non existant, and we meet only with licensees and Doctors, the only real difference being that the licensee had not paid for the costly ceremonies accompanying the doctorate.

The overall result of these differences was that during the Middle Ages while almost all the scholars from outside Italy that we shall mention for their biological contributions, were teaching Arts, in Italy all scientific studies were firmly bound to the schools of medicine.

A we shall see further on, the great development of the Medieval universities gradually waned, teaching became somewhat sclerotic and research moved into other institutions. This was partly the result of the penetration into the faculties of a growing number of monks, especially from the mendicant orders: Franciscans and Dominicans.

Usually Dominicans were the more faithful interpreters of peripatetic teachings, while Franciscans, at least in the early times, while using the Aristotelian logics, were rather linked to the Augustinan thought and to the Christian neoplatonism of Scotus Erigena. This lasted for a very long time and we shall see how significant it was and perhaps still is.

Indeed, as an example, still in 1715 in the faculty of arts and medicine of Padua we find two chairs of theology, one *in via Sancti Thomae* and the other *in via Scoti*.

Once the great debates of the Middle Ages and of the early Renaissance were over and the Universities, partly also for political reasons, had settled into a conformistic routine, the best of scientific researches migrated into the framework of Academies and the Universities had to wait for the 19th century to recover a central position in research.

We shall see in the further chapters how central was the function of the Medieval and Renaissance universities in fostering the rebirth of biology.

A last point at least worth mentioning as a matter of curiosity concerns womens' education. In most of the Italian Universities at least, women were always accepted both as students and doctors, though, obviously there were not many of them. Let us remember a few: Betisia Gozzadini, born in 1209, was the first to hold a chair of Laws at Bologna; at the beginning of the 14th century, again in Bologna, both the wife and the two daughters of Andrea Calderini 'read' laws; one of them, Novella, who was teaching Roman Laws, was so beautiful, that she was obliged to give her lectures veiled, not to trouble the students. Again in the Middle Ages we have women teaching at the school of Salerno and, as we shall see, women were holding chairs of physics, philosophy and even human anatomy in the 18th century. Sometimes women even studied far from their home town, such as Pellegrina Amoretti from Oneglia (a little town near Genova), who got her Doctorate in both Civil and Canonical laws in Pavia in 1777 (and was praised for that in an ode by Giuseppe Parini, possibly the best Italian poet of that century).

It was only with the French revolution that in Italy women were barred from entering the University, to be admitted again around 1880.

APPENDIX TO CHAPTER IV

A section that may be useful in order to understand and evaluate several important problems as late as the 18th century, and I think, even contemporary debates

This appendix deals with some problems of logics that are of very general significance in scientific reasoning. In biology they had a special and self evident significance at times, but, even when scholars are not consciously debating them, are still important: for instance, they underlie the whole of any debate on systematics, both ancient and modern. To understand them is also necessary for a correct appreciation of the debates on embryology at least up to the middle of the 19th century. Moreover the stand that on them took some ancient masters framed their whole scientific and philosophical attitude and theories and, because of their direct or indirect influence, did in fact influence their followers. Since the 18th century, when medieval philosophy practically ceased to be studied by biologists (and generally by scientists), scholars bacame often unconscious of the, sometimes devious, ways by which these debates were still in the background of their own theories.

Had I thought that the following pages did not matter for understanding the developments of biology, I would, obviously, have avoided the trouble of writing them, but, as I am also aware that they might look as a sort of long digression, I have labelled these pages an appendix: as such the reader who is merely interested in 'the story' of biology, may just bypass them, and come back to them later on, when the story of the individual subjects of our studies, will make clear to him the purpose and use of these pages.

I think that for almost all of my readers the arguments of these pages will appear novel and, perhaps somewhat strange: indeed, even in such high schools where they are mentioned at all, the established tradition requires that they are dealt with an obscure language proper perhaps for initiates: sometimes the Latin jargon of Medieval logicians, more often than not by such old fashioned, but time honoured translations from Latin that are almost the best to make the whole unintelligible to the average reader.

It must be added that the problems dealt with in these pages were at times interwoven with important religious matters.

A matter of further difficulty is that in the past a number of apocryphal books were credited to great masters of Antiquity or to Saints and famous theologians. Such spurious texts have been eliminated by modern textual criticism, and so they have vanished from standard textbooks, leaving the reader puzzled as to how someone, say, for instance St. Thomas Aquinas or Sir Francis Bacon, got some ideas.

Finally, for reasons that I fail to understand, such teaching in history of philosophy that is provided in high schools, has for over a century completely ignored the work of the eclectic philosophers of the Roman times, though their work is quite relevant for the development of logical thinking, and the only character that they deem worth mentioning for the first centuries of our Era is Plotinus. Neoplatonism, as we shall see, had a great impact on many aspects of the development of sciences in spite of being a totally unscientific doctrine, but this does not justify the total obliteration of all other schools.

On what is science

While everyone is told that Aristotle was the first to make a systematic study of logics, texbooks seldom mention that he was also the first to study in his books *De anima, De memoria et reminiscentia* and in the *Parva naturalia* what we now call psychology. If anyone studies comparatively both the *Organon* and the books on psychology, he will easily perceive that Aristotle was fully conscious of a basic problem: "what is Science?" Aristotle holds that logic, that is the basic content of the *Organon,* is just an instrument useful to attain knowledge, but that logic may be merely be applied to the study of propositions or statements that may be said about a given subject.

Aristotle agreeded with Socrates and Plato that the object of science are general propositions, those that we now call principles and laws of science. Statements concerning only individual peoples or events, though they can obviously be either true or false, are not the object of scientific investigation. Another basic principle for Aristotle is the 'principle of no-contradiction': a statement or an ensemble of statements to be a scientific statement must never imply either an internal contradiction or contradiction between two of the propositions in the ensemble. Contraditory statements were termed *absurdae* by medieval scholars, hence the 'theorem of pseudo-Scotus': *Ex absurdis sequitur quodlibet* (= from contradictory premises one can darive any conclusion he likes), that is, one can not conclude anything with certainty.

Aristotle was aware that, in order to logically analyse a proposition to verify its scientific validity, its terms must be properly defined. Such definition must be 'according gender and specific difference', a generic quality being a sufficiently comprehensive one, while the specific difference must be a quality or a set of qualities which are inclusive of the object defined and exclusive of any other: Thus, as an example, let us take the statement 'the Moon is a celestial body satellite of Earth' here the 'gender' is 'is a celestial body', the 'species' is 'satellite of the Earth': to say that the Moon is a celestial body is an extremely comprehensive statement, while to say that it is a satellite of the Earth is both inclusive as the Moon happens to be the a satellite, and exclusive, as anything which is not a satellite of the Earth, by definition is not the Moon.

This pattern of definition was clearly stated by Aristotle and is basic in all the following development of sciences; it was precisely recalled by Linnaeus as a premise to biologic systematics, and still is embodied in the International Rules of Nomenclature both for plants, animals and bacteria.

It is obvious that, should we refuse any of these principles, then Aristotelean logic would not work. For instance, should we agree either with Protagoras' principle that reality is a purely contingent phenomenon and that it is identical with sensation or with Heraclitus' instability, then a science as conceived by Aristotelean logic becomes impossible, as the same thing could either be or not be at the same time according different observers or according the times of observation.

However Aristotle, besides being a logician, was also, as we saw in chapter II an excellent naturalist and observer. So in the *Metaphysica* and in the *Topici* he studies the possibility of a science of changing things, especially of those changing in time. He paid attention too to the problems posed by hypothetic propositions, for example, should I say 'Tomorrow there will be a battle' can this statement be said to be true or false?

Finally Aristotle was perfectly aware of the fact that the qualities or attributes (that is what defines a species) do not really exist as realities separated from their subject (substance). He writes: "health exists when the men is healthy, and the figure of the sphere of copper exists just when the sphere of copper exists" and not much later he writes a statement which is crucial for the naturalist: "There is no reason to believe in the existence of the 'Eidos' (which we have seen may be translated both as idea and as species): man is born from man" and then "Every quality does not exist by itself, none of them may be separated from the 'ousia' ... qualities appear to exist only because under each of them there is an individual being ... and this is the Substance, that is the individual bearing its various attributes. Good, sitting, do not mean anything without this substance. It is thus clear that the existence of qualities depends ultimately on the existence of the sunstance ..."

On the other side should, as Aristotle himself holds, 'substance' be an indeterminate 'apeiron', then science is not concerned with substance. But is it possible to have a science of something that does not exist, that is of characterizing qualities separated from the substance which is characterized? To extricate himself from this quandary Aristotle, as the Greeks often did, employs a verbal trick. He says that we should distinguish between 'dynamis', potentiality, and 'act'.

Potentiality is what may be, but presently is not, for instance the absolute red colour, or, perhaps, a complex 'eidos' such as 'horse'. The 'act' is that that actually is: the red pot or an individual horse.

Therefore, according Aristoteles, the indeterminated substance is in itself only a potentiality, which becomes reality when unites with the 'eidos'.

Moreover it is necessary to point that in the Stagirite's concept, the 'genus' is, when considered in tandem with the 'species' or Eidos, a substance *sui generis*. In fact real objects, and let us take as an example living beings, are the members of a sort of series: animal-mammal, mammal-carnivore, carnivore-canid, canid-wolf, wolf- an individual wolf in a pack. At each successive level the comprehensive member of the tandem is the genus and the circumscribed one is the species. Conceptually, therefore, in a way, 'substance' becomes more and more real the more it becomes individualized.

On the other hand Aristotle conceived the universe as both eternal and unchangeable, therefore this 'materialization' of sustance is not a historical process, it merely happens during the development of every individual object, both living or not.

However, this concept leads Aristotle into another difficulty : had he admitted of the reality of the universals only in the single individuals sharing of any given quality, this would have forced him to agree with the individualistic position of Protagoras and thence deny the very possibility of a science of 'universals'. He had to find an escape.

Given the age, his proposed solution is extremely brilliant: he concedes that it is conceptually permissible to conceive the process of individuation as progressing by separate steps, given such premise, it is possible a science of potential universals, considered as factors of the 'steps' by which the real thing comes to be.

As we shall see these problems were extremely relevant in the framework of a general theory of sciences in the scientific debate until during the 19th century physicists begun to propose entirely different views on what 'substance' is (and finally practically did away completely with it). Biologists however still have some difficulties to come to terms with the physicists view of 'matter'.

Aristotle, with his usual intellectual honesty, had openly avowed the difficulties and limitations of his logic, which he had tried to solve, and these were soon seriously tackled by the second Stoa.

The Stoics, rather than try to reconcile the idea that only single individuals really exist and the principle that science may deal only with generalities, tried to build a logic and a science of the particular.

They took as a strating point the tenet of Antistenes, a pupil of Socrates, that only individual objects really exist. The stoic conclusions are therefore 'nominalistic', though not as extreme as that of the Epicureans, who maintained that universals are mere sounds ('fonai') or as Roscellinus and other medieval nominalists, that they are mere words: *flatus vocis.*

The stoics were rather 'terminists' as a Medieval thinker would put it: they thought that the concepts underlying the names, such as Dog, Goodness, were real in a special way and of a different reality from that of the individual objects, of which names were the 'signs' (a concept that is extremely important in the current semiotic debates). Thus science was possible, as it aimed to establish rules that go beyond the transient reality of single individuals.

However it was clear that, under these premises Aristotelic syllogistic logic was inadequate and that it was necessary to solve the problems that Aristotle had pointed out and left unsolved when he had listed his famous 127 'non syllogistic' propositions, for which his logic was inadequate to establish whether they were true or false.

The stoics added to the Aristotelean syllogisn ('cathegoric syllogism') based on the relationships of comprehension and extension, the concept of 'necessary connection' or more precisely of 'obliged connection' ('hypothetic syllogism').

This was framed in the following basic schemes: (1) if A is also B must be, but as A is, then necessarily also B is. (2) if A is also B must be, but as B is not then also A cannot be, (3) if A is not, then B must be, but as A is, then B can not be. (4) Either A or B are, but as A is, then B cannot be, (5) either A or B are, but as B is not, then A must be.

This is a typical dichotomic logic which, just as Aristotelean logic, assumes contemporarity, and is, therefore inadequate for the analysis of processes or propositions assuming a span of time.

The Stoics also examined in their 'theory of signs' the necessary connections of terms or evidences: a scar certifies a previous wound, smoke is a signal of fire. This kinds of connections open different problems of temporal relationships, as the scar presupposes the wound, but the wound is no more when the scar is.

Now the theoretical premises for analyzing this kinds of logical connections were found by the Stoics in their metaphysics: these assume that the world is a network of interlinked chains of causes and effects, involving the whole natural world. Diodorus, indeed, states that the possible is indistinguishable from the real and that anything that could possibly happen must have, in fact happened.

We may well forget about this 'fatalistic' attitude of the Stoics as irrelevant for our purposes; we must instead undeline that they followed Aristotle in taking empirical experience of matters as the test of truth for one of the members of their 'hypothetic syllogism'. Truly Sextus empiricus, who is our main source on the logics of the stoics, says that they had an additional standard for truth, but does not tell us what it was. Anyway their absolute naturalism and pantheism gave the Stoics total faith in empirical experience.

Such faith in experience was much less with the Epicureans, and the Neoacademics had no faith at all in experience. Neoacademics, such as Arcesilaus, remark that empirical perceptions and observations are no guarantee of truth: an optical illusion, for instance, is a sensation, but it produces a mistaken judgement. Therefore the acceptance of some empirical evidence is the result of its rational evaluation, an operation that requires for the man who is to judge that he has some theoretical standards to apply to the individual case, and these are 'universals'. But these, in turn depend on the validity of previous experiences and so on *ad infinitum.*

At this point the basic problems of philosophy and of theory of science were clearly stated and it seemed that there was no solution of them.

In the following centuries such philosophers who refused a trascendent, metaphysical, source of truth have suggested many theories of knowledge and standards of truth, or at least of probability, but all of them proved weak in face or rigorous logical criticism.

Thus Baconian inductivism, that has been almost standard practice in science for the last three centuries, was soon criticized by Hume. In recent times Karl Popper's has proposed his theory of falsifiability, which is currently used by many biologists: the theory basically assumes that though it is impossible to 'prove' a theory, yet it is possible to show when a theory is false, and, therefore, supposing that we were able to reduce the issue to two alternative theories only, by falsifying one we would, in fact, prove the other, a 'discovery' that had been done by the Stoics twenty centuries ago! Unfortunately also this theory has been elegantly falsified!!

Practical experience shows that usually, but not always, inductivism works fairly well for normal research routine; that Popper falsifiability is extremely useful to get rid of some wrong theories, that a bit of intuitionism is usually found at the core of major advances in research and that a sound dose of generalized scepticism is always good, so that the ideal is a well proportioned cocktail of all of them.

Later developments

Anyway, coming back to our problems, we saw in chapter 3 how, beginning in the 2nd century AD an increasing amount of mysticism creeps into the writings of Pagan thinkers and, obviously, much more in those of Jewish and Christian writers.

As Jewish thinkers were relevant for the development of Western thought only after 1,000 AD, we may forget about them for the time being.

Pagan mysticism mainly expressed itself in two 'philosophical' schools: Neopythagoreans and Neoplatonists.

Neopythagorism is still poorly known and it is not clear if it had any influence on Medieval thinkers, possibly mediated by mystic mathematicians of Arab or Persian nation, like the sufi 'Umar Khaiyam, or, perhaps by some Jewish cabalists.

The lasting influx of Neoplatonism is, instead, quite clear.

The platonic 'eideia', which are obvious universals, are conceived in classical Neoplatonism as being a product, an hypostasis or an emanation of the highest God and, in turn, produce the actual universe (several neoplatonists, like the Emperor Julian the apostate, worshipped the traditional Gods in their aspect of astral Gods). In pure Neoplatonism the Platonic image of the Demiurgus, imagined as a god-like entity who creates things shaping them in the likness of archetypical ideas has but little place. The Christian gnosis of Neoplatonic pattern sees the Demiurgus as the Christ while, on the contrary, pagan gnosis conceived of an evil demiurgus who tries to squeeze into the chain of successive emanations in order to take the place of the supreme God. In either case all these thinkers consider universals as absolutely real.

Pace most textbooxs, Plotinus, the creator of Neoplatonism is a non entity from the logic-scientific standpoint. His assumption, and obviously he has no evidence for it, is that all created things have an impulsion to reunite with their creator. They all aim to climb back along the descending chain of successive steps in creation. Each such step is the passage from one universal to one or more less comprehensive universals. Each such descending step is considered as marking an increasing inferiority, a degradation in respect to the previous, more comprehensive step. Thus the soul of every man wishes to climb back the chain of the universals until it may merge back into its prime source: God.

Such a mysticism may be interesting from a purely religious standpoint, but for our purposes its main significance is that it could easily interact with the Hermetic tradition. This considered that there was a parallelism between all transformations that could be seen either in nature or in the crucibles of the alchemists and of the goldsmiths and other craftsmen and the transformations and purifications that the soul of the 'philosopher' undergoes when he becomes able to understand the essentials of things beyond the appearances of nature or of written texts.

In the early centuries of our era both Christian and Jewish thinkers, just as happened later with a number of Islamic thinkers, were either diffident or openly hostile to philosophy. However, luckily for Neoplatonism, it was basically adopted by three most notable thinkers. These, albeit quite different among themselves, were able to turn it into a spiritual influence lasting for centuries. These were Proclus (410-485), Simplicius (c. 529) and Boethius (480-c. 526), who were almost contemporaries. Another capital factor is the influence that Platonism had on St. Augustine and, thanks to his great authority, on the whole development of Christianity.

The three above mentioned philosophers aimed to a philosophical synthesis based on solid historical foundations, and, at least under this last aspect our debt to them is immeasurable: indeed most of what we know on the lives and works of a large number of Greek philosophers and scientists is preserved in their books. Had the writings of these three authors perished, we would hardly know anything but the name of most Greek thinkers.

All three are usually considered to be basically peripatetic scholars, and, as far as the formal framework of their ideas is concerned, it is Aristotelian, but this is an aristotelism that precisely on the matter of universals is strongly tinged by Neoplatonism. Both Proclus and Simplicius belong with the eclectic trends of the last years of the pagan Academy (we know that when Justinianus I closed in 529 this last stronghold of Paganism, Simplicius for a while emigrated to Persia). Boethius, instead, is a Christian so much influenced by Neoplatonism that several scholars have doubted, I think wrongly, that he was really a Christian.

As far as the problem of the universals and of the reality of general concepts Proclus (within the framework of a basic and capital discussion on geometry) holds to what was to become the classical Thomist definition (Thomism is the term for the ensemble of the philosophy-theology of St.Thomas Aquinas, which for almost seven centuries has been the accepted basis of Catholic doctrine); St Thomas received it as mediated through Avicenna (see chapter 5) and St. Albertus Magnus (see chapter 6). Proclus and his followers assume the universals to exist *ante rem* (that is before the existance of things) in God thinking them, *in re* (that is in the actual things) as the material concretisation of the 'form' or 'eideia'; and *post rem* (= after the material existance) in the mind thinking of the phenomenon. Thus the universal does in fact exist, but is merely a 'mental instrument'.

This was the solution actually preferred by most scholastic philosophers, but in the medieval debate rather than the text of Proclus, which was hardly quoted, it was the writings of Boethius that were of basic significance. Among them the text chiefly discussed was his comment on the *Isagoge* of Porphyrius, itself a comment on Aristotle's 'categories'. Porphyry clearly poses the problem of what genus and species are: Are these mere mental images or have they any empirical reality?

Basically Medieval thinkers had to choose among three pairs of possibilities, and they had to choose such a way as to satisfy both logics and Christian orthodoxy:

(A) as far as universals were concerned the alternative was beteween pantheism and theism whith a God having personality and will;

(B) between the concept of an individual immortal soul and the acting intellect as it may be deduced from the study of book 3 of the *De anima*;

(C) as for the the connection between matter and form the consequences arising from the necessary acceptance of the story of creation.

The problems of the universals was therefore crucial for the solution of the other problems, although the obligatory choice in the B alternative, was itself conditioning the possible choices for A.

Thus the problem of the universals was central to medieval thought. Here, while outside the universities we may notice a preponderant and diffuse influence of the strong realism of Scotus Erigena, who worked at the court of Charles the Fat, joined with the equally strong realism of Arab thinkers, within the universities nominalism was the predominant trend, and while it was assumed to be Aristotelian, but was, indeed, very close to the stoic tradition.

Scotists-Arab neoplatonism had its leading figure in Ramon Llull (Rajmundus Lullus). With Llull the universals and especially those which may be thought as God's attributes are *res immutabiles* (= unchangeable things) which, like the roots of a tree merge into the stem, thus materializing the extant thing. Such stem is, however, comparatively indifferentiated as it materializes, but immediately if divides itself into material *genera*, which in turn subdivide into *species* distributed and subdivided like the leaves of a tree, all similar among themselves, but never identical. This realism applies to material things as well as to moral entities, as into them merge and then branch off again the *virtutes,* that is the powerful qualities of the different universals. Thus, for instance the 'morality' of any individual will be made, in each one, of different amounts of 'justice', 'love', 'honour', 'courage', 'pity' and so on. Within this basically Neoplatonic lay out, just as in Plato, universals can really exist *ante rem, in rem, post rem.*

The positions of nominalists are more varied and range from extreme nominalism, which is credited to Roscellinus, who goes back to the Epicurean positions and holds that universals are mere *flatus vocis*, to authors like Abelardus who holds that they are potential entities, *ideae ante rem*, which really exist *(in actu)* only in the things (*in res)*, but that are permissible to the philosopher as pure abstactions, *positae in nudis intellectibus.*

Intermediate positions are more close to Aristotle: they hold that the same attribute or quality (or set of qualities) is at the same time both a universal and a particular: it is a universal in so much as it is common to many, but it is also a particular as, in so far as it pertains to an individual being, it is a *unicum*.

It is obvious that there is either an interaction or, at least, a link between the universals taken as individual qualities, attibutes, and 'substance' or 'matter'. The term itself '*substantia'* literally means something that stands underneath (the attributes) and for Christian thinkers it is important to decide whether substance is an indefinite 'apeiron', as supposed by Aristotle, or a 'quantity' as in the Platonic tradition. Christian scholars received the Aristotelian tradition as mediated by Ibn Gabirol (Avicebron), but its consequence was to credit any kind of being and not only those that we usually call material objects, but also such beings as angels, with some sort of substance, however indeterminate, which was needed as a sort of 'support' for qualities, as qualities can not really exist alone. So an angel would be composed of substance and qualities, just like a horse, the difference being that the angel has, for instance, no weight, while the horse has weight, but not reason, and so on.

Curiously enough this theory was attributed to St. Augustine and it was not perceived that it necessarily lead to pantheism (and, indeed, that of Spinoza is the direct heir of it). This position had its main champion in Duns Scotus. He opposed this concept of substance, termed *materia primo prima,* to the Platonic-Tomist thesis by which matter had in inherent quantity, *materia signata,* and was proper, exclusively proper to each particular being, and that Scotus called *Materia secundo prima.*

A modern naturalist who may have the patience to think a little over these quarrels, which apparently, because of the language they use, are entirely void of interest for us modern, will, nevertheless be surprised to find here the hard core of current debates on the general principles of systematics and of systematic methods.

On the other side, if you just think it over, you will realize that he is in error, who thinks that this type of discussions is irrelevant in times of experimental sciences. Experiment requires a previous question which we expect to be answered by its results. But the question is necessarily a hypothesis, and this is just a hypothetic universal: you can not make hypothesies about some single event concerning a single individual. Just to make an example: until 1600 no one doubted that spontaneous generation might occur. Even great naturalists such as Aristotle, though they believed that it was rare and limited to only a few groups of animals, did not doubt its existence, and, therefore no one thought of making any experiment to verify it. As soon as some new evidence allowed for the hypothesis that every organism must necessarily come from another organism (Harvey was probably the first to maintain it in explicit terms), within a few years Redi first and thence other experimenters, prompted by the apparent increasing complexity of the problem, set to work to verify which of the two alternative hypotheses was true.

It must be plainly admitted that 'no universals, no science'; Plato, Aristotle, Duns Scotus, St. Thomas and all the others were quite right.

A further factor of great relevance in making the problem complex was the problem of Revelation of the Biblic-Evangelic text (for the Muslims of the Quran). Here the arguments were interwoven with the Hermetic tradition.

Assuming that the Bible was the word of God, what was written there just meant what was the literal meaning of each sentence, or was there some further significance?

Some years ago the coffin of a king of Judah was discovered near Jerusalem and it bore an inscription that, beside the name of the king, included some furher words. Now ancient Hebraic did not write or otherwise mark the vowels and, as a consequence, the scholars started to dispute as to which vowels should be inserted to give the correct meaning to the inscription. The problem was hotly debated for a while, until it was found that there was simply written: 'Do not open'!

The Hermetic tradition tells that prophets and sapients, possibly even the Gods, never utter messages which should be taken at their literal value, the sacred message is, indeed, always supposed to have a double significance: one literal and one symbolic, and that the really significant message is the hidden one. As brilliantly argued, for instance by Umberto Eco, Hermetic 'reading' allows a sort of unlimited 'drift' in the interpratation of messages, and that, therefore it requires a 'key' for its correct interpretation (we shall meet with 'keys' well down into the 18th century, and whenever a book is titled *Clavis* (= key) one is justified to suspect a more or less covered hermetist). Obviously everyone has his own pet 'key' and gets different interpretations, which may offer the good excuse to slaughter each other.

Thus the 'Cabbala', the 'key' largely using combinatorials created by Spanish Jewish scholars of the Bible around the end of the 13th century, was incredibly popular also outside Jewish communities, and there strongly interacted with the Neoplatonic tradition.

We shall leave here the development of these problems as, as far as their impact on biology is concerned, we shall discuss them in their proper context. A brief mention must, however be done of the 'terminist' solution proposed for the problem of the universals, and mention will also be done to the pioneering work of the Italian naturalist-philosophers Pomponazzi and Telesio.

The best formulation of terminism may be found in the writings of Occam. Occam holds that only individuals really exist and that the Universal is created by our minds; for instance an individual animal or a population of animals are real, but the systematist's 'species' is our abstract creation. However the *signium* (= mark) is that by which we recognize a particular thing and becomes the symbol for it; this exists in nature, *naturaliter,* and has an objective value; however the *intentio secunda* is the abstact universal, that is the general qualitiy, such as 'red' or an ensemble of qualities, such as the 'idea of horse'. These are concepts that we ourself created and do not directly depend on the things themselves. Universals do not exist *naturaliter*, they are instead *secundum institutionem voluntariam* (= created by our will). Nevertheless science is still possible as, though only individuals concretely exist, the abstraction, directly rooted as it is onto some qualities of each individual, is a univìversal derived from the particulars, therefore, as a universal may be the object of sciences, and yet it is rooted and in some way participates in reality.

Both Pomponazzi and Telesius precisely reversed the platonic-scotist model of Lullus, where the divine 'virtues', whith their perfection, merge to materialize the extant things and, as they are variously distributed, they thus cause an infinite variety of different assortments both for quality and quantity of the different 'virtues' or better 'divine attributes' in each material or spiritual object or individual. Ponponazzi and Telesio first assume a double standard of truth, that is they completely separate everything that concerns religion and revelation from the field of science. Whether this was merely expedient to their attempt to avoid charges of heresy and blasphemy (rather unsuccessfully) or whether that corresponded with their real beliefs is not known. Anyway, while professing themselves to be good Catholics, as philosophers they advocated entirely opposite ideas. In philosophy they both tried to develop a theory of science based only on sensation and on the individual phenomena and which could all the same reach normative levels.

Their attempts are rather crude and their results are close to those of the Epicureans, however they are significant as forerunners of Sir Francis Bacon's inductivism.

The odd thing is that inductivism in sciences never met with seriorus resistance: no one had ever doubted the value of empirical or experimental evidences and the, so called 'systematic doubt' was the natural result to the growing amount of information, which was continuously providing some evidence either contradictory with that already available or with its interpretation. Thus new adjustments and checks on existing theories were plainly always needed. On the other side inductivism was never able to replace entirely the more ancient traditions, and that is self evident even in the writings of Bacon himself and with the enthusiasm with which he ransacks, in order to exemplify his theories, the writings of typical 'magicians' in the old tradition, such as Gianbattista Della Porta.

Anyway scientists, and especially biologists, more and more had a tendency to overlook the historical background of their own interpretations and thus continued to carry on in their writings subtle bias, centuries old, of which they were not conscious.

Some responsibility for this development certainly belongs to the schooling systems.

Apart for Italy, where in late medieval times there were some attempts to organize some elementary schools funded and controlled by the town's administrations, and this was, anyway, done on a limited scale, popular education had always been monopolized by the clergy and by the monastic orders. Moreover since the middle 16th century even in private tutoring, the wealthy, who previously had usually employed lay tutors, more and more relied on the family's chaplain both in Catholic and in Protestant countries.

Christian churches had long been cleverly teaching that Pagan art, history, philosophy and, above all religions, though they might be admired, were not to be taken seriously as far as their religious content was concerned. When children begun to study pagan thinkers, they were ussually sufficiently 'immunized' from any radical doubt against the Jewish-Christian revelation. Both the Reformation and the Counter-reformation were also worried, and consequently acted, against the danger of a Pagan rebirth, as it appeared possible in the upper classes of Italy in the transition between the 15th and 16th centuries. Thus, partly as a follow up of the old tradition and partly because neoplatonism was the only pagan 'philosophy' compatible with Christianity, it was preserved as an important part in the cultural-religious education. We shall see how its influx is notable in the development of various trends in analysis, especially concerning evolution.

As it would be interesting to examine how these different influences acted on some important biologists of the Renaissance or of the 17th century, and we shall try to do precisely that in the following chapters, though, unfortunately, none of the more relevant scholars has been sufficiently studied to allow for a complete appreciation of all aspects of the problem. The best approximations for the appreciation of these probelms are either Newton or Leibniz, and, at present, Newton is the better studied of the two. A brief digression on him may be useful for the better undersatanding even of biologists.

Many manuscripts by Newton have been published comparatively recently and several were destroied in the 19th century as their owner though that they would detract form the conventional appreciation of the great Sir Isaac.

Troughout his life Newton was able to fuse his deep religious faith, which was apparently a very conventional and fundamentalist matter (though it has been argued that he was covertly an Arian), with his advanced scientific research.

In the European educated media at the end of the 17th century it was impossible to ignore the problems arising from the interaction of increasingly efficient methods of observation and of rational analysis of the evidence on one side, and the Biblical text on the other. To Newton, as to the vast majority of scholars of his age, the main problem is neither that of a double truth, as developed by Medieval Averroists, nor that of 'reconciling' scientific evidence and faith. To them both the Biblical and Evangelical texts were unquestionably 'true' both in their substance and in their wording, but, as they did not match here and there with empirical evidence, it naturally followed that, true to the Hermetic tradition, they must have been written in a symbolic language, to understand which it was necessary to find the proper 'key'.

We have said that this assumption was a solid and honoured Medieval tradition, and this not only in Christian media, but also for the Jewish and Muslim traditions. The result had been a vast cryptic literature which was often written such a way as to be even more obscure than the evidence that it aimed to explain.

Cartesius (Descartes) had been acutely conscious of the fact as in his youth he had, naturally vainly, attempted to get in touch with the supposed rosicrucian wise men. Cartesius' philosophy, mathematics and physics were all a radical and openly avowed attempts to eliminate all traces of Lullism form all fields of science and philosophy.

In England the ideas of Cartesius were considered with interest, but England being a protestant country, also with some diffidence. Several people were worried that Cartesius' brutal opposition of *res cogitans* and *res extensa,* implied a too materialistic view of the world. Such was the attitude of Newton: interested in mechanicism, but a little diffidently.

The development of the physico-matematical thoughts of Newton was slow (he was also a perfectionist and always afraid of all kinds of criticism) and interwowen with pauses when he studied Biblical chronology.

Most of learned Europe and Newton himself expected 'the end of times' and the beginning of 'the New Kingdom' within a reasonably short time (Newton thought that he had calculated some alternative probable dates for it and his last option for the second coming of Christ was 1948). The 'Magicians' or 'Natural philosophaers' work was considered as a sort of dutiful moral preparation for a better world where the biblical prophecies would have been fully clarified and were to find their fulfilment.

Thus the complex activities of Newton, including his long and painstaking alchemical researches, may, in a way, be considered as a sort of perpetual prayer.

Both the failures and the supposed achievements of his alchemical researches, gradually brought Newton further and further from the crude Cartesian mechanicism. Meandering through brilliant and accurate experiments and meditations of the obscure texts of Sendivogius and company, Newton attained the basics of the classic concepts of 'mass' and 'force', where 'mass' is conceptually rather close to the *materia signata*, while the 'force', being the *Principia mathematica* in Latin, is naturally called by the traditional and traditionally ambiguous *vis*. It is indeed one of the merits of Newton's book to have finally done away with the ambiguities of old, which combined under the same name the concepts of 'attribute', 'form', 'acting force' and others. It is just by using his redefined concepts and some previous hints and intuitions both his and of previous scholars on magnetism etc. that he built his great synthesis: Newtonian mechanics.

We have such an ingrained habit of living in a Newtonian world (as his mechanics are perfectly adquate for our daily experiences), that its is difficult for us, after three centuries of tuition based on the words of the *Principia mathematica,* both to imagine the world as it was seen by people before Newton, just as we still feel uneasy with the two new worlds of relativistic and quantum physics.

But what is significant for our argument is that to Newton himself his concepts were 'Theoria' exactly in the religious Aristotelic meaning of 'contemplation of the Gods'. Indeed Newton needed, for his physics, the new concept of 'absolute space', a fixed reference system, as opposed to 'Relative space', which is but the manifestation of the relationships temporarily obtaining between the observed objects. For a while Newton called his 'absolute space' *Sensorium Dei*, thus provoking the outcries of the equally pious Huygens and Leibniz (who were both relativists) who charged him of being 'impious', as, being the absolute space measurable, Newton claimed to be able to measure at least some attribute of God!

Equally clear is the furious reaction to the *Principia* by Cartesian mechanists who feared the resurrection, in new shapes, of ghosts that they thought to have laid forever, such as actions at distance by celestial bodies.

Again partly depending on ancient traditions are the anti-Newtonian arguments of Goethe, Geoffroy St. Hilarie or Ocken on one side, of Cuvier on the other: problems that we shall discuss in due time.

This may have been a long digression, but I think it useful in order to understand several problems which sometimes quite clearly and sometimes obscurely, underlie much of the biological debate since the end of classical times.

CHAPTER V

The Islamic culture and the Western world

SYNOPSIS OF THE MAIN HISTORICAL EVENTS AND OF SCIENTIFIC ACHIEVEMENTS

Isidore of Seville (c.570-636), the venerable Bede (673-735)
622 Hegira: Mohamed flies from Mecca to Medina.
640 the Arabs conquer Alexandria, supposed final destruction of its library
642 battle of Nihawand: the Arabs crush the Sassanian empire.
752 battle of Poitiers: Charles Martel stops the penetration of the Arabs in France.
763-809 Caliphate of Harūn al Rashīd ibn al-Mahdī: Culmination of the Abassid power , Baghdad is the most splendid capital of the East
1085 the Christians capture Toledo.
1095 beginning of the 1st Crusade (1096-1099).
1258 the Mongols capture Baghdad, destruction of the Abassid caliphate.
Jabir ibn Haiyan 8th century, Māshā'allāh +820, Rhazes 865-925, Alhazen 965-1039, Abū 'Alī al-Ḥusain ibn Sīnā (Avicenna) 980-1037, Al Biruni 973-about 1050, Al Ghazaali (Algazel) 1058-1111, Abū 'l-Walīd Muḥammad ibn Rushd (Averroè) 1126-1198, Rabbi Moses ben Maimon (Maimonides) 1135-1200, Al Farabi 870-950, Omar Khayam + about 1123
Pierre Abelard (Petrus Abelardus) 1079-1142
Translators: Armand from Carinthia, Gerard of Cremona etc. c. 1132-1187, Adelard of Bath c. 1100-c. 1200

Islamic biology

When we try to follow the development of Medieval thinking on matters of natural history, there is indeed a problem for any student unfamiliar with linguistic and archivist research and who does not have a lot of leisure: for most texts both European and even more Arab or Persian there are no recent translations or even editions.

Any elementary history tells us that the Arabs, and before them the Persians largely absorbed Greek science and that it was precisely mostly by the intermediary of Arab commentators that Greek science was to exert a powerful influence on Western thinkers of the late Middle Ages. However, when we try to check the sources for this tradition, we find that, even for what concerns basic thinkers of the Islamic culture, such as Avicenna, several of their books both of medical or biological subject were never translated or that such Latin translations that were prepared were never printed and, though to some extent used by the European scholars of the 13th-14th centuries, thence laid completely ignored in the old libraries.

Much the same is true of the probable influence of the Jewish-Spanish culture: both the scholars of the Talmud, and the highly reputed Jewish physicians did indeed write occasionally on animals and plants and therefore are sometimes quoted. Such are the talmudists Saadia (882-942), Rabbi Hananel ben Hushiel (11th century), Rabbi Gerson ben Juda (who was teaching in Metz during the 12th century and should not be confused with the famous philosopher Gersonides = Rabbi Levi ben Gerson), Hai Gaon, Rabbi Solomon Ben Isaac (also known as Rushi of Troyes), Shem-tob ben Joseph ibn Falqera, Jacob ben Mahir (who in 1302 made a translation of Averroes).

Kalonymus ben Kalonymus wrote a treatise on animals, which is a mere paraphrase of some Aristotelian treatises. Among the physicians we are remembered of Assaph (9th century) and Sabbatai Donnolus (10th century), this last certainly having some influence on the early development of the School of Salerno, but I must say that I have failed to find out what they actually wrote in the fields of botany and zoology. In fact I only found that the often mentioned *Safer ha-yaqan* by Donnolus is a mere list of antidotes.

In the end I was quite doubtful whether the traditional accounts are really reliable and I think that, indeed, the tradition is valid as far as medicine, mathematics, physics and alchemy are concerned, but that the real importance of such non-Christian authors for the late Medieval scientific renaissance was almost irrelevant as far as their transmission of Greek science is concerned. Apart for a rather brief period, Europeans were rather interested in what the Arabs themselves had to say in the way of comments or additions to the ancient tradition. Indeed the original Greeks texts, with few exceptions, rapidly became available in their original language.

What is clear is that the bulk of the philosophic and medical Islamic culture was actively studied in Europe, just as there was a significant reciprocal exchange of traditions and influences in the field of courtly literature (the ‘courtois’ culture).

Such books as were written in Persian were almost completely ignored, except when they became available as Arab translations. Thus it happens that European scholars debated at length some of Avicenna’s (Ibn Sina) theories, while completely ignoring others.

By the death of the Prophet (632) the religion he had founded had spread all over the Arabian peninsula. Thence begun the great expansion of Islam. Under the leadership of the first caliphs who succeeded Muhammad, the Arabs conquered first the neighbouring countries of the Middle East: Babylon, Syria, Iran; then they spread through Egypt and to all of North Africa, captured Sicily, Sardinia, the Balearics, almost the whole of Spain. By the end of the 8th century the Arab rule had reached almost its maximum limits in the Mediterranean. Muslim strongholds existed in Provance and along the coast of Southern Italy.

Arab rule had a varied destiny: the Emirate of Bari and the settlement on the Garigliano, in Southern Italy, or the Proveçal strongholds were ephemeral events lasting a few dozens of years, the Spanish conquests lasted for centuries, in Africa and Asia it is still there. In the Middle East Muslim influence advanced and retreated repeatedly in face of Byzantine resistance and of the Crusaders onslaughts. Finally once Turkish rule was able to more or less unite all the Muslim word west of Iran, it slowly advanced through the Balkans until the end of the 16th century, when finally checked on the sea at Lepanto (1572) and under the walls of Vienna (1529 and 1683). Since then Muslim power was on the wane until, at the beginnings of the 20th century, almost all Muslim countries were under the direct or indirect rule of European powers.

However, while in Spain cultural contacts between Moors and Christians lasted for centuries, in the Middle East they were rather ephemeral: practically the brief span of the Frankish kingdom of Jerusalem and of the even more ephemeral Frankish principalities around it (1099-1187). Otherwise all contacts were practically mediated through the Byzantine Empire, who acted as a cultural philtre. By the time of the final crumbling of Byzantine power, European culture had long since passed the times when it was open to the Arab cultural influence.

During the early phases of development of Byzantine culture, after the final separation from the Western empire (this being said with the proviso that, in fact, Byzantine cultural strongholds lasted for centuries in Italy and, until the Muslim conquest, in North Africa), the Greek scientific and cultural heritage was basically intact, though it was less and less available. This is largely borne out by the lists of the books quoted by Byzantine authors. Surviving Byzantine codices (actually mostly preserved in Western European libraries, which acquired them between the 13th and 15th centuries), show that there were three periods during which older works were actively copied. These were separated form one another by lapses (each one lasting a couple of centuries) of apparent lack of interest. The last active period was that of the ephemeral Byzantine revival after the recapture of Costantinople by the Greeks in 1261.

For a number of reasons, some apparently still poorly understood, the Nestorian and Jewish communities of Egypt and Syria of the 5th-7th centuries were actively engaged in the translation of Greeks texts into Coptic and Syriac.

Meantime, though almost continual internecine wars were undermining the Sasanian Empire, there cultural life remained quite active, though not as flourishing as for a brief period under Kushrau I (531-579) when first the teachers of the school of Edessa, closed by the Emperor Zeno (489), and then those from the schhol of Athens, closed by Justinian I (529), moved to the Sasanian cultural capital of Gundishapur. Some of them, however, including the greatest of them, Simplicius, later returned to Athens[1].

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1 In spite of his great importance both as a historian of philosophy, as we are indebted to him for a good deal of what we know of the Greek philosophers apart from Plato and Aristotle, and as a thinker in his own right, we know almost nothing of Simplicius' life.

As we have already said, the invading Arab armies (634) found Syria, Palestine and Egypt but weakly occupied by Byzantine troops, who barely succeeded in keeping control of these countries against the growing impatience of their inhabitants both with imperial taxes and religious policy. The Byzantine garrisons, after a couple of battles, retired into some fortresses and the relief expeditions sent from Constantinople were signal failures, while the local populations rather welcomed the advancing Arabs. Damascus fell in 635, Alexandria in 642, Cyprus was attacked in 660, Costantinople itself was repeatedly attacked until the final defeat of the Arabs in 718. The Arabs advanced beyond Egypt in 647 and reached the Atlantic for the first time in 681, but in North Africa the Byzantines and their allies put up a strong resistance, with alternating victories, until the final capture of Carthage by the Arabs in 698. In Spain the Visighots repulsed a first naval attack in 675, but the Arabs crossed the strait of Gibraltar in 711 and were master of almost the whole of Spain by 713. They then entered France and established themselves in Narbonne in 720, thence raiding the whole of Southern France, raids with continued in Provance well after the Arab advance northwards had been decisively crushed by Charles Martel at Poitiers in 732, almost exactly a century after they had begun their expansion. However Arabs were still on the offensive: for instance Avignon fell to them in 737. Sicily was raided since 720 and its invasion, begun in 827 was completed by 878, while Corsica was captured in 806 and Sardinia in 810. An Arab emirate was established in Bari in 840 and raiding Arab parties were a continuous threat to the Italian and French coasts and these occasionally penetrated well inland for the whole of the 9th century, so much that they reached the outskirts of Rome in 846 and had temporary strongholds here and there along the coasts of both Italy and Provance.

Meantime the Arabs had invaded Mesopotamia and in two major battles (Kadisyia, 637, and Nihavand , 643) crushed the Sasanian Empire and gradually annexed Iran (The last Sasanian king, Yezdegerd III, actually was killed about 10 years after having been defeated at Nihavand). Thus, in scarcely more than 30 years the Caliphate ruled an empire stretching from the Atlantic to the borders of India. It was, indeed, an immense castle of sand, as it was shortly to break up into several major and minor pieces, but, at this point in time the Arabs found themselves to be utterly incapable to rule such vast countries. So they quickly recruited into their administration such prominent local people who were ready to embrace Islam, and, in a somewhat subordinate position, also a good many Christians and Jews; on the other side they were immediately acutely aware of the need to acquire the local traditional cultures. Thus the Arab engaged into the rapid translation into Arabic of as many ancient texts as possible, both from Greek and from Syriac versions.

Arabic thus quickly became a learned language and, throughout the early Medieval times the Islamic world was culturally the most advanced, overtaking even the Byzantines.

In spite of the strict bounds set by the Islamic religion, which has as the fundamental tenet that all truth is in the Koran, scientific culture developed rapidly, though increasingly challenged by the more orthodox groups both among the Sunnis and the Shiites.

Apart from the contributions of travellers and geographers, Arab scientific literature appears as basically consisting in comments on Greek sources. This is however to some extent misleading, as the Arab thinkers were considerably open also to other influences, especially from other Eastern cultures, and incorporated a good deal of new evidences. The only field in which Islam was absolutely uncompromising was in its absolute prohibition of anatomical investigations on Man.

This was by itself a considerable stumbling block in the path of biological investigations, and we shall see, indeed, how the first real steps towards a new development of our discipline were made precisely in the field of human anatomy.

A further limitation of Arabic science is its extreme tendency for concreteness and for the interpretation of all evidence in the light of its real or presumed practical or moral advantage for Man. This totally guides all natural sciences, including astronomy, where the mathematical spheres of Ptolemy, which he conceived as simple explanatory models, are believed by Arab thinkers to be absolutely real.

The medical schools of Nisibis and Edessa played an important role in the transfer of Greek medical knowledge into the Arab world. These schools had been founded by the influence of the heretical patriarch of Constantinople Nestorius and flourished especially at the end of the 5th century and their teachers later fled to Mesopotamia. The Sasanians, who had founded a cultural centre in Gundishapur welcomed them and there and in other schools of their empire which soon fell under the Arab sway, the teachings of the Greek masters were preserved and almost worshipped.

The Arab conquerors soon established additional schools in Baghdad, Samarkand, Damascus and in other towns. The great Spanish centres of learning: Cordoba, Seville, Toledo, Murcia and others were established somewhat later.

There theology, philosophy and medicine were the main subjects of teaching.

The basic plan of such a ‘Madrasah’ is a group of buildings around a Mosque, with housings for the teachers and the students, libraries, hospitals and wards.

Thus the Arabs collected, preserved and spread again the great inheritance of classical medicine, while somewhat adapting it to their peculiar spiritual requirements and adding to it some interesting contributions.

While the Arabs thus busied themselves, in Western Europe the preservation of the classical heritage was basically the task for copyists working in the cloisters, who, partly because of factual difficulties and partly for a cultural policy, worked practically only on Latin texts of limited scientific value.

However the common tradition that the Medieval knowledge of the Greek classic was that which had been received through its Arabic elaboration is false.

Truly enough some Greek books have been preserved only in their Arab translations (this is particularly the case for the works of Galen, whose Greek originals were discovered only in the 16th century and some of which are still known only in their

Arabic translations), and is equally true that some work had been done since the 10th-11th century to translate texts from Arabic into Latin: Gerbert of Aurillac (pope Sylvester II) repeatedly requested such translations from Spain.

Again important groups of translators from Arabic worked both in Toledo and Palermo immediately after these towns had been recaptured from the Muslims. But in fact in the vast majority of instances less than 20 years lapsed between any Greek text was translated from Arabic and the date when it was first translated from the original Greek. Arabic texts were translated and studied for centuries because of a direct interest in their original contents: in the comments and additions that they provided to the Greeks.

For a history of biology the problem is that almost all the important contributions made by the Arabs are in medical fields, and thus they have but a marginal interest for us.

However, we shall briefly mention the most significant of them.

Perhaps the earliest worth mentioning is Mesuè senior (Yūḥannā ibn Māsawaih), who died in 875, who was also known to our Medieval scholars under the name of Johannes Damascenus (John of Damascus). He was the physician of the Caliph of Baghdad and wrote several books, the best known in the West being titled *Aphorisms* and was printed for the first time in Bologna in 1489.

Again of merely medical relevance is the work of Seraphiun (Yuḥannā ibn Sarābīyun), a Syrian physician who approximately in the fifties of the 9th century wrote a book also titled *Aphorisms* and another titled *Pandectae* (both published in Venice in 1496). His books were commonly used in the early Renaissance. In his books the plants from which medicaments may be obtained are just mentioned, but not described.

Giovannizio (Ḥunein ibn Isḥāq, 809-873) was the official translator for the Caliph Ma'mun and in this capacity he translated all sorts of books: from the Bible of the Septuaginta to Plato, from the treaty of veterinary medicine of Theomnestus to mathematical works of Archimedes and Menelaus. He also wrote some one hundred original works patterned in the Greek fashion. A comment of his on Galen was commonly used in the Italian universities up to the 15th century.

The work of Rhazes (Abū Bakr Muḥammad Zakarīyā al-Rāzī, 850-923) is of great significance in the history of medicine, but is irrelevant for the history of biology. He was a Persian and a favourite of Shah Al-Manṣur of Ghazni, to whom is dedicated his most famous book (he wrote about 200) dealing with medicine, mathematics and astronomy the most famous being the *Kitab al-Manṣuri* (*Liber medicinalis Almansoris*). Actually the 9th volume of the book deals with the treatment of all diseases known at the time; under the name *Nonus Almansoris* it was usually read and commented by the reader of the *Lectura almansoris,* a special chair in our universities up to the end of the 16th century. Equally important in medical teaching was the *Kitab al-Hawi fi'l-tibb*, known in the West as *Continens medicinae*.

The greatest Arab scientists was unquestionably al-Biruni, who was also a 'Hakim' (= philosopher-physician) in the true meaning of the word. But, while we have his works on physics, mathematics, etc. of his medical treatises there only remain an incomplete medical-apothecary encyclopaedia.

Avicenna

Avicenna (Abū ʻAlī al-Ḥusain ibn Sīnā) is one of the most famous figures in the history of Arab culture.

He was a Farsi from near Bukara, born in 980, died in 1037. He was for some time an adviser to the court of a minor Persian prince, who later persecuted and threatened him with death. He was a man of universal culture, great versatility and prodigious memory. He wrote a number of books both in Arabic and Persian on philosophy, mathematics, geometry, astronomy, medicine and natural history and some of them in Persian (Farsi) have not yet been translated and printed. His Arabic treatises had a great influence on the Medieval culture, both Islamic and European.

His philosophy is basically Aristotelean but strongly tinged with neoplatonism. and, as with all other Islamic thinkers, completely anthropocentric. The strict prohibition of dissection of human corpses enjoined by the Islamic law compelled Avicenna, as all other Muslim authors, to depend entirely on Galen for his human anatomy, while he completely subscribed to the humoral theories of Hippocrates. He summarised all this second hand knowledge in a great opus in five books: his famous *Canon of Medicine*, which was greatly popular not only with Muslim physicians, but also in the Christian West, where it was made available by a Latin translation by Gerard of Cremona (12th century)

For the pure biologist possibly the most interesting contribution by Avicenna is his clear discussion of fossils. He considers both the classic alternatives: that they are petrified organisms or that they are animals which were being spontaneously generated from mud and that could not complete their development. Avicenna is positive that they are the remains of true organisms, which have been transformed after death by a *vis petrefaciens*: a special power active in special environments. His thesis, after all, is basically correct as, in order to fossilise an organism must be entombed under favourable local conditions. The Arabic writings of Avicenna were well known and valued by Medieval European scholars.

On the whole, as a naturalist, Avicenna stands as a brilliant compiler, with a sound personal experience.

Averroes

Probably the most important Muslim scholar of the period, at least as far as original thinking and influence on later developments of Western culture are concerned, is Averroes (Abū ʻl-Walīd Muḥammad ibn Rushd). He was born in Cordoba in 1126, was governor of Andalusia and died in North Africa in 1198.

Like Avicenna he was a philosopher, a judge, a physician and an astronomer. As far as history of biology is concerned he contributed nothing original and, therefore he might be conveniently ignored; however his comment on Aristotle, as soon as it was translated into Latin had a tremendous impact in the faculties of Arts and even more in medical faculties. He had indeed the quality of being notably faithful to the original thinking of the Master and, moreover, he developed some important implications of Aristotle's ideas, which occasioned important conflicts in the Medieval universities. The influence of Averroism in the medical media was to have an important impact both on medical practice and on the framing of later European scientific culture.

Averroes is often critical of Avicenna, charging him with misunderstanding Aristotle, while, as far as biology is concerned, his discussion of the concepts of potentiality and act in nature is especially important (see appendix to chapter IV). As we said Aristotle considers the marble block as containing *in potentia* the statue, and applying this concept to embryonic development, as he thought that the materials who made up the embryo 'potentially' contained it. Averroes, for all his love for Aristotle, does not agree. For him nothing exists *in potentia*, that is as a possibility, which does not exist *in actu*. The seed of plants and the embryo of animals actually contain the plant or the animal, within the marble there is no structure or figure, and therefore Aristotle's comparison does not hold. To this extent Averroes' discussion and conclusions are a step towards a modern approach.

In Western Europe scholars passionately took sides in the dispute on the merits of the two great Islamic thinkers and through the 13th and 14th centuries the two schools were openly and sometimes violently opposed. The Averroists were repeatedly condemned by the Church. The Church itself, after much debate, adopted Avicenna's position, which is basically identical with that of St. Thomas Aquinas.

Thus the philosophy and science of Aristotle were first preserved for Western thought by Boethius, but the debate on the merits and limits of Greeks sciences was really sparked by the interpretative debate among the Muslim thinkers.

A follower of Averroes was Maimonides (Rabbi Abū ʻImrān Mūsa ibn Maymun ibn Abd Allāh or in the Hebrew Rabbi Moshé ben Maimon). He too was born in Cordoba in 1132 and died in Cairo in 1204. An encyclopaedic philosopher as his master, he has a key role in the history of Jewish philosophy. Christians were mainly interested in his comments to the Aphorisms of Hippocrates and on his letters on dietetics. These works show much original thinking and a lively criticism of Galen and of other ancient masters. They were much read both in Medieval and Renaissance times and were instrumental in the preparation of that systematic criticism of the classical tradition that was typical of the 16th century.

Other Muslim authors

We know of other Muslim authors who wrote on zoology, but few of their works survive.

Among those who were known in Europe, we may take as a typical example Giahiz (Abū ‘Uthmān ibn Bāhr ibn Maḥbub al-Giāḥiẓ, c. 776-c. 847), who wrote a *Kitab al-Hayawan* (= *book of animals*); this book, on one side, shows how interested was the author in the behaviour of the about three hundred species considered, which, however is always regarded as valuable for its significance for human morality, while al-Giahiz has no interest whatsoever in the morphology of the animals themselves. Thus the book is an interesting source of factual and traditional accounts on animals. A curious feature in Giahiz, is that he believes that climate has some influence on the aspect and behaviour of animals, some sort of ‘little evolution’ which is at the origin of local races. Some sort of transformation is also possible through hybridisation, but this has usually a bad effect, at least as far as morality is concerned. We shall find almost the same ideas expounded by Buffon, and one wonders whether the French naturalist knew of a book which has always been popular in the Arab world.

Another author who may be worth remembering is Sakārja ben Muḥammad al-Qazwīnī, who is the author of a compilation, largely based on previous and mostly lost authors, where he described several animals for which we have no previous descriptions.

While Islamic literature is, as far as zoology is concerned, very poor, its scientific literature numbers a good many important authors on botany (such as Ibn Haǵǵiāǵ in the 11th century, Ibn al-Ṣūrī in the 12th and others). All their works, just because of the strict practical interests of Muslim naturalists, are basically medical herbals, which are notable additions to the Greek texts, as they contain descriptions of many plants which were unknown to classic authors.

Other additions to botanical knowledge may be found in Arabic agronomic books.

Finally we should not forget the importance of Arab geographers (and above all of Ibn Battuta) who provided a good many accounts of animals and flora of different regions, thus setting out the evidence on which Europeans begun to elaborate in the late 15th century.

Such, for instance, is the chapter on Egyptian animals in the *description of the marvels of Egypt* written in 1203 by ‘Abd al-Latīf ibn Yūsuf al-Baghdādī, who also gives a detailed account of the Egyptian method for the artificial incubation of chicken’s eggs.

For the sake of fairness we may also quote a *Life of animals* by Muḥammad al-Dīn al-Damīrī, who died in Cairo in 1405. While some of the descriptions given by these authors show a genuine interest in the animals and relate some original observations, their works can not be considered as books on any significance in the development of biology.

Concluding remarks

Averroes is one of the latest Muslim authors who actively supported the philosopher's side in the raging debate between them and the fundamentalists. a debate that saw the final triumph of the followers of Al-Ghazzalī (Abū Ḥāmid Muḥammad al-Ghazzālī, known as Algazel to European scholars) (1058-1111), and by the end of the 13th century the Muslim world ceased to provide any active contribution to the evolution of sciences. Around the middle of the 13th century the Arab powers begun to crumble (and to be substituted by the Ottoman Turks) and their culture underwent a stasis, so that all European interest in it soon vanished.

An additional problem for the development of scientific culture in the Muslim world was the long standing ban on printing because of religious preoccupations. Thus the first printing facilities were introduced in Istambul only in 1727 by Ibrahim Muteferrika, who was actually a Hungarian who had converted to Islam, and did not outlive their patron. In Egypt a press worked through the three years of Napoleon's occupation, but was eliminated as soon as the last French soldier went. Printing only reappeared there around 1850!

Thus a number of possibly significant texts were lost and several, even by celebrated authors, still sleep, mainly in European libraries, waiting to be printed and translated.

To summarise: the Arabs did not introduce new ideas into the medical and biological sciences, but the preservation of texts and their comments, as well as the development and dignity that they bestowed to the study and practice of medicine, as well as some technical improvements in surgery and a notable development of pharmacology, were significant contributions to the development of medical practice and, to some extent, were to help in the development of biological thought too.

CHAPTER VI

Medieval times from the end of the Western Roman Empire to the end of the XV century

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SYNOPSIS OF THE MAIN HISTORICAL EVENTS AND OF THE MAIN SCIENTISTS AND PHILOSOPHERS

476 deposition of Romulus, nicknamed Augustulus, conventionally the last Western Roman emperor.
490 the Ostrogoth Theoderich conquers Italy
529 Justinian I closes the school of Athens, its teachers flee to the Sassanian court at Gundishapur.
Alexander of Tralles c. 500, Manlius Severinus Boethius is killed in 530 by order of Theoderich, Simplicius goes to Gundishapur in 530
668 the Longobards invade Italy.
622 Hegira: Muhammad flies from Mecca to Medina.
640 the Arabs capture Alexandria, supposed final destruction of the Library.
642 battle of Nihawand: final defeat of the Sasanian Empire.
700-1200 Islamic culture flourishes.
Isidore of Seville c. 570-636, the venerable Bede 673-735
752 battle of Poitiers: Charles Martel (the hammer) blocks the Arabs in France.
800 Charles the Great (Charlemagne) is crowned Western Emperor.
Alcuin 735-804, John Scotus Erigena c. 810-870
887 Charles the Fat is deposed, practical end of the Carolingian dynasty.
961 Otto I becomes Emperor.
1066 William of Normandy defeats the Saxons at Hastings and conquers most of England.
1073 beginning of the quarrel between the Pope and the Emperor on feudal and bishoprics investitures.
1085 the Spaniards capture Toledo from the Moors.
1095 beginning of the first crusade.
Pierre Abelard 1079-1142, Translators from Arabic: Armand of Carinthia, Gerard of Cremona etc. c. 1132-1187, Adelard of Bath c. 1100-c. 1200
1158 Diet of Roncaglia, the Emperor Frederick I Barbarossa (= Redbeard) grants special privileges to the school of Bologna and generally to students and masters.
1176 Frederick Barbarossa is defeated by a coalition of Italian communes at the battle of Legnano.
Period of the Universities foundations: after Bologna (1119) and Paris (1131 bulla of pope Gregory IX, 1194 bulla of pope Celestin III, 1200 charter by king Philippe Auguste); Montpellier (1181), Oxford (before 1208), Padua (1222), Naples (1224), Cambridge (1229, this last follows a school which has been mentioned sporadically since 630) etc.
1130 alcohol, which was already known by the Arabs, is first produced in Germany.
1145 paper is produced in Europe for the first time.
1176 Petrus Valdus begins his preaching and is declared a heretic.
1180 coal begins to be substituted for charcoal.
Frederick II of Swabia 1194-1250, Robert Grosseteste 1168-1253, Jordanus Nemorarius c. 1200, Leonardo son of Bonaccio da Pisa, also known as Leonardo Fibonacci c. 1170-1240 writes the

***Liber abaci* and other books and practically introduces in Europe both algebraic methods and the Arab-Indian numeral notation, Albert of Böllstadt (Albertus magnus) 1193-1280, Thomas Aquinas 1225-1274**
1205 beginning of the rule of Gengiz Khan.
1208-09 crusade in France against the Cathars heretics.
1215 King John grants the first *Magna Charta Libertatum.*
1269 first document describing the magnetic compass.
1281, 1331, 1334 first documents mentioning guns and artilleries.
Peter of Moricourt c. 1269, Vincent of Beauvais +1264, Mondino de' Luzzi c. 1275-1326, Roger Bacon 1214-1292, Ramon Llull (Rajmundus Lullus) 1235-1315, Witelo c. 1250-c.1300
1340-1440 the Hundred Year's war.
Duns Scotus 1265-1308, Theoderic of Freiburg +1311, William of Occam +1350, Jean Buridan +1360, Nicholas Oresme 1323-1382
1389 the Turks conquer Serbia.
1397 Michael Chrysolora teaches Greek in Florence.
1400-1434 Hussite wars, Western schism, councils of Constance and Basel.
1450 Francesco Sforza becomes duke of Milan.
1453 the Turks capture Costantinople.
1454 John Gutenberg prints the Bible, first book printed by movable letters.
1455-1485 War of the Roses.
1462-1500 Ivan I becomes the first czar of Russia.
1486 Bartholomeu Diaz sails beyond the Cape of Good Hope.
1492 Columbus reaches the Caribbean Islands (thus discovering America), Lorenzo 'the Magnificent' dies in Florence.
1498 Vasco da Gama reaches India via the Cape of Good Hope.
Nicholas Cusanus 1401-1464, Erasmus from Rotterdam 1465-1536, Giovanni Pico count of Mirandola 1463-1494, the 'Merton Group' works around the middle of the 15th century, Leonardo da Vinci 1452-1519, Giovanni Marliani +1483, Berengario da Carpi c. 1460-1530, Georg Peurbach 1423-1461, Johannes Regiomontanus 1436-1476, Nicolaus Copernicus 1473-1543, Girolamo Fracastoro 1484-1553.

Biology and medicine during the early Medieval times

We saw how during the late Roman Empire biological studies were practically the mere perpetuation of previous knowledge, while medical studies still made some significant advances. We have also seen the reasons of the extremely limited contributions to new knowledge by Islamic scholars. Now that we come back to the development of biology we cannot completely overlook the evolution of medicine, as it was precisely chiefly amongst physicians that ran the main biological debates and were made the more significant advances. It was but very slowly that biology acquired the dignity of an autonomous branch of the sciences.

We have already mentioned the essentials of that transitional period which runs from the end of the Western Empire to the Longobard conquest of Italy and to the expansion of Islam, which were roughly contemporary.

We may well divide the following centuries in two periods: the first corresponding with the period of the consolidation of the main 'barbarian' monarchies: Longobards in Italy, Franks and Burgundians in France, Visigoths in Spain, Saxons in Britain, soon followed by the explosion of Islam, which almost swept away the Spanish Christian rulers and seriously threatened both France and Italy.

This phase is practically ended in the West by the time of the Spanish capture of Toledo (1185) and of Jerusalem in the East (1199) by the Crusaders. The following phase, the late Middle Ages, practically merges into a very gradual transition with Humanism and the following Renaissance.

In chapter IV we have seen the story of the social development of schools and we might feel that this should suffice, but for the opportunity to mention briefly the deep, albeit subtle, influence of the teachings of John Scotus Erigena (or Eriugena). The life and deeds of this Irishman are intimately woven with that brief, but burgeoning flourishing of cultural activity started by Charlemagne, and continued by his successors. We owe to their encouragement the preservation of most that survives of the Latin writers, as almost all the existing manuscripts and many of those copied by the scholars of the 15th-16th centuries and since lost, are copies made in Carolingian times of codices of the 4th-5th centuries. The Carolingians equally made an effort to promote the establishment of new schools and the diffusion of literacy.

When the Carolingian dynasty foundered into the worst chaos Europe had witnessed in centuries, the real 'Dark Ages' followed and the beginning of recovery may be seen with the advent of Emperor Otto I (961) about a century later and this practically corresponds with the beginning of the already mentioned debate on 'universals'.

As we have seen, both the late Roman schools and such schools as developed during the Middle Ages were in great need of summaries and digests (the *summulae*) and, as these were the books more commonly preserved, we are often ignorant of the precise source of such notions as are expounded there.

As far as Natural Sciences, and more specially zoology and botany, are concerned, books are fairly rare. Apart from Roman and Greek texts, more or less complete, among which the commonest, just for its practical value, is the familiar Dioscorides, copies of which range from the wonderful *Dioscorides Vindoboniensis*, a lavish Byzantine manuscript, which many figures are excellent copies of Crateva's originals (1st century BC)[1], especially prepared for a Lady of the Roman senatorial family of the Anicii, or the equally Byzantine and almost as good *Dioscorides neapolitanus*, the later (8th century), but still reasonably good, *Dioscorides Longobardorum*, to extremely poor and incorrect copies.

[1] Among the several Greek novels which were popular in Hellenistic and Roman times and which still survive, there is a curious novel in the form of fictitious letters (a kind of novel that was revived in the XVIII century and which is still popular), which chief character is a physician Crateva, apparently a great-grand-son of the historical physician of Mithridates VI, and who may well have been a real character and to whom some scholars credit the famous pictures.

Among the few new books produced, we may quote the *De natura rerum* and the *Etymologiarum, sive de originum libri XX,* written by St. Isidore of Seville (the popular Sant'Isidro) for the education of a Visigothic king, the first written approximately in 612 and the second in 630; the *Periphysion aut de divisione naturae* by John Scotus Erigena; the *De natura rerum* by the Venerable Bede (674-735); the *De Universo* by Rabanus Maurus (c. 820). From the Byzantine world we may quote a compilation by Timothy of Gaza, which is but a summary of the writings of Aristotle, of Helianus and of Oppianus of Apamea and, later, some essays by the Emperors Constantine VII Porphyrogenitus (10th century) and by Constantinus IX Monomachus (11th century) summarising and commenting on writings by Aristotle.

We maintained that the *De divisione naturae* by John Scotus had a deep, albeit subtle, influence.

The book was written in about 870 and is basically a philosophical-theological book, in which Scotus tries to synthesise his views of Christian theology with Christian neoplatonism as it had evolved on the basis of the *Timaeus* (the only platonic dialogue then available in the West in the Latin translation by Calcidius), of the Christian neoplatonists Gregory of Nyssa, Origen, Maximus the confessor, a few others and the most celebrated, even if apocryphal, 'pseudo-Dionisius the Areopagite' (apparently a book originally written in Syria); all this was framed within the principles of St. Augustine and the Aristotelian 'categories' as illustrated by Boethius. Within this framework natural sciences are considered to be essential to a correct understanding of Creation, of God and of redemption.

Scotus does not add anything new to the information that he gathers from his sources, which, however, he interpreters in the freest way. So, for instance he argues, possibly taking his hints from Martianus Capella, that Jupiter, Mars, Mercury and Venus rotate around the Sun and that the Sun, with its surrounding planets, moves around the Earth, which is a curious anticipation of the 'Tychonian system'.

Scotus' book was largely ignored for the next three centuries, but his theories, mediated by Honorius of Autun and by St. Bonaventure of Bagnoregio, were the core of Ramon Llull's theories of the world and of lay sciences: such theories, as we shall see, had a deep influence on all subsequent developments. At all events the *Periphysion*, though supported by authorative theologians, such as the great cardinal Nicholaus Cusanus (Nicholas of Cusa), was formally condemned by the Church, basically as it had been often quoted by such heretic movements as the Amalricians and the Albigensians.

Finally there is no doubt that, through St. Bonaventure, John Scotus had a notable influence on Duns Scotus (about 1266-1308), who was nicknamed *Doctor subtilis,* and who was considered for centuries as the only alternative to the Aristotelianism as developed by St. Thomas Aquinas. Indeed we have mentioned in Chapter IV how even in the 18th century, for instance at the faculty of medicine of Padua, they had two chairs in Theology, one *in via Scoti* and the other *in via Thomae*.

I do not dare to say that there were reciprocal influences between Duns Scotus and Llull, but it appears quite probable that they met in Paris.

The ideas of John Scotus, in some obscure way, may well have had some relevance to the development of the thinking of Jewish cabbalists (whose basic text, the *Zohar* was written in Northern Spain in just those years when Llull was active there). Thus, as we shall see, Scotus had a relevant influence on the development of biology, but an entirely indirect one.

We said that the Early Middle Ages were a period of scientific stasis, yet there were a few additions to the zoological and botanical lore. Cosmas Indicopleustes (c. 500-550) provided the descriptions of previously unknown animals, such as the warthog. So did the unknown compilers of the Greek *Geoponika*, a book on agronomy written between 944 and 959, and which was later translated into Latin. So did some physicians, such as Aetius, Alexander of Tralles, John the 'actuarius' and Demetrios Papagomenos, who described a number of parasites both of Man and of other mammals.

Of some significance may well be the anonymous books on veterinary medicine (the *Hippoiatrias*) and on hunting. Unfortunately all these works are almost unavailable, and the historians who mention them take care not to give details of what precisely they say!

A book that is always mentioned in histories of biology, though it does not deserve it, is the *Physiologus*. This booklet derives from a Greek original, presumably as old as the 2nd-3rd century AD and it is known not only in Latin manuscripts, but also in translations into most European languages and even in Arabic and Amharic! It simply tells moral stories about animals, some real and some fantastic, the animals mentioned in the Bible being preferred.

The book has no scientific claims and was never considered anything but a book for entertainment. But for the fact that it is by far the commonest early medieval text dealing with animals, one does not understand how it came to be considered at all in histories of science.

Almost as common and patterned on the *Physiologus* are the many somewhat later *Bestiarii*, some being merely moral, some being mainly concerned with the symbolism of 'courtois' love (Love bestaries), a kind of literature which has been popular until recently. From the period 1,000-1,400, we also know a number of books being somewhat like encyclopaedias, written both in prose and in verse, and dealing with natural history and more specially with animals and plants. In Italy some of them antedate the corresponding books in other European languages, thus we have the *Fior di Virtù* and *L'Acerba* by Cecco d'Ascoli (who was executed as heretic), the *Tesoro* by Brunetto Latini, the teacher of Dante Alighieri, the *Dittamondo* by Fazio degli Uberti. Corresponding books by British authors are by Philip Thaun and by Alexander of Neckam, several were written both in French and Provençal. All these books basically relate information, both true and false, gathered from classical sources, but they provide some here and there new information. So, for instance, Fazio degli Uberti tells us that, in his times, beavers (which he actually calls *beveri)* still occurred in the delta of the Po.

When discussing all these books, which are generally scoffed at by historians, one must remember that they were not planned as academic treatises: they were what we now call 'popular science' or the cultural equivalent of today's scientific serials on TV. Most of them, taken the science of the time, are surprisingly accurate.

Again another group of treatises pertain to such 'courtois literature': the books on hunting, and we shall further pay some attention to the *De arte venandi cum avibus* by the emperor Frederick II. Several such books provide some good and new information.

So, as soon as the economic and cultural flourishing of the 12th-14th centuries began, we meet with a remarkable number of books, both good and bad, dealing with both plants and animals. It is notable that some fictitious stories and myths were so widespread that they are quoted almost in every medieval compilation.

Several such fantastic stories, for instance, have a honourable place in the *Physica* or *Liber simplicis et compositae medicinae* written by the Benedictine nun Hildegarde of Bingen, who died in 1179 in the monastery that she had founded near Bingen in Germany. As stated by its title, the book is basically a book of recipes, and, even as such, it is nothing special, thus one wonders whether its renown may be due to the fact that it was compiled by a saintly woman.

Beginning with the second half of the 12th century there is a dramatic increase both among scholars and laymen in the interest in natural history, and especially medical botany, and including astrology and alchemy. This runs parallel with the development of the Universities, of Communes and of guilds already discussed in chapter IV.

Obviously we are not concerned with the political and social framework that was instrumental or, at least, which allowed for the flourishing of studies, such as the first great successes of the Spanish 'Reconquista', the development of trade with Byzantium, the flourishing of the Arab-Norman and Swabian culture in Southern Italy.

It is notable that at this times a number of North European, including many Normans, either came and studied in Italy or definitely settled here, and, at the same time several scholars from Italy acquired prominent positions in England.

Among the scholars who greatly contributed to the translation and comment of Arabic texts are Adelard of Bath (born about 1170), who wrote the important summary, *Quaestiones Naturales,* and Alexander of Neckam, who produced both a basic translation of the books of Aristotle and some original treatises. It is also worth mentioning that the Byzantine empire had a brief cultural and artistic revival after the collapse of the 'Latin Empire' and the recapture of Constantinople by the Greeks. At this time Byzantine scholars translated from Arabic into Greek texts which were lost in their original Greek. Again, and especially in Spain and Provance, at this time, many texts, both Greek and Arabic, were translated into Hebrew and by the teachings of Jewish masters, became available in Western Europe.

It is notable how the European scholars that were hunting for Greek texts were interested primarily in scientific, medical and technical books, the fashion for the literary and historical texts following practically only from the second half of the 14th century.

The search and diffusion of ancient books on philosophy and on different applied sciences and techniques, was paralleled by a corresponding production of new treatises in the different national languages.

A second group both of translations and of new compilations are more scholarly. They all belong to the rationalistic attitude prevailing in the Universities and which was then battling against the preachings of such mystics as Bernard of Clairvaux, Pier Damiani and others. The general attitude may be synthesised by a sentence attributed to Ramon Llull (actually it is not found in all the copies of the *Book of the Lover and the Loved*): 'They asked the lover (= the Christian) what the world was; He replied 'For those who know how to read it, it is the book by which my beloved (= God) is known'. They then asked whether my beloved is in the World; the lover replied 'yes, but just as the author is in the book.'.

Anyway, besides this general attitude, the scholarly production of this age, as far as biology is concerned, can be grouped into two sections. For one the model was Pliny and, to mention the more prominent, this was the pattern for Thomas of Cantimpré (who wrote the *Liber de Natura Rerum* between 1233 and 1248 and around the same time wrote a book on bees) and of Vincent of Beauvais, who completed his *Speculum naturale* around 1250. Aristotle, instead, was the model for such as St. Albert the Great (Albertus Magnus).

Incidentally, it appears that Pliny was not available to the writers of the 7th-8th centuries, though we have a manuscript of the 5th-6th century preserved by the Abbey of Nonantola. The first author to quote directly from Pliny was Bede, and thereafter the *Historia Naturalis* was for centuries immensely popular.

Both Thomas and Vincent are mere compilers and, just as Pliny, they do not care to investigate whether there is any logical implication in the nature and behaviour of living beings. They just want to list everything which may be assumed 'to be known'. At most their comments touch on the morals of the stories they tell, just as in the *Bestiarii.*

As modern encyclopaedias, their books had an immediate success and were most frequently copied. However, for reasons that we shall see further on, the book of Vincent was often believed to be the work of Albertus Magnus. Moreover both Vincent's and Thomas' books were immediately translated in various European languages and had a number of imitations.

St. Albert the Great (Albertus 'Magnus') is a scientist of absolute value. He is very much akin to Aristotle in his approach to the problems of natural history, both in his own work and in the soundly critical way he quotes his sources.

Albert of Böllstadt ('Magnus') was born in Swabia from a noble family around 1200 and died in Cologne in 1280. He studied in Padua and later became a Dominican monk. He taught in several different schools and places and for some time directed the new *Studium generale* which had been founded in Cologne by the Dominicans and there he had as a pupil the famous Italian saint and philosopher Thomas Aquinas. When we consider the number of different appointments and charges that were enjoined on him by the Church, his many long travels, mainly done on foot because of his vows, the number of books that he was able to write on every possible subject, from theology to botany, from zoology to morals, is truly incredible and earned him the nickname *Doctor universalis.* As it was then standard practice, his work is mainly in the way of commentaries on Aristotle, that he read according a moderately Averroistic outlook. He thus showed a considerable moral courage, as both earlier and in his own times the works of Aristotle had been repeatedly condemned by the Church. Nor was Albert free from some influence from hermetic sources. Those he freely acknowledges in his *De natura et origine animae,* where he repeatedly mentions Hermes Trismegistus, believing him to be a great-son of Prometheus and the original source of stoic philosophy. Influences of neoplatonic and neopythagoric origin are equally clear in the thought of Albert.

As the cultural influence of Albert was immense (for instance both the theology, philosophy and natural history of Dante Alighieri may be ultimately traced to him), he is largely responsible in giving to most medieval philosophy and science, a basic un-aristotelean pattern within the frame of a formal aristoteliansm.

The Aristotle used by Albert was that of the translation by Michael Scotus (c. 1220), and the first 19 books of Albert's *De animalibus,* which, as a whole, number 26 books, are a re-elaboration of the books on animals by Aristotle. The next two, which we shall discuss further on, as they are the most important, are *Qaestiones* and the last 5 are an account largely based on Thomas of Cantimpré (which possibly explain why also the books of Vincent of Bouvais were often credited to Albert.

The first 19 books on animals include some new data, but are chiefly significant for the improvement that Albert suggests on classification and that are definitely an advance on such a classification as it may be deduced from the Stagirite's books.

Albert's main original contributions appear in books 20 and 21. The *Quaestiones* were apparently prepared for a course given in 1258, and were collected by Conrad of Austria. However, their final text was completed just after 1260, as there are references to the new translation of Aristotle by William of Moerbeke, which was circulated for the first time in 1260 and that had not been available to Albert when he had written the first 19 books of the *De natura animalium.*

A comparison of the *Quaestiones* by Albert with the similar and almost contemporary ones written by Petrus Hispanus (later pope John XXI) in 1235-1248 and by Gerard de Breuyl, shortly after 1260, which both deal basically with the same problems, shows a striking difference.

While the other two deal with different problems only under the aspects of theology and logics, Albert, quite naturally pays the due attention to logical and theological aspects of the 'Questions' that he debates, but also introduces new empirical evidence; when possible at all he discusses the practical implications, such as medical, of each topic; finally and most significant, he is extremely reluctant to call into the play the divine providence and miracles and always strives for a logical interpretation of the evidence.

As far as animals are concerned, Albert may be credited with the discovery of insect haemolymph, with the description of the gangliar system in crayfishes and spiders, of the allantoid membrane. He made also accurate dissections of the eyes of moles, gave better descriptions of the earliest blood-vessels of the embryos of fishes and birds. He also experimented on the behaviour of ants by removing their antennae. As an example of the kind of problems that he discusses, he provides a brilliant explication of how it happens that though in the Ark there must have been only one pair of sheep and one of wolves and while the sheep produce only one lamb per year, and the she-wolf produces several cubs, yet sheep always outnumber wolves. It is indeed notable that he is able to provide a reasonable answer without recourse to Divine providence.

Albert paid special attention to monsters, as they had always been thought to be 'signs' of the gods. Albert dismissed several Plinian monsters as delusions and, anyway ruled out any diabolic intervention in real instances. Again he ridiculed such traditional lore as the story of the self-castration of beavers, of the transformation of Goose-barnacles into geese, of the incubation by the sun of the Eagle's eggs and so on.

As a whole Albert is a staunch supporter of the theory of the *Scala naturae,* that is that all natural objects form a continuos chain, each species being intermediate between two others. Some of his 'intermediates' are a little surprising, such as the Elk (= Moose in America), that he considers as an animal intermediate between the horse and the Red-deer, but as a whole his systematic is a definite improvement on that hinted by Aristotle.

Two more books by Albert on natural history are significant: a *De mineralibus* (where he proposes a classification of minerals) and a *De vegetabilibus et plantis.* On plants, again, Albert made some notable observations, such as providing the first real description of the growth rings of trees, or his remarks on the different kind of symmetry in flowers.

However, while the alchemical works of Albert were celebrated for centuries (in fact he did indeed do some important work, such as the preparation of pure Arsenic, but he was also credited with a number of alchemical treatises that he never wrote), his zoological work was largely ignored and had no real impact on subsequent developments. The same holds for his botanical work which was apparently extensively quoted only by the Bolognese Pietro de' Crescenzi in his book on agriculture and related subjects.

About three centuries lapsed before Ulisse Aldrovandi resumed work on systematic where Albert had left it.

Two more significant aspect of Albert's activities deserve our attention.

Whether Albert is the author of a curious booklet *De secretis mulierum* (The secrets of women) is disputed, but, anyway, he was greatly interested in the problems of reproduction. Albert had no sympathy for women, but he considered that the foetus did not develop, as suggested by Aristotle, from the male semen only, nourished by the menstrual blood, he holds instead that it develops from the mixture of both the male semen, which, anyway, is the responsible for the 'form', and the feminine 'sperm' (actually vaginal and vulvar secretions); thus, considering that good quality and abundant materials are prerequisite for having strong and well developed products, St. Albert considers that good sexual satisfaction by the parents is to be praised as it makes for better children.

Last but not least, Albert's opinion on fossils is clear-cut, and is the same as that of Avicenna: that is that they are the remains of once living organisms which were turned into stones by some local 'power'.

It is obvious that the necessary question then arises: how does it happen that marine organisms are found high in the mountains? Albert has nothing to say on it, but his almost contemporary, Ristoro d'Arezzo, suggested in his *The composition of the World*, written in 1282, that they had died there during Noah's Flood. This explanation was that mostly followed in Italy (with notable exceptions as we shall see), rather than the alternative that they were mere mineral formations or organisms which, while naturally growing within the rocks had been stopped in their development before coming alive. The two being practically the only alternatives discussed until the 18th century, but for Leonardo and a few people who had learnt of his ideas, like Fracastoro.

Albert was thus instrumental in greatly advancing the so called 'Christian Aristotelianism', which had been first promoted by Boethius and which was brought to perfection by Albert's pupil, St. Thomas Aquinas.

In the late 19th century and in the early decades of the 20th century historians with a penchant for positivism had a tendency to undervalue Albert, who was certainly no revolutionary thinker and had no trouble with the Church, while they extolled his contemporary, the Franciscan Roger Bacon, who with the Church had serious problems indeed. As a matter of fact both of them were great in their own way and the persecutions suffered by Bacon were largely due to his political stances, rather than to his philosophical ideas.

Somewhat younger than Albert, was Ramon Llull (Rajmundus Lullus, 1232-c. 1316). Llull was a Catalan and in his youth had been a knight, a courtier with the King of Majorca, had married and had two children. He later became a Franciscan monk and became famous under the nickname *'Doctor illuminatus'* (the enlightened doctor).

Also Llull produced an immense quantity of books on every kind of science (his geometry is especially important). There is no question that Llull was a mystic, but he was also a great logician, especially interested in combinatorial logic. As usual he aimed at a general synthesis of all knowledge. His work is strongly tinged with neoplatonism. Llull linked all sciences with the influx of divine virtues as mediated by the celestial spheres and the qualities of the four elements, and his ideas are basic for the understanding of all Medieval astrologic medicine, but his influence goes, both directly and indirectly far beyond it: Lullian combinatorial logic was quite influential for over three centuries and none less than Leibniz was deeply interested in it; the *Geometria nova,* which in some ways foreshadows topology, was so influential that Descartes, when he proposed the principles of analytic geometry, considered his new approach as an alternative to Llull's and the only one which could substitute for it.

Pico of Mirandola and Paracelsus quote Llull as an undisputed master and Jordanus Brunus, himself a lullist, charges Paracelsus to be a plagiary of Llull. Although Llull, in the genuine works of his condemns alchemy, his general theories were such as being liable to fit readily into the alchemical tradition and so, in the following centuries, he was commonly believed to have been a great alchemist and magician.

As we shall see further on, as Paracelsian influences were extremely important in all branches of biology until the middle of the 18th century and even in some schools in the early 19th, lullism had a lasting influence.

Llull did not contribute anything new to biology and his botanical writings are merely concerned with the medical use of vegetable remedies, but the combined influence of his logic and his mnemotechnic were instrumental in shaping even some aspects of modern systematic.

The long term influences of both Albertus magnus and of Llull on biology certainly deserve more attention than it is usually paid to them, as I strongly suspect that their ideas have filtered through the centuries into even some modern scholars, who probably even ignored their names.

A mention deserves, at least, the Byzantine Manuel Philes (1275-1345) who wrote a *Peri zoon idiotetos* where he describes several animals for the first time. As the already mentioned Byzantine *Geoponika* was translated into Latin just about this time and became rapidly popular; a better comparative study of Byzantine and Western European literature could well increase our understanding of the cultural exchanges in the age which prepared the cultural development of the early Renaissance.

The revival of scientific interests was immediately perceived not only by physicians, but also by the many writers on agriculture and related subjects. Among them the exemplar and outstanding one is Piero de' Crescenzi of Bologna (1233-c. 1321). His book *Opus Ruralium Commodorum Libri XII* was written between 1304 and 1309. Although its author acknowledges most of his sources and is one of the few authors who often quotes Albertus magnus, he does not quote either the Jew Moses of Palermo or the Calabrian Jordanus Ruffo, who were both active as compilers from Arab sources at the court of Palermo at the time of Frederick II and to whom Piero is largely indebted for his sections on veterinary and on animal husbandry).

The abundance of textbooks on agriculture or on hunting is correlated with a general improvement of climatic conditions and with the evolving of new agricultural practices, as well as with the quick selection of new breeds of domestic animals, much improved on the poor average quality of their early medieval counterparts. New breeds of horses, dogs and sheep appear. The selection of new breeds of horses was obviously the result both of improvements in the ploughs and in the harnessing of carts, but largely also on the evolution of armoury and the development of different specialised types of cavalry.

Among such rich and varied literature a special place befits to *De arte venandi cum avibus* by the emperor Frederick II of Swabia, the most famous book on falconry ever written.

Frederick II of Hohenstaufen (1194-1250), king of Sicily and later Emperor, is famous as a protector of arts, an open minded and illuminated autocrat, the politician who first attempted to establish a strong centralised state and the enemy of several popes. He is also the founder of the University of Naples (which he established as Bologna had turned Guelph). He ordered Michael Scotus to translate the whole Aristotelian corpus from the Arabic, a translation that, though never completed, was very influential in the diffusion of the philosopher's ideas; however, several spurious and late texts were included, the result being a considerably neoplatonized Aristotle.

The curiosity of the Emperor in natural history, induced him, in order to see after how many years they could be caught again, to experiment the marking and release of fishes by rings put to their opercula. At the British Museum Natural History there is a painting figuring with its measurements such a marked, gigantic pike, caught again 267 years after the Emperor had it marked.

Frederick's treatise on falconry is somewhat indebted to an Arab treatise that was translated for the Emperor by Theodore of Antiochia with the title *De scientia venandi per avibus;* as Frederick was fluent both in Arabic and Greek and there are Byzantine books on falconry, it is possible that, should these be made available, some other debts might be discovered, but the emperor's book still is an extremely original work which amply proves Frederick to have been a first class naturalist. The emperor clearly distinguishes and describes a number of bird species, both falcons and others; there are sound considerations on the bird's geographical distribution and migrations. Other new observations concern various aspects of bird biology and morphology. It was the emperor who discovered the pneumatisation of the main bird's bones and who correctly identified the different bones of the bird's legs with their homologues of mammals.

Another book on hunting that deserves a place in a history of biology because of the several good observations it includes, is *Le miroir de Phoebus, des deduits de la Chasse, des bêtes sauvages et des oiseaux de proie* by Gaston de Phoebus.

It must be finally noticed that all such medieval practical textbooks are less encumbered by the traditional respect for *auctoritas* than the contemporary typical scholarly works.

Medicine before the flourishing of Universities

Though this book is not concerned with the developments of medicine, and we shall omit all references to strictly medical matters, throughout the Middle Ages and until well into the 19th century, so close were the links between more general biological studies and medicine, that some notice must be taken of the development of medical knowledge.

Since the earliest stages of the development of monasteries, medical plants were grown in the convents' orchards and handbooks on the collection and preparation of medicinal plants were produced throughout the Middle Ages. Possibly the oldest such book known is that by Benedetto Crespo, Archbishop of Milan in the 8th century. During the early Middle Ages the ancient knowledge of plants was preserved both by copying the books of Dioscorides or by summaries produced by usually unknown or obscure compilers. Medicine was mostly studied and practised in the convents, but as shown by the legislation, there were also some lay physicians. Their value was, however, extremely poor, at least judging by the accounts of their activities provided by some Arab physicians, who had an opportunity to see them at work during the crusades.

However a lay medical school was soon to develop in Salerno, South of Naples. An ancient tradition was that the school had been founded by four masters: one Latin, one Greek, one Arab and one Jew, who, however, were each lecturing in their own languages.

This is legend, but it still holds the truth that it was in Salerno that the four different medical traditions actually merged. The truth is that by the 7th century numbers of sick people were attending a Benedictine monastery in Salerno. By the 9th century we have definite evidence of the school and we know that in 904 a Salernitan physician was at the court of the king of France. At this time there was an entirely lay school run by a 'Hippocratic college'; often masters were paid directly by the students. who already at that age, were coming from many different countries.

Among the earliest physicians of this period is Garioponto or Guarimpoto, probably a Longobard, who died about 1050; he wrote a sort of medical encyclopaedia titled *Passionarium*; the book is still historically important as in his attempt to translate Greek terms, he Latinised a number of terms also from the common language and thus introduced in medical terminology terms such as 'gargarise', 'cauterise', 'cicatrise' etc. which are still with us.

Other famous doctors were the Jew Benvenuto Grafeo, who wrote a celebrated *Paractica oculorum,* and Alphanus, a Longobard who earned fame as a benefactor during the Norman siege of Salerno and who was later, apparently, an adviser of Robert Guiscard.

Equally dated round 1000 is the famous Salernitan *Antidotarium,* which was repeatedly copied and which contains some significant additions of Arab origin to the classic pharmacopeia.

It was actually the diffusion of Arab culture around 1100 which spanned the most flourishing period for the Salernitan school. This is also the period of the 'Latin' kingdom of Jerusalem and of the greatest development of trade between Europe (largely through the Italian commercial city-states) and the Saracens.

The man who was largely responsible for the diffusion of Islamic medical knowledge was Constantine the African, a native of Carthage. He was a learned man, equally fluent in Arabic and Latin and, according the medieval fashion, nicknamed *Magister orientis et occidentis.* He may rate as the most celebrated master of the Salemitan school. Later in his life he became a monk and retired to Montecassino when the abbot was the Longobard Desiderius, who later became pope with the name Victor III. Constantine died in Montecassino in 1087.

Constantine apparently translated a number of treatises from Arabic, including Galen's *Microtechné* and Hippocrates' *Aphorisms*, thus reintroducing both Hippocrates and Galen to the Western scholars.

At this time the degrees granted by Salerno were acknowledged through the West as entitling to practice medicine.

Thus, while the Salernitan school may be rooted in a monastic or cathedral establishment and several of its masters later in their life took the orders, it always functioned as a lay establishment. Actually, while the church, as we have seen, was progressively restricting the opportunity for clergymen and monks to practice medicine, the complete secularisation of the Salernitan school was accelerated and it was finally consecrated when Frederick II, when chartering the University of Naples (1240), granted to Salerno the monopoly of medical teaching for the whole of the Sicilian kingdom (which actually included the whole of Southern Italy). Frederick ordered that the medical curriculum was to last for five years and divided it into a first three years curriculum corresponding with the licence of Arts, and two years of medical theory and practice. In fact Frederick prescribed the dissection of human bodies, but we do not know whether the Emperor's directions were actually implemented. Most of the ambitious and progressive plans of the emperor collapsed with his untimely death after some serious defeats by the Italian Guelph leagues.

In Salerno as in other places the study of anatomy was usually practised on pigs. There were sound reasons for that: first the size of pigs was approximately the same as that of man, second pigs had such a paramount importance in Medieval stock raising that they were the commonest animals available; moreover the preparation of their meats for conservation and marketing had originated true guilds of butchers specialised in handling their carcasses and who were, therefore, ready-made dissectors available to help the teacher. Some such guilds also specialised in some minor human surgery. In fact in Italy it is still common to call an incompetent surgeon a 'Norcino', but few remember that the town of Norcia was an important centre in the pig trade and that the guild of 'Norcine' butchers was famous (they, for instance, were established in such numbers in Rome that they had their own church) and they commonly practised surgery for cataract blindness (one wonders: in times when anaesthetics were almost unknown – actually the Salernitan doctors used strong opium preparations either to prepare for surgery or to soothe pain – and there was no antisepsis, who was the bravest: the patient ready to undergo eye surgery or the Norcino, who practised it?).

Anyway we owe to this tradition one of the most famous texts from the Salemitan school: the *Anatomia porci,* a mere score of pages, wrongly attributed to a Copho, who probably never existed. The little tracts on the same subject by Master Maurus (c. 1170) and by Master Ursone (c. 1180) are definitely better.

After the middle of the 13th century the Salernitan school begun to decay until it became practically a ghost school. This did not prevent that stronghold of conservatism that was the Sorbonne, to ask for the advice of the school well into the 18th century. The death of the school was officially certified by its formal abolition by king Murat in 1811.

Usually the teachings of the Salernitan school are recorded in simple verses and are of very practical kind.

I reproduce here a couple of them from the most famous collection: the *Regimen sanitatis* for the sake of curiosity (but wise indeed they are and could profitably be used today).

Si vis incolumen, si vis te reddere sanum	If you want be healthy, if you want to recover
Curas tolle graves, irasci crede profanum	Take away serious preoccupations, believe that to be in rage is irreligious
Parce mero coenato parum; non sit tibi vanum	Drink but little pure wine at dinner, do not care
sugere post epulas; somnum fuge meridianum	to get up after a good meal; avoid sleeping in the middle of the day
Non mictum retine nec comprime fortiter ano	Do not try to postpone urination, nor try to keep belly gases
Haec bene si serves: tu longo tempore vives	If you keep well (these rules): you shall live long
Si tibi deficiant medici: medici tibi fiant Haec tria: mens laeta, requies, moderats diaeta	If you have no physician available: your physicians will be these three things: a merry mind, relaxation, mod erate feeding

In France another important medical school flourished shortly after that of Salerno. This is the School of Montpellier, which also profited of his position on the Mediterranean with its good communications with both the Arabic and the Jewish culture through the Balearics (whose king's overlordship it acknowledged for a while), Spain and Sardinia.

It seems that in Montpellier the teachers, up to 1220, were not organised and each one taught independently, later a school patterned on that of Salerno came into being.

The main difference was that in Montpellier a regular university gradually developed around the school of medicine. As a consequence the medical faculty of Montpellier was more independent than usual and it carried a greater weight in the university's affairs. As proof of its independence it is certain that, in its early times, even Jewish masters coming from Spain were teaching there.

The most notable figure of the school of Montpellier in the 13th century was Arnaud of Villeneuve 1240-1311 or 1337), nicknamed 'the Catalan'. He was a good friend of Lullus and is probably the originator of the syllabus of the Monpessulan faculty, which was officially sanctioned by pope Clement V with a bulla of 1309. The curriculum envisaged the usual three degrees: Bachelor, Licenciate, and Doctor. The main authors that the pupil had to study were Hippocrates, Galen, Rhazes and Avicenna. As the whole curriculum lasted six years. One may, perhaps, wonder how long it should last now; after almost seven centuries of scientific development; would sixty years be enough?

Late in his life Arnaud was suspected of heresy, was arrested by the Inquisition, who actually ruled one of his books to be heretical. However two popes came to his rescue: Boniface VIII, whom Arnaud had cured by an astrologic talisman, and immediately afterwards Clement V, and he was released.

As usual in Arnaud's writings we find some criticisms of Galen and of Avicenna, based on personal observations. This shows that, contrary to what is commonly assumed, teaching in medieval schools usually was not a slavish repetition of the teachings of the old masters. There were, indeed teachers who swore *in verba magistri* and some of them had the chance of being remembered in textbooks. Some such living mummies were certainly able to use of their powers in the faculty against some brilliant colleagues. However the truth is that what gave to illuminist and later historians the sensation of a static intellectual environment was rather the peculiar teaching organisation: the stereotype reading and commenting on classical sources, where all the new ideas were lumped into the comments.

While in France, and, as we have seen in Montpellier, medical teachings are directly recruited into the university curricula, in Italy the medical faculties became established as acknowledged university curricula by a more roundabout way.

Late medieval medicine and its connection with universities and the early anatomical schools

While in chapter IV we have briefly sketched the history of the development of universities, we must here deal a little more with the development of medical faculties, as it was there that biology developed during the late Medieval and Renaissance times.

Many of the most ancient universities, such as Bologna, Paris, etc., were originally schools of Laws and of Theology, however in all of them, by the end of the 13th century, Medicine was taught as well.

The teaching of medicine in universities, however, met with some resistance from the masters of the senior schools and, in order to introduce it, the physicians had to adopt for their teachings a good deal of the methods and rules of the other faculties.

During all the early development of the universities the standard practice in teaching was the *mos italicum,* the Italian way, that is: the master, in the schools of laws, took as the object of his lecture some passages of the Justinian codex and 'glossed', that is discussed and clarified its meaning and significance (the derogatory meaning now currently inherent to this word, came to be when traditional methods were abandoned and especially when the method was thought to be linked with scholasticism). Much later, and in Italy with difficulty, the *mos gallicum* became fashionable, this being much more flexible and based on the comparative comment of different texts at the same time.

As I said, the medical faculties were obliged to adopt the legist's pattern of teaching in order to get official recognition.

At this point it may be useful to exemplify the general attitudes by the life and scholarship of a typical and celebrated medical master of the 13th century

Pietro d'Abano (1250-1315) was one of the most famous masters of his age. After a long stay in Paris and some rather obscure travelling, he went to Padua, where he continued to teach until his death.

Pietro was thrice tried for heresy, he was acquitted in the first two trials, while he died of a natural death during the third. Probably because, as he was dead, no one cared much of the outcome of the trial, he was finally condemned, his corpse was apparently exhumed and burnt in 1316. In order to understand which was the attitude of many lay administrations towards the Inquisition, it is interesting to note that when he was charged with heresy for the second time, the Commune of Padua ruled that all the expenses for his defence were to be charged to the civic administration and shared equally among the different quarters of the town (in fact the authority of the Inquisition in the Italian states, and especially in the Republic of Venice, was quite limited in comparison, for instance, with what was happening in Spain).

We do not know precisely which were the charges against Pietro, as the documents of the trials did not survive. It seems probable that he was charged of Averroism, and he was certainly a moderate Averroist, just as many masters of the Paduan university were both then and later. However, scholars, like Nardi, who made a special study of the philosophy of Pietro, do not think that his opinions could be strictly judged as heretical. Indeed, as I said, he was twice acquitted and even when tried for the third time, he was not jailed or otherwise restricted.

His two main treatises are the *Conciliator controversiarum, quae inter philosophos et medicos versantur* (= The peacemaker in the quarrels debated among philosophers and physicians), which aimed to solve the problems arising from the comparison of Classical and Arabic sources, and the *Lucidator dubitabilium astronomiae* (= The clarifier of what is debatable in astronomy). Pietro purposely did not contribute anything original. Indeed he was a classical scholar and translator, and had a good command of Arab sources (in their Latin translations), he thus aimed to identify everything common to the best Classical and Arab schools and, at the same time, to extol medicine as a science (and astrology, which was strictly linked with medicine), against the opinion of Aristotle and, even more, of extreme Aristotelians, who qualified both these as mere 'praxis'. Pietro is thus a typical example of a late medieval physician-scientist. He passionately advocated the dignity of sciences also for applied sciences, among which he included some astrology and magic. He was not an alchemist, but, on the testimony of people that he deemed as trustworthy, he considered some alchemical transformations as possible. Anyway when dealing with strictly biological subjects, such as reproduction or the theory of critical days, though still remaining within the mainstream of Aristotelianism, he is conspicuously original, though not necessarily right. Thus, when discussing reproduction, Pietro holds that generation depends both on factors intrinsic to the reproducing organisms and to external factors, inclusive of astral influences. However he considers that environmental factors are sufficiently strong to determine the spontaneous reproduction only of the simplest animals (which animals he deems 'simple' is another matter, we would judge his standards at least peculiar); anyway environmental factors are too weak to make spontaneous generation possible in the complex animals, such as mammals.

His defence of some magical practices and his rejection of others does not concern us, as they are considered merely in the framework of medical practice. Anyway Pietro is absolutely clear in separating 'natural magic' ('white magic' for the commoners), which merely aims to use of the natural features and powers, and is therefore 'science' and a good thing, from 'black magic', which tries to use occult powers, and must be absolutely condemned. It is precisely his characterisation of the 'magus' as a scientist which is typical of an attitude from which gradually developed science as we presently know.

We may omit the details of the reasons by which Pietro d'Abano maintained the need of astrology as an essential tool in medical practice, but we must remember that, in those times, no one seriously doubted that either the celestial bodies directly influenced earthly matters by their combined and varied influxes or that, at least, the Almighty, had planned for a precise correspondence between terrestrial events and astronomical configurations etc.; secondly: our ancestors had a considerable empirical knowledge of what we now call 'biorhythms' and of the changing biochemical proprieties of plants according their developmental phases and that it was 'obvious' to correlate such facts with celestial events.

Thus Pietro, while not an original thinker, is a paradigm of the philosopher-physician, caring both for his daily practice and for the theoretical background of such practice. Moreover he strongly maintains that, while there are some assumptions and logical developments that are needed by those whom he calls 'theologizantes' (such as the hypothesis of the tenth sky), these are not necessary for the 'philosopher', who should never use of hypotheses which are not based on observable facts; an attitude to which almost any scientist would subscribe.

If Pietro d'Abano is a remarkable example of the learned physician of late medieval times, another such person deserves at least a mention. This is the Florentine Taddeo Alderotti (c. 1215-1295) who from 1260 was lecturing in Bologna. He specialised in comments on Hippocrates, but his most notable work was written for the Florentine political leader Corso Donati; this is *Della conservazione della salute* and has the distinction of being apparently the first medical treatise written in a modern language.

While Padua in the XIV century is important as a centre of more or less heterodox Averroism, Bologna has the distinction of being the first where active study of the human anatomy was resumed.

Surgery had always been a basically empirical practice. Thus, this was one of the reasons why in most of Europe, physicians, who strove for being included into the lesser nobility, underrated surgery as requiring just the practical skills of the 'barber-surgeon'. In Italy, where the typical feudal nobility of the landed gentry was soon politically overshadowed by the merchant-patrician of the towns, though barbers were entitled to practice some minor surgery and there were a number of regularly certified 'surgeons' who had not graduated in the universities, yet surgery was always part and parcel of the physician trade. As surgery requires anatomy, so the rebirth of anatomy was a purely Italian achievement.

We have mentioned how the Emperor Frederick II had recommended the dissection of human corpses, and Guglielmo da Saliceto, in his *Cyrurgia* of 1275 had equally considered human anatomy as necessary and probably practised it. However evidence for early autopsies is obscure. There is no doubt that at least one autopsy on a man dead in an epidemic was performed in 1285 in Pavia and that, at the same age, in case of suspicious deaths the corpse had to be inspected by a medical panel and it is possible that autopsies were practised (one such was certainly performed in a case of suspect poisoning in 1302).

There is no doubt that the credit for the first 'anatomy' for scholarly purposes, by his own account, was done by Mondino de' Luzzi in January 1315.

Mondino was the son of a Bolognese apothecary, he was born in about 1270, was public doctor of medicine in the University of Bologna from 1314 to 1324, and died in 1326. His tomb in the church of St. Vitale is still extant and conforms with the standards of those times for University doctors, as its front shows Mondino teaching his pupils. Mondino was also an active and respected politician in his native town.

Appointed as professor in 1314, it is clear that he immediately felt the need for a better training in anatomy, so that already in January 1315, he was dissecting corpses. His *Anothomia* was issued in 1316 and, to be fair, it is a somewhat cursory work, only envisaged as a support for surgery. It is still completely subservient to Galen's teachings, but, at least, it provides practical rules for the dissection. It is interesting to note as an example of the trend in Medieval Italian Universities, that Mondino was helped in his dissections by two helpers: a certain Otto Agenius Lustrulanus, of German origin, and a young woman, Alessandra Giuliani from San Giovanni in Persiceto, who had specialised in the preparation and injection of vessels, and who died when barely 19 in 1326, the same year of Mondino's death.

The *Anotomia Mundini* became almost immediately a standard book for over two centuries throughout Europe. Shortly after the invention of printing it was published by John of Ketham in Venice (1493) as *Fasciculus medicinae* and it continued to be printed until 1558.

Again, well after the times of Mondino, the most authoritative and almost unique anatomical source was Galen. However, only the first 8 books and part of the 9th of his *Administrationes anatomicae* were available, besides some other minor treatises on specific anatomical problems and scattered remarks in his other works. Only in 1906 was discovered a complete Arab translation of its 15 books. Some spurious anatomical texts were attributed to Galen for a long time. Moreover that which was available to the European physicians only through Arab translations and commentaries depended on translations which were somewhat unreliable, just as it happened with the Latin translations from Arabic used in the schools. As remarked by Berengario da Carpi (see chapter VII), discrepancies both in text and in interpretations were far from rare. Thus when the teacher found discrepancies between what was being found in the corpses and Galen's opinions, it was easy for him to explain away the difficulty (a) by supposing that the original text had been either misunderstood by the Arabs, or corrupted by the copyist, (b) by assuming that the corpse that he was examining was abnormal, (c) sometimes even by assuming that some anatomical structures might have changed since Galen's times.

Though, as we shall see, at least in Italy, conditions were comparatively favourable to serious anatomical investigations, little of value was achieved throughout the 14th and early 15th century.

Anatomies were never forbidden either by the Church or by the common law (the bulla of Boniface VIII that is sometimes quoted as prohibiting anatomies, in fact is aimed only at stopping the practice of boiling the corpses of pilgrims to the Holy Sepulchre, in order to save the bones, which were thence sent back to the pilgrim's family for burial (and there were good profits in this sort of operations). Rather, when the 'Black death' ravaged Europe around 1350, not only were anatomies performed on the corpses of people dead from plague) by order of the public authorities in Florence, Perugia and many other towns, but even the pope Clement VI ordered such anatomies with the hope of discovering something useful.

In the Florentine archives of the 14th and 15th centuries there are several wills, both by men and women, who ordered that their corpse was to be opened, generally specifying that they thus hoped to accrue some advantage for the health of their children. No doubt this was happening in many other places.

In 1410, when pope Alexander V suddenly died in Bologna, his corpse was opened to discover the *causa mortis.* Likewise, in Florence, Lorenzo 'the Magnificent' and

Catherine Sforza, mother of Giovanni dalle Bande Nere, were both dissected within a few hours from their death.

Official 'Anatomies' were however comparatively rare. The commune had to provide just a few corpses every year (in Padua, Bologna and Florence just 2, one man and one woman), preferably people dead by hanging. Anatomies were done only in winter, when temperature allowed for the dissection to go through several days (and this was the origin of the tradition in Bologna, of the 'Carnival anatomy', which developed into a fashionable celebration attended by the high clergy, the nobles and their Dames (see chapter IX).

Though, as we said human anatomy was practised since the beginning of the 1300s, it was formally allowed by pope Sixtus IV at the end of the 15th century and the permit was reiterated by Clement VII some years later.

However, free anatomies had long been official: for instance, the Florentine statutes provided that if the university could get some extra corpses, the University's officials had to certify that the house where the dissection was performed was suitably located and fit, that each attending student had paid a gold florin in advance to cover the expenses for the sector, the subsequent burial of the corpse etc., being however entitled to a reimbursement if some money was left in the end. Such high charges were felt as unfair for most students, and so, for instance, the Venetian Doge, ordered in 1475 that all such fees were to be charged to the state treasury and that the relatives of people dead in the public hospitals were to be thus encouraged to leave their relative's corpses for anatomy. Otherwise the students used to steal corpses from cemeteries, so that these were usually guarded by watch dogs. For instance in Bologna we know that in 1319 four students (a team of four was the standard one for such adventures) were prosecuted for stealing the corpse of a girl and dissecting it with the master Alberto de' Zancaris. However, they were prosecuted for the theft of the corpse, not for its dissection, and, though we do not know precisely the ruling of the judge, it must have been lenient, as one of the students, Jacopo da Piacenza, went on to graduate, became bishop of Zagreb and personal physician to the king of Hungary. At a somewhat later age the great Vesalius wrote that 'corpses not given might be taken!'.

Such favourable conditions, however, as we shall better see in the following chapters, were rather peculiar to Italy, and this goes far to explain the extraordinary flourishing of Italian anatomy in the Renaissance, just as the attraction that the Italian Universities had for students from all over Europe.

While human and animal anatomy were, so to speak, 'incubating' their flourishing in the next century, physiology was still that of Galen, and, as well as pathology, was based on the theory of the four humours. Therapy, and, indirectly botany, were, again, the classical ones with some Arab additions. Indeed, as most remedies were prepared from vegetables (called 'simples'), the profession of herbalist or 'simpler' was an important and lucrative one. Therefore such books as we have already mentioned and which were usually termed *Horti* or *Hortuli'* (that is 'gardens' or 'orchards') were quite popular. Among them, apart from Dioscorides', two deserve mention: one is the *Hortus sanitatis,* an anonymous compilation originating from Mainz and, even more important, a Greek text of the 4th-5th century commonly attributed to an otherwise unknown 'Apuleius platonicus', which was commonly copied through the middle ages and which has the distinction of being the first illustrated botanical book printed (almost certainly in 1482) and now surviving in 18 copies.

As far as I can judge by leafing through Dioscorides and 'Apuleius', as well as through the famous late 16th century *Ricettario Fiorentino*, almost all the medicines prescribed did indeed include in their preparation mixes of really active drugs for the diseases for which they were recommended; these, however, were added with a number of other useless, but often costly, items.

The developments of biology in the late medieval and early renaissance times

The transition between Medieval and Renaissance times was a very gradual one, moreover it did not happen at the same time throughout Europe. Thus all dates suggested are just conventional.

Undoubtedly there were important changes in outlook between 1300 and 1500. These were sensational in the arts, but were quite significant also as far as sciences are concerned.

While the Middle Ages were anxious to recover the scientific texts of the Greeks and the Romans, the humanists, beginning with people like Petrarch (1304-1374) and Boccaccio (1313-1375), were much more concerned with the recovery of historical and literary works. Nevertheless the search also for scientific sources went on and much work and ingenuity were spent in critical editions of the ancient scientific texts available and in better translations.

While the earlier Middle Ages had done with a somewhat Platonised Aristotle as their guide in scientific endeavour, by the middle of the 15th century the increasing knowledge of both the genuine Aristotle and of Plato and the Neoplatonists, opened an increasing gap between the two schools. Georgios Gemistos Pletho (1355-1450) and the Florentine Academy begun a true Neoplatonist revival among the upper classes, while Aristotelianism remained entrenched in the universities.

There is certainly a certain amount of truth in the common opinion that the period of transition between the Middle Ages and the Renaissance was a period of increasing individualism, at least as far as one's opinions and judgements were concerned, as in the practical sides of life people's ambitions and actions remained very much the same. However, it was then fashionable to challenge traditional authorities, including the scientific ones. The recovery around l450 of a considerable corpus of documents allegedly due to Hermes Trismegistus (the thrice great Hermes), a mythic personality, who was supposed to have lived at the times of Moses, was to have a great and lasting influence. In fact these texts are of Egyptian origin and were composed in the 2nd-3rd century AD, which easily explains some influences of Jewish and Christian origin, which struck the late medieval readers. Some knowledge of the hermetic texts had long been widespread (for instance St. Albert the Great considered Hermes as a descendent from Prometheus, the Titan who stole the fire to the Gods to give it to humans, but that the pious medieval scholar, in true euhemeristic tradition, considered to have been a historic hero). However, the availability of the complete hermetic corpus had sensational effects. While a few soon decried the hermetic text, both as late and valueless, thinkers like Marsilio Ficino (1433-1499), Pico della Mirandola (1463-1494) and soon a host of other influential philosophers begun building on them a new Biblical-Neoplatonic-Hermetic theory of the world which was to have a lasting influence on sciences, including biology.

The flowering of arts, economy and culture which from Italy begun to spread through Europe sparkled new fire into the religious and intellectual debate. Political and mundane factors were also prominent, but, while the establishment of the Catholic church and most of that in the universities stood by the Biblical-Aristotelian synthesis arrived by the scholastic debates, the lay upper classes were increasingly attracted by the new philosophy, in spite of the bland condemnations by the Church.

However, both within the monastic orders and the common people there was a growing intellectual unrest. There is no question that the Reformation begun with the thuds of Luther's hammer nailing his theses on the door of the cathedral of Wittenberg *(*1517*),* but the symptoms of the brewing crisis can easily be found in the preaching and writings of a number of monks and of a few laymen throughout Europe for many years before. These people were seriously concerned that the growing influence of Greek science and philosophy was undermining the true Biblical-Evangelical faith. They were close to the early 'Fathers' and even more to Augustine. However, as we shall see in the next chapters, while the split caused by the Reformation caused a decline of the hermetic influences in the Catholic environment, saw them largely recruited into the scientific protestant environment.

Throughout the transition between the Middle Ages and the Renaissance, the considerable technical developments of the late Middles ages, were absorbed and investigated by both scholars and artists. It is usual to quote Leonardo da Vinci (more correctly: Lionardo, as he was christened and always signed), who is, indeed, the foremost example of such attitudes, but it would be easy to quote a number of other outstanding personalities of a 'universal genius'.

However, as it is Leonardo who, among them, was the most interested in the study of truly biological problems, we shall deal with him. As a comprehensive appreciation it may be said that (a) Leonardo was from some forty to eighty years in advance on his times in the various branches of sciences, (b) that with the possible exception of palaeontology, Leonardo's work is entirely irrelevant in the history of the advancement of sciences, as he was never able to organise and publish the results of his work, which thus became a favourite hunting ground for erudite research; something like the archaeological research that has shown that Norsemen had indeed discovered America over a hundred years before Columbus, but just to leave it alone for the Spaniards to land there for good by the beginning of the 16th century.

Leonardo da Vinci *(*1452-1519*)* thought of himself as of a self taught man, but it is clear that, though his Latin was rather poor and he had no Greek, he was extremely up to date in all the recent technical advances and well aware of the main scientific problems which were currently debated. He thus planned, though he never practically achieved, a number of treatises which broadly correspond in scope with those produced in the decades immediately following his death. As an artist and an engineer, which were his main qualifications, he was well aware that his age was at least equal, and in many fields much more advanced than classical antiquity and that while geometry was still very much that of the Greeks, mathematics was acquiring the technical tools to make classical mathematics very soon obsolete. This easily explains the scientific approach of Leonardo: a disregard for traditional authority against the new evidence and the need for an adequate mathematical groundwork for all sciences.

While there is evidence that practically all the Florentine artists contemporary with Leonardo practised anatomy (for instance Michelangelo was supplied with corpses by the Prior of the Augustinian convent of S. Spirit, who made available those of patients deceased in this hospital), Leonardo definitely set out to prepare an immense anatomical treatise in 120 books, for which he prepared hundreds of superb drawings. Conscious of his insufficient cultural background, Leonardo planned to write it with the co-operation of Marcantonio della Torre, professor first in Padua and then in Pavia, who, however, died in 1511, when only 33. In his anatomical studies Leonardo used many new techniques, like injection of coloured liquids in the vessels, of melted wax in the cavities of soft organs, like brain, inclusion of collapsible organs, like the eye, in coagulated egg's white, serial sections. In true engineer's outlook he tried to investigate the function of bones and muscles by mechanical models, interpreting the structures as levers, pulleys, pillars etc. Thus he made a number of new observations which were later rediscovered by different anatomists. Just to mention some of them: he drew the frontal sinuses, Highmore's cavity, noticed that the sacral bone is composed by five vertebrae and not by three, as it was often believed in his times. He made serial sections of the brain, and paid a good deal of attention to the heart and vessels (though he did not think of any amendment to Galen's theory of circulation). He made excellent figures of the human uterus and of foetuses and of their membranes. Most people know of his studies on the flight of birds and on the possibility of flying machines. He was obviously interested in several aspects of physiology and may have written around 1515 a little treatise on respiration, for which there are some surviving notes. As an artist he was interested in vision and made several investigations on problems of vision, etc.

His drawings of animals show a keen observer, but his studies of plants are more interesting and advanced: indeed he studied phyllotaxis, investigated the possibility of using growth lines to establish the age of plants and considered the movements of lymph.

Leonardo's studies on geology and palaeontology may well be the only ones to have been influential on the subsequent development of this branch of science. It appears that his ideas were familiar to Gerolamo Fracastoro, as we shall see in the next chapter. In his notes and drawings there is a number of passages giving correct interpretations of geological structures and of the fossils. He not only thought the fossils to be organic remains, which was an idea current in Italy, but he flatly refused the possibility that they were the remains the Biblical flood. His remarks on the growth lines on seashells and on the fact that one could find small shells growing on larger ones, and the taphonomy of the fossils were to him proof that the animals themselves had been living for a long time where they are presently discovered. Leonardo's discussion of fossils is part of a general theory on the growth of mountains, based both on ptolemaic astronomy and Aristotelian views on 'natural' motions, and, although it is entirely wrong, yet it is interesting as it envisages the possibility that there will develop irregular pressures inside the Earth and that such local underground pressures slowly push up the mountains.

A famous, but rather cryptic, text of Leonardo on a 'dragon' having lived in a distant past and whose bones now lye buried under rocks, has been argued to show that Leonardo had thought that fossil bones might have been the remains of past and strange animals.

Had Leonardo been able to write and publish the many treatises that he was planning, there is no doubt that they would have contributed the most significant advances in several sciences and especially in biology for centuries. As they are, they are proof of a frame of mind that, if more conscious and advanced in Leonardo than in any of his contemporaries, yet was typical of the age and portentous of the scientific explosion to follow in a few years.

Indeed the closing years of the Middle Ages opened the age of the great geographical discoveries (in 1488 Bartholomeu Diaz passed the Cape of Good Hope, in '92 Columbus reached the Antilles, in 1497 Vasco da Gama reached India by sea); in the meantime the growth of Turkish power helped to deflect the main trade routes from the Mediterranean. Thus a new flood of information was heralded.

Meantime some Italian mathematicians made substantial advances in algebra, such as were prerequisite for the development of the new astronomy in the next century.

Even more significant for the growth of sciences was the first practical success in printing, when Gutenberg, in 1455 printed the Bible with a machine using movable letters and produced with the financial and possibly technical support of a Doctor Faust, a learned, but somewhat shadowy figure, who later was to deprive Gutemberg of much of the profits accruing from his printing device[2]. Within ten years books were printed in Italy and other countries besides Germany. In 1477 the earlier 'herbal' by 'Macer Floridus' (without figures) was printed and in 1482 an illustrated edition of the famous 'Apuleius platonicus' was printed in Italy, the first illustrated scientific book printed.

[2] The real Doctor Faust, who was the originator of the legends embodied in Goethe's, Marlowe's and other's dramas and operas is a shadowy figure of somewhat later date, but it is possible that some memory of Gutemberg's associate may have been incorporated into the myth.

CHAPTER VII

The Renaissance

SYNOPSIS OF SOME CRITICAL HISTORICAL EVENTS AND OF THE MAIN SCIENTIFIC THINKERS

1453 end of the Hundred Years War.
1455-1485 Wars of the Roses.
Giovanni Marliani 1483, Berengario da Carpi c. 1460-1530, Otto Brunefels 1488-1534, Georg Peurbach 1423-1461, Johannes Regiomontanus 1436-1476, Nicolaus Copernicus 1473-1543, Girolamo Fracastoro 1484-1553, Theophrastus Paracelsus 1493-1541
1516 Charles V becomes king of Spain, in 1519 is elected Emperor, abdicates in 1556.
1534 Act of Supremacy: establishment of the Church of England.
1566 Netherlands rebel against the Spaniards.
Nicolò Tartaglia 1500-1557, Girolamo Cardano 1501-1576, Leonard Fuchs 1501-1566, Guillaume Rondelet 1507-1566, Michael Servetus (Miguel Servet y Reves) 1511-1553, Andreas Vesalius 1514-1564, Conrad Gesner 1516-1565, Pierre Belon 1517-1564, Andrea Cesalpino 1519-1603, Giovanni Benedetti 1530-1590, Jacopo Zabarella 1533-1589, Fabrizio d'Acquapendente 1537-1619, Tycho Brahe 1546-1601, Giordano Bruno 1548-1600.
1558-1603 Elizabeth I queen.
1571 the Turkish fleet is destroyed at the battle of Lepanto.
1588 the English defeat the Spanish Armada.

The 16th century

The 16th century saw throughout Europe the development of the *renovatio*, the 'Renaissance, which had been in progress in Italy since the previous century and which for scholars went with the elated sensation of living a new era, when finally the old and glorious antiquity was renewed, unbound by either the constraints of a dogmatic tradition or the binding effects of a culture which had been deprived for centuries of the benefits of a good deal of what should have been its natural cultural inheritance. Thus the 16th century was characterized by several factors which were instrumental in speeding up a rather overall cultural change.

We have seen how already in the 15th century there was a growing interaction between the world of scholars and that of the technicians, and how this was fostered by the increasing influence in the courtly, learned media of such scientists-artists who are best exemplified by Leonardo, the greatest of them, but who were quite numerous.

The traditional antinomy between the *Bios theoretichos* and the *Bios praktichos*, which went back to Aristotle and which had been dwindling for some time because of the increasing influence of the merchant and artisan guilds, was quickly obliterated by the discovery of printing.

Indeed during the Medieval centuries a good deal of empirical knowledge had been built up by the 'practitioners' (one may just mind the elaborated knowledge of statics and of the technique of the simpler engines in order to build the masterpieces of Medieval architecture or of ships worthy of the high seas). However most of this knowledge was transmitted either orally or by rare manuscripts holding 'the secrets of the trade' and, that just as secrets, were handed from one generation to the next by the craftsmen.

But as soon as printing became possible, there was a quick proliferation of practical handbooks on all arts and crafts, and these are often translated into a number of local tongues. Some of them, such as the books by Agricola on metallurgy and on mines, are now considered as the forerunners of the entirely new scientific fields of geology and mineralogy.

At this time a number of practical needs, for instance the problems relating to seafaring in the high seas or those of applied hydraulics, required for their solution more and more the contributions of basically theoretical scholars.

In the field of biology, herbals and books of anatomy are the reply to the practical needs: surgery and pharmaceutics, but are written either by members of medical faculties or by people rather closely associated with them and, therefore, who strive for scientific accuracy or, as they said at the time, for philosophic rigour. They can thus rank as scientific texts rather than mere guides for empiricists. Their authors were striving not only to satisfy the immediate needs of readers who asked for some reliable information, but also by a sort of urge to excel, striving for personal 'glory'.

However, the reader must remember that the total number of people involved, as far as biology was concerned was incredibly small: as a whole there were between 60 and 80 universities in the whole of Europe and that means that the faculties of arts and the faculties of medicine could hardly reach about 100; most universities were tiny and local establishments, where hardly any research work was done and, even in the major ones, a medical faculty would probably number about a dozen members and most of them were professors involved with the practical teaching of the treatment of diseases, thus leaving perhaps 2-3 people in an average faculty to delve in anatomy, physiology, botany or zoology. This means that at any time during the 16th century, there were hardly 400 people in the whole university establishement of Europe who may have been engaged either in active research or in the revision of ancient knowledge. No one has made a systematic study of the number of publications on biological subject produced during the 16th century and of their authors, but, if we deduct simple compilations and abridgements, such as were produced for the student's usage, and we shall mention some, I would not be surptised if the whole production of original research through the century was the work of less than 300 people.

The many cultural trends typical of this age of change not only had a different influence on the various authors, but were also prompting the print of a number of scientific-magic books.

Thoughout the 16th century and the following one, scholars, with almost no exception, firmly believed in the validity of the Bible (New Testament included) as a work of divine revelation and argued for taking the study of Nature into the religious debate of the Reformation, or, at least, they maintained that the understanding of Nature is a way to the contemplation of God's works.

On the other side the men of the Renaissance are consciously strongly individualistic and that, in a number of instances, prompts them to that typical individualistic activity that is Magic. It was only very slowly that, during the next century the magicians evolved into corporate academicians.

The current and necessary religious debates of this age tend to merge with the debates on magic and the result is the increasing separation between 'black magic' and 'natural magic', which more and more approaches the canons of modern sciences.

Thus began a process, which would win the day around the middle of the next century, and which condemned all secrecy in matters of science and discredited any 'esoteric knowledge' transmitted by obscure vocabulary to just a few adepts. It was indeed a long process, plagued at times by collective crazes, that resulted on one side in a pullulation of magical and alchemical texts and in the 'witch-hunt' craze of the first half of the next century.

Paracelsus

Paracelsus, both as a man and for his ideas, could well be ignored in a history of biology, for, in spite of the multiplicity of his interests, he never dealt with pure biological problems as such: he was a physiscian and merely a physician. However, the influence of Paracelsism over the whole of the scientific thinking during the period ranging from the second half of the sixteenth century until well beyond Newton's times, was such and so complex that it is necessary to pay some attention to this strange man.

The influence of Paracelsism was great mainly in the 17th century century as we may well say that throughout the 1600 the scientific world was divided between Cartesian mechanists and Paracelsians. Indeed we largely owe to Paracelsism that Cartesian mechanicism was never able to rule Western thought, and we shall see how a good many of the more important biologists up to the age of 'Enlightenment' were more or less thinking along Paracelsian lines. Thence, during the '700, hand in hand with 'Great Alchemy' wanes Paracelsism, (but for Germany, where there was a 'revival' of Paracelsism linked with the development of 'Naturphilosophie' and the romantic movement). Since the 18th century Paracelsus is more and more ridiculed and decried and positivist and materialist authors of the 19th century hardly have a good word for him.

There is no question that it is difficult to give any credit to a gentleman who, in his writings, quite seriously teaches us, among other things, all that is necessary to implement in order to create a 'homunculus' in an alambic and thence how to grow him to adulthood!

There is no doubt that Paracelsus' ideas in matters of biology were just as completely crazy as those of Cartesius, but they are crazy to us modern, they were reasonably plausible, given the knowledge of these times, to Paracelsus' contemporaries,

Paracelsus, as a physician and as a scholar was hotly discussed even during his lifetime: some extolled him as the great innovator, who went well beyond all the existing schools, while for others he was a despicable quack!

Theophrastus Philippus Aureolus Bombastus von Hohenheim, who later took the name of Paracelsus Eremita, was the son of a physiscian and was born in Einsiedeln, Swizerland, either in 1493 or, perhaps, in 1490. The Bombasts von Hohenheim were a noble and powerful family in the region and Paracelsus' father may have been an illegitimate scion of it.

Just as the famous 'magus' Cornelius Agrippa, Paracelsus was probably a pupil of abbot Johannes Trithemius of Spanheim (1462-1516), a neoplatonist, a learned astrologist and occultist or, better in the terminology of the times 'a magus of natural magic' and, also, a pioneer student of cryptography (curiously the manuscript of Trithemius' book on cryptography was bought by John Dee, physician and astrologer of Elizabeth I and a friend of Harvey, and was to become the the basis for the ciphering methods of the British Elizabethan secret service).

There is an unverified tradition that later on Paracelsus travelled extensively through Europe, went to Rhodes and, perhaps, even to Istanbul and Egypt. He attended some courses at different universities: listened to Berengario da Carpi in Bologna and to Nicolò Leoniceno in Ferrara. There is an unverified tradition that he got his medical degree in Ferrara and the tradition that he met there with Savonarola is mere legend, as Savonarola had moved to Florence before 1490 and was executed in 1498, when Paracelsus would have been about 5!

In 1526, thanks to the lobbying by the printer Froben, he was appointed both as professor of Physics and Medicine in Basel and as public physician of the town. In Basel he got an excellent repute as a practitioner and crowds flocked to his lectures, that he was giving in German, to the great distress of the Faculty.

Indeed Theophrastus Bombastus Paracelsus was just the bombastic character equally proficient in getting as many dedicated enemies as were his devoted admirers and this makes it impossible to assess him as a man. He went out of his way to advertise his refusal of traditional medicine by publicly burning the books of Galen and

Avicenna (but he praised Lullus). His quarrelsome habits and the death of his friend and promoter Froben, made Basel uninhabitable for him, and he left in 1528, possibly to escape jailing, as he had called the Bishop to trial for some payments and lost his case. Since then he was always on the move through central Europe and about him it is hard to tell facts from legend.

To give a hint of the man who said of himself: 'I am the Monarch of all the physicians' and of his ways, let us quote this sentence: 'The physician enlightens matters because he knows the cause and also the ways to digest and prepare the medicines; but which sort of light are you able to give, you doctors of Montpellier, of Vienna, of Lipsia? Just about as much as a golden fly on the results of a bout of dissentery!'.

Paracelsus meddled, on the strength of his fame (but always isolated), in the religious debates of the times (it was just that of the raging Protestant reformation). He died in 1541 in Salzburg and the Bishop-prince honoured his coffin with solemn ceremonies.

Throughout his agitated life Paracelsus wrote a number of books in Latin, German and in an abominable mix of the two, all crammed with new words of his own creation. He dealt with philosophy, with many medical problems, with alchemy, mineralogy, magic and prophecy. In fact his prophetic writings and the *Der grossen Wundartznei* (1536) are almost the only ones which were printed in his lifetime and almost all of his enormous production was published after his death.

There is no question that his outright region to medical tradition had a great significance for the following development of sciences, but what did he actually advocate?

Paracelsus was an outspoken advocate of 'natural magic', which Renaissance development was rooted in the philosophy of Marsilio Ficino and Pico della Mirandola. He was equally sure of the significance both for the individual and for the world as a whole of astral influences. He was unquestionably a competent alchemist and he must get credit for insisting, against most of his colleagues, on getting for each experiment precise quantitative measurements. Moreover he introduced into medical practice several chemicals of mineral origin, but as to that he was not the only one. Anyway Paracelsus employs mathematics with very different aims from those of tradition (and of later science): his is a sort of mystic mathematics, more akin to Pythagoreanism or Kabbala than to any orthodox usage. The mystic aspects in Paracelsus were certainly significant in spreading his teaching through the next century.

He and most of his followers were troubled by the influence of Aristotle, a philosopher who advocated a number of theories absolutely incompatible with Christianity, and of a physician, Galen, that had coarsely disapproved of Christians. The traditional attempts to reconcile these two thinkers with Christianity were, to them, a signal and hypocritical failure. If Christanity was rooted in the Bible and especially in the Prophets, all Christian science had better to look either in the sacred Books or search directly in the signs that God had put into Nature to help mankind. Indeed, as we shall see in the next chapter, most paracelsians were either Protestants, or, at least sympathized with the different reformed creeds, while Catholic schools remained the strongold of Galenism or went over to Cartesian mechanism.

In his *Volumen medicinae paramirus*, which is perhaps the best studied of his books, Paracelsus says that Nature is the macrocosm, while man, who is its most perfect part, is a microcosm made by the same substances and ruled by the same laws. Man duplicates in himself all the phenomena of the macrocosm and thus suffers form all sorts of cosmic, astral and telluric influences, and so far his ideas are not significantly different from Ficino's). For Paracelsus (but not for all later paracelsians) organic bodies are made by the traditional four elements. Beyond them there are three principles, that he calls 'salt', 'sulphur' and 'mercury' (which, obviously, are not the substances commonly known by these names, but are rather elements provided with their respective general properties. Moreover there is a fourth class of active principles, the 'archaei' which are endowed with the vital force, and are in fact the quintessence of life. Each organ works by virtue of its own archaeus. The universe, besides material entities, is alive with active spiritual entities, who, however, have no soul (they are, according to him, purely 'mercurial'), such as sylphs, nymphs and so on. Thus Paracelsus classifies diseases according their supposed cause: *ens astrale, veneni, naturale, spirituale, deale*. Each one of the main organs is supposed to be under the special influence of a celestial body: the liver is linked with Jupiter, the heart with the Sun, the brain with the Moon, the spleen with Saturn, the lungs with Mercury, the kidneys with Venus.

In his pharmacopaea Paracelsus grants considerable credit to a traditional lore, that of 'signature', which had been largely adopted by both classical authors and medieval physicians of Scotists tradition. This assumed that the active priciples obtainable from plants, animals or minerals are, so to say 'advertised' in the plant itself in some visible way linked with either the organ on which they act or with the kind of disease they cure. So, for instance, the plant *pulmonaria* is good for lung diseases; *Hypericum* which has perforated leaves, is good for wounds by pointed weapons; peony, the pistil of which resembles a human brain, is good for nervous troubles.

The ideas of Paracelsus on diseases are strongly tinged by his personal neoplatonism and, as a whole, Giordano Bruno, with precise reference to the *Volumen medicinae paramirum,* charges Paracelsus with mere plagiarism of Lull. Bruno's judgement is a weighty one as he was fully conversant with both authors. Personally, as far as I can judge and considering the absence of adequate comparative studies, I think that Paracelsus gave a rather personal interpretation of Lullism, with a strong medical bias and tinged by his personal battles in the turmoil of the Reformation.

Thus, thanks to the subsequent influence of Paracelsian ideas, Lullism, in its Paracelsian make up, was to become, often unknown to subsequent scholars, a powerful brake on the spread of Cartesian mechanicism. Moreover Paracelsus, just because of his refusal to accept traditional authority, was an experimentalist and bequeathed such a penchant to his followers, with great benefit for the development of biological sciences.

It is clearly hopeless to search in the writings of Paracelsus for any methodical approach to problems, yet there is in his ideas a hard and coherent core in his firm belief that biological phenomena have a close correspondence with alchemical transformations. This does not mean that he thought biological phenomena to be, in fact, chemical processes in a modern sense: he could not possibly have had any hint of something alike modern chemistry, but this belief prompted, as we shall see, a host of experiments and researches which, in time, developed into biochemistry.

Vesalius and the reformation of anatomy

As we have seen, Leonardo's anatomy precisely fitted into a widespread interest in his times. Almost all histories of medicine and biology consider the publication of Vesalius' *Humani corporis fabrica* as the turning point from classic-medieval to modern anatomy and almost as often Vesalius is characterized as a great innovator. That Vesalius' work had an enormous impact is certainly true: it was a sort of blueprint for the following development of anatomy as well as being by itself a most considerable advance on previous knowledge. Yet, in truth, advances in anatomy had begun to accrue just in Leonardo's times. One may remember people like Alessandro Achillini (1463-1525) who, besides other facts, appears to have first described the malleus and incus in the middle ear and the excretory duct of the submaxillary gland, which is commonly known as 'Wharton's duct'. Even more deserving of consideration is Berengario da Carpi (1470-1530), his actual name was Jacopo Barigozzi, another contemporary of Leonardo (1452-1519), who made a number of 'anatomies' and published a radical revision of the anatomy of Mondino, in which he corrected a number of mistakes. Even better is his short *Isagoge breves in anatomiam humani corporis*. Berengario described several hitherto unknown structures such as the thymus, the sphenoid sinus, the coecal appendix, etc. In fact, as Berengarius writings were not much read outside Italy, many of his discoveries were later 'rediscovered' by other anatomists. Berengario gives also a discussion of the function of the heart's valves which may rate as a first step towards the understanding of the blood's circulation. Last but not least, Berengario's works have excellent illustrations.

It is quite possible that Vesalius came to Italy because here there was since long a good tradition for the dissection of human corpses both by university teachers and by artists, and because of the much better quality of illustrated books produced by Italian printers. He he was also probably aware that, at least in some Italian states, freedom of investigation was better guaranteed than elsewhere. Falloppio, who was himself a pupil of Vesalius, and thus an excellent judge, calls Berengario "*Restaurator* *anatomicae artis, quam Vesalius postea perfecit"* (the restorer of anatomical art, which later Vesalius completed).

There is no question that in biology one cannot fix, even in a conventional sense, a date for the transition between the medieval times and the Renaissance. In fact from the middle of the 15th century to the beginning of the 17th, that is through some 150 years, there was a gradual transition in every science and the, so called, scientific revolution lasted through all the 17th century.

In fact rather than in the triumph of the experimental method (which has been always generally appreciated), the 'scientific revolution' witnessed the conflict between an anthropocentric approach, which we may qualify as 'medieval', strongly tinged of hermetic-neoplatonic streaks and well suited for any of the different possible interpretations of the Scriptures (literal, symbolic, etc.), by its assumption of a strict correlation, by a divine design, between the universal 'macrocosm' and the human 'microcosm', and an attitude much closer to the classic thought and especially to the Aristotelean-Democritean ideal, in which man has an ever smaller share in the great book of Nature.

The divine qualities *Bonitas, Magnitudo, Duratio, Potestas, Sapientia, Voluntas* etc. through which, in typical Medieval thought, creation and its laws came into being, during this long period were slowly replaced by an impassive 'Nature'.

The common tradition that the experimental approach was undervalued in the Middle Ages fails to appreciate that, while the value of empirical evidence had never been doubted, students simply lacked the technical means to develop such experiments as were conceivable.

The Italian mathematicians between 1500 and 1600 produced the mathematical instruments needed to develop both astronomy and physiscs. Much in the same manner the results of geographical explorations and the new optical instruments paved the way for biology to explore new paths.

Usually during the period preceding the Lutheran reformation, the Catholic Church was quite tolerant and often even encouraged both philosophic research and scientific speculation. For instance, Copernicus was even invited by the pope to cooperate in the reformation of the calendar. Even during the early years of the Reformation the Catholic Church was much less intolerant than were Luther, Calvin and other reformers (by the way Luther himself considered the fossils as evidence of Noah's deluge). It was slowly, and especially during the second phase of the Tridentine Council, that dogmatism and intolerance gained the upper hand in the Catholic world. This occurred just at the time when the multiplication of the protestant churches, opened a multiplicity of paths through which scholars could manage to foster their sciences in spite of the many local synods.

In Italy the tradition of Academic liberties was quite strong, even in spite of the influence of the Church and new ideas were burgeoning. Perhaps the first steps had been taken at the court of the Medicis by the open neoplatonism of Marsilio Ficino.

Cosimo senior, having bought a Greek manuscript of Hermetic texts, in 1460 charged Ficino with its publication as a sort of preface to the publication of the Greek text of Plato's dialogues. Thus the basic Hermetic text, the *Tabula smaragdina* became available in its Greek original, while, until then, had been available only the translation from Arabic by Hugo Santallactensis of approximately 1140. Shortly before the Greek platonist Georgios Gemistos Pletho (1355-1450) had been teaching in Florence to a selelect audience and advocating a revival of classical pagan religion. Thus a number of prominent scholars, such as Giovanni Pico, count of Mirandola, became platonists or, rather, awowedly neoplatonists. The Church was worried and, for instance, condemned several theses of Pico (much to his distress and surprise), but almost without any practical consequence.

Thus the interest of Copernicus for the heliocentric model was aroused by the neopythagoric-neoplatonic influences of Pico della Mirandola and Maria da Novara (a man in spite of his name) who, in turn, trod the path opened by cardinal Nicholas Cusanus (1401-1464), who had considered the possibility of a moving Earth and was himself a student of Lull.

Because of local interest in naturalistic-medical studies, three universities: Padua, Bologna and Pisa, were especially prominent in the development of Anatomy.

Padua was especially lucky as it was a domain of the 'Serenissima' republic of Venice, which was especially jealous of its autonomy with respect to any foreign authority, even the Pope's (for instance a statute prohibited any member of the Senate of the Republic who had as a relative either a bishop or a cardinal to participate in any debate when Church's matters were involved). Thus the Republic was always keen to avoid that religious problems could interfere with the functioning of institutions, such as the Patavine University, which brought both fame and money to the state.

This was so much so that during the 16th century over 5000 students matriculated in the 'German Nation' alone, and they included even Poles, Ukrainians and Russians.

Indeed in Padua, at least in the faculty of arts and medicine, admission of both Jews and Protestants was always free and they could even hold chairs (between 1517 and 1619, 80 Jews graduated in Medicine and a further 149 graduated between 1619 and 1721!).

In truth up to the middle of the 16th century even the Popes were fairly open minded: so, in 1555, pope Julius III with a special bulla granted to the Jew Leone Benaia his Doctorate in Medicine with full freedom to practice both for Christians and Jews. When the more bigoted Pius IV, in 1565, ordered that, for the Bishop to be able to grant the doctorate in a church, the candidate had to formally profess the Catholic faith (after all a reasonable implementation of the original medieval statutes), the Venetian Republic immediately ordered that non-Catholics were to get their honours by a palatine count and shortly afterwards arranged that such degrees were to be granted by an appropriate magistrate at the Collegium Venetum and even adapted the text of the degrees granted to the faith of the grantee. So, for instance, the degree of a Jew was granted ‘In the name of the Eternal God, common year 1565’ and that of none less than William Harvey, himself a protestant, was ‘In the name of Christ, Amen, in the year from the Virgin's parturition 1602’ so as to avoid any mention of the Pope or of the Catholic Church.

Resuming our narrative as far as anatomy is concerned, just as the works of Erasmus and others had paved the way for Luther's theses, so the work of the Italian anatomists may be considered as a sort of preparation for that of Vesalius.

Andreaé van Wesele (Andreas Vesalius) was born in Brussells from an illegitimate branch of a noble family who were traditionally physicians on December 31, 1514. After a thorough preliminary education, he studied medicine in Paris, where anatomy was taught by the famous teacher Jaques Dubois (Jacobus Sylvius, not to be confused with the later Sylvius, a Duch anatomist of the 17th century, whom we shall mention further on). Sylvius had been originally a linguist and had gained a renown by his knowledge of Latin, Greek and Hebrew. Later on he had become a learned, good and passionate anatomist, who discovered, inter alia, the venous valves of the Azygos vein and made some significant contributions to animal anatomy. One of his pupils tells us that, as he was rather poor and had no servants, when he was able to obtain some, possibly stinking, piece of a corpse from the gibbet, in order to show it to his students, he used to carry it hidden in the ample sleeves of his gown. On the other side Rableais, who had been a fellow-student with him in Montpellier, is positive that Sylvius had definitely a prickly character, with whom it was difficult to get along. Besides Vesalius, Sylvius had as pupils such greats as Servet, Gesner and Estienne. Had Sylvius avoided his controversy with Vesalius, he would be honourably remembered because of his several notable discoveries both in human and animal anatomy.

When Vesalius was studying in Paris, he attended lectures by other notable teachers, such as Jean Fernel (1497-1552), a notable mathematician, whom some sources claim to have been sceptical of medical astrology, though, as a physician to Catherine de' Medici, he cured her sterility with magic-astrologic practices, as shown by some talisman-medals, apparently done on his specifications. Also Fernel was a good anatomist, though his discoveries are usually overlooked. In fact he described the rachidean channel of the medulla (which escaped Vesalius). A third distingushed anatomist who was also available in Paris at that time was Johann Guinter (or Winther) of Andernach (1497?-1575). At a later time, he quoted evidence from Vesalius, only to recant later. Guinter was especially friendly to Servetus. Vesalius, instead, did not think much of him.

Vesalius, as a Belgian, and therefore a Spanish subject, had to leave Paris because of the war between Francis I of France and Charles V (1536). He moved to Louvain, where he continued his studies. However, though he tells us how, little by little he was able there to pick up a complete human skeleton, he was unconfortable there and thence he worked for a little while as surgeon in the Imperial army. Then he moved to Padua. There the Faculty, on December 5, 1537 granted him, at the age of 22, his medical doctorate. The next day, after dissecting a corpse, 'doing an anatomy', as it was termed, he was appointed directly as a professor to the chair that his promotor had vacated for him! It is obvious that upon his arrival the faculty of Padua was already aware of the exceptional merits of Vesalius.

Thus Vesalius begun his celebrated lectures in Anatomy. Of these lectures we still have an account by Vitus Tritonius, one of his pupils. The frontispice figure of his great work shows his protrait in the very act of demostrating some anatomical details. His eloquence, passion and proficiency soon gained him such acclaim that there was no classroom sufficiently large to hold all his audience. Such was his fame that twice he was invited to practice a dissection in Bologna. Of his second visit there, we have an amusing account by Baldasar Hesler: Vesalius was acting as sector in a course of January 1540, while in the chair was Matteo Corti. Corti was a Galenist, who maintained that Mondino had been wrong whenever he criticized Galen, meanwhile, Vesalius, while claiming himself to be a Galenist, was showing to the students, by his dissection, the mistakes of Galen!

In October 1539, the Venitian government, under pressure from the students, increased the salary of Vesalius from 40 gold Ducats, to the unheard of amount of 60! Moreover Vesalius had the full cooperation of the local authorities so that he could avail himself of a number of corpses of executed criminals. Vesalius himself tells us that, on some occasions, the executions were scheduled when they were most convenient for him. Thus at least once he was able to examine the heart and pericardium of a corpse within minutes of the execution!

In 1538 Vesalius published in Venice six anatomical plates, which were extremely successful in the schools. Though they were much better than any previously available plate, they still include several of Galen's mistakes. One of these, for instance, is the *rete mirabilis* in the hypophyseal region. This does, indeed, exist in several mammals, such as Artiodactyls, but not in man.

Later Vesalius wrote a commentary on some books of Galen included in the complete works of the Pergamene physician being printed in Venice by Giunta. It must, indeed, be remembered that, when his experience told him that the Greek had been wrong, Vesalius made no bones about criticising the anatomy of Galen, but in his medical practice, he remained basically a faithful galenist.

In 1543 Vesalius went to Basel for the final correction of the proofs of his magnum opus: the seven books of the *De humani corporis fabrica*, which was issued in June 1543, by the editor Oporinus (Johann Herbst, 1507-1568).

Some comments are useful here: Oporinus had been a pupil and secretary to Paracelsus and, when his master had left Basel, he had followed him for a while. Moreover Oporinus was the second editor to publish a Latin translation of the Koran, which in practice was the first, as the sale of the previous Venitian one had been prohibited. Oporinus' edition of the Koran had two introductions, one by Luther and the other by Melancthon. So (a) Oporinus was, as a printer, fully qualified to print Vesalius' work, (b) as in his book Vesalius criticised Galen, it was difficult for him to employ as printer the Venetian Giunta, who was just publishing the entire Galenic corpus. (c) finally the Paracelsian Oporinus was an ideal editor for a strongly innovative work, as the work would stand as a counterweight to the illustrated edition of Galenus published in Basel eleven years previously by that editor Froben, whom we met as sponsor of Paracelsus. Thus the book appeared acceptable both in Catholic and Protestant lands.

On May 12 Vesalius performed a public anatomy and the skeleton that he finally prepared is still preserved by the University of Basel. Leaving Basel, Vesalius paid a short visit to his native country and then went back to his teaching in Padua.

Shortly afterwards Vesalius was again invited to lecture and dissect both in Bologna and Pisa. In Pisa he was received with great honours by the Grand-duke Cosimo I de' Medici, who was anxious to rejuvenate this ancient University, which, after the final conquest of the town by the Florentines, has ceased all activities. The Grand-duke was planning to bring the University to its maximum splendour in order to help the languishing economy of the town and, for this purpose, he even abolished the Florentine University. Vesalius was offered a chair, but he finally settled on going back to Padua.

Shortly afterwards Vesalius was appointed as personal physician by the Emperor Charles V with a big salary and Vesalius, who was sensitive to such temptations, although not yet 30, left his chair. During this period of his life Vesalius proved a successful military surgeon. In 1556 Charles made Vesalius a Count Palatine.

Though the vast majority of physicians immediately accepted Vesalius' new anatomy (which, by the way were publicised in Germany by Fuchs, and in England by a Thomas Geminus with a *Compendiosa totius anatome delineatio*, 1545, which is but a poor summary of the *Epitome,* a fact about which Vesalius complained) yet there were obviously several conservatives who were critical of him. Most vocal was old Sylvius, who in 1549, criticised Vesalius without naming him, and again in 1551, nicknaming hin Vesanus (= insane) and who charged Vesalius with impiety, to infect the whole of Europe by his ignorance, and went so far in his foolish cricicism as to condemn the usage of figures in anatomical treatises, probably as it had been just the quality of the illustrations that had helped in the immediate success of Vesalius' writings. If one asks himself why Sylvius was so vocal against Vesalius, when he had certainly the possibility to check to his own satisfaction, the truth of his opponents descriptions, one can answer that this is just an example of that blind commitment to one's own consistency that is so common among academics and politicians. Sylvius had once written. "After Apollo and Aesculapius, they (vid. Hippocrates and Galen) were the supreme authorities in the field of medicine, perfect on every account, and both in physiology and in other branches of medicine, they never wrote anything that was not absolutely true".

After the abdication of Charles V (1555), Vesalius spent some time in Brussels. Later (1559) he moved with his family to Madrid in the service of Philip II. As a physician Vesalius was a Galenist and his renown as a practitioner was as good as that as an anatomist. He kept in touch with the best medical scholars of the day and so, in 1564, he corresponded with Giovanni Ingrassia, whom we shall meet further on. In 1564 Vesalius was again in Venice, officially for the publication of a criticism to the *Observationes Anatomicae* of his pupil and successor in the Patavine chair Gabriele Falloppio (and by that publication he did himself a disservice: he apparently had not checked the facts and thus he criticised Falloppio, who had recently died, just where the latter was right). It appears that the whole affair was a mask for an underground dealing with the Republic to get back the Patavine chair, while keeping the Spanish king in the dark. Probably the dealing was successful and Vesalius left for Jerusalem, apparently with the promise to be back by the beginning of next term, but on his return, he died in Zante in obscure circumstances towards the end of 1564. The reasons which prompted Vesalius to leave Spain and to go to Jerusalem have never been explained, in spite of much legend and speculation.

The *De humani corporis fabrica* is a superb folio with many excellent plates, some being the masterpieces of a Belgian pupil of Titian, Jan Stephan van Calcar (1499-1546); many further drawings are in the text and, as a whole the iconography is vastly superior both in beauty and precision to that of any preceeding book. The text is both original, clear and alive with personal experiences. It includes also many helpful technical details. and quite a few personal reminiscences.

Vesalius' first original observations concerned the lower jaw: when still a student in Paris he had noticed that, contrary to Galen's dictum and to what actually occurs in many mammals in which the jaw is formed by two suturated bones, the human jaw is from birth made of a single one (an Arab author of the 13th century, had already noticed Galen's mistake). In his *magnum opus*, Vesalius did not make any oustanding discoveries, but corrected a number of traditional mistakes.

The most important of them, as it implied the revision of a basic chapter of physiology, concerned the structure of the interventicular septum. In order to account for the supposed mechanism of blood circulation, Galen had been forced to assume that the interventricular septum was porous, so as to allow blood to ooze from one ventricle to the other. Actually in the first edition of the *Fabrica* Vesalius concludes that, though he had been unable to find the supposed pores, yet he admired the power of God who had provided so that blood could pass through invisible pores. However, in the second edition (1555) he made up his mind and bluntly says that while in previous years he had not dared to completely deny the possibility of a passage of blood through the interventricular septum, he was now satisfied that no such porosity existed and that the septum is as dense and strong as any other part of the heart. He was thus satisfied that no oozing occurred.

Naturally it was immediately remarked by a number of authors that should it be so, thence the whole theory of blood movement collapsed, while there was no alternative theory available.

Neither the *Fabrica*, nor the *Epitome* which was published at the same time, are perfect, as they still include a number of mistakes. Yet they were both a great improvement on previous knowledge and two most stimulating books. One of the implications that it took time for scholars to realize was that the new anatomy required a complete rethinking of all human physiology, for which a good morphology is the requisite foundation.

Vesalius naturally dissected a fair number of animals and sometimes reports on them; indeed he occasionally follows in the steps of Galen as he attributes to man structures that he had, in fact, seen in his animal dissections, but that do not occur in Man. Moreover, as he was a pure physician, with no other interest than man, his observations on animal anatomy are entirely marginal to his research interests. It is also remarkable how he apparently failed to perceive how his own discoveries in human anatomy required a revision of Galenic physiology as well.

As we said Vesalius was a great master and students flocked to his lectures from everywhere and among them was John Caius, who attended his lectures for eight months in the Winter 1539-40 and that, back in England, was the first to translate Galen into English (1544-49) and later richly endowed one of the most famous colleges in Cambridge: Gonville and Caius.

Contemporaries and followers of Vesalius

The Italian anatomical schools were to be the best in Europe for about another century.

After Vesalius had left Padua, Realdo Colombo was appointed in his place. Colombo was born in Cremona in about 1520 and died quite joung in 1559. He was first a professor in Pisa, thence he moved to the Papal court in Rome, where he met with Michelangelo, whom he supplied with materials for his anatomical studies. Colombo wrote a *De re anatomica* (1559) which is significant in the development of the new theory of blood circulation. As the book was published a few months after his death and on the testimony of Valverde, who was his pupil, there is little doubt that he actually recognized the little circle not later than 1548.

The next anatomist to hold the chair previously held by Vesalius' was Gabriele Falloppia (or Falloppio) from Modena (1523-1562). He was professor first in Ferrara and next in Pisa, before his final appointment in Padua. In Pisa he certainly performed dissections on living criminals condemned to death, of whom he tells us that had been made insensitive by strong dosing with opium.

On this peculiar, albeit brief phase in the history of human anatomy, the Florentine ducal archives preserve a curious set of documents: in the first the governor of

Castrocaro informs the central administration that a woman sentenced to death is seriously ill and that, therefore, he begs for the executioner to be promptly dispatched, so that the culprit could be executed instead of dying a natural death. The duke's administration replies that, as the Duke had ordered her to be sent for the dissection in Pisa, the governor was ordered to call for the best physicians available and to see whether she could be cured. In the meantime he was to keep her in good spirits. Should the college of physicians judge that there was no hope for recovery, then, upon such advice, the executioner would be dispatched! In the end she was sent to Florence and thence to Pisa. As a whole Florentine archives record that not less than 13 criminals were sent to Pisa 'for the anatomy'.

Falloppia's *Observationes anatomicae* ' (1561) are of the highest quality and testify to a personality even more independent from tradition than that of Vesalius himself. The name of Falloppia is linked with the description of the uterine tubes (Fallopian tubes), a number of other details of the urogenital system, and an excellent account of the anatomy of the ear where are described for the first time the labyrinth and the cochlea. He also made important studies on the eye muscles, on the cranial nerves, etc.

On the other side Falloppia was so certain that fossils were just spontaneous formations in the rocks, that he claimed such an origin even for some Roman sherds!

Gerolamo Fabrizi from Acquapendente, better known as Fabricius (1537-1619) was a pupil of Falloppia and held the chair of surgery and anatomy from 1566 until 1609, when he left the chair of surgery to his pupil Casseri, while keeping the chair of Anatomy until 1613 (a rather unusual arrangement as the two teachings were usually combined in a single chair). In 1613 Fabricius finally retired as a most honoured and famous master. While Fabricius was holding the chair, his success as a teacher prompted the building of the first 'anatomical theater' which is still preserved in the main building of the University of Padua.

Among the many students who attended Fabricii's lectures, the most famous was certainly William Harvey, to whom we owe the complete description of the blood's circulation.

Through his many years of scientific activity, Fabricius dealt with a number of problems and, late in his life, he published a lot. His first treatise is the *De visione, voce et audito* (Venice, 1600), while perhaps his most famous and one of the latest ones is the little tract on the valves of the veins *De venarum ostiolis* (Padua, 1603), where he described the venous valves and misunderstood their function. Fabricius, as all the anatomists of his age, made extensive investigations in animal anatomy and recorded in considerable detail a number of entirely new observations. However he was always propted by the need to compare his findings with the conditions in Man. Anyway probably his most important contributions for the history of biology are his embryological books: *De formato foetu* (Padua, 1600) and the posthumous *De formatione ovi et pulli* that are the first real embryological monographs. There Fabricius accurately describes and figures, as best as the naked eye can allow, the main developmental phases both of the development of the human embryo and in several animals, including different Selachians, Mammals, Reptiles and chickens.

Some passages in his *De brutorum loquela* (on the language of animals, 1603) have been read as implying some sort of transformism. Whether this is correct is arguable, but the book undobtedly stands as the first comprehensive account of communication in animals, a distant ancestor of Darwin's studies! The great interest of Fabricius in animal anatomy and physiology is, thus, documented by several of his books and it seems that he had planned a comprehensive account of animal anatomy that, if it had been printed, would really have made him the Father of Comparative Anatomy.

As a man Fabrici had such a difficult and quarrelsome character that, in spite of his great renown, in his late years he used to go around the town only when accompanied by a half a dozen armed escort!

Fabricius was a good aristotelean and usually tried hard to make his observations match with the Stagirite's theories. Though his observations are certainly not perfect, yet they represent an unquestionable advance over previous knowledge and his contribution to physiology, especially of vision, speech and movement are especially outstanding.

Fabricius was equally successful as a physician and surgeon as an anatomist and as a result he grew both rich and famous.

On the advice of Fabricius, Giulio Casseri (1552-1616), who had first entered Fabricius' service as a footman and had then became his dissector, was appointed, in 1509, to the chair of surgery, which he, on the final retirement of Fabricius, finally combined for but a few years with that of anatomy. Casseri is foremost an exquisite technician and both his descriptions and figures are excellent. He studied the cranial nerves, the middle ear etc. A significant section of Casseri's work concerns the anatomy of all domesticated animals and of some invertebrates. He was prompted in this endeavour, at least as far as mammals are concerned, not by scientific aims, but by very practical needs as a teacher of surgery. Indeed at the time (a) the opportunity for students to practice anatomy on human corpses was still limited and (b) as anesthesia was practically still non extant (the properties of opium were known at least since the 13th century, but it was seldom available and dangerous to handle, so that, for instance, military surgeons made a practice to stun wounded soldiers with a mallet before applying surgery). Thus, it was absolutely necessary that the surgeon should have been extremely deft and quick, so that constant practice on animals was mandatory at the time and this, in turn required a precise knowledge of the differences between man and other animals.

Thus accrued a wealth of precise information on which much later could be built true comparative morphology.

Casseri, however, was not a mere practitioner, as shown by his investigation on the anatomy of invertebrates, which he did as auxiliary to his investigations on man. Thus, in the framework of his study of the sound producing and hearing mechanism in Mammals, he described the stridulatory organs of cicadas, which were investigated again only in 1740 by Reamur, who was apparently unaware of Casseri's description.

Giulio Cesare Aranzi (1530-1589) was an embryologist. Born in Bologna, Aranzi was for many years professor of Anatomy there. His main contributions are in the field of fetal annexes and fetal circulation. We owe to him the description of the *ductus Arantii*, a terminal branch of the umbilical vein, which in the fetus connects the umbilical vein and the inferior vena cava (*De humano foetus liber*, 1564).

Another Bolognese anatomist was Costanzo Varolio, born in 1543, who, after having been for a while a professor in his native Bologna, was called by pope Gregory XIII to teach in Rome at the "La Sapienza". Varolio died when still quite young in 1575 just after the completion of his studies on the anatomy of the brain and any medical student is still required to remember the *pons Varolii* for his examinations.

Bartolomeo Eustachi (or Eustachius) was also a professor in Rome. He was born in an unknown year at the beginning of the 16th century and died in 1574. He is the author of a book on the ear (1572) and, again, everyone knows the 'Eustachian tubes' connecting the middle ear with the pharynx. Eustachi wrote a little volume on the kidneys (1563) and of another on the teeth (1563). In fact most of his discoveries remained unpublished. When he died he left 54 splendid plates ready for publications. These were rediscovered and published by Lancisi in 1754, when they were obviously obsolete. Moreover a study of the original copper-plates suggests that some original plates had been in fact completely lost or damaged and that these had been substituted by Lancisi himself, so that the actual content of Eustachi's later discoveries is somewhat in doubt.

Eustachi was a Galenist and as such criticized Vesalius, but when, as it happened, his own investigations proved Galen wrong, he was prompt to correct the Greek master, so that he was, in turn, attacked by the really orthodox Galenists.

Another contemporary of both Vesalius and Eustachi was the Sicilian Giovan Filippo Ingrassia (1510-1580), born in Regalbuto, near Enna. He was professor of Anatomy and Medicine at the University of Naples, a chair which he left to become *protomedicus generalis* (that is chief of all medical services) in Palermo. Ingrassia's investigations are manifold: in the field of anatomy his contributions mainly concern the skeleton (*In Galeni librum de ossibus doctissima et expectantissima commentaria*, a posthumous work published in Palermo, 1603). There he proved that Galen had basically described the skeleton of monkeys. He was also among the first to pay attention to cartilages and to describe the cranial pneumatic cavities.

Some scholars credit Ingrassia with the discovery of the stapes, while, for others this was made either by Eustachi, or by Colombo or Falloppia. As a matter of fact, Ingrassia's description was published 23 years after his death and it seems that he actually made his researches around 1546.

Nowadays Ingrassia is mainly recalled because, as 'protomedicus' he was involved in both fighting the plague of 1575-76 (*Informazioni del pestifero e contagioso morbo* etc., Palermo 1576) and in criminal investigations, so that he is considered as a pioneer both of forensic medicine and of public hygiene.

We may just mention some of the other Italian anatomists of repute of the Renaissance: Arcangelo Piccolomini (Ferrara, 1525-1586) and Giovan Battista Carcano (Ferrara, 1515-1579) who both made some significant contributions.

For the diffusion of the new anatomy much more important than the these last mentioned anatomists were a number of foreign scholars who, some after having studied in Italy, fostered the revival of anatomical studies in their native countries.

We have already mentioned the British Caius (1510-1573), other significant scholars, some of which graduated in Italy, who brought the new anatomy to their countries were Felix Platter (1536-1614) born in Basel and graduating there. Later he studied in Montpellier with Rondelet and in Paris with Fernel; back in Basel, he was the first after Vesalius to make there a public anatomy and in 1560 was appointed as a professor, a chair that he kept until death.

Significant contributions to human anatomy were given by Gaspard Bauhin (1560-1624) of Basel, by Pieter Paaw (1534- 1617) of Amsterdam, who studied in Paris, Orleans, Rostok and Padua, by the Spaniards G. Postio (dates not precisely known), and G. Valverde (c. 1560-?), from Amusco, in Leon, who studied in Padua with Colombo and in Rome with Eustachi. Valverde, after his return to Spain, published in 1556 a Spanish summary of Vesalius' treatise, even copying some of his plates, so that the infuriated Vesalius heaped scorn on Valverde. Finally among the foreingn supporters of Vesalius, it is worth remembering, also because of his complex biography, Juan Rodrigo, nicknamed Amato Lusitano (1511-1568). This last, born in Castelo Branco, Portugal, was the son of a marrano family (Jews that had converted to the Catholic faith) and graduated in Salamanca. Later, to avoid the growing controls of the Inquisition, emigrated to Antwerp; he then moved to Italy, in the attendance of the Duke of Ferrara Ercole II d'Este, and was a professor in Ferrara until 1547. Italians were usually very tolerant with Jews. For instance, for political-economic reasons in Leighorn they even had a member in the town council while, at the same time, were mistreated in Florence by the same ducal authority; in Padua the Venetian officers, when the Jews were theatened by riots, even employed the army to protect them. However the legislation was pityless with relapsed Jews, that is with baptized people who went back to Jewery. However, until the bigoted pope Paol IV Carafa, imposed a mounting pressure on all the Italian States, at least in the states of Venice, Mantova, Ferrara, Florence, Lucca, Urbino and even in the papal town of Ancona actions were sporadically taken and only on denociation by third parties. As things begun to change Lusitano in 1547, having renounced his chair, by a roundabout voyage, moved to Thessaloniki, then a Turkish possession, and there he abjured Christianity and went back to Judaism. Another important human anatomist was the Belgian Rambert Dodoens (Rambertus Dodonaeus, 1518-1586) from Malines, but by far the most important of all was the Dutch Volcher Coiter (1534-1576) from Gronigen and whom we shall discuss further on.

The renaissance of Botany and Zoology

The renaissance of naturalistic studies was slower that that of Anatomy for obvious practical reasons: good medicine requires good anatomy and, anyway, man is a most interesting subject.

However, medical treatment requires appropriate remedies, and these, since immemorial times, were mainly obtained from plants. We have already mentioned the medieval compilations on plants, the transmission of Classical knowledge and the cultivation of medical plants.

During the 15th century there became available new and better translations of Dioscorides and Theophrastus, the work of 'litterati' such as Theodore Gaza, Ermolao Barbaro and Marcello Virgilio. The Venitian Ermolao Barbaro (1453-1493) deserves a mention also because he published in 1490 the *Castigationes plinianae* a critical analysis aimed at the emendation of Pliny's text, which survived only in very corrupt manuscripts.

Such critical work was prompty followed by more technical commentaries.

Nicolò Leoniceno from Lonigo (1428-1524) was professor of medicine in Padua, Bologna and Ferrara, where his lectures were followed for a while by Paracelsus. He made a basic citicism of Pliny (*Plinii et aliorum doctorum*, *qui de simplicibus medicaminibus scripserunt*, *errores notati*, 1492). Leoniceno was a man of immense learning and both a physician and a humanist (he was a good friend of the great poet Ariosto) and, as it was common in the Italian universities, had no qualms in criticizing the ancient masters. It is obvious that this was a prime source of debates.

In the long list of physicians who for one or another reason criticized the classical authorities, another who deserves a special mention for his importance in the history of Mathematics, is Girolamo Cardano (1501-1576) magus, mathematician and successful physician.

Antonio Musa Bresavola published an *Examen omnium simplicium medicamentorum* where he warned that many plants in common usage in his own times had not been known to the ancient authors.

By far the most celebrated and read commentary on the 'simples' was that by Pierandrea Mattioli, a Sienese, usually known by the latinized name Matthiolus (1500-1577). Matthiolus was court physician to king Ferdinand and later to Maximilian II in Prague. His Comments on Dioscorides were first printed in Italian (1544) as was common with books of mainly practical use which had to be easy to consult even by mere practical herbalists. Nevertheless, his book enjoyed an enormous success: the Italian version had some 18 editions, the Latin one 10 and was printed for the last time as late as 1724! There were translations in French, German and Bohemian. The book was hailed by many and severely criticized by some, which is fair with a book that, side by side with excellent accounts, occasionally credits different simples with incredible virtues. Matthiolus was certainly a very active naturalist, who not only made his own collections, but was actively exchanging correspondence with many other botanists (Luca Ghini, Francesco Calzolari, Bartolomeo Maranta, Ulisse Aldrovandi). The figures in his book are usually very good and he had the unquestionable merit of being the first to note systematically the actual localities where were collected the different plants.

For the first time during the first half of the 16th century were established regular *lecturae simplicium*, chairs specially devoted to the study and preparation of herbal and mineral drugs. The first such chair known was established in Rome, where Giuliano da Foligno was appointed in 1513. It was at about the same time that the first collections of dried plants were begun and that botanical gardens were established. These were originally devoted only to the growth of plants of medical interest (Giardini or Orti dei Semplici). The fisrt 'garden of simples' was established in Pisa in 1543 by the Duke Cosimo I de' Medici on the advice of Luca Ghini. Luca Ghini had been first professor in Bologna from 1534 to 1544, but moved to Pisa in the context of the already mentioned effort by Cosimo I to strengthen the University of Pisa. Ghini also succeeded in geting the Duke to establish a Garden of Simples in Florence (end of 1545). While the Garden of Pisa was removed from its original location in 1563 to make room for shipping yards and again shifted to its present location in 1593, that of Florence is still in its original location.

In the same year, actually July 1545, was established the Garden of Padua, thus antedating Florence by a few months. The Paduan garden was the results of the efforts of the local 'lector of Simples' Francesco Bonafede and its first curator was Luigi Squalermo, who, however, always signed 'Anguillara' from his birthplace (c. 1512-1570), another pupil of Ghini, whom both Mattioli, Falloppia, Cesalpino and Aldrovandi charged to be a perfect ass and worst, while Gesner and Belon appreciated his work. He did, indeed travel and collect plants in many countries.

Aldrovandi had to wage a long battle to establish a Garden in Bologna and he succeeded only in 1568, over 20 years later.

In the same years a number of private collections of living plants were established in Italy, one such, which is reported as especially rich, was owned in Milan by senator Scipione Simonetta (1524-1585), a magistate and politician, who later died in Madrid as head of the council for Italian affairs of King Philip II.

While the habit of drying medicinal plants for later preparation of drugs was common since the earliest antiquity, to collect dried samples for study is, again, a 'discovery' of the 16th century. There is an unverified tradition that again credits Luca Ghini for the preparation of the first *Hortus siccum*, but this is improbable, as the very first such herbariun in existence is the Herbarium of Gherardo Cybo in Rome, dated 1532. In Florence we have a herbarium prepared by Cesalpino for a bishop Tornabuoni before 1563 and which includes 768 different plants and Bologna houses Aldrovandi's personal herbarium, holding over 5,000 specimens.

Coming back to Luca Ghini (1490-1556), he was born in Croara d'Imola, and, as usual was both a physician and a professor, first as *extraordinarius* (1534-1539) and later (1539-1544) as *ordinarius* in Bologna, whence he moved to Pisa, finally he went back for a short time to Bologna, where he died. While both as the promotor of Botanical gardens and as a teacher he had a profound influence, he published very little and only on medical problems. It appears that he was preparing a big botanical treatise, but, when Matthiolus' book appeared, he gave up the project and made a present of all the assembled materials to Matthiolus himself.

However we know much of his teaching as we have notes on his lectures by both Aldrovandi and Cesalpino, who were his pupils for a while. From these we gather that he was really a very good botanist. Also Luigi Anguillara, the just mentioned first curator of the Botanical gardens in Padua, was a pupil of Ghini. It is not clear why both Aldrovandi, Cesalpino etc. considered Anguillara as their special 'bête noire'and heaped insult on him.

Not only academics contributed to the revival of botanical investigations: some private apothecaries, such as Bartolomeo Maranta from Venosa (1500-1571), himself a pupil of Ghini, and Francesco Calzolari (1522-1609) from Verona made valuable contributions. Calzolari is especially notable as he provides a first description of a local flora in his account of surveys made on the Monte Baldo near his native town (*Il viaggio di Monte Baldo*, Venice, 1566, and *Iter Baldi Montis* Venice 1571). Calzolari is also often remembered as the owner of one of the earliest collections of natural history specimens, animals, plants and minerals, some being very rare or coming from distant lands. We have two descriptions of this 'Museum': by Olivi (1593) and by B. Ceruto and A. Chiocco (1622) and we shall discuss it again in the next chapter.

Three people are usually considered as the 'fathers' of German botany and were all of a Paracelsian penchant, yet their books are in the way of commentaries of classical authors with the addition of personal observations. All three of them were Lutherans and their personal stories have something in common.

Otto Brunfels, from Mayence (1484 or 1489-1534) was first a Carthusian monk, who later became a Lutheran preacher and finally settled in Bern as a physician. He wrote a book (*Herbarium vivae eicones*, Strasbourg, 1530-1536) which was an attempt to describe and illustrate medicinal plants from life. The figures were due to Hans Weidlitz. However the book suffers from the author's attempt to identify plants from central Europe with the Asiatic species described by Dioscorides. Apart from his botanical work, Brunfels was a notable figure in the debates of the Reformation and, during his life, was especially known as a supporter of Nicodemism, opposing Luther. As an astrologer he wrote an *Almanac and prognostic from 1526 to the end of this and of any other world* (an end that he speculated that was rather close at hand).

Hieronimus Bock (in Latin Tragius) (1498-1554) from Baden, wrote a herbal in German (*Neu Kreutter Buch*) (Strasbourg, 1539) with rather poor figures by David Kendal. Later the book was translated into Latin and was quite successful: new editions followed until 1630. Here the descriptions of the plants are accurate and are supplemented by information as to their habitat. The species are still arranged according the scheme of Dioscorides with minor and reasonable changes.

Leonard Fuchs (1501 or 1505-1566), after whom was named the genus *Fuchsia*, was a professor of Medicine in Tübingen from 1535. His most important work is the *De historia stirpium commentarii insignes* (Basel, 1542) which has some 500 excellent figures which were drawn for him by different pupils of Dürer. This book is to some extent the botanical equivalent of Gesner's book on animals. Here too the different species are arranged by alphabetic order, an eminently practical arrangement for quick consultation by apothecaries and physicians. An important section of the book is its glossary and there are listed several terms that were to stay in botanical terminology and which in this book are defined clearly for the first time.

In France, Montpellier was an important centre of botanical studies.

Charles de l'Escluse (Latin Clusius, 1526-1609) from Arras was teaching there for several years when not travelling through Europe to collect plants. Later he settled in Leyden, where he established the Botanical gardens. Some of the plants originally planted there by Clusius are still thriving. The main work by Clusius, and a good one at that, is the *Rariorum plantarum historia* of 1576.

Another two scholars who studied in Montpellier were Jacques Daleschamps (1513-1588) from Caen, who wrote a general history of Plants, and Mathias de L'Obel (Latin Lobelius, 1538-1616) from Lille who systematically studied the flora of that region. Also his 'systematics' are somewhat different from Dioscorides': this author's arrangement is largely followed, but, when he comes to herbs de L'Obel separates such plants that have wide leaves with reticulate nervature, from those with narrow leaves and parallel nervature. Thus he hints at the separation of monocotiledons from dicotiledons, though de L'Obel could not possibly have thought in terms of natural groups, but was simply aiming to a device helpful for the quick identification of plants and for helpful memonic devices.

The two brothers Jean (1541-1613) and Gaspard (1560-1624) Bauhin belonged to a huguenot family who had been obliged to flee from Amiens in France and had settled in Basel. Gaspard, whom we have already mentioned, had studied with Fabricius and with Aranzio, his main botanical contribution was a *Pinax theatri botanici* (1623) that, by its date, should be considered in the next chapter. There he describes some 6000 different plants, a considerable advance on previous books, though it is probable that authors like Fuchs, who described a mere 500 species, in fact listed only such species that they deemed to be of practical significance and omitted all species devoid of pharmacological potential. In Gaspard Bauhin book there are some additional improvements on the past. Though without any explanation, he groups most species into groups that are approximately corresponding with what were later considered as natural families. The title of the book is especially notable for its explicit reference to those 'theatres of the World' which were linked with both the development of museums and with that of mnemonics, on which we shall have much to say in the next chapter.

Anyway the most interesting botanist of the late renaissance is unquestionably Andrea Cesalpino, whose most important book is the *De Plantis* (Florence, 1583). Cesalpino was born in Arezzo either in 1524 or 1525, studied and graduated in medicine in Pisa, where he had as teachers Realdo Colombo and Luca Ghini. He was later appointed as Lector (i.e. professor) of simples and followed Ghini as curator of the Botanical Garden. Some time afterwards he was appointed as full Professor of practical medicine, a chair that he held until 1592, when, probably disappointed by the appointment of Girolamo Mercuriale (1530-1606) to a lectorship in Pisa, he obtained an appointment at La Sapienza in Rome and pope Clement VIII appointed him as his Archiatra (that is Chief physician). Cesalpino died in 1603. A herbarium prepared by him survives and is treasured by the botanical collections of the University of Florence.

Cesalpino was a man of both superior intelligence and vast culture, as a whole he was a rather orthodox Aristotelean and had both the merit and the fault of being a theorist to a far greater extent than the other authors thus far mentioned.

According Cesalpino, who here follows Aristotle, plants are like the simplest animals and live 'upside-down, with the head (= the roots) buried into the earth. To him the fact that the roots correspond with the head is made evident by the fact that it is by the roots that the plant gets its nourishment just as animals get it from the mouth. As for the location of the vital spirit of the plants Cesalpino holds that their vegetative soul, which in animals is located in the heart, is, instead, located in the medulla, in what we now call the 'collar', at the transition between the root and the aerial part of the plant.

Generally Cesalpino follows Theophrastus granting that plants usually have no sex and that they may occasionally appear by spontaneous generation. Yet they have reproductive organs: both fruits and seeds. To nourish itself and to reproduce are the two basic functions of a plant and, therefore they must be the first two biological features used to characterize them.

This framework may well be taken as being 'orthodox' and even traditionalist, but it is just within this frameworth that Cesalpino makes some some very precise remarks on the physiology of plants. Moreover no one doubts that he had the merit of having been first suggested a true classification. Cesalpino still follows the traditional subdivision into trees, shrubs, bushes and herbs, but subdivides each one of these groups into several categories, mainly on the evidence of their fruits and seeds. Apparently Cesalpino was the first to remark that some seeds have two embryonic leaves, those that we call Dicotiledons, and other which have only one of them. In spite of having used some characters which are still considered as significant, Cesalpino was avowedly bound in his systematics, by the requirements of the particular type of mnemonic that he followed and so, in spite of his recognition of some groups that modern systematists still deem to be valid, his classification ends up being entirely artificial, at least judged by the modern standards. It is also notable that, though an Aristotelean yet Cesalpino tackles the problems of systematics by an approach entirely different from that of Aristotle himself.

As far as we can know (we said in chapter II, that the botanical works of Aristotle are lost), while the Stagirite used to group the organisms starting from the empirical evidence available on the individual organisms and then proceeding gradually towards increasingly comprehensive groups, Cesalpino elaborates his classification beginning with the hypothesis of a mnemonic 'theatre': thus he first establishes a hierachy of easily remembered features, and thence allocate the different species by subsequent subdivisions, following the range of characters in the pre-set order. In a way this may even be considered as a first step both towards those dichotomic tables which were to be the first great achievement of Lamarck, as well as towards a kind of operational methods that have been repeatedly surfacing in the practice of systematics.

Several botanists, mostly German, were interested in Cesalpino's ideas, but interest soon waned and Linnaeus in not far from the truth when he says that Cesalpino was lonely walking in the house that he had built: we shall come again to Cesalpino when dealing with the problem of blood circulation.

Though chiefly important as a zoologist, one should not forget the French Pierre Belon who wrote in French (as it was usual with texts of practical science) a book of applied botany, which, in 1589, was translated into Latin by Clusius with the long title: *De neglects stirpium cultura, atque earum cognitione libellus, edocens qua ratione sylvestis arbores circurari et mitescere quaeant* (= *Booklet about the cultivation of overlooked bushes and on their recognition, teaching [also] which method forest trees require for their clipping and domestication*).

Possibly the first botanical garden in Germany was established in Nurenberg by Joschim Cammermeister (Camerarius, not to be confused with the later Rudolph Jakob Camerarius), who had taken his doctorate in Bologna in 1562, after having studied in Wittenberg. He published a notable *Hortus medicus et philosophicus* in 1588.

Finally we should remember both for his renown when alive and as teacher of Belon, the German Valerius Cordus (1515-1564). He was trained as a physician-botanist by his father Euicius, and, when not yet twenty, he published a *Dispensarium* on plants of medical proprieties. He also wrote a big *Historia plantarum*, which was completed in 1540, but was in fact published under the editorship of Gesner in 1561.

During the Renaissance, zoological books appear somewhat later than the botanical ones. Yet some of them are truly notable. The zoological books may be ranged under two headings: some deal only with a few groups of animals, the others aim to cover the whole animal realm.

Among the authors of 'monographic' books, three deserve special notice: the French Belon and Rondelet and the Italian Salviani.

Of Pierre Belon we have just mentioned his book on forestry. He was born in 1517 near Le Mans from a poor family. Yet he succeeded in obtaining the patronage of rich supporters, so that he was able to study at Wittenberg with the famous botanist Valerius Cordus. Belon later was able to raise the money to visit Greece, Turkey, Egypt and Italy. In Rome (1549) he met with both Rondelet, who was there in the retinue of Cardinal de Tournon, as well as Salviani. As it happened all three were about writing a history of fishes and so they exchanged materials and informations. Back in France, Belon received his doctorate in medicine from the University of Paris. King Charles IX, the one who engineered the 'Massacre of the Night of St. Bartolomew', the massacre of Protestants, granted him a pension and a house at the Bois de Boulogne. Belon was busy translating Dioscorides, when he was murdered in 1564.

In fact Belon published several works on fishes and other aquatic animals, but his memory is mainly linked with a *Histoire de la nature des Oyseaux avec leur déscription et naif portraicts retirés au naturel* (Paris, 1555). This is practically the first true printed monograph on ornithology. This book includes the famous drawing showing the basically correct comparison between the skeleton of a man and that of a hawk, arranged in an appropriate positions. Moreover Belon suggests a classification of Birds based on the morphology of the bill. Finally Belon, denies the reliability of several old traditions, for instance, and independently of St. Albert the Great, he dismisses as a crazy notion the tradition that Geese originated from the Goose-barnakle, a legend that was still believed by some naturalists in the next century.

In more than one way Belon may be considered as a comparative anatomist *ante litteram*, yet his books include some strange 'mistakes', so, to give again an example, in his *L'histoire naturelle des estranges poissons marins, avec la vraie peincture du Dauphin*, Paris 1551 (= *The natural history of the strange marine fishes, with the true picture of the Dolphin*), he was the first to provide an accurate description of three species of Dolphins. He describes apparatus by apparatus the similarities between dolphins and man and other mammals and notices how the fetus is attached to the mother by a placenta, yet, and against Aristoteles, who had made the 'Cetae' a special group, Belon squarely places dolphins with fishes! The only explanation, to me, is that, as Belon lists among fishes also the hippotamus, the beaver and the otter, he was not interested in systematics as such, and placed any aquatic animal among fishes!

Guillaume Rondelet (1507-1566) was born in Montpellier and was professor there of anatomy. He had followed his courses together with Rableais, with whom he struck a firm friendship. Rableais has left us an amiable satire of his friend in his 'Gargantua and Pantagruel' under the nickname of 'Doctor de Rondilibus' and when Rableais was charged with heresy, the two friends certainly met in Rome in 1549, when Rondelet arrived in the retinue of Cardinal De Tournon and Rableais was there to clear himself of the charge of heresy [1]. Rondelet was a famous teacher and a number of students,

[1] Which, by the way, as he himself relates, he got by the good offices of Cardinal Jacopo Simonetta.

both French and foreigners, flocked to his lectures, among them were Coiter, Bauhin, L'Ecluse, L'Obel and, briefly, both Gesner and Aldrovandi. His two important books: *De piscibus marinis* and *Universae aquatilium historiae* (Lyon, 1554) follow the usual pattern of the times: they are rather lengthy, and much care is taken to verify the descriptions of Aristotle. His descriptions of some 250 marine animals, including some rare or curious species, such as *Argonauta*, are usually good and include a number of completely new details. Yet his figures are poorer that those by Belon and by Salviani and he lists also some fantastic creatures such as the 'bishop-fish'.

Rondelet was keenly interested in physiology and was the first to remark the importance of air also for the respiration of fishes. Having discovered the natatory vesicle of fishes, he supposed that it functioned as a sort of lung. He also supposed that air dissolves in water and then is captured by the gills. As usual at the time Rondelet placed the Cetaceans with fishes and he may have found confort in that by his observations on different species of little sharks (now included in the genus *Mustelus*), as in some of them the embryo is linked to the mother by a placenta and in some is not (and Rondelet provided the first figure of the placentation of sharks!). Rondelet was also the first to figure the dissection of a sea urchin! He is also the author of an important *Pharmacopaea*, which was printed several years after his death.

While Rondelet's figures are rather poor, Salviani's are excellent. Ippolito Salviani (or Salviano) was born in Città di Castello possibly in 1514 and was a practicing physician in Rome, where he was a physician to popes Julius III, Marcellus II and Paul IV. He died in 1572. Salviani described only about one hundred species, some rare and even entirely new, like the 'pork-shark' (*Oxynotus centrina,* often quoted also as *Centrina salviani*). His descriptions are very exaustive: besides the description of the animal itself, they include information as to its habitat and habits, its qualities as food, its preservation and when appropriate, medical usages. As Salviani, like almost all the Renaissance naturalists was a practicioner, the inclusion of such practical aspects in his descriptions is but natural and usual, as we shall again see when dealing with Gesner and Aldrovandi.

The works of these three naturalists paved the way for a renewed interest in new descriptions of animals not only from exotic countries, as we shall shortly see. However, none of them was interested in arranging his animals and his discoveries in any systematic order beyond grouping the obvious similarities.

While Belon, Rondelet and Salviani were actively engaged in the pursuit of new evidence, the next two to be mentioned contributed comparatively few novelties and their works are rather monuments of erudite research.

Edward Wotton (1492-1555) was born in Oxford from a comparatively poor family, yet he was able to study at his native town University. His book *De differentiis animalium* (Paris, 1552) is the result of several year's work and is a notable methodical account of Aristotle's systematics and the reasons therof. The book had some influence on the subsequent developments of zoology.

Much more important for the history of zoology, was Konrad von Gesner, born in Zürich in 1516 and dead there in 1565. He was the son of a protestant craftsman, who was killed in the battle of Kappel in 1531. The boy Konrad, however, was so brilliant that some friends paid for his studies first in Basel, then in Paris and Montpellier. He had a professorial chair in Lausanne and later was a practitioner in Zürich, but he always suffered from a shaky economic situation. He was also a sportsman, particularly keen on mountaineering. Gesner was certainly keen on personal investigation and was the first to use magnifying glasses in biology, so that we owe to him the first description of the skeletons of Foraminifera. Yet his craving for completeness tends to swamp his own observation by a mass of bookish information. Being a good classicist, he also taught Greek for a while in Lausanne and published editions of various classical authors, a list of languages and dialects, one of all the authors who had written in Latin, Greek or Hebrew. He had also written a book on botany, which, on account of its originality and scientific merit, is possibly better than his treatise of zoology, but it remained unpublished for almost two centuries after his death, in spite of the fact, which is borne out in Gesner's letters, that he put much store by it.

A major weak spot in Gesner concerns his attitude to fossils: probably because of theological preoccupations, he firmly denied the possibility that fossils were the remains of formerly living beings and considered them as merely 'figured stones'.

Gesner basic work is his monumental *Historia Animalium.* The publication of this five-volumes folio treatise begun in Zürich in 1551, but was completed only in 1587, 22 years after the death of the author after his notes and sketches. As a whole this enormous treatise amounts to over 3,500 printed pages with hundreds of figures. For each species Gesner gives: (1) its name in all the languages he knew; (2) a description of the external characters of the animal and its native land; (3) its habits, instincts and way of life; (4) techniques for its capture and, possibly, domestication; (5) Possible usage as food; (6) medicinal employment; (7) literary, moral and allegoric significance. Finally a list as complete as possible of quotations of the books in which the animal had been mentioned.

Gesner's systematics are nothing else than the Aristotelian one: viviparous quadrupeds, oviparous quadrupeds, birds, fishes and any other aquatic animal. Within these groups animals are listed in strict alphabetic order, but for birds. Cetaceans are united with fishes and bats with birds. Gessner lists as real a number of imaginary animals from ancient traditions.

Plainly Gesner's book is an expanded and updated 'Thomas of Cantimpré' supplemented by some original observations squeezed in here and there.

Several of his original figures are quite good and it is notable that while he complained that the editor, to recoup something of his expenses, was selling badly coloured copies of his figures even before the book was on sale, yet he made no bones about copying any good figure that he could find. This, for instance, he did with Dürer's rhinoceros, published by Dürer himself for the first time in 1515!

Very few people were interested in invertebrates during the 16th century and so an otherwise scarcely notable scholar takes a place of honour: Thomas Moffett (1553-1604), a London physician, wrote an *Insectorum sive minimorum animalium theatrum* (A theatre of insects that is of the smallest animals) which was published by E. Wotton well after the author's death (London 1634), which makes him a contemporary of Aldrovandi.

Moffett (there are doubts as to the correct spelling of his name: one finds it written also as Muffet, Moufet, etc.) tells in the preface of his book how some friends had advised him against such studies, as such imperfect animals were not worthy his time, effort and money and that their study was neither honest nor useful, and how he had, nevertheless continued in his work. All that is laudable, but, apart from the reasonably good plates, there is not much original to recommend the book. For instance Mouffet is worse than Aristotle as he lists the caterpillars of butterflies among the apterous insects and the butterflies and moths among the winged ones. It is possible that he had never seen a scorpion, as he portraied it with some sort of wings. He lists among insects animals that are not insects at all, a usual mistake of the times (it was common practice by good scholars up to the middle of the 18th century). Mouffet candidly reports as true the Plinian story that bees are born from the carcase of a rotting bullock: the king (actually the queen) is supposed to arise from the noblest part of such corpse: the brains!

Mouffet was a committed Paracelsian and worked hand in hand with Paracelsians in Basel, where he cooperated in the pubblication of the posthumous volumes of Gesner treatise. The title of his book *Theatrum …* suggests that his arrangement of the topics in his book was ruled by the requirements of some method od mnemonics. The book itself is dedicated to Tycho Brahe and to Petrus Severinus, both Danes and Paracelsians, with the difference that while Tycho's astronomical work paved the way for Kepler, Severinus was both an extremely vocal and influential advocate of Paracelsian medicine, who did not contribute anything of significance to the development of biology.

Slightly junior to Gesner was Ulisse Aldrovandi (1522-1605) from a noble family of Bologna. When still a youth he was briefly jailed in Rome by the Inquisition on suspicion of heresy. He was duly acquitted and some of his biographers suggest that this experience was one reason for his subsequent interest in animals: a scholarly field where chances of running into theological troubles were minimal. Personally I do not believe it: never in his immense correspondence and in his notes he shows any trend to such speculations that could lead him into trouble, while everything points to a keen urge to revise the Aristotelian tradition so as to account for the flood of new evidence that was rapidly accruing.

Aldrovandi graduated in Bologna in Philosophy and in Medicine and soon became a professor in his town's University. There, after many difficulties, he succeeded in establishing a botanical garden parallel with the Museum that he was assembling at his own expenses. His famous probity earned for him some important appointments for the control of the quality of the drugs prepared by the apothecaries. He thus ran into an epic struggle with the Apothecaries' Guild: Aldrovandi was adamant on the quality of the drugs employed in the preparation of the various concoctions. A particular bitter case concerned the quality of the vipers used to prepare the Theriaca or Triaca, a sort of extremely complicated concoction recommended by Galen for most diseases. Aldrovandi charged the Bolognese apothecaries of cheating their customers by using vipers that he judged to be not up to the standards recommended by the Pergamene physician.

Aldrovandi was an immensely learned man who studied and wrote on the most disparate subjects, both scientific and literary. For a fair appreciation of his personality it is necessary to consider not only his published books, but also the thousands of pages of his correspondence, files and notes that he bequeathed to his native town together with his museum. These were partly used by his pupils to prepare the edition of the volumes of his monumental history of animals that he had not completed.

Apart from several little tracts, he was able to publish only four volumes of his *magnum opus*: an *Ornithologia* in three volumes (Bologna, 1599-1603) and a *De animalibus insectis* (Bologna, 1602); nine more volumes, basically an edition of his preparatory notes, were published between 1606 and 1668. His books are both extremely accurate and splendid, with a number of illustrations that were prepared by artists in his service (and Aldrovandi often complains in his correspondence that he would need some finacial support to go on in his work). Aldrovandi activities as a teacher and his enormous network of correspondents, ranging from mighty potentates to any kind of learned or curious person, made him quite influential in late Renaissance Italy. Towards the end of his life he was in touch with some of the future members of the Accademia dei Lincei, but he died in the interval between the first attempt to establish the academy and its real organisation in 1610.

All Aldrovandi's activities were centered on his University, that he vainly tried to reform and update.

As we said Aldrovandi was a rather orthodox Peripatetic and in his works he did not use the very conventional criteria of Gesner. He was constantly preoccupied about the utility for mankind of the evidence that he was collecting and publishing. So, rather than following any organic principle for classifications, he followed different criteria in grouping the species of different groups. Thus some of his groupings are an advance on the traditional ones, other are a step backwards. Contrary to Wotton and, perhaps because of some influence by Cesalpino (the two had both studied under Luca Ghini), in spite of his admiration for Aristotle, Aldrovandi did not follow consistently his classification, but on this more subsequently.

Each species is described with every possible detail and quotation, but from both his published accounts and from his notes it is clear that he usually tried to verify first hand his evidence, and he is usually more cautious than Gesner when he has to rely on second hand information, so that his work is, as an average, a definite improvement over Gessner's. He definitely was at his best with insects, where he clearly identified some natural groups, such as the Orthopteroids and the Diptera.

Aldrovandi also deserves the credit of being the first to consider systematically also the internal anatomy of the animals and in his works we find several good figures of the skeleton of different vertebrates.

A peculiar problem which has puzzeld several scholars is that of Aldrovandi's 'dragon'. In 1572 a strange reptile was captured in the neighbourhood of Bologna and was given to Aldrovandi for study and description. This he did and a further account of it is to be found in his tract *De draconibus*. The animal examined by Aldrovandi was apparently a snake of moderate size (about one meter long), but the middle of the body, judging fron Aldrovandi's description and figures, was inflated as it could be in a snake which had just eaten some prey, but the really strange thing is that the animal had two legs under the inflated portion! Aldrovandi dissected the animal and had the skin prepared. Unfortunately, though it was seen by many people, the specimen, just as many others from Aldrovandi's collection, later disappeared, probably having decayed and was destroyed just as it happened to the Oxford stuffed Dodo, which, having been badly attacked by parasites was destroyed but for the head and feet. An odd, but not altogether unusual, thing, is that the many authors who commented on Aldrovandi's dragon have not examined the over 300 pages of his preparatory notes and drafts for his tract. For instance, judging from his published illustration, the 'dragon' had a pair of legs, while in his manuscript Aldrovandi comments on the fact that the two legs were offset by several inches, though one was to the right and one to the left side of the ventral squamation. Unfortunately though I was able to examine the photocopies of the whole document, the handwriting proved too difficult for me to actually read it, so that I have not been able to make up my mind as to what the animal actually was.

However, on the evidence of his 'dragon' Aldrovandi accepted several traditional and more or less fantastic beings. Indeed it is both hard to see what on earth could Aldrovandi's specimen be and it is equally difficult both to believe that he was the victim of a hoax or that he was a conscious accomplice in a fraud.

As far as fossils are concerned Aldrovandi, followed the ideas of Fracastoro (see further on), and therefore was fully convinced that fossils were the remains of once living organisms, though he makes gross and curious mistakes, just as when he figured a fossil bivalve as a petrified heart or when he figures some fossil molars of bisons or aurochs, judging them to be the teeth of giants!

For most of his life Aldrovandi tried to build a 'universal museum' and for years unavailingly petitioned kings and princes to grant him the necessary funds and support. He succeeded nevertheless in assembling a remarkable collection and an immense quantity of notes and files that he left to his town and that with both losses and additions, still basically survive. We shall discuss in the next chapter the general problem of the reasons and significance of natural history collections through the 16th–17th centuries.

In an essay Aldrovandi openly criticized both Lullian influxes on mnemonics and arrangements of evidence aimed solely to be of help in memorizing facts. Much for the same reasons, he equally criticized the museographic schemes derived fron Julius Camillus' 'Theatre', as being marred by Neoplatonism and Hermetism.

Aldrovandi advocated and practiced a merely empirical and pragmatic approach both in the arrangement of his archives and of his collections: his was a search for an arrangement such as to make easily recoverable any datum available. His aim, just like that of the Stagirite, was make all knowledge both available and testable for any man, leaving each individual person to use it as it best suited his particular purposes. This is a principle strongly advocated by several modern systematists, who just advocate a system having the maximum informational content.

As a whole, Aldrovandi ranks as a moderate reformer, just as anxious for continuity as he was keen to increase knowledge and expunge old mistaken notions.

His general renown made him an influential personality both in Italy and abroad as it is clearly certified by his carefully kept files of the visitors to his collections.

The Dutch Volcher Coiter (Groningen, 1534–Champagne, 1576) was undoubtedly the most important of Aldrovandi's pupils. He studied also in Montpellier with Rondelet, in Rome with Eustachi and in Padua with Falloppia. For a while he was professor in Perugia. Later he went to Germany, where for some time had a professorial chair, but he worked also as a military surgeon, and actually died when a surgeon with the army of Casimir of Palatinate, who had been campaigning in France in support of the Hugenots and of their leader, the future king Henry IV.

The main works of Coiter were published between 1572 and 1576, when he was back in the Netherlands. Coiter, sometimes on the precise advice of Aldrovandi, studied the anatomy of a number of animals, mostly vertebrates. His descriptions are very good and his figures are accurate, though here and there there are some curious mistakes, such as in a fine figure of the skeleton of a monkey, which however, has the position of the pelvis entirely wrong! Coiter was interested also in embryology and made extensive studies on the development of the skeleton. He made also some investigations on the reproductive organs and, but he was not the only one, saw Graaf's follicles and argued that they were eggs at different stages of development. Finally he published, included in his osteological works, a short tract *De auditu instrumento* where he gave some significant contribution to the knowledge of the anatomy of the hearing apparatus. Not only was Coiter certainly a first class anatomist, but his extensive studies of animal anatomy justify considering him as a forerunner in Comparative Anatomy.

A monograph that, given the importance of the animal at the time, is worth mentioning is the anatomy of the horse, written by the Bolognaise senator Carlo Ruini (1530–1598): the first systematic investigation of the anatomy of an animal, which, both for its completeness and for its beauty has been often compared with Vesalius' *Fabrica.* Ruini's book was blatantly plagiarized by several people.

As we said, during the Renaissance the chairs of anatomy and of surgery were usually blended in the Italian Universities. Thus is is not surprising to find that several physicians, who were basically surgeons, contributed significant anatomical discoveries. Such is the case of Leonardo Botallo (1530-after 1571). Botallo was born in Asti but spent most of his active life in Paris, where he had gone as personal physician to the queen Caterina de' Medici: the discovery of Botallo' ductus arteriosus, which bears his name and that obliterates after birth, is generally credited to him. Apart from the *ductus Botalli*, Botallo made also a significant contribution to pathology by his book *De curandis vulneribus sclopetorum,* (Lyon, 1571) where, contrary to the common opinion of the time, he argued that the wounds from firearms were not poisonous by themselves, and that the infections and gangrene that often followed such wounds were not due to a special poisonous power of the bullet itself. Indeed such wounds, as they were caused by soft balls of big gauge, usually involved the retention of the bullet itself and of fragments of dress carried by the bullet and, finally, these big and comparatively slow bullets caused serious contusions, followed by local necrosis of the surrounding tissues, all these making infection so much the easier.

Another such notable surgeon-amatomist was Guido Guidi (Vidus Vidius) a Florentine, who died in Pisa in 1569, after having been for years a physician for king François I of France and a professor at the 'College de France' which that King had established in 1530, in despair of the obdurate conservatism of the Sorbonne's medical faculty. Later the Tuscan grand-duke Cosimo I called him to read philosopy and medicine in Pisa as part of his already mentioned plan for the revival of that University. Guidi is still remembered for his discoveries on the Vidian canal and the Vidian nerve. However, he was also the first to perform successful plastic surgery (his masterpieces were reconstructions of noses by transplants from the patient's arm) which he described in his *Chirurgia e Graeco in Latinum conversa.*

The Renaissance problem with physiology

The early renaissance scholars, be they 'litterati' or scientists thought that they were living in a happy age of *renovatio*, the rebirth of classical, perfect times and while they were thinking to look backwards to the distant past, they were, in fact, opening new alleys. This was, incidentally, instrumental in enhancing the prestige of Neoplatonism and Hermetism and by that weakening the authority of the Aristotelean canon and suggesting the need for some sort of valid compromise between the different schools.

Indeed, as Galen's physiology and anatomy were carefully integrated and, as Galen had been himself a man of universal culture and interests, they could be even considered part of a comprehensive framework reaching from philosophy to politics. Thus scholars, in the second half of the 16th century became more and more conscious that they were facing a crisis and the reactions ranged from the enthusiasm of the early Hermetists, who thought they had rediscovered the true, pristine, science attuned to the renovation of Christianity (they were soon mostly recruited into the camp of the Reformation), to the attitude of conservatives, usually well entrenched in the Universities' chairs, who were terrified by the chaos that they foresaw.

Rather soon the scholars became conscious that the development of culture and sciences was also bringing about their disruption.

The first acute problems, as far as biology is concerned, arose with the new anatomy. Surgeons (and in Italy at least surgery was part and parcel of the physician's profession) were immediately enthusiastic about the new anatomy and, apparently, did not bother much about its implications for galenic physiology: the medical remedies did, in fact work all right. Indeed, when you peruse such books as the Pseudo-Apuleius of 1483 or pharmacopeae such ae the famous 'Florentine pharmacopea' of 1567, you find that, albeit complicated and often including useless products alongside the good ones, the simple and compound drugs used did indeed contain properly administered drugs. On the other side, and especially in France, where surgery was kept apart from medical practice, physicians were immediately aware that, should the anatomy Vesalius's and company's be right, it would imply the collapse of Galen's physiology, which stood as the basis of the theories on which medical practice was based. This explains how the reaction to the new anatomy by otherwise competent people such as Sylvius was so acrimonius: they could not see how to devise an alternative physiology and, indeed, this was plainly impossible given the evidence available.

The wise ones of the time, concentrated on descriptions: these could be checked and they did not take sides in the debates. Many strove for a compromise (and a fine example is Guinter from Andernach). Natural Magic was the choice of many of those who stood against traditional Aristotelianism, and this will be one of the main subjects of the next chapter, just as the pervasive influx that had, especially in the next century, the realisation that the traditional mixage of classical and basically Aristotelean philosophy and Christianity could not stand if the essentials of the Bible, the Thorà, were thrown in and give its basic function of foundations on which the Gospels stood. As these probems were crucial throughout the next century, we shall leave also these important points for a fuller discussion further on.

In the late 16th and even more in the 17th century Paracelsian physiology made a bid as a substitute for Galenism, but as it was itself inconsistent with the new anatomy, it was not a satisfactory answer to the problem and it took over a whole century of debates (and worst) to find a way out of the impasse.

The big debate arose first and foremost on the problem of the blood's circulation, and, as Harvey's discovery was to hammer the first definite nails in the coffin of Galenic physiology, we must first give an outline of the problem as it appeared at the time.

According to Galen the food, once eaten is first modified in the gut (the Plinian term *coctio,* from *coctura* = to cook, was standard), thence having been thus roughly elaborated, it goes from the gut to the liver passing through the *vena porta*; in the liver the food is further tranformed into blood and gets some natural spirits which give it nutritional powers, the *vena cava* receives the blood from the liver and sends part of it directly to the different organs to feed them, and conveys part of it to the right half of the heart. There the blood, through a network of pores, oozes into the left part of the heart. Meantime, during the diastole, the heart pumps in air from the lungs, through the 'venous artery' (the pulmonar vein). Such air has a twofold function: it mixes with the blood providing it with the 'vital spirit' (actually *pneuma)* and thus transforming it from the dark blue venous blood into the bright red arterial one, and it cools the heat that God put into the heart at the beginning of life and that must last there until death. At systole, that portion of the blood which has not oozed (and thus is still imperfect) into the left half is pushed back form the right half of the heart into the great veins to reach the organs and provide them with some gross nurishment. This 'imperfect blood' reaches the lungs by the 'arterial vein' (= pulmonary artery). From the left half of the heart the aereated blood, which is now 'vaporous, thin and sincere', is pushed through the aorta, to the various organs, which it thus supplies with the necessary pneuma, needed both for all vital processes and to complete nourishment. This is the scheme; however Galen thought that a small amount of blood leaving the right ventricle reaches the left atrium passing through the pulmonary artery and the pulmonary vein, thus implicitely admitting that the two are connected. He also assumed that the bicuspid valve still let some blood form the left heart to flow back at each systole into the lungs, where it discharges some ashes which are continuously forming in the blood. It is notable that, although the reflux of the blood from the left heart into the lungs is a mistake, yet the idea that the blood discharged some noxious residues into the lungs, to be expelled in respiration, was essentially right. Thus according Galen throughout the circulatory system, both in the veins and in the arteries, there obtained a regular flux and reflux of blood.

The core of Galen's theory was that the essential function of blood is to nurish the organs, that is that the blood itself is transformed into the tissues substituting their worn parts. Galen's system was wrong on the following points: (i) it assumed the porosity of the intervetricular septum, (ii) it assumed the passage of air through the pulmonary veins, (iii) the systolic reflux both in the arteries and the veins does not exist, (iv) the chilum does not pass by the *vena porta.* Moreover Galen considered the liver as the main organ producing the blood, while the heart's main function is to heat the blood and help in the mixing of the blood with pneuma. It is thus obvious that the advances in anatomy falsified the whole theory.

Galen also assumed that in the liver such parts of the food that cannot be transformed into blood, are transformed into yellow bile and collected in the gall, or into atrabile, which goes to the spleen, or, finally into urine, which is collected by the kidneys (as the study of urines had a great significance both in diagnostics and prognostics, a much debated problem throughout the Middle Ages and the Renaissance, was whether urine and bile were formed in the liver and simply collected in the gall, spleen and kidneys, or whether both the urine was elaborated in the kidneys and the atrabile in the spleen thus both acting directly as depurators of the blood).

The medieval physiology of the nervous system was very crude: It assumed that the arterial blood, rich with animal spirits reaches the hypophyseal region, where it circulated in a *rete mirabilis*, which, in fact, exists in some mammals. There it was further purified and enriched by animal spirits, then it got into the nervous system and ran through the nerves, thus causing movements and controlling the different functions. Nerves are conceived to work, with respect to the brain very much as blood vessels with respect to the heart and, like them are thought to be like thin canals. Animal spirits are elaborated from the vital spirits supplied by the arterial blood running in the meningeal menbranes and also from air, which reaches the brain through the pores in the ethmoid. Animal spirits are stored in the cerebral ventricles and from there they run into the nerves, and through them reach muscles and sense organs.

Occasionally, in connection with the function of the brain, the location of the soul is debated by philosophers, as well as the problem of how the brain can develop its highest functions: imagination, thought, memory. Anatomists and physicians usually, and perhaps wisely, rarely touch on these problems.

The physiology of reproduction and of other functions is usually a compromise between the hypotheses of Hippocrates, of Aristotle and of Galen. Almost always Aristotle's theories are at least partly rejected, and the different scholars side with either of the other two. Usually sperm is considered a particularly refined kind of blood, which is perfected in the testes. Both male and female are considered to share in the reproduction, as vulvar secretions are considered to be the feminine sperm. Thus Aristotle's theory that the female supplies only the material of generation (menstrual blood) while the male sperm carries the 'formal principle' (Entelecheia) is seldom considered. It is commonly believed that the two semens meet in the uterus and there coagulate to start embryogenesis. Most authors hold that the white and cool parts of the body (membranes, skin, nerves, brain and vessels) derive from the male sperm (*partes spermaticae*), while the hot and red liver, heart and meat (= muscles) come from the feminine sperm (*partes sanguinae*), thus following Galen.

Throughout the 16th century there is a running debate whether during embryogenesis first appears the heart (Aristotle) or the Liver (Galen). Another debated topic is the following one: the embryo is fed by the mother through the fetal vessels, and its development is directed by a *vis vitalis* or *anima altrix.* The critical point, therefore, is: does the *Anima altrix* develop itself into the rational soul, in parallel with the development of the body, or does the rational, true soul substitute for the *anima altrix* at some given moment? The implication of the debate concerned the problem of baptism for abortions, as you cannot baptize a being who does not yet have a true soul.

Curiously the problem is still cryptically lingering in parliamentary debates concerning the latest time when abotion is permissible!

The problems of inheritance of features were rarely debated, most people following Galen in holding that maternal semen transmits the more general characters, those of the species, while individual characters are carried by the male semen. A we shall see later on, this idea, filtered through Cesalpino, was further elaborated by Linnaeus and stands at the very root of Linneus' quasi-evolutionism.

Sex is conceived as being determined either by which side of the uterus (which was still often believed to be bicornuate) housed the embryo or, more often, it is supposed to depend on whether the male semen came from the right or from the left testicle.

Such is the broad outline of Renaissance physiology which had to come to terms with the new anatomy!

Harvey's predecessors

As we said the probem of circulation of the blood was the first on which some real advances were made during the 16th century. The reconstruction of the story of the discovery of circulation has occasioned considerable debates, often tainted by nationalism. By now the picture is quite clear.

The ancients, and especially Galen, had a vague idea of the possibility of the pulmonary (= little) circulation. This was revived by the Arab Ibn al-Nafis in the 13th century and by Leonardo, who may even, in an obscure paragraph of his notes, have thought of the great circle. However these had no impact on contemporary science.

The first precise statement concerning the pulmonary circulation is by Miguel Servet y Reves (Servetus, 1522-1553), a physician-phylosopher-pantheist theologian, in a strange booklet (*Christianismi restitutio*, 1553). Servetus' family belonged to the lesser nobility and he had studied in Paris at the same time as Vesalius and might have studied also in Padua as it is certain that he visited different places in Italy. Having settled in Strasbourg, he promptly published a book, *De trinitatis erroribus,* which obviously infuriated both Catholics and Protestants, so that he had to flee for his life. He then settled in Lyon, but when he published his *Christianismi Restitutio*, he had to fly again from the Inquisition (his image was later burned in Vienne) and went to Geneve, where the Calvinists duly burnt him at the stake, together with most copies of his booklet. At present, apparently only three copies of the original edition of the book survive, one being incomplete.

In his book Servetus maintains that the whole Creation is a manifestation of God, just as Jesus is, and in two brief passages he mentions blood and criticizes Galen's theory: Servetus maintains that the blood goes fron the right ventricle to the lungs, where it passes from the Arterial vein (= pulmonary artery) into the Venous artery (= pulmonary vein) where it is purified from ashes and mixed with air; finally, being completely purifired and aereated, it is sucked back into the heart in the left atrium. Servetus argues that the connection of the pulmonariy arteries and veins is in the lungs because there would be no reason for the 'venous artery' to be so large if its function was merely to nourish the lung, as, indeed, before birth, when the lung is not functioning, the lung itself is fed by a tiny vessel: the arterial duct of Botallo! Servetus also flatly denies the porosity of the interventricular septum and the passage of blood from the right to the left ventricle; at most he considers the possibility of a very limited and functionally irrelevant oozing through the septum.

As Servetus book was basically a book on theology, and as almost all the copies were destroyed and as the little circulation is there discussed in but a few incidental paragraphs, it is safe to assume that it was completely ignored by contemporary anatomists and that its significance in the development of sciences was nil.

The impact of the teaching of Realdo Colombo was quite a different matter. There is no doubt that Colombo lectured on the little circulation for some time previous to its publication in 1552 in the *De re anatomica*, shortly before his death. Colombo's contributions are duly acknowledged by Harvey. Realdo Colombo rejected any possibility of permeability of the interventricular septum and maintained that the 'venous artery' carries only blood and neither air nor blood mixed with air, as it was currently believed by many. As for Galen himself, we said that he did not think that it carried simple air, but, following the principles of the second Stoa, that it carried blood enriched with pneuma, that is a qualitative fraction of common air, a mix of *vires* (powers, virtues, proprieties), which were further purified and used by the different organs.

In his description of the four main vessels attached to the heart, Colombo remarks that two are such as to bring blood to the heart during the diastole and two to carry it out at systole. Colombo clearly describes the 'lesser circle', yet he still subscribes to the opinion that the veins are responsible for carrying 'nutritional blood' to the different organs of the body.

A better notion of the circulatory system, yet curiously faulty, was provided by Andrea Cesalpino, whom we have already considered as a botanist. Cesalpino was a pupil of Colombo and in his *Peripateticarum questionum libri V* (Venice, 1572), and in *Quaestionum medicarum* (Venice, 1593) proposed the following theory: he holds (and he is the first to use the term 'circulation'), that the blood passes regularly from the arteries to the veins everywhere in the body, by a network of capillaries. He thinks indeed that Galen's *vasa per capillament resoluta* are not tufts of blind ended thin vessels, but that they are a true network. He remarked that, when a vein is bound in an animal and then, after a little time is cut, the first blood bleeding is very dark, but that is becomes bright red as bleeding continues, as may be expected if there is a passage of blood from the arteries to the veins. Finally, Cesalpino remarked that if one binds the veins anywhere in the body, these vessels bulge between the ligature and the origin of the vein from the capillaries, while the portion of the vein from the ligature towards the heart becomes empty, contrary to what is expected by Galen's theory. However Cesalpino's conclusions are patently absurd: he did, in fact maintain that the blood flowed from the heart to the tissues during the day, to go back to the heart at night! Moreover, at least in the *Peripatetic questions* he appears to be still believing in a limited permeability of the interventricular septum.

Advocates of Cesalpino have charged Harvey with not mentioning their author, though his books appear to have been available in Padua when Harvey was studying there. As a matter of fact, while Colombo and Fabrizio (both quoted by Harvey) were teachers in Padua, Cesalpino was teaching in Pisa, so that his books may have had, at most, a limited circulation in Padua. Unless Harvey had already a specific interest in blood circulation, for which there is no hint, he would have no special reason to read books by someone who was basically a botanist and even less to take the trouble to buy and bring home costly books of apparently little use. There is thus small reason to doubt that Cesalpino's books were not available to Harvey during the many years that he spent in England thinking and experimenting on circulation.

The last person who, to some extent, paved the way for Harvey, was the already mentioned Fabrizio d'Acquapendente (Fabricius). Harvey qualifies Fabricius as *peritissimus anatomicus et venerabilis senex* (= an exquisite anatomist and a most respectable elder). Fabricius had discovered the valves of the veins in 1574 and had been lecturing on them until he published his discovery in 1603, when Harvey was already back in England (*De venarum ostiolis*). However, Fabricius misunderstood the function of such valves as he thought that their use was in slowing the flux of the blood from the heart to the organs! This was a curious error as he describes precisely the experiment that proves that they are there to prevent the reflux of blood to the tissues: Fabricius proved that, by lightly binding the arm, so as to block the superficial veins, these expand towards the perifery as blood coming from the capillaries continue to reach them, moreover the position of the valves themselves becomes noticeable as especially turgid points. This is precisely the same experiment that, correctly understood by Harvey, the Englishman quotes among those that support his theory!

The fossils

Before ending this chapter we must refer to the debate on fossils.

We have incidentally mentioned that Aldrovandi followed Fracastoro in his appreciation of fossils. Actually Girolamo Fracastoro (1483-1553), a Veronese noble and physician, had been a friend of Girolamo della Torre, the anatomist and good friend of Leonardo, who was to cooperate with him in the great treatise of anatomy that Leonardo had planned. Della Torre's untimely death had wrecked the project, but Fracastoro's ideas are so close to those that we find in Leonardo's notebooks, that it is arguable that they originated with him. On the other side he stated them in 1517, as certified by Torello Sarayna, a lawyer, who published his book on the antiquities of Verona in 1530. Fracastoro's ideas were further extensively expounded, giving him due credit, by Ceruti and Chiocco in their book *Museum Francisci Calceolarii*, published in 1622, over a century after they had been originally argued by Fracastoro. On these second hand testimonies, Fracastoro's thesis was that fossils were the mineralized remains of animals and plants that had been left stranded by the retreat of the sea. He argued this last point by the actualistic evidence that the sea had considerably retreated in his own times both in Egypt and near Ravenna. He had also criticized the thesis that the Pholads, a specialized burrowing bivalve mollusc that lives in deep galleries that they burrow into rocks, were actually born by spontaneous generation inside the rocks themselves, an argument that had been considered as evidence that, as living animals could be generated inside rocks, many could have began there their development without being able to complete it. On the other side Fracastoro followed Leonardo, arguing against the idea (then popular at least in Italy) that fossils were the relicts of Noah's flood. He maintained that the 'flood' must have been only a local phenomenon, and that, in any case its reported length, 140 days, was far too short a time to explain the enormous thickness of sedimentary rocks and, moreover that it could not possibly account for the finds of obviously immotile or hardly motile seashells at great distance from the sea. Such seashells, moreover, showing different developmental stages, proved that they had been living and breeding where they were now found.

Much the same hypotheses as Fracastoro's were argued at the same time by Alessandro degli Alessandri, a Neapolitan jurist (1461-1523) who, in his *Dies geniales* (The Days of the Origin) of 1522, had maintained, on the evidence of marine fossils found in the interior mountains, that Calabria had been once mostly covered by the sea. And again the same ideas were propounded by Ferrante Imperato in his *Historia naturale* of 1599.

In order to explain the mechanisms of fossilisation Nicola Manetti, in 1520, argued, following Lull's ideas on the influences of celestial bodies, that the remains of organisms could be petrified by such influxes.

Most naturalists of the 16th century took sides in the debate on fossils. There were the partisans of their inorganic nature, such as Giovan Battista Olivi, from Cremona, who in 1584 maintained that fossils were merely figured rocks, or Libavius (1560-1616) who argued for a sort of panspermy: there were ubiquitous 'germs', which originated both crystals and fossils, according to the local conditions. We have also already mentioned how Falloppia and Gesner were both loath to believe in the organic origin of fossils. The second party, holding the 'Flood's' origin of fossils included among many others the physician, magus and great matematician Girolamo Cardano or, as already mentioned, none the less of Martin Luther! Finally there were others, besided Fracastoro or degli Alessandri, who correctly though the fossils as evidence of past living organisms, but thought them to be possibly much more ancient than Noah's flood. Such were the French Bernard Palissy (1510-1589), the German Georg Bauer (= Agricola) of Nettelschein (1494-1555) or the equally German Enalius (Christopher Entzell, 1517-1583) the son of a craftsman and later member of the town council of Saalfeld who, in a *De re metallica* of 1551, published some excellent figures of fossils and argued for their animal origin; and such was, finally, the already mentioned Aldrovandi, who considered the fossils as evidence of a very ancient sea.

Time scales were not a problem at this point as scholars were aware of very rapid 'petrifications' in hydrothermal deposits.

The beginning of European world colonialism and the early zoological and botanical explorations

Up to the end of the 15th century information as to the animals and plants from the Far East or the interior of Africa were scanty, mostly vague and often mere legends. Spices, drugs, precious objects were available, but usually they arrived through a chain of middlemen. Obviously both the ancient sources, like Herodotus, were studied, as well as the reports of the few who had traveled into the interior of such fabled lands, such as Friar Giovanni da Pian del Carpine, Marco Polo and a few others.

In the 15th century the Portuguese, soon followed by the other European kingdoms, launched a steady program of systematic explorations aimed to open new routes for trade. Having at last developed the first vessels worthy of the high seas, squadron after squadron sailed along the African coast and deep into the ocean, and were finally able to reach India and establish trading stations there and all along the African coast. The Italian states were soon aware of the danger that the opening of the oceanic routes was for their trade and prosperity, but were handicapped by being sealed up into the Mediterranean at a time when the steady military expansion of the Turkish empire and the activities of its vassal pirate little states of North Africa produced a state of semi perpetual warfare, both on great and minor scale, which reached its climax with the great battle of Lepanto (1571). Anyway both the main Italian states, and the great Western European monarchies promoted the study of also the neighbouring lands.

The discovery of central America in 1492 (wich just followed the final vanishing of the Viking settlements in Greenland, which, anyway had gone practically unnoticed by the scholars) opened an entirely new world of unexpected animals and plants. Indeed some scholars debated whether these were not the product of a separate creation, different from that recorded in the Bible. Theologians even debated the problem of how could the native Americans descend form Adam and some argued that they could not possibly be true men in the theological meaning.

Actually it was the acute need to catalogue properly all the flood of new evidence that was partly instrumental in the development of true biological systematics.

Through the 16th century were published a growing number of travel accounts and descriptions of new animals and plants. These were sometimes single chapters in the context of some general account of a given country, sometimes were regular monographs.

As a typical example we may recall that we said how Belon explored the countries of the East Mediterranean between 1546 and 1549. In 1553 he published an interesting booklet 'Les observations de plusieurs singularitéz et choses memorables trouvées en Grèce, Judée, Egypte, Arabie at autres pays estranges'. This book, as was usual up to the 19th century, is a mélange of anything of interest: curious habits of people, animals, plants, drugs, strange crafts. As for biology, Belon on one side scorns as legend some traditional lore, on the other he considers as being reliable some incredible stories. Rather than decrying such mistakes, one should think how difficult it has ever been for travellers to tell truth apart from phantasy on second-hand reports and both the state of the art and the flood of new and extraordinary evidences that was current at the age and made even wild stories quite credible.

Another French explorer, Pierre Gilles (Petrus Gillius, 1490-1554), who was a Provençal from Albi, made the most adventurous trips, but, unfortunately, lost all his collections. He actually travelled on commission from the French king François I, who had allied himself with the Sultan against Charles V and was anxious to open the Middle East to French influence,

Important additions to the fauna and flora of the Middle East were made by Prospero Alpini, born in Marostica, near Vicenza in 1553 and died in Padua in 1616. He had graduated in medicine and was later attached to Giorgio Emo, Venetian consul in Cairo from 1581 to 1584. Back in his country, he was appointed lector of simples at the University of Padua. His most important contribution is the *De plantis Aegypti*, where, amongst other things, he first described coffee! Alpino described also several new animals and wrote also some historical contributions. His medical treatise *De presagienda vita et morte* is hailed as the first systematic tratise on semeiotics.

We said that for biology, by far the most important event of the Renaissance was the discovery of the Americas. The first systematic account of the natural history of the newly discovered lands is by Gonzalo Fernandez De Oviedo y Valdez (1470-1557): the *Historia general y natural de las Indias* (Salamanca, 1535), where are described for the first time a number of animals and plants such as the tapir, the tree-sloth, the manatee, colibris etc. and, among the plants, Maize, Pine apple, Cassava, Cactuses.

The works of the Jesuit José de Acosta are even more important. Father Joseph left from Cartagena in 1570 to preach in Peru; thence he traveled North into Mexico and sailed back to Spain in 1587. He died in Salamanca in 1600. His *Historia natural y moral de las Indias* earned him the nickname 'Plinius of the New World'.

Among other things he holds a quite correct hypothesis as to the origin of the Amerindians: after having carefully examined the different theories of his time: separate creation, migration by sea, migration by land, he argues that there must have been a connection between Asia and America in the extreme North.

All the different activities and problems discussed in this chapter were, moreover, prompting the diffussion of natural history collections,which will be discussed in the next chapter.

Medicine in the XVI century

Throughout this century natural history and medical practice were still so closely linked that, in order to understand both some advances and some problems, we must briefly discuss various aspects of Renaissance medicine in its connection with problems of general biology.

A very important problem which aroused much debate was that of contagion in epidemic diseases. This was largely influenced both by the comparative diminishing impact of some diseases, such as leprosy (though certainly many cases formerly diagnosed as leprosy were of different nature, such as skin tubercolosis), and on the other by the outburst of epidemics of previously unknown diseases, such as syphilis. The appearance and spread of previously unknown diseases compound the debate on contagion with that raised by Paracelsus and his followers concerning the chemical nature of diseases themselves. As, however this later aspect of the debate came to the forefront only very late in the 16th century and reached its achme in the next, we shall examine it in the next chapter.

While the reduction in the incidence of some diseases may have been due either to improved sanitary conditions, to better implementation of sanitary police regulations or to the combined effects of both, the appearance of syphilis, which was spreading like wildfire through Europe in its acute form, raised urgent problems. Though there are still debates as to the origin and first appearance of syphilis in Europe, it was just in the very last years of the 15th century that the disease was recognised as such. The Italians, who were among the first hard hit ones, made the accusation that it had been introduced in the wake of the French army of Charles VIII, who had crossed the whole peninsula marching on Naples, and so called it either ‘French disease’ or ‘Gallic pest’; the French, claimig that their soldiers had been infected on arrival in Naples, called it ‘Neapolitan disease’. If the disease was imported into Europe by Columbus’ sailors, it must have rapidly spread to the Western Mediterranean sea-ports, Naples included, while the armies on the move, which were always accompanied by numbers of whores, helped in its rapid spread, and the police regulations that had helped to keep in check other venereal diseases throughout the middle ages, in spite of being drastically reinforced, proved powerless in this case.

The same Girolamo Fracastoro, whose ideas on fossils have been already mentioned and that had got his doctorate in Padua (where he had been a friend and fellow student of Copernicus), wrote a short poem *Syphilis sive de morbo gallico* (Verona, 1530) which actually established the name of the new disease. The poem imagines that a shephard Zyphilus is punished by Apollo with the new disease, which symptons are accurately described. While various reasons that do not concern us here led the physicians to treat syphilis with preparations of mercury, of guaiacus or of sandalwood (and Paracelsus strongly criticised the excessive ministrations of these concoctions), the debate on the cause and propagation of the disease was rife. Fracastoro dealt with these problems in a book (*De contagione ed contagiosis morbis*), where he describes the main epidemic diseases knowm in his times: plague, petechial typhus, syphilis, *etc.*

Fracastoro assumes three types of contagion, by direct contagion, as it happens with mange, indirect, where *fomites* such as dresses. linen etc. carry the *seminaria prima*, finally Fracastoro considers that a third type of contagion does not require either a direct or an indirect contact between the diseased and the healthy subjects. Such diseases are transmitted at a distance, so he thinks, as its *seminaria* attach themselves to some humor to which they have some affinities, and such minute particles may be transported in the air and enter the healthy body through respiration and thence enter the blood vessels; Fracastoro holds that such a kind of contagion occurs in the plague, smallpox etc.. He holds that all such *seminaria* are, in fact, alive (*contagium vivum*) and, though his ideas as to the nature of the *semimaria* were vague and cannot be considered as a true anticipation of the later discovery of bacteria, they were obviously of great importance. As, at the time, it was clearly impossible to prove it, Fracastoro's theory was not welcomed by the majority of physicians, yet it was almost immediately supported by several among the most progressive ones either contemporary or scarcely later than Fracastoro. It was thus enthusiastically advocated by such people as the already mentioned Filippo Ingrassia and Prospero Alpini.

CHAPTER VIII

The 17th Century

SYNOPSIS OF THE MAIN EVENTS AND CHRONOLOGY OF THE MOST IMPORTANT SCIENTISTS

Ulisse Aldrovandi 1522-1605, Thomas Hariot 1560-1621, Caspar Bauhin 1560-1624, Sir Francis Bacon lord St. Albans 1561-1626, Galileo Galilei 1564-1642, Johannes Kepler (Keplerus) 1571-1630, J.B. von Helmont 1577-1644, Robert Fludd 1574-1637, William Harvey 1578-1657, Tommaso Campanella 1568-1639, Pierre Gassendi 1592-1655, René Descartes (Cartesius) 1596-1650, Pierre Fermat 1601-1665, Johann Rudolph Glauber 1604-1670, Evangelista Torricelli 1608-1647
1618-1648 Thirty years war.
The Academies: Accademia dei Lincei: 1603-1630; Accademia del Cimento 1657-1667; Royal Society: 1662; Academie des Sciences: 1666.
Robert Boyle 1626-1691, Blaise Pascal 1623-1662, John Ray (Wray) 1627-1705, Marcello Malpighi 1628-1694, Christian Huygens 1629-1695, Anton van Leeuwenhoek 1632-1723, Robert Hooke 1635-1703, Jan Swammerdam 1637-1723, Nehemiah Grew 1641-1712, Isaac Newton 1642-1727, G.W.Freiherr von Leibniz 1646-1716.
1644-1653 English revolution
1653-1658 Cromwell's dictatorship
Edward Tyson 1651-1708, Johannes Camerarius 1665-1721, Jean Bernoulli 1667-1716,
1683 Last attempt of the Turks to capture Vienna. They are beaten by J. Sobieski, king of Poland.
1689-1725 Peter I, the Great, is Czar of Russia.

Some general remarks on the XVII century

It is usual to date the beginning of the 'scientific revolution' to the start of the 17th century, but the reader is begged, also better to understand the biographies of several scholars, to note that this was also the century of the bloodiest and most destructive wars in the history of Europe up to the First World War. To most of the combatants many of these were wars of religion: England went through two revolutions, the bloody war between Charles the I and Parliament and the 'glorious revolution' that sealed the destiny of the Stuart dynasty, besides she fought in turn against the Spaniards, the Dutch and the French; France was at war with all the countries surrounding her and, in the intervals Catholics and Huguenots killed each other; Spain was occasionally at war with France and fought for years against the British and the Dutch; Germany was devastated by the 'Thirty years war' which involved also the French and the Swedes, the Empire, or rather Austria fought in the North against the

German Protestants and their allies and to the South almost continuously against the Turks at times allied with Venice; Venice itself was single handed at war with the Turks for over 20 years, while the rest of Italy was a battlefield for the French and the Spaniards, with an occasional intervention of some German-Austrian armies. The African pirates spent their summers raiding the coasts of the Central and Western Mediterranean, while the Knights of Malta and the Tuscan Knights of St. Stephen tried, on a lesser scale, to retaliate. Because of the religious background of these wars, moreover; not only people were enthusiastically killing of each other in the name of God, but, in addition, whenever the Calvinists or the Turks got hold of any 'image' they busily smashed it. Countless masterpieces were destroyed or whitewashed in order to cleanse the churches, while, when not otherwise busy, commoners and local authorities all over Europe, but mainly in Germany, found the time to indulge in witch-hunting, and succeeded in burning over one million people. Finally the Jews were the victims of a number of outbursts of expulsion and widespread vexation. Almost only Italy escaped to some extent all these niceties: a good many wars were waged also in Italy, but at least witch-hunting was fairly sporadic and, especially in Central and Northern Italy the local governments usually afforded the Jews a reasonable amount of protection.

Historians of science often indulge, to some extent according the nationality of the writer, to extol one or another of the 'Founding Fathers' of the Scientific revolution itself. As for the list of such 'Founding Fathers' there is a widespread consensus: Francis Bacon, Galileo Galilei, Nicholas Copernicus, René Descartes and, as an option, William Harvey (not that his importance is doubted, but as some aspects of his personality are hard to accept as those of a good 'revolutionary', it happens that in many histories of sciences, he is reckoned as the last of the previous century, though his 'magnum opus' on the blood circulation was published in 1628.

As for myself, I think that to date the beginning of the 'scientific revolution' as having occurred in 1600 is as significant as to date the end of the Antiquity and the beginning of the Middle Ages by the deposition of the Western Emperor Romulus Augustulus, an event that passed quite unnoticed for the contemporaries.

In fact two of the 'Fathers' were conscious of their stand as innovators: Francis Bacon and Galileo Galilei (who, however, qualified himself as 'Pythagorean philosopher'), whereas their contemporaries Kepler and Harvey, though just as conscious of the importance of their discoveries, were intellectually much more akin to the corresponding trends of the Renaissance.

Sir Francis Bacon, later Baron Verulam and Viscount St. Albans and for some time Lord Chancellor of England (1562-1626), was basically a learned politician, with a lasting interest in philosophy. He turned entirely to philosophical speculations when he was dismissed from the Court. His personality is hard to define, perhaps the best definition is: 'an outstanding amateur', and it was just because of his amateurish approach that he had little scruples against producing a true 'Manifesto' combating all kinds of traditional science. Yet he was no radical innovator: as a junior contemporary of Giordano Bruno (who had lived several years in England) he may well have been to some extent under the latter's influence. Being basically a politician (William Harvey, who was his personal physician said once: 'he writes philosophy as a Lord Chancellor!'), he distinguishes himself from all his contemporaries because of his systematic stressing of the public utility of sciences. In a way one could say that Bacon was a 'utilitarist' *ante litteram*. In fact the basic break of Bacon with scholarly tradition is just by his insistence on the public function of sciences.

In his times both the Catholic and the Protestant clergies were worried that a widespread debate on scientific-philosophic matters by laymen could help in the diffusion of heresies. On the other hand, lay scholars both of true Aristotelic tradition or of the neoplatonic-hermetic trend were much afraid of any widespread knowledge. They saw in Natural Magic mainly a path to individual moral perfection, to glory and to the opportunity to help individuals of their choice. They thus fought 'the excessive' diffusion of knowledge, either by writing in Latin, or even more often by using a deliberately allusive and obscure language, a language for adepts, so much that 'hermetic language' became a byword for unintelligibility – as a typical example just try to read the book *Del senso delle cose e della magia* (= *On the meaning of things and on Magic*) – by the Italian philosopher Tommaso Campanella (1568-1639).

Francis Bacon, instead, not only commonly wrote in English, but he emphasised the importance of the general diffusion of knowledge.

Although his chief work is the unfinished *Novum Organum*, Bacon discussed in a number of publications the basis for the interpretation of Nature, the logic of science, and the promotion of a practically aimed science. He identified as *Idola* such mental attitudes that commonly lead to error either of method or of scientific reasoning.

Bacon repeatedly and unsuccessfully endeavoured to establish a big research institute. This should have covered all sciences and be provided with botanical gardens, chemical laboratories, and zoos, and thus foster applied research (Bacon recommends the pursuit of what we now call genetic improvement of plants, comparative anatomy and the search for the philosopher's stone!). Basically Bacon was fully convinced that he was trying to establish a new philosophical school.

There is no doubt that Bacon was an inductivist and an experimentalist and that he introduced into philosophy some typical features of the later 'English empiricism'. Though neither deep insights nor really rigorous logic rate him as a 'great' philosopher, his work nevertheless had a great and positive influence. His ideas were largely implemented by the later scientific academies and especially by the Royal Society, some 50 years after his death.

It is somewhat peculiar that, though Bacon was urging experimental research, almost all of his work relies on published sources and while largely ignoring the greatest discoveries of his contemporaries, including those of his own physician Harvey, he ransacks, both for the good and for the worst, the writings of such people as Gianbattista Della Porta. Although Della Porta was an important scholar in his own right, yet he fits into the tradition of Natural Magic rather than as a forerunner of a modern approach to sciences.

Galileo Galilei, in his attitudes, is much more like a modern scientist. He has but a marginal interest for the historian of biology, as his only significant contribution in this field is to have shown how skeletal structures can be analysed in physico-mathematic terms of statics. However his technical achievements, such as the earliest compound microscope, his thermometers and clocks etc., provided just the instruments needed to open to discovery entirely new fields of biological studies and considerable improvements in more traditional ones, as they provided the possibility of accurate quantitative studies.

Galileo's philosophy is ambiguous. He was a good mathematician and well learned in different fields, literature included; but his real genius, besides his ability in either creating or improving of a number of instruments, was his uncanny ability to devise experiments, quite often purely theoretical, but nevertheless proper to clarify problems. Adding to these qualities his total self confidence, he was the proper type to become a 'charismatic' personality.

Galileo was a good and practising Catholic, but he had as much faith in the scientific truths that he was discovering and thus he soon became such a staunch and orthodox Copernican, that he always maintained the perfect circularity of the celestial orbits and never accepted Kepler's discoveries, in spite of the fact that he was corresponding with Kepler and had got his papers!

Unfortunately for him, because of his inner urge to reconcile his faith with his scientific theories, he trod the dangerous path of theology and maintained that the Scriptures had been written for the common man and therefore should not be taken literally by the philosophers, but should be explained by them in the light of the results of scientific investigations. Practically this was a mere variant on the old Medieval theory of the Two Truths, which stated that when theological and philosophic truths were at odds, both should be followed by the scholar 'as expedient'.

Galileo maintained that the task of science was the precise description of phenomena and their interpretation within the framework of coherent theories, much as it was the model of the ancient Greek thinkers. He also maintained that the interpretation should avoid recourse to any empirically unobservable factor.

Galileo was also a great master both as a writer and as a teacher, as well by his academic lectures and by his informal talks; he thus bred a number of excellent and devoted pupils.

His trials by the Inquisition are a familiar story and their unfortunate ending was due as much to his overconfidence as to the general political-religious situation. As matters stood at the time, the Inquisition had no alternative but to condemn the scientist. As cardinal Bellarmino had written to Galileo some years before the final trial, the Church could not admit to factual errors in the Bible except when faced with overwhelming evidence and that was not available. In fact Galileo thought to have it in his theory of tides, which failed to convince his judges, and with reason as it happens to be the one completely wrong Galilean theory! Anyway the judges considered both the fame, the age and the powerful protectors of Galileo and their sentence was, for the times and charges, extremely lenient. He was never really jailed and his confinement as a sort of forced guest of friends first in Rome, then in Siena and finally at his own home in Florence was almost a formality, as he could practically continue to meet his friends.

Anyway, as all the necessary theoretical premises were lacking, the Galilean model of science was at the time almost impossible to implement in biological studies. However, as we shall see, the 17th century saw the first real attempts to a quantitative approach to biological problems and to deal with them by regular experimental programs.

It was during this century that the two schools of biology, the 'mechanist' and the 'vitalist', did gradually identify themselves and their debates were to last into the 20th century!

We have seen how the foundations of Galen's biology were grounded on general premises of stoic derivation. Until the end of the 16th century the celestial world on one side and the terrestrial elements on the other were considered as being essentially different; each one was supposed to be endowed with its peculiar proprieties, which provided them with given powers to act on other bodies in specific ways.

The real peculiarity of this attitude was not with its basic premises: as a matter of fact such powers that the medieval Latin called *vires* or *virtutes* gradually evolved into the familiar concept of Newtonian 'forces', in the concept of waves, and so on; the basic difference was that for the medieval mind there was an infinite number of them, each body or part of it being endowed with a number of different and highly idiosyncratic 'powers', while modern physics has been and still is striving to simplify and unite them into as few 'forces' as possible.

It must be added that in the late Renaissance, both in alchemy and in physiology, no one conceived of quantitative combinations among the reacting materials and the results of the chemical reactions were generally seen as true transformations.

All that slowly changed through the 17th century, often greatly disconcerting some scholars. Thus, for instance, when Newton proposed the concept of absolute space, he argued that this was an attribute of God! Thence both Leibniz and Huygens, both as pious in their own fashion as Newton was in his way, rose in anger arguing that Newton was daring to measure God. Similar preoccupations arose with the introduction of gravity: several scholars of the mechanistic, Cartesian school, were alarmed by this obscure force capable of acting at distance, as they feared that it could evolve into a 'scientific astrology' and even support the belief in the occult powers of the operations of witchcraft.

Italy, which in the 16th century had been culturally the most scientifically advanced country in Europe still held such a position in the early years of the 17th century, and, indeed, several basic advances in biology were either the work of Italian scholars, or were made by scholars which had studied in Italian universities. However, and increasingly through the 17th century, new important centres for scientific research flourished throughout Europe, while the increasing poverty and troubled Italian life, in a country that was more and more becoming largely a pawn in the hands of the great continental powers, begun to tell also on its intellectual life.

While most of the scientific literature of the 17th century is still in Latin, an increasing number of great and lesser scientists wrote also purely scientific works in their national language: So, again in Italy, Galileo and Redi wrote works that, besides their lasting scientific impact, are also masterpieces of Italian prose. It is probable, considering the content of most books written in the national languages that, as it had been already common practice with philosophic-religious books, the common language was chosen for such innovative writings for which the author wished to get the support of a more general public rather than only that of the academicians.

The scientific academies

The reborn interest for all Greco-Roman antiquities that was the pride of the 'Humanists' could not but lead to the imitation of everything classical. So, under the powerful suggestions of the teachings of Georgios Gemistos Pletho (possibly the last avowed pagan) and of Marsilio Ficino and with the support of Lorenzo 'the Magnificent' a 'Platonic Academy' was born in Florence in the last quarter of the fifteen century (incidentally in Italy during the 15th any person of consequence was qualified as 'Magnifico', nowadays only the University chancellors – Rettori – are still 'Magnificents'!). The 'Platonic Academy' was basically a literary-philosophic group, but after its example 'Academies' soon multiplied for any conceivable purpose, ranging from the most trivial to the most exalted.

Naturally, naturalists being sociable and talkative people, who enjoy discussing their 'trade' with friends, scientific academies were bound to appear as scientific research was developing. Moreover most of the best scientists, even if they got their salaries as university professors, yet did not feel at ease within the boundaries of the naturally conservative intellectual structure of the Universities, with their rigid and often obsolete curricula and would have readily subscribed to that golden maxim of Goethe: "A school is an institution for the purpose of perpetuating obsolete knowledge".

Thus during the 17th century active research became more and more the domain of the Academies, while Universities happily multiplied throughout Europe as almost mere teaching establishments, whose professors, when not members of some Academy of repute, cordially hated their colleagues who obtained fellowship of such Academies!

Academies are also the first to practice group research, as foreseen by Bacon and they also often undertook the publication, first of separate books, but rather soon of periodicals collating the results of the fellow's studies. Thus 'Acts', 'Transactions', 'Journals' etc. appear during the second half of the century and some of them are still continuing.

Through the 16th century the number of scientists is rapidly growing as well as the number of their contributions.

The 'Accademia dei Lincei' is usually believed to have been the first scientific academy, but that is not entirely true. In fact Gianbattista Della Porta (1538-1615), later a member of the Accademia dei Lincei, had founded in 1560 in Naples an *Academia Secretorum Naturae* where membership was restricted to people who had made some discoveries. However Della Porta's academy was soon stifled by the joint suspicions of the political and religious authorities.

Thus the 'Accademia dei Lincei' was practically the first to produce some consistent work. Yet it did not last long.

Being, *de facto* the fist one, its story deserves a brief outline. It was founded on August 17, 1603 by prince Federico Cesi, son of the Duke of Acquasparta, by Johan van Heek (Heckius), a Dutch physician, by Francesco Stelluti, from Fabriano, a naturalist who produced also a good translation of the Roman poet Persius and was himself occasionally a poet (some of his short poems are published in a collection by several authors, including Bartolomeo Simonetta and the famous Giovan Battista Marini, who was to give his name to a poetic fashion, 'Marinismo') and by a relative of Cesi, count Anastasio De Filiis, born in Terni. Prince Cesi was then barely 17 years old, but he was the heart of the group and set for it the task of discovering, the secrets of Nature with a penetrating gaze, such as that of a lynx (hence *Lyncaei*).

Almost immediately the new academy was suspected to be impregnated by Hermetism and Heckius was suspected both of being an heretic and a magus and in 1604 was expelled from Rome. Shortly afterwards both Stelluti and De Filiis went home. Yet the four friends kept closely in touch and Cesi began in 1605 to prepare both new programs and statutes. First by his *Lynceograpkism* or basic statute, and later by the *Praescriptiones Academiae Lynceorum* of 1624: the academicians were enjoined to constantly endeavour to study mathematics and natural sciences for the purpose of discovering the essences of things, but they were also to cultivate philology and literature

Heckins was able to come back to Rome only in 1614, but by 1610 the academy had been completely re-organized and it was both operational as it was recruiting new members. In 1610 was recruited into the academy Gianbattista della Porta, who is still remembered because of important discoveries in optics, but was also an alchemist and a Paracelsian magus. Della Porta wrote a famous treatise, originally in four books, but which by successive revisions grew to twenty and got its final title *Magiae naturalis libri viginti,* where one can find classic discoveries described side by side with curious magical practices.

Della Porta was quite influential on the philosopher Tommaso Campanella (1568-1639), on the magus Cornelius Agrippa and on the famous mathematician (but also a physician and magus) Girolamo Cardano. Della Porta strongly stressed the basic difference between 'natural magic' which is but the exploitation of natural forces, and therefore is beneficial, and black magic, which has recourse to evil spirits for criminal purposes.

In 1611 Galileo Galilei joined into the academy, then several others, both Italian and foreigners (for instance the French De Peiresch, whose extensive correspondence is, in some way, a parallel to the famous one of Pêre Mersenne) until the total number of 32 was reached in 1625.

Thanks to prince Cesi's money, the Academy published several immensely important books, such as the letters of Galileo on the solar spots (1613) and his *Saggiatore*. In the field of biology Stelluti produced the famous print of the bees, which is the first image of an insect drawn with the help of a microscope. But the most ambitious plan of the Academy was the *Mexican treasure*, which was to be an exhaustive description of the flora and fauna of Mexico, a work into which co-operated several academicians. A few trial copies were printed before the death of prince Cesi, but the final version was printed only in 1651.

Meantime, soon after the death of Cesi the Academy had collapsed, in spite of efforts by Stelluti, Cassiano del Pozzo (who bought the library) and a few others. The present Accademia dei Lincei is a revival of the middle 19th century.

Shortly after the demise of the Accademia dei Lincei, its place as a scientific institution, was taken by the Accademia del Cimento, established in Florence by Cardinal Leopoldo de' Medici. The cademicians included the grand-duke, himself a keen alchemist, but were mostly pupils of Galileo. Ordinary fellows ('Operatori') included people like Vincenzo Viviani, Francesco Redi, Lorenzo Magalotti, Giovanni Antonio Borelli and as 'correspondents' Nicholaus Steno, who, after a long stay in Florence, had returned to Denmark.

Most of the research activities of the academy concerned physics, but they include such basic contributions to biology as those by Francesco Redi.

Magalotti, in 1667, collected all the reports on the research done during ten years by the fellows of the academy (*Saggi di Naturali esperienze = Essays on experiments in Natural History*) and in the same year the academy was dissolved!

Meantime other academies had been born elsewhere on a more lasting basis.

The Royal Society of London was born around 1645 from regular, but private, meetings of scholars (the so-called 'invisible college', who, side by side with their scientific interests, enjoyed an innocuous opposition to Cromwell's dictatorship (but were spared the attentions of the 'Lord Protector' by his son-in-law, and later bishop, Wilkins, whom we shall mention again). In 1662, after the restoration of Charles II, the Society was chartered, got the name that it still bears, and started its official, glorious life, which was consistently linked with the progress of biology.

Among the earliest members of the Society were Newton (who was only reluctantly persuaded to join, and who soon became its president, and the Honorable Robert Boyle, that, besides being usually considered amongst the 'fathers' of true chemistry, performed, with the help of his pneumatic pump, several important experiments on respiration. Among the Society's early foreign fellows the outstanding biologists were Antoni van Leeuwenhoek and Marcello Malpighi. The Society, contrary to the French Academy, and in true English tradition, had for a long time a rather informal structure and membership was open not only to prominent scholars, but also to gifted amateurs. The Society started in 1665 the publication of the world famous 'Philosophical Transactions'.

In the same years several informal groups had existed in France, and in 1666, on the advice of Colbert (who, in turn, had been convinced by Claude Perrault), Louis XIV gave a charter to the 'Academie Royale des Sciences' as a parallel to the literary academy established by Cardinal de Richelieu (now Academie Française), thus granting official standing and support to an informal group of scientists, who had the habit of meeting at the home of Melchisedec Thevenot (1620-1692), a diplomat and a sponsor of sciences.

Among the earliest members of the Academie were, besides the obviously inevitable Perrault, Jean Pecquet and the Abbé Mariotte, who was not only a physicist (remember the Law of Boyle and Mariotte), but also a keen biologist.

As Colbert had planned the Academy as an instrument for the economic development of the country, the Academy immediately had statutes, financial support for research and salaries for the academicians.

Being good public servants of a state where the king proudly exclaimed 'L'État c'est moi' (= I am the state), the academicians were organised in a precise hierarchy, received a salary, met every Saturday, and were to work on plans outlined by Perrault. The Academy supplied the laboratories, the money to obtain the instruments and for the actual research, as well as it paid for the publication of results. Thus were produced the splendid 'Mémoires pour servir à l'histoire naturelle des animaux' (Paris, 1671-76), almost completely by Perrault and his helpers, and the parallel 'Memoires pour servir a l'histoire des plantes' developed by Dodart and, under the direction of Fontenelle, the regular publication of the 'Histoire et Mémoires de l'Academie Royale des Sciences'.

All this had, however, been preceded by the publication in 1665 of the weekly 'Journal des Savants'. This had been initiated by Denys de Sallo (1626-1669), well before the foundation of the Academy. The Journal was promptly killed by order of the government (March 1665), but its publication was resumed next year. The Journal is thus just slightly older than the Philosophical Transactions

The Academie itself was re-organised in 1699 and has since been the maximum forum of French science.

Finally we must record as especially important in the history of biology, besides the academies of London and Paris, those, albeit rather ephemeral, of Amsterdam and Copenhagen.

German states, soon followed the French example and, as they were trying to recover from the ravages of the 'thirty year's war', they stressed the practical purposes of their academies, but apparently did not achieve much.

In America an attempt was done by Increase Mather who organised at the end of 1600 the Boston Philosophical Society, which did not survive long. In fact it was Benjamin Franklin who, by organising 'Junto' (later American Philosophical Society), gave a real start to American Academies.

In the meantime the Universities supplied to their teachers only the classrooms and, at most, the 'anatomical theatre'. As experimental research was increasingly demanding in terms both of instruments and of room, most scholars were in a quandary: this is aptly exemplified in the preface to a book written in 1592 by Pierpaolo Simonetta, then professor of surgery and anatomy at the university of Pavia and formerly chief surgeon with the Spanish squadron at the battle of Lepanto: he complains that the university did not even refund the professor for the money spent for the animals used in the student's dissections!

Thus, as Academies could count either on the prince' contributions or on those of wealthy members, they became the promoters of scientific research.

On top of that all, one must always remember that the whole 17th century was scourged by wars, both international and internecine, that were either outright on religious issues or that had a religious background and this had its own impact on all aspects of scientific development.

As an example of the situation widespread in Europe I shall quote the story of the Academy of Science of Bologna. The events that I shall summarise occurred in the very last years of the 17th and in the early ones of the 18th centuries, but they are all the same typical of an almost general situation.

We have mentioned the crisis that during the 1600 spread through all sectors of Italian life and which struck the universities as well. But for Padua, where the Venetian government prohibited Paduan citizens from holding chairs at the university, the professor's recruitment had been increasingly local. Moreover the age old competition that monks offered for chairs had been increasing, as they could be satisfied with lower salaries because they were backed by the convent and had no family. So it happened that, favoured by the counter-reformation, monks, including Jesuits, had almost monopolised the chairs of philosophy. In several little towns such as Camerino, the university which had been established in 1370, practically died out, and all teaching was done in the convents, but also in the most famous faculties, the number of students decreased.

This is easily understandable: by now almost every state, albeit small, had got a university, which was adequate for the standard teaching, so that it was not worth while to spend all the money necessary to graduate abroad. Moreover many states even prohibited their citizens from studying abroad.

Coming back to Bologna; around 1690 every Bolognese citizen holding a doctorate, was entitled, on demand, to be registered as a professor at the University (usually without salary), therefore, while the total number of the students was around 500, there were some 150 professors. In principle every professor was supposed to give 100 lectures per year, but the total number of school-days, once holidays and other celebrations were excluded, was short of 100. Moreover, while in theory there should have been some 15,000 hours of lecturing, the available schoolrooms could not possibly hold more than 30 lectures per day, so that the very maximum of lectures that could be given was no more than 3,000. Moreover, through the 17th century the curricula remained unchanged and, obviously, they had become quite obsolete. Thus the best professors used to lecture at home for really keen students. Such unofficial courses were free from any official constraint and usually excellent.

Obviously there started a tug-of-war between logic, which demanded a reformation, and entrenched interests which resisted any change. So, gradually the number of professors was reduced to about 70, who actually lectured only when the students really demanded it. For instance Galvani (see next chapter) though he received his salary, yet was officially exempted from lecturing, and similar situations were not uncommon in all the European states.

The decadence was such by the end of the 17th century that Archdeacon Anton Felice Marsili, chancellor of the University (he had succeeded, after a short interlude to Rev. Alessandro Simonetta, 1600-1671) and a good naturalist in his own right as we shall see further on, had published in 1689 a list of the many abuses and illegal transactions which were common at the time: waste of resources, irregular grant of degrees and in the appointment of professors, absenteeism. His proposals for reformation were frustrated by the boycott of the vast majority of the professors (and among the leaders of the opposition to any reform one is surprised to find the great Malpighi!). Some years later the junior brother of Archdeacon Marsili, General Luigi Ferdinando Marsili, began his own campaign for correcting the situation. We shall deal further on also with the notable scientific merits, both as a naturalist and as a geographer of General Marsili; here we shall briefly describe his battle against the academic establishment. The general in 1709 made new proposals, which included the gift of his own library and collections, and these were promptly rejected by the academic corporation. But General Marsili recruited the support first of the pope Clemens XI and, even more important, that of the 'Cardinal Legato', that is the Cardinal-Governor of Bologna, Casoni and later of the next Legate, his good friend Prospero Lambertini (who was later pope Benedict XIV) and counter-attacked with the support also of a small group of dedicated professors, who had, on their own accord, established around 1690 the Accademia degli Inquieti. Thus in 1711 the general was able to establish an Accademia della Scienza dell'Istituto di Bologna (= Academy of Sciences of the Institute of Bologna). However, he soon begun to complain, with little justification, that the academicians were lax in their scientific endeavours. By its statutes, the Academy had to provide both lectures and demonstrations complementary to those of the official courses, but based on different methods and theories. General Marsili provided the new academy with a library and instruments, additional funds were later provided by Cardinal Lambertini, while the town and the University were bullied into providing housing and beginning the construction of an observatory. Thus the new institute was organised as a Museum-cum-teaching establishment. Slightly later, as a Pope, Lambertini established 24 salaries for professors and scholars, on the condition that the recipients of those salaries (Benedettini) produced at least one scientific paper per year (perhaps the first instance of the 'publish or perish' system)!

This story in fact reached its conclusion well into the 18th century, but such situations were so common that, for instance, when, during the 18th century, in Holland was mooted the proposal to establish a national academy, the University of Leiden protested alleging that since the French academy had been established, no one heard any more of the Sorbonne! In England up to the middle of the eighteenth century, with but rare exceptions (Newton can not be counted as it is proved that he went for lecturing punctiliously according the timetable, but, as usually there was no one to listen, he was soon back to his rooms and to his studies), both Oxford and Cambridge led an obscure life, their main merit, according some English historians, being their jealous battle in defence of their privileges and liberties against all attempts of the government to infringe onto them, and thus providing the premises for the great liberal achievements to follow.

Before we leave the subject of the Academies it is necessary to stress again how, during the 17th century almost all the main scholars mentioned in the later sections of this chapter were usually working within the framework of the Academies and their own work was surrounded and supported by the results of the researches of many other valid scholars to whom we owe a good deal of the many great and lesser discoveries of this century.

Museums

The 'Museum' (home of the Muses, home of learning) was properly, in classical times, that in Alexandria, which was linked with the Library. However the Museum never housed any collection, it was really a mere centre for studies and the collections, if they existed at all, must have been housed in the Library.

Nowadays, instead, Museums are basically conceived as collections assembled and preserved both for educational purposes or as study materials. The change in the meaning of the name is of little significance by itself, but important it is indeed the historical significance of Museums.

The tendency to collect objects of all sorts is general in mankind and ownership of certain objects is often and acknowledged 'Status Symbol': Such an accumulation of treasures, of works of art, of objects of any description or, even, of more or less authentic relics of saints has been always common practice (Charlemagne paid a lump sum for the foreskin of Christ!), but the advent of Humanism and later of Renaissance made books, works of art and antiquities the most treasured items, and proud owners more or less regularly exhibited them to some sort of public. Actually the first time the word 'Museum' is used in approximately the modern meaning is by the establishment of the Capitoline Museum by pope Sixtus IV in 1471, which was conceived for the purpose of assembling Roman antiquities.

Interest for natural objects, especially when they could appear 'curious' is as ancient as human records go, but gained enormous impetus during the 16th century by the flow of 'things never seen before' which came with every ship arriving from the distant lands reached by European shipping (and by the conquistadores). It was at this time that the Germans coined the word 'Wunderkammer' by which these 'proto-museums' are generally known nowadays, and any cultivated and sufficiently well-to-do gentleman in Europe was almost expected to own one of these. Actually, as we have seen in the previous chapter, the transition between the hodge-podge assemblage of all sorts of curios and museums in the modern sense was begun by Aldrovandi and his friends.

Several modern scholars who have dealt with the development of the earliest scientific museums have stressed the social background of their development and the supposed 'elitist' or snobbish attitude of their owners. I think that these authors often missed three important points: (i) they should have seen the early museums in the framework of the whole of the society and culture of their times. Being private enterprises, collections were just an expression of the personality of the collector, which, again was largely a product of his education; the choice of items collected, their arrangement and the selection of the persons welcome as visitors depending on the purposes of the owner. (ii) the record of visitors is biased: Palaces and gardens being the age's most obvious status-symbol and as a concealed status symbol is no status symbol at all, they were traditionally open to all sorts of visitors. However, common people, who were working from sunrise to sunset to scrape together a living, would but rarely knock on the door. Should one knock, he might be shown around by some lay servant and that would be the end of it, but should a gentleman or a scholar come, then politeness required that the house-lord himself or at least some special official was to show the guest around and that the visit was duly recorded (such records could always come of use). At least up to the Second World-War, ladies kept their albums at hand, and visitors were asked to sign them and be kind enough to write a few lines (and when going somewhere for the first time you had better to have in store a few kind maxims or verses in order to allow you to pencil an appropriate line onto your guest's albums). The second point, and this is really important for our purposes, Museums were usually conceived as a 'sylloge' or anyway as a material embodiment of the kind of work that the creator of the Museum either was writing about, or thinking of. They were thus ordered, and to a considerable extent still are, with an eye to that curious thing that was Mnemonic, on which we shall have much to say when dealing with the development of Systematics in the 17th-18th century.

The first true natural history museum was established by Ulisse Aldrovandi and was, in fact, a complement to the botanical gardens that Aldrovandi was planning and later established in Bologna. Aldrovandi's Museum was to some extent an improved version of a project that had been mooted in Ferrara, but that was abandoned when an earthquake so damaged the town that the money earmarked for the Museum, was diverted to the urgent needs of reconstruction. In fact Aldrovandi's museum was not only the first one in its planning, but was also the first real public museum, as Aldrovandi bequeathed his collections, archives and library to the town and, housed in the Town Hall, both were for a long time a sort of tourist attraction. A good deal of Aldrovandi's collections still survive, now housed by the University. Some specimens still remain also from other private collection of the early 17th century, such as those of Manfredo Settala in Milan, of Calzolari in Verona, of Imperato in Naples and of Kircher in Rome. The aspect of such 'proto-museums' is preserved for us by some plates in different books. Thus that of Ferrante Imperato, a rich apothecary in Naples, is illustrated in his book *Dell'istoria naturale libri XXVIII* (Naples, 1599), a would-be extensive 'reasoned catalogue' of minerals, fossils, animals and plants, but which is scientifically rather poor. Among the earliest and best known similar proto-museums in Europe is that of the Dane Ole Worm (1588-1654), a scholar who is still remembered because of the association of his name with the Wormian bones, supernumerary bones in the human skull.

So great was the significance of the Museums in the development of Biology, that we may still devote some further pages on them.

Aldrovandi was fully convinced of the potential significance of extensive collections for the advancement of science, thus for years he pestered all manner of potentates and especially the Spanish king to get the funds necessary to assemble a 'Universal Museum', but meantime he was spending all his money to collect as much material as possible, paying for the artists that were preparing the illustrations for the books which were based on the collections, the secretaries that filed his notes and correspondence. He established an incredible network of hundreds of 'pen-friends', ranging from Princes, such as the Grand-duke of Tuscany, to obscure provincial amateurs, who were all engaged in exchanging information, specimens, drawings, etc.; in fact a surprising parallel with the modern 'global village' of computer networks!

Another illuminating story is that of the Jardin des Plantes and Muséum d'Histoire Naturelle of Paris.

Actually the first French botanical garden was that of Montpellier, which, for a while, was directed by a good scholar, Richard De Belleval (1564-1632), but that was destroyed during the siege of the town in 1662.

In Paris the Protomedicus (first physician) of the king, Jean Heroard de Vaugrigneuse, and Guy De la Brosse (1586?-1640), also a royal physician, urged the King to establish a botanical garden of medicinal plants. That took time, but, finally, king Louis XIII, on 6 January 1626, issued the first charter for the establishment of a 'Jardin royal des plantes médicinales'. However, as most physicians of the king were Paracelsians and the new establishment was planned also as a teaching establishment, the Medical faculty promptly began obstructing the project. Thus it was only in 1635, nine years later, that the Parliament of Paris ratified the appointment of Guy De la Brosse as the first 'Intendent', that is director. The Royal charters had determined that, whereas not granting official degrees, the Jardin's personnel was to provide public teaching of botany and of medical preparations and later (1643) also of anatomy, entirely free of charges and to be given in French. Yet the medical faculty of the Sorbonne, led by Gui Patin (1601-1672), dean of the Faculty and whom we shall meet again battling against Harvey, initially strenuously opposed the beginning of the courses, then tried to control the appointments of the personnel. Thus the first years of the new establishment were a never-ending battle against the Medical Faculty, which was such a stronghold of traditionalism that already François I, had established the 'Collège de France' as an alternative to its blind conservatism.

Some 10 years of almost complete eclipse followed the death of De la Brosse. Finally the Minister Antoine Vallot (1594-1671), in 1654 appointed Denis Jonquet to the Jardin and afterwards a great-grand-son of De la Brosse. Guy-Crescent Fagon (1639-1718), a naturalist of value, a man of both genius and great culture. Fagon was appointed as 'Intendent' in 1665, and he had the great merit of assembling at the Jardin several good naturalists, mainly, again, Paracelsians. Chief among them were the botanist Joseph Pitton de Tournefort (1656-1709) and Étiènne François Geoffroy, who belonged to a lineage of apothecaries going back to the first years of the 16th century and an ancestor to his homonym Étiènne Geoffroy St. Hilaire, the famous friend-enemy of Cuvier: In his late years, finally, Tournefort appointed Antoine-Laurent de Jussieau to the staff of the Jardin, although he was then still almost a boy.

Étiènne Geoffroy has only an indirect significance for Biology, as he was a chemist, but he was the leading figure at the Jardin in turning chemistry from the esoteric clouds of typical alchemy into a plain strictly scientific enterprise.

Fagon gave the Jardin a vigorous development, so that, just after his death, its name was changed first into Jardin royal des Plantes, and then to Jardin du Roy. It was just after Fagon's times that the 'Droguerie' of the Jardin, became the 'Cabinet d'Histoire Naturelle'.

The development of the Jardin, finally freed from all boycott, was continued under the direction of Charles-François de Cisternay du Fay (1732-1738), when a number of other excellent naturalists joined the staff (foremost the brothers De Jussieau) and reached its apogee under the long leadership of Buffon, who completely re-organised it. But an account of Buffon and his times will take a good share of the next chapter.

As museums owned by institutions rather than by private collectors, both Paris and London are antedated by two Jesuit institutions. In Rome the Jesuits created the

'Museum of the Roman College', which is also known as 'Kircherian Museum' after the name of its first organiser, Father Athanasius Kircher, a notable figure whom we shall mention again, and the Jesuit Museum of Vienna. Both were conceived as joint institutions with the Jesuit colleges. A number of specimens from the Kircherian Museum still exist, scattered amongst different museums in Rome, but mainly at the Ethnographic Pigorini Museum.

In London the Royal Society soon began to assemble collections, which later, after the purchase by an Act of Parliament of the famous collections of Sir Hans Sloane in 1759, merged with it and became the original nucleus of the British Museum.

A great advance in the preservation of natural history specimens was made by the usage of alcohol as a preserving medium, recommended chiefly by the Honorable Robert Boyle (1663). Thus delicate specimens which were seriously damaged by the previous practices of desiccation, could be stored properly for further study. Another practice introduced in this century, probably by Severino, was the injection of fluids in cavities. It was used by many scholars and reached a sort of peak by Frederik Ruysch.

Actually Museums gradually became almost a fashion. By 1704 D.M.B. Valentini in his *Museum Museorum* could list 159 of them and they have since played a key role in the development of sciences.

The development of botanical and zoological systematics

The close connections that occur between the development of museums and the development of sytematics make it advisable to discuss the evolution of systematics before other aspects of 17th century biology. This in spite of the fact that the achievements in this field were much less spectacular than the advances in some others.

Throughout this century the development of explorations and of trade contributed a steady and increasing flow of new evidence, while scholars at home were increasingly at pains to organise it.

John Johnston was a Scot by origin, but was born in Poland (1603-1675). He made extensive travels and finally settled in Silesia, where he died. He followed rather faithfully in the steps of Aldrovandi, from whom he borrowed much of his evidence. His several volumes on animals, published between 1650 and 1665, are practically mere compilations, but he introduced some improvements on the order followed by Aldrovandi. Actually his works were well received, went through several editions and translations and were still judged worth republishing by H. Ruysch in Holland in 1718. It is important, in order to understand the biological compilation by Johnston, that these were part of a general survey of all recent progress in the different sciences, seen in the framework of a general theological view of the World. Johnston thought that in his time great advances had been made towards a reliable 'scientific prophecy'!

In some ways his outlook resembles, but with a much stronger Paracelsian tinge, to that of Sir Francis Bacon.

The already mentioned Clusius assembled much new information on both exotic animals and plants, that he published in his *Rariorum plantarum historia* (Antwerp, 1601) and *Exoticorum libri decem* (Antwerp, 1605). There he synthesised both the information already available with new data on animals and plants both from the East and from the West Indies that he had gathered directly from both naturalists and voyagers. His *Exoticorum Libri decem* had a considerable influence. His figures are mostly derived from Gesner or from Aldrovandi and are generally good. Among the new species described and figured, is the Dodo, the big, flightless columbid that the Dutch discovered in Mauritius, when they first settled there in 1598 and that, within a century was 'dead as the Dodo'. As we mentioned Clusius was also the founder to the botanical gardens in Leyden, in the Netherlands and some of the plants he planted there are still thriving.

While already in the Middle Ages the Arab pharmacopeiae had made Europeans familiar with a number of oriental plants, during the 17th century there was a real flood of information and specimens of new organisms and many scholars, scattered all over the new institutions which were burgeoning everywhere, were at pains to describe them. Accordingly it is both tedious and hopeless to try to list them. Only three or four will be mentioned as examples.

In the 17th century insects begun to attract a growing interest and were the subject of a number of books, thus we may mention as an especially attractive personality Sibylla Merian (1647-1717). She was a Dutch, grand-daughter, daughter, sister, wife and mother of famous engravers. She specialised, through her whole life in illustrating insects with, as a complement or background, plants and other animals. In 1698, when already 51 she went for two years to Surinam or Dutch Guyana, to study its animals, and especially its insects. Thence she prepared a basic book, which she published in 1705. While some of her books were published in the 17th century, some were published posthumously in the 18th and a few even as late as 1986.

The development of trading stations overseas and the establishment of the earliest little colonies prompted both governments and trading companies to encourage the collection of information and specimens of fauna, flora and minerals by their officers abroad. The Dutch, in spite of the long war for independence against Spain, were off to an early start. Indeed as soon as the Spaniards were finally chased from the United Provinces, the Netherlands went through a true economic boom. Dutch independence was formally acknowledged by the Treaty of Westphalia in 1648 and already in 1652 Van Riebeck founded Cape Town as a refitting station for the Dutch convoys sailing to Indonesia and Ceylon. Thus the Dutch East India Company was among the foremost in prompting its representatives overseas to collect both information and materials about any sort of natural production of their stations and the result was the publication of several local faunas and floras. Among these the work of Georg Eberhard Rumpf (Rumphius) (1627-1702) while stationed in Amboina is especially important. Nothing remains either of his private collection, destroyed in an earthquake, or of those sent to Holland, lost in a shipwreck, but he also sold a collection to the Grand-duke of Tuscany and some specimens survive in the museums of Florence and Pisa. Rumph's book on the natural history of Amboina is a splendid work and many of Linnaeus species are actually based on Rumph's descriptions. Georg Markgraf (1610-1644) in Brazil made the equivalent of Rumph's work in Indonesia.

Thus scholars were increasingly faced with two main problems: the first was how to catalogue all these organisms so as to retrieve easily the information available (and this was a main stimulus for the development of systematics), while the second was a theological one: could these new organisms, men included, from distant and isolated lands, be, perhaps, the result of a creation different from that reported in the Bible? How could they, after being chased from the Earthly Paradise have reached these distant places? This was a crucial problem for famous thinkers, like Tommaso Campanella.

So far as scholars were dealing only with local faunas and floras, though with some difficulties, given the great variety of organisms discovered, and especially of birds and insects, yet the traditional systems might still be used, but it was clearly impossible to use them any more in general works. Indeed Gesner, as we said, was the last one to stick by the Plinian tradition when ordering the animals in his treatise; and even he made some changes when dealing with birds, by grouping some as the ducks and the falcons, without considering either alphabetic order or size. Subject indexes had already appeared in books of history by the middle 16th century, and are increasingly used during the 17th century, but this was clearly an inadequate solution.

Moreover scholars were groping with another theoretical problem.

In the chapter on the history of higher education, we have seen how important it was for the student his proficiency in memorising information. On the other side scholastic nominalism had shown the limits and faults of common language. Thus scholars asked themselves whether it was possible to develop a completely objective and unambiguous language and link it both with the possibility of developing memory as well as of uniting all knowledge into a single synthesis.

Such problems had been debated since Lullus and we cannot deal with their manifold consequences, sufficit to remember that no one less than Leibniz, who spent a good deal of time and energy on these problems, listed also Spinoza, Borelli, Descartes as prominent scholars in the field of mnemonics.

One of the most popular techniques used to help memorising information, was to arrange a sort of walk or progress, through a set of mental associations between images and ideas. As we said in the preceding section on Museums, it was obvious that the arrangement of the collections must mirror a logical argument or narrative, such as it can be found in a book summarising and updating the available information. Thus the visitor could build a reciprocal connection between the object seen and the information read, each one helping the other in building retrievable knowledge. It was also assumed that it was desirable to offer such an arrangement that could suit several alternative intellectual itineraries. In fact Leibniz created for the purpose some odd neologisms: 'com2nations' = combinations, 'con3nations'= contrinations and so on, according the number of elements that formed the main axis of an argument.

This implied that the evidence offered should be suitable for visual association and, on one side prompted the common usage of the term *Theatrum* (theatre) for books which embodied such arrangement of evidence as suitable to be 'shown', on the other made external characters preferable for categorising the evidence.

This goes far to explain the choices of Aldrovandi and later scholars, who, to begin with, implemented and improved on Aristotle's classification or Alberto's *Scala Naturae*. Seen with our eyes, familiar with evolutionary theory, it is easy to understand how such morphologic evidence as "all mammals with horns have a cloven hoof and no upper incisors", or even supposedly physico-psychic traits such as: the simplest Invertebrates have only a vegetative soul, Mammals have an appetitive, and possibly in some species, even a rational soul, will naturally lead to group the species into approximately natural groupings.

Even a cursory consideration shows that, while the study of mnemonics is currently considered as an historical curiosity, and after Leibniz combinatory has taken a very different path, contributing to the development of mathematical logic and of computer science, yet modern Museums still follow the outline of a visible discourse. It is equally clear that the effort implied by the 16th-17th centuries approach was to lead towards modern systematic.

As we already said, one of the earliest schemes for the classification of plants was proposed by prince Federico Cesi, who had in a way foretold some later principles of classification in his *Tabulae phytosophicae.* Though Cesi had a clear idea of plant sexuality well before Camerarius and had suggested some advanced criteria for plant classification, his results were by and large ignored.

Attempts to improve the systematic of plants into a comprehensive framework were made by Gaspard Bahuin (*Pynax theatri botanici)*, by Rivinus (1652-1723) and by Johachim Jung (Jungius) from Lubeck (1587-1657). Jungius studied in Padua and later taught in several German Universities. Both his important contributions; *Dixoscopiae,* 1662, and *Isagoge phytoscopica*, 1679, were published after his death and passed practically unnoticed. However one of his unpublished manuscripts happened to be seen by Ray, who was much impressed. Hence, because of Ray, Linnaeus became interested in Jungius.

In fact Jungius recognised and described some families (Compositae, Labiatae, Leguminosae), he also proposed several terms which are still in usage, Jungius also proposed a theory of flowers that foreshadows the traditional one that the various parts of the flower are nothing but modified leaves. Finally, Jungius was an 'ancestor' of binomial nomenclature.

Undoubtedly the most important systematist of the 17th century is John Ray (who originally spelt his name as Wray). Ray was born in 1627 or 28 at Black Notley, in Essex. His father was a blacksmith, but one with a reasonably flourishing trade. So, young John could attend University partly thanks to what we would now call an additional scholarship. John graduated in Cambridge and became a fellow of Trinity College (which then meant that he had a sort of scholarship for life, without the obligation to do any work); he joined the clergy and, though somewhat late in life, was ordained a parson. However, when Parliament, under pressure from Charles II, ordered to swear to the Uniformity Act, Ray renounced his fellowship rather than submitting. Luckily for him, during his years as at Cambridge, he had struck up a friendship with a rich gentleman and an amateur naturalists: Francis Willughby (1635-1672), who officially hired him as a tutor for his children, but in fact made him a scientific collaborator. The two of them travelled extensively through Europe, both to visit interesting places (*e.g.* Ray collected fossils at various localities in Italy and Germany) and to meet important scientists (and Ray was thus in touch with many of the major scientists of Europe) and prepared a project for a great general revision of both the animal and plant kingdoms. According the original plan Ray was to do the plants and Willughby the animals. This project came to match a project by John Wilkins, later Bishop of Chester, a good friend of both, a student of mnemonics, a pioneer in both linguistic and semiotic studies (in youth Wilkins' abilities in combinational, had made him one of the main cryptographers in the British Secret Service under his father-in-law Oliver Cromwell, and he had freely availed himself of his exceptional position to shield his friends of the 'invisible College' from the attentions of the Lord Protector's police. His consummate political abilities got him a bishopric under Charles II, in spite of his close relation with the Lord Protector and of having, at the restoration, lost his Mastership at Cambridge.

Wilkins was trying to build a 'universal language' based on rigorous concepts of logic and combinatorial mathematics. So, Ray and Willughby prepared for Wilkins an outline of classification based on a rigorous implementation of divisional logic. Both of them, however were unhappy with the results. Meantime Willughby died when aged only 37, and bequeathed to Ray a small pension as one of his executors and left him in charge of the education of his sons. The widow of Willughby thence urged Ray to complete and edit her husband's planned works on animals. After spending some more years at Willughby's mansion, Ray, having married, retired to his native home, where he looked after his family and continued to study until his death in 1705.

Ray had soon attained a good reputation and had been made a Fellow of the Royal Society, who offered him the appointment as Secretary, which he refused, as he refused any appointment that could take him from his studies. His well balanced judgement, his considered care of the sentiments and prejudices of everyone, earned him always not only respect, but he was genuinely liked by almost all who had to deal with him.

Ray was primarily a botanist and his first book, *Catalogus plantarum circa Cantabrigiam nascentium*, is just a regional flora. However, when Willughby died, he began work also on animals and botanical and zoological works were published somewhat alternately: *Francisci Willughby Ornithologiae libri tres* (1676), *Methodus Plantarum Novum* (1682), *De historia piscium libri quatuor* (1686), *Historia Plantarum* (published in several volumes between 1686 and 1704), *Synopsis methodica animalium quadrupedum et serpentini generis* (1693), *Synopsis methodica stirpium britannicarum* (1696), and the posthumous *Historia insectorum* (1710) which was printed from Ray's notes just as he left then and thus is rather in the form of a draft.

The books on birds and fishes (which, as it was traditional, included the cetaceans) were published under the name of Willughby and, probably, Ray's contribution was the general lay-out within which he edited the copious notes and drafts of his friend.

Ray was a born systematist and an open minded one. In his groupings of plants, which he gradually evolved in his successive contributions, he took full account of the criteria suggested as a basis for classification both by Jungius and by Cesalpino. Curiously, as both his books and his correspondence testify to a singularly complete knowledge of international scientific literature, he was not aware of the work of Camerarius on the sexuality of plants, though he quotes other works of this same author. Anyway, both in his published works and in his correspondence, Ray was fairly certain that sex was to be expected in plants, even if not yet proven. He was also fully up to date as far as plant anatomy is concerned and he closely followed Malpighi (see further on).

Ray's systematic of plants was a definite improvement on past classification, both formally, by his methodical way of describing genera and species, and as he defined several good natural groups. His genera, however are extremely comprehensive and correspond, in modern taxinomy, rather with families and even orders, and, as may well be expected, some are a mix-ups of the most heterogeneous things, grouped by just some superficial likeness.

Ray definitely believed in the immutability of species, as these had been created by God, who had accomplished his work at the end of the sixth day. However he admitted for some plants a limited 'degeneration', which could turn some good plants into weeds. It was indeed difficult for him to believe that a benevolent God could have created weeds! In his correspondence and in two rare books he discusses at length the nature of fossils. These are: *Miscellaneous Discourses concerning the Dissolution and Changes of the World* (1692), reprinted with amendments in the same year with the new title *Physical-theological Discourses* (1692), and *The wisdom of God manifested in the Works of Creation* (1691), an encyclopaedic work, which had four edition in Ray's lifetime and which he regularly enlarged, so that it passed, from the 249 pages of first the edition of 1691 to 464 in the fourth, that 1704, which is the one that had several reprints through a century and a half. This book had a great influence on English scholars through the following century and here, as in other works, Ray acknowledges his indebtedness to the Cambridge Neoplatonists, to the Paracelsians and to thinkers like Ficino, Curdano etc. Ray was fully persuaded of the importance of fossils and, after carefully weighting the pros and cons for their organic origin (he quotes with praise, Steno, whom he had met in Montpellier), he declares most of them to be the actual remains of plants and animals. However he is worried by the problem of reconciling their evidence, assuming their origin from once living beings, with the Bible's accounts of creation and of its subsequent history; so he occasionally wavers and is tempted to agree with his friend Martin Lister.

Incidentally, another British botanist contemporary with Ray, and worth remembering, mainly because of his fascinating personality, was William Dampier (1652-1715). He is nowadays mainly remembered as a privateer, which he was for most of his life when not in command of some of H.M.S. in charge of explorations by the Admiralty! His contributions to the exploration of the coasts of newly discovered Australia and of the islands of the South Seas are truly remarkable, but it is notable that throughout his life, even under the most difficult situations, he unceasingly collected and accurately described plants, so that Linnaeus gave his name to various genera and species.

Another important botanist contemporary with Ray is the already mentioned French, Joseph Pitton de Tournefort (1656-1708). He had an extremely keen eye for affinities, so that his groupings are usually quite good, albeit based on a rather superficial analysis of characters. His most famous book, *Institutiones rei herbariae* (1700) is significant as it specially stresses the employment of the concept of 'genus' to group closely related taxa.

Thus, in botany at least, the concepts of Family, Genus and Species had been, by the end of the century, extensively discussed and were used in a markedly different way from that of Aristotle and rather akin to that of Plato's 'eideia', and were quite close to the ideas of Linnaeus. Even the adoption of a binomial system of nomenclature had been suggested by Augustus Quirinus Rivinus (the translation of his German family name Bachmann) (1652-1723), a German from a distinguished family of scholars, who, however in his *Ordo plantarum* suggest to base all classification on the sole evidence of the flower's corolla.

Anyway it is clear, when taking a close look at the classifications used by 17th century botanists, that they basically followed the example of Camerarius: they first grouped such plants that they intuitively thought to be more akin to each other, and then, when writing their books, they looked for such characters as could be used in order to reach identifications by following in the steps of Aristotelean logic (which Aristotle himself did not consistently follow in his biological works), so as to pass from a more comprehensive to a less comprehensive group.

Zoological systematics were not much improved by the scholars of the 17th century: whereas, as we shall see, both morphology and physiology made spectacular advances. Aristotle's systematic framework, being the result of the exceptional qualities of the Master, was still reasonably satisfactory, at least as far as Vertebrates were concerned. The following table, summarising the classification of Mammals used by Ray, clearly shows how it was just an improvement on Aristotle.

Table

- Ungulates
  - Solipedes (Horses, asses)
  - Bisulci
    - Ruminants
      - With perennial horns (Cattle, Goats, etc.)
      - With deciduous horns (Deers)
    - non-Ruminants (Pigs)
  - Quadrisulci (Rhinoceros, Hyppopotamus)
- Unguicolata
  - with two toes (Camels)
  - with 5 toes
    - Fused (Elephants)
    - separated
      - narrow nails
        - More than 2 incisor teeth (Carnivores)
        - only 2 incisor teeth (Mice, Rabbits)
      - broad nails (Monkeys, Man)

The advances in physiology and anatomy during the 17th century: William Harvey

The most momentous discovery in the biology of this century, as it was to pave the way for a complete revision of all physiology, was the description of the double circulation of the blood by William Harvey, who also contributed significantly to the development of embryology.

William Harvey was born in Folkestone in 1578 to a well-to-do family of Freemen of the Five Ports (in the niceties of British heraldry thus conferring a semi-noble status, which entitled them to a coat of arms, but not to peerage). At 16 he matriculated at the University of Cambridge and was a pupil at Caius College. Having got his Bachelor's degree of Arts in 1597, he went to Padua, thence considered the best in Europe for medical studies (Fabricius was teaching Anatomy and surgery and Galileo Mathematics). It is not certain when he reached Padua, but he was certainly a student there from 1599 to 1602 and got his medical Doctorate. Then he returned to England and settled in London, married the daughter of one of the physicians of Queen Elisabeth and of James I and was received in the medical guild. He was quite successful as a physician (we have mentioned that Bacon was one of his regular patients).

However, it is also certain that some of his colleagues did not think much of him as a practitioner, though none had doubts on his abilities both as a teacher and as an anatomist), In 1615 he was appointed as Lumleian lecturer of Anatomy and surgery to the Royal College of Physicians. At the time he had already developed all the essentials of his theory on circulation, as it appears from manuscript notes that he expounded it for the first time on April 16, 1616. However he delayed the publication of his studies for another twelve years. In 1618 he was appointed among the physicians of king James I and then of Charles I. When the civil war broke out, he stood by the king and retired to Oxford, where he got an appointment. He died there in 1657, having retired after the final defeat of the king.

When Harvey was in Padua he became interested in the debate on blood circulation and he resumed experimenting on it after his return to England. As we said, he had got at least the essentials of his theory by 1616, but he published his results only in 1628, in a pamphlet of but 72 pages (*Exercitatio anatomica de motu cordis et sanguinis)* published in Frankfurt. The choice of Frankfurt may look strange, but it may be explained by two considerations: first Frankfurt's fair was the most important market of Europe for books, so that publication there went far to ensure the best diffusion for the booklet. Second: there are reports that when news of the new theory spread to England several people thought that he had gone crazy and that he then lost a number of clients. As it might well be expected that public opinion would be ill at ease with the new theory, Harvey may well have thought it wise to delay such a reaction by publishing late and abroad.

Harvey, as a preparation for his booklet, made a number of experiments and dissections, including dissections of invertebrates, such as land-snails and crayfishes, both of which have a pulsating organ. He correctly concluded that the heart is not a double function pump, as required by Galen's theory, but it is a purely force pump. This implied that Galen's theory could not stand. Harvey than proceeds to prove his theory both by different experiments and by arguing the following three basic propositions: (i) the blood is pushed by the heart's contraction and flows continuously through the heart and the arteries in such amounts "*ut ab absuntis suppeditari non possit, et adeo ut tota massa brevi tempore illinc pertranseat",* that is, the amount of blood that passes through the heart and vessels is disproportionately greater than expected by the traditional theory; as such an amount could not possibly be produced by the food ingested and a simple calculation shows that it is the same blood that flows through the heart many times a day, (ii) the blood pushed by the arterial pulse enters into each part of the body "*majori copia multo quam nutritioni necessario sit vel tota massa suppeditari possit"*, that is: the amount of blood that daily reaches the tissues is much more than the total amount of blood in the body and, apparently, exceeds the amount necessary for the nutrition of the tissues; (iii) *"Ab uno quoque membro ipsas venas hunc sanguinem perpetito retroducere ad cordis locum,* which means that the blood returns to the heart only by way of the veins.

The proof of the first proposition is the key and the most original part of Harvey's work. He measured the total amount of blood pumped into the arteries by each pulse, that is the difference between the capacity in Man of the inflated and of the contracted ventricles, which he found to be two ounces, if the heart pulses 72 times per minute, in an hour there must pass through the heart some 540 pounds of blood, which is a good 3 times the total weight of the body of a normal man. As such an amount cannot come from nowhere or disappear afterwards, it follows that the only possibility is that it is the same blood that continuously circulates by the action of the cardiac pump. As for the other two propositions, the proof is by and large similar to that proposed by Cesalpino and as the consequence of the correct interpretation of the function of the valves in the veins: by strongly binding an arm its arteries swell proximal to the ligature, while they become empty and cease to pulse distally. When, instead the binding is looser, it is the turn of the veins to swell, but that occurs in the distal part of the arm, while proximally the veins become empty. This proves that in the arteries the blood runs from the heart to the organs and it comes back by the veins. The experiment shows that the valves in the veins function to prevent a backflow of the blood. Fabricii's experiments with a loose binding of the veins, if correctly understood gave precisely the needed proof!

Thus Harvey elegantly proved that the blood is continuously circulating through both the little and the great circle.

As expected by Harvey many physicians, including a few quite good ones, strongly opposed his theory and some took time to be persuaded. Besides exploding a cornerstone of traditional physiology it appeared as the whole mechanism would keep the blood flowing round and round for no apparent purpose, an apparently absurd thing for a provident and purposeful nature to do!

We shall mention but a couple of Harvey's critics: such were the Milanese Giovanni Della Torre, Caspar Hoffmann of Altdorf, and even John Vesling, then teaching in Padua, took time to be persuaded. The stronghold of opposition to the novel theory, anyway, was, as it could be well expected, the medical faculty of Paris. Its Dean, the same Gui Patin whom we met battling against the Jardin, and who, later, was to rage against Van Helmont, declared circulation "paradoxical, impossible, inintelligent, absurd and noxious to man's life". Jean Riolan junior (1580-1657), son of a good anatomist by the same name, took the field for "the ancient and true medicine" and argued that, should Harvey be right, the liver's function instead of being that of making blood, would be simply to divide blood from bile, but then we should change a lot of ideas on physiology and think that Hippocrates and even Aristotle had been wrong! How impossible! However Riolan had made a good point in his pamphlet noting that the new theory, anyway, failed to explain the different colour of blood in the arteries and in the veins and Harvey had to concede it in his reply: Harvey had not seen the passage of the blood from arteries to veins. That was to be done by Malpighi in 1661.

An especially strange position was that taken by René Descartes (Cartesius), and this goes far in showing how amateurish he was in matters of biology: Descartes subscribed to the circulation, as Harvey's mathematics were as simple as faultless, but he maintained that it was not the heart's beat that kept the blood flowing, as the heart could not possibly be a muscle!! Cartesius was a master at failing to appreciate the main advances of contemporary sciences: he did not believe in the cynamatics of Galileo and declared Kepler's laws impossible!

Indeed Harvey's discovery had far reaching implications. It complemented the new anatomy of Vesalius and his followers and absolutely demanded a completely new physiology.

Thus Harvey's discovery created many more problems than it apparently solved. As the evidence on which to build a new physiology simply was not available, it is understandable that many scholars were reluctant to sail such uncharted seas and unfathomable depths.

In fact a real understanding of the physiology of circulation depends on an understanding of the chemistry of respiration, which was practically impossible at the time (though some Paracelsian alchemist-physiologists somehow got close to it). Moreover biologists were saddled with the problem of the liver's functions, once it was demoted from chief hematopoietic organ (the poor Galenists could not know that, indeed, the liver *is* an important hematopoietic organ during embryonic life). Thus it was necessary to discover the true physiology of digestion. As far as hematopoiesis is concerned, the solution of the mystery depended on a good cellular theory, which was still two centuries in the future.

Though dealing with an entirely different branch of biology, Harvey's researches in embryology deserve to be discussed here, as they greatly enlighten his scientific personality. Again as the result of many years of researches, in 1651 Harvey published *Exercitationes de generatione animalium*. This book gives a notable account of the development of Mammals, while for birds and other animals it does not add anything of significance to the observations of earlier scholars and especially of Fabricius, whom Harvey duly quotes. Among the Mammals he had especially studied deers. His book is important as there Harvey not only maintains that the development of all animals is gradual and occurs by progressive additions and structural improvements, faithfully following in the steps of Aristotle, but he also resolutely states his persuasion that even in animals, such as mammals, in which no egg is visible, yet eggs must exist. He is sure that even in man eggs do really exist, even if he had been unable to find them. Nevertheless, again following Aristotle, he admits that some of the lower animals may be generated by *generatio aequivoca.* Though certainly not as momentous as the booklet on circulation, Harvey's book on the reproduction of animals does not deserve the comparative neglect which befell it. In fact it complements his main discovery by showing how whenever he thought that the evidence required a new hypothesis, he was ready to advance it, even if he could not completely substantiate it, while failing such new evidence there was no reason to abandon the more traditional theories.

Harvey is in many ways a sort of transitional figure: he was definitely an Aristotelean, but he was ready to correct and improve the tradition whenever new evidence required, just as all the best scholars of the age, who, while considering with great veneration the classical tradition, were excellent observers.

Throughout the 17th century there was a growing demand for rigorous verification of scientific hypotheses. Meanwhile the scientists strove to frame scientific debates within the framework of logically well chained arguments supported by positive evidence, almost all of them were pious people who were sincerely anxious to reach a unified picture of the world, where the Word of the Sacred Books, its metaphysic interpretation and scientific theories should support each other.

Nevertheless the great complexity of biological phenomena, in which, as we now know well, historical events always played a great role, has always put ever new stumbling blocks on the way of the development of rigorously deterministic models of the biologic world. So again and again such 'final solutions' maintained by famous scholars were proved wrong. At least to some extent, the ideal of turning biology into an entirely rigorous science still eludes us. We are moreover faced by some developments of modern mathematics and physics, that indicate that with the increasing complexity of any system, and biological systems are by far the most complex known, there creeps in an amount of indetermination, where stochastic events have a considerable play.

Quite often the scholars of the 16th-17th centuries were hardly aware of all the implications of their work. Probably some were so busy with their current work, that they lacked the time to consider the niceties of purely theoretic problems. Others wisely considered that times were not yet ripe to go into such problems on the scanty evidence then available.

Great as were the problems raised by Harvey's theory of circulation, evidence supporting it soon begun to accrue.

Richard Lower (1631-1691) must have been an extremely bold physician: he was the first to attempt the transfusion of blood between men and even from sheep to man. As everything went right with his first attempt in 1667, he became enthusiastic of the new method and probably killed some more than he saved, but having been promptly imitated by others (for instance by a certain M.G. Purrmann, 1648-1721) one can guess the results. Anyway Lower made some valuable studies on pulmonary circulation and, in 1665, he described the change in colour of the blood when aereated. The same observation had been made slightly earlier by Carlo Fracassati (1630-1672), a Bolognese professor. Both Lower and his contemporary George Ent (1604-1689) thought that such changes were due to a 'nitrous' part of the blood, whose function was to preserve life. This was in line with some contemporary alchemists, who were working on the hypothesis of a 'nitrous-aerial spirit' having a respiratory function (see pp. 235, 304).

Further anatomical advances which were either contemporary or that soon followed complemented Harvey's discovery. The posthumous booklet by Gaspare Aselli *De lactibus, sive de lacteis venis* had been printed in Milan in 1627, one year before the publication of Harvey's *Exercitatio*. Gaspare Aselli (1581-1625), from Cremona, was first a military surgeon, then a physician in Milan, and finally a professor in Pavia. He discovered the chylous vessels when, during the dissection of a dog which had recently been fed, found that the mesenteries were crossed by vessels coming from the intestine and stuffed with a sort of milky liquid, the chyle. Aselli gave a good description of the mesenterial chylous vessels and understood that they, and not the veins carried the digested food, but Aselli was mistaken in assuming, by following the Galenic theory, that these vessels had to carry the food to the liver. The *De lactibus …* is the only printed work by Aselli, other studies of his remained as mere drafts. It was Johannes Vesling, who had moved from Paris to Padua, who provided a really adequate description of the chylous system. Shortly afterwards (1647), and again in the dog, Jean Pecquet, a former student of Montpellier, discovered the subsequently named cistern of Pecquet and the thoracic duct by which the chyle reaches the succlavian vein and is discharged directly into the general circulation. This was the only discovery of Pecquet, who shortly afterwards became the personal physician of Fouquet. Fouquet was the powerful minister (intendent general) of finances of Louis XIV and Pecquet was an advocate of the medical powers of brandy, finally to vanish when his master was jailed for life by order of the king.

Pecquet's discovery was published only in 1651. In the same year a young student from Uppsala, the Swede Olof Rudbeck senior (1630-1702) distinguished the lymphatic from the chylous vessels and suggested the name *vasa serosa* for the lymphatics. He identified the lymphatics in a number of organs, recognised the lymphatic glands and studied the nature of the lymph. His discoveries were published as a dissertation in 1653. Rudbeck was later appointed as a professor in Uppsala and was the first there to make public dissections of human corpses. As we shall see in the next chapter, however, still in Linnaeus' times, when Rudbeck junior was a professor, public 'anatomies' were so rare that Linnaeus had to travel to Stockholm to see one. Rudbeck was also interested in botany and, late in his life, in a rather forlorn historical-archaeological project aimed to show that Sweden was the most ancient civilised country in the world and that Uppsala stood where the mythic Atlantis was. As most of his unpublished notes were destroyed in the great fire that almost razed Uppsala late in his times, apart from his already mentioned early studies, his lasting merit rest in his long and strenuous battles with the faculty to update medical teaching.

In the same year 1653 that Rudbeck published his researches on the lymphatic system the Dane Thomas Bartholinus (Bertelsen) (1616-1680), who was son of the Copenhagen anatomist Caspar, and who had studied in Leyden, in Naples with Severino, in Padua and had finally got his doctorate in Basel, described, independently of Pecquet, the thoracic duct and gave good descriptions of the lymphatic system.

Thomas Bartholinus was a remarkable teacher, but, possibly because of his chronic bad health, apparently his anatomical investigations were largely dependent on the ability of his sector, his former student, the German Michael Lyser, and, after Lyser's departure to take a chair in Leipzig (where he did nothing notable), Bartholinus completely ceased all personal anatomical research. In 1660 he obtained dispensation from all teaching obligations and spent the rest of his life in elaborating on other peoples' researches, antiquarian studies, extolling his own merits and securing appointments for his relatives and protégés!

Thus by 1661 practically the whole circulatory system was adequately known, as in that year Malpighi was able, thanks to the microscope, to see the passage of blood from the arterioles to the veins through the capillaries in the lungs of the frog and in the tail of tadpoles.

Other advances in Anatomy

All along this century human anatomy underwent a steady development over much of Europe.

Francis Glisson (1597-1677) was first a lecturer of Greek at Cambridge, a chair that he abandoned for political reasons. He thereafter practised medicine. Being a true Aristotelean he published some general works of little interest, but he also produced two excellent monographs, one on the liver (1654) and another on the stomach and gut (1677).

His junior friend, Thomas Wharton (1614-1673), who was also a practitioner in London, is the author of the first systematic account of glands (*Adenographia universalis*, 1656), distinguishing them from organs that he did not consider to be secretory: the gut, the brain and the tongue. He did, instead consider as glands, and accurately described, the kidneys, the testicles, the thyroid. He rediscovered the excretory duct of the submaxillary gland which is still known by his name. Wisely Wharton refused the hypothesis of Descartes that the pineal gland is the seat of the soul and supposed that its use was to clean the brain from its excretions, which was then usually considered to be the function of the hypophysis. It is most notable that at a time at which there was no technique available to study the histology of tissues, such internally secreting, ductless glands, had been correctly identified as secreting organs.

Another important British anatomist was Thomas Willis (1621-1675). In the civil war he sided with the cavaliers, was appointed professor of natural philosophy in Oxford, which he left after the final defeat of Charles I, and moved to London where he became a most successful physician. He was among the earliest fellows of the Royal Society. In spite of the time that his practice detracted from that available for his studies, he continued with his anatomical investigations, usually with the help of various assistants both acting as dissectors and helping with the descriptions. These he fully acknowledged in his papers. Thus Willis' papers may be considered as among the earliest records of team-work in the history of biological research. Several of his figures were actually drawn by the same great Christopher Wren (then professor of Astronomy) that was also helping Hooke. The main work of Willis concerns the brain (*Cerebri Anatome*, 1664; *De anima brutorum*, 1672). Willis had a keen interest in comparison and thus he investigated many animals, both vertebrates and invertebrates.

As a whole he followed Descartes' theories on the functioning of the nervous system, but he flatly refused one of Descartes main tenets as well as the other common opinion that the main functions of the brain were located in the walls of the ventricles; he maintained, instead, that memory and the higher functions of the brain are located in the cerebral cortex (a discovery that was independently made a few years later by Swedenborg). Willis repeated some of Galen's experiments, such as cutting the vagus in a dog and confirmed Galen's results of almost 1,500 years before! On the other side, as he had basically adopted Cartesius' ideas on the functioning of the nervous system and was unwilling to admit any rational power in animals, Willis' ideas on the physiology of the nervous system were soon to become obsolete.

Another notable investigator of the nervous system was Raymond Vieussens (1641-1715), who studied at Montpellier and was later the director of a hospital. His *Neurologia Universalis* (1685) gained him a well deserved reputation. He was among the first to propose some changes in the traditional myth of the 'psychic pneuma'. Vieussens thought that actually in the nerves there flowed a 'nervous spirit', more or less as blood flowed in the vessels; such 'nervous spirit' being produced by the nervous tissue by refining blood. Just as for Willis, Vieussens ideas were largely derived from theories which were quite common among alchemists. The modern reader is free to considers such ideas as brilliant intuitions of the concept of neurosecretion and of neurotransmission, or as a by-product of the theories of Great Alchemy, by now defunct for over two centuries.

Among the Italian anatomists we may mention Bellini and Valsalva.

The Florentine Lorenzo Bellini (1643-1704) had studied in Pisa and there obtained a chair of Anatomy. However he was later charged with atheism and impiety and had to forfeit his chair. He took up a private practice in Florence with remarkable success, so much so that he was often consulted by the Grand-dukes and, in spite of his opinions, by the Pope. He had studied with Borelli and with Redi, was a fellow of the Accademia del Cimento and a painstaking scholar; he was also a good poet and an elegant writer in prose. His main contributions concern the taste buds and the structure of the kidney.

Anton Maria Valsalva, born in Imola (1666-1723) was a pupil of Malpighi and a teacher of Morgagni, and was professor in Bologna. He is mainly remembered for his excellent studies on the anatomy of the ear.

In the first half of the 17th century Italian Universities still attracted several foreigners. The Venetian government was as keen as ever to uphold the excellence of

Padua and thus appointed there Adrian van der Spiegel (Spigelius, 1578-1625) from Bruselles and a pupil of Fabricius, Johannes Wesling (or Vesling, 1598-1649) from Minden in Westphalia; who published in 1641 a *Syntagma anatomicum* which enjoyed a vast and long reputation as an excellent textbook, and J.G. Wirsung (1600-1643) from Munich, their names being still linked with important anatomical features: 'lobe of Spigelius', 'duct of Wirsung', etc.).

Anyway by far the most important scientist who came to Italy from Northern Europe is Nicholas Steno (Niels Stensen, 1638-1686). Steno was born in Copenhagen and studied there. He was a friend of the brothers Thomas and Erasmus Bartholin. When Copenhagen was besieged by the Swedes, as many other students, he took an active part in the defence. Later he went to Amsterdam, where he studied with Blasius and there discovered the excretory duct of the parotid gland (*ductus Stenonis)* and thus quarelled with Blasius. Blasius wrongly claimed the discovery for himself and characterised poor Steno as a 'wretched boy', charging him of deceit, ingratitude, bad manners, foolishness, injustice, blundering, perfidy, incivility, falsehood, treachery, calumny, scoffing, malice, arrogance, perversity, audacity, shamelessness, impudence, fatuity and depravity: a fairly comprehensive list for such a saintly person such as Steno actually was! While in the Netherlands Steno studied also with Sylvius (De la Boe) and thus became a friend of all the major young Dutch biologists of the time, he was also a good friend of Spinoza. During his stay in Holland he published a monograph on the anatomy of the brain.

Back in Denmark (1664) he applied for the chair of anatomy, which, however was granted to Matthias Jacobaeus, a nephew of Thomas Bartholin senior. Steno went then to Paris and here he took the opportunity of his life. The Grand-duke of Tuscany had invited Swammerdam to Florence, but the rather misanthropic Swammerdam did not care for courts and so turned over the invitation to his friend Steno. Steno thus came to Italy carrying as an additional introduction a letter of recommendation by Thevenot, one of the founding members of the French Academy. The Grand-duke Ferdinand II welcomed the Dane scholar, appointed him as one of his physicians and granted him all the means and opportunities for his studies. At that time Steno was able to meet in Florence with such notable scientists as Vincenzo Viviani, Francesco Redi and Lorenzo Magalotti. He also had no problem in getting in touch with Malpighi and others.

While in Florence Steno converted to the Catholic creed and became a priest (1667). Twice he returned to Denmark, the first time hoping for a chair, which he was refused. The second time (1672) as he had been appointed *Anatomicus regius*, but he did not find himself at ease, partly also because he was a Catholic. So he returned to Florence, were he was again welcomed by the new Grand-duke Cosimo III. It was during this second stay in Tuscany that Steno made his fundamental contributions to Geology and Palaeontology. However Steno's interest in the sciences was vanishing, while he felt ever and ever more the call of his religious ministry. In 1677 Steno was appointed as Bishop *in partibus infidelium* of the nominal seat of Titopolis and in real charge as Apostolic Vicar for Northern Germany and Scandinavia. He thus returned to Denmark. In spite of his many calls as Apostolic Vicar (he even wrote to his friend Spinoza in a futile attempt to convert him), Steno gave guidance to some brilliant pupils in their anatomical researches. As Vicar he travelled widely both in Germany and Scandinavia and finally died in Schwerin utterly destitute. The Grand-duke of Tuscany asked for his corpse, which was finally buried in Florence in the Church of St.Lorenzo where also the Grand-dukes are buried.

Steno was an outstanding scholar in many fields, as an anatomist he made notable studies on the glands, the lymphatics, on the heart and on the muscles. These last studies are especially interesting as Steno tried to investigate them from the mechanical standpoint, thus opening the field that was going to be that of Giovanni Borelli. Steno was also the first to discover the connection between the yolk-sac and the gut in the embryo of chickens: he rediscovered the placentation in some sharks, which was finally confirmed by J. Müller in 1840.

His contributions to the birth of truly scientific palaeontology will be considered further on.

Animal anatomy

We have repeatedly stressed how, since Galen, the anatomy of different animals had been studied, first as a substitute for dissections that were not possible either because of actual vetoes on human dissections or because of the insufficient availability of human corpses. Practically only Aristotle had studied animal anatomy as valuable because of its intrinsic interest, as, in spite of his training, Aristotle was totally indifferent to the practice of medicine, whereas all the Anatomists listed thus far were primarily physicians, and, with but very few exceptions, the trend continued through the 17th century. Nevertheless this was also a century of considerable advances in animal anatomy (to speak of comparative anatomy at this stage is, to say the least, optimistic).

During the 17th century animal anatomy was known as zootomy and had a considerable development.

We have seen how at the turn of the 16th century people like Casseri (1559-1615) made a number of important contributions especially on the organs of voice and hearing, which Casseri described in several mammals, in frogs, fishes and even in some insects. Casseri's contributions, however, are purely descriptive. We have also seen how animals were currently used in researches, for instance by Harvey, Pecquet and Bartholin, who all used dogs.

The credit for having written the first book entirely devoted to animal anatomy, goes to Marco Aurelio Severino. Severino was born in Tarsia (near Cosenza) in 1580, he went to Naples to study medicine. There is a story that he met there with the heretic philosopher Tommaso Campanella, but this is probably a legend. Anyway Severino was certainly a committed Paracelsian. In Naples he became a professor of anatomy and surgery and there he earned a reputation as a successful physician and surgeon, and actually boasted that he was a rather aggressive surgeon. Also as a teacher his reputation was notable and even foreigners attended his courses. Apparently for fear of being tried by the Inquisition, he fled Naples for a while, but later he returned there as a honoured master until his death in 1656.

His most quoted book is titled *Zootomia Democritea*, and was published in Nurnberg (1645). German publishers being usually chosen whenever the author aimed to bypass the obligations and controls of the Inquisition. Democritus was commonly assumed to be a sort of anti-Aristotle and, thus, Severinus was publishing his books as a sort of anti-peripatetic manifesto. His next book is an even more rude attack on Aristotelean philosophy as it is its title: *Antiperipatetica* (Naples 1655).

Severino's *Zootomia* is important rather for its plan than because of its content. As it had been long common practice, Severino acknowledges a general body plan common to both man and animals in general, and believes that this has been planned by God. As a good Paracelsian he considers that man (the microcosm) is the archetype of the living world and that it subsumes all the structures found in any animal. Thus Severino considers that the anatomy of animals (zootomy) must be studied in order to understand human anatomy (which he calls 'andranatomy'). Obviously his wish to find in all the animals the same simplified organs that occur in Man led Severino into a number of gross mistakes. Nevertheless it must be admitted that, albeit for reasons totally unknown to Severino, the idea of searching in different animals for structures having the same morphologic significance, was a sound one. Severino's zootomy is conceived as entirely subservient to human anatomy, his figures are sketchy and sometimes more or less fantastic. As a whole his *Zootomia*, while referring to a number of dissections made on many mammals, birds reptiles amphibians, fishes, arachnids, insects, crustaceans and molluscs and including some new observations, is a rather poor work, certainly not up to the standards of a Belon, who, by sheer instinct, hit on better principles and methods of comparison.

An interesting chapter of the *Zootomia* is the last one, which provides a description of both techniques and instruments used for dissection. There Severino recommends the usage of magnifying glasses and claims to have discovered the methods for the injections into vessels and cavities of substances which there solidify, so that by successive destruction by maceration of the surrounding tissues, one obtained a perfect cast of the cavities investigated. The method was later much perfected by Spigelius and by Ruysch and is still used as it has proved extremely useful for descriptive purposes.

As we have seen the second half of the 17th century there is a marked increase in the amount of investigations on animal anatomy. Several include microscopic investigations and, as such, will be considered further on.

Among the best animal anatomists of this period, perhaps the most important are Stefano Lorenzini of Florence (dates of birth and death unknown) who published an excellent monograph on the torpedo fish (1678), but whose scientific activities were wrecked by political suspicions that cost him a twenty years imprisonment without trial; the Dutch Gerard Blaes (Blasius, 1626-1692) whom we have mentioned for his quarrel with Steno, and who in his early treatises contributed a number of new details to the anatomy of several species and then wrote a vast compilation: *Anatomia animalium* (1681) which has been considered as the first systematic treatise of comparative anatomy, but is just an extensive list of facts, without any attempt to real comparisons. In England we have Samuel Collins (1618-1710), who wrote *A systeme of Anatomy* (1685), which includes a good deal of information on animal anatomy. This volume is to a considerable extent a compilation notwithstanding the claims of the author, who seems to have mainly contributed the verbose and mostly pointless speculations which make up for a good deal of the book.

By far the best English animal anatomist of this time was Edward Tyson (1651-1708): he was born in Bristol, died in London and studied both in Cambridge and Oxford. He was professor of Anatomy in London, was a reputed practitioner and, as a physician, he deserves mention as, when he became the director of the Bethlehem Hospital (the asylum commonly known as 'Bedlam'), he drastically changed the hospital practices, greatly improving the lot of his patients. As an anatomist he is the first Englishman to concentrate only on animal anatomy. Not all of his studies were published during his life. His contributions include the anatomy of a cetacean, of snakes (and especially of the Rattle-snake), of a species of Peccary and was the first to describe the anatomy of a Marsupial (the Virginia opossum). His studies on the Tape-worms (Cestoda) and on the Round worms (Nematoda) are better than those by his contemporary Redi. Anyway his most famous paper is his 'Anatomy of a Pygmie', which is the first description of an Anthropoid Ape (his 'pygmie' was a young Chimpanzee), and his conclusion is that the animal is more similar to man than any other monkey or ape. As Tyson systematically compares his findings with all that was known on the anatomy of other animals, his work is definitely not ancillary to human anatomy and Tyson fully deserves to be qualified as the first true comparative anatomist.

Martin Lister (1638-1712) was an important student of Molluscs especially as far as their anatomy is concerned, but he gave even more important contributions to geology and these will be mentioned further on.

Some of the main scholars mentioned in this chapter made some contributions also in the field of morphology, and we cannot mention the many minor ones, who provided some significant advance on special subjects.

As we said a number of significant researches were carried out using the facilities of the academies and were often the result of a regular team-work. Also when the scholars were working 'at home', their results were often published by the academies and such publications soon became periodicals.

Thus the Royal Society published papers by Ray, Allen Moulin, P. Blair, of the great microscopists Leeuwenhoek and Malpighi, etc. The journal of the rather peripatetic *Academia Naturae Curiosorum* (later of Halle) published several papers on the anatomy of insects and other miscellaneous subjects by Johannes von Muralt (1645-1733), a Swiss of noble birth (the Da Muralto were originally from Locarno, then part of the Duchy of Milan, and had fled their home town when converted to Calvinism) and of other authors; the *Acta medica hafnienses* of Copenhagen, founded by Thomas Bartholin, published papers by Caspar Bartholin junior, his son and the discoverer of the 'glands of Bartholin' (1650-1705), Olof Borch (Olaus Borrichius, 1626-1690, an enthusiastic hermetist), Steno, Holger Jacobsen (Oligerus Jacobaeus, 1650-1701), who studied both lower vertebrates and invertebrates, but whose main reason for advancement was apparently to be the son-in-law of Thomas Bartholin.

The Academie Royale des Sciences, included a notable team of naturalists and anatomists, the most notable being Claude Perrault.

He was born in 1613 to a family boasting of several notable personalities; his father was a famous lawyer and one of his brothers is the author of the famous collection of fairy tales, including the familiar Cinderella, the sleeping beauty, etc.) Young Perrault was somewhat uncertain as to his real calling: He first graduated in medicine and practised for a little while, tried for a while soldiering, finally decided to be an architect and a naturalist. His project for the facade of the Louvre was preferred to that by Bernini, who had been invited to Paris by Louis XIV (1665)! Apart from this most varied record, as a member of the Academie, he was the promoter and co-ordinator of the team of biologists and anatomists of the Academie. There is a tradition that he died from a contagion got during the dissection of a camel from the Jardin du Roy. If so that was exceptional as usually the man who actually did the dissections was Duvernoy, while Perrault merely wrote down the notes (which he later expanded and edited) and La Hire was making the drawings. It seems, however that the whole team fell ill. Perrault died when 75 of age.

As we said the dissections were a team-work and this is well illustrated in a contemporary print; they were performed either at the Academy or at the 'Ménagerie du Roy', while the results were published, sometimes anonymously, in superb folio-volumes, richly illustrated. The volumes were then presented by the king and the Academy to assorted 'Very Important Persons'. Obviously some compromise was necessary between the 'Grandeur' of the Roy Soleil, who made such splendid presents and budgetary constraints. So the 'Mémoires' were published in but a few copies and at irregular intervals and immediately became collector's items. Actually even the title of the 'Mémoires pour servir à l'histoire naturelle des animaux' changed a bit from one issue to the next and volumes were published in 1667, 1669, 1671, 1676 etc. The Mémoires describe the external morphology and the anatomy of several animals native to France, but even more of exotic species and were mainly the work of Perrault. We shall come back to Perrault when dealing with the problems of reproduction as he was one of the first enthusiasts of the strange theory known as 'panspermy', a theory holding that the germs of all living species are extremely minute particles occurring everywhere and that they develop whenever they enter into a suitable receiving organism!

Thus during the 17th century the efforts of many scholars produced a great increase in the knowledge of the morphology of many organisms, though this was still haphazard. Actually the term 'comparative anatomy' appears in this century in a paper by Malpighi and is first used in the title of a book by Nehemiah Grew.

The microscope opens new worlds

By the beginning of the 16th century magnifying glasses and spectacles were commonly used for a number of purposes and it was just natural that their potential use in biology was very soon realised. In the previous chapter we saw that Gesner was apparently the first to use lenses for biological investigations. Actually the first mention of the magnifying power of lenses occurs in a brief passage of Seneca, where he mentions the magnification realised by looking through a glass bottle filled with water., Pliny mentions glass lenses used to light a fire. Spectacles appear in Europe by the end of the 1200 and in a little poem by the Florentine Giovanni Rucellai (1475-1525) titled *Le Api* (the Bees), he mentions the possibility of observing a magnified insect by means of a lens and a concave mirror. The poem was written apparently in 1523/24, but was printed in 1539.

It is not clear who was the actual inventor of the compound telescope (that is with a combination of lenses. The most probable candidate is the Dutch Zacharias Janssen between 1590 and 1600. The first telescopes ('cannoni') reached Italy from France and, as he himself relates, prompted Galileo to build in 1609 his first telescope. Soon afterwards Galileo built an 'occhialino' (= little spectacle) which, by adjusting the position of the lenses could be used either as a telescope or to magnify things close by. Anyway the first substantial improvements in microscopes were by Eustachio Divini (1620-1695) and in 1668 the Philosophical Transactions announced that with his microscope Divini had discovered 'an animal smaller than any of those seen hitherto'.

Important improvements in the construction of microscopes are also due to Father Filippo Buonanni S.J. (1638-1725), who eventually obtained the post that had been of Kircher at the Museum of the Roman College, and who we shall appear again on the wrong side in the debate on spontaneous generation.

The development of the theoretical aspects of the optics of microscopes and telescopes was the work of Johannes Kepler, G. Fontana and Christian Huygens; their studies allowed a reduction in the optic aberrations of the early instruments, which were such as to partly justify the critics who maintained that such observations which did not fit with their theories were false (actually I have had an opportunity to look through one of Galileo's original telescopes, and it is a very poor instrument indeed!). The technical improvements on the instruments were mainly due first to Dutch craftsmen and later to the English ones.

Let us now consider the work of the 17th century microscopists. Some of them, such as Malpighi preferred compound microscopes that, though provided more blurred images, usually allowed for greater magnifications. Others, such as Leeuwenhoek, who were able to build themselves exceptionally good and strong lenses, or, anyway, could get such high quality glasses, preferred somewhat lesser magnifications and clearer images (however, some of Leeuwenoek personal instruments had exceptional magnifications, exceeding those of the contemporary compound microscopes).

One of the first scholars to use the microscope for scientific purposes was Father Athanasius Kircher S.J. (1601/2-1680), of whom we shall have much to say.

Possibly the greatest of the early users of microscopes is Marcello Malpighi (1628-1694), born in Crevalcore, near Bologna. Malpighi matriculated at Bologna in 1645, then he left studying, resumed his curriculum and graduated in 1653. He later was a pupil of Massari and when Massari died followed in his chair in 1656. Much later, on his return from Messina, Malpighi married Massari's sister. The fact that he got his chair at such an early age does not mean much, as we have seen how the Bolognese legislation provided that anyone who was both a Bolognese citizen and a doctor was entitled to a chair. However there is no doubt that Malpighi had already won for himself a good reputation, as the same year Ferdinand II, grand-duke of Tuscany, who was talent scouting for his University of Pisa, offered him a chair with a good salary. Malpighi taught in Pisa for barely three years and there he co-operated with Borelli for a brief time, just to quarrel later on for rather trivial reasons. Thereafter he went back to Bologna. There his merits were soon recognised and he was even appointed a fellow of the Royal Society, which published almost all his papers. Though in appearance a quiet man, in fact he engaged in furious arguments with some colleagues, especially with Tommaso and Giovanni Girolamo Sbaraglia, Mini and Ovidio Montalbani, who thus earned themselves a mention in the history of biology, and deservedly as the 'bad guys'. Anyway the quarrel with the Sbaraglias had a very personal background, as the two families had a long standing quarrel on matters of estate boundaries and, later, a brother of Malpighi murdered in a riot a close relative of the two professors.

Anyway Malpighi became disgusted with Bologna and moved to the University of Messina in 1662, but there stayed but for a short time and soon was back in Bologna, where he remained until 1691, though his enemies made life hard for him: they even organised a group of students who ransacked Malpighi's house, broke the furniture and his instruments and even beated the aged professor himself.

When the Royal Society heard of the misfeasance, it offered to Malpighi its own microscopes. On top of all this, in 1689 a former pupil of Malpighi promoted four theses describing his work as practically useless and occasionally wrong. These theses were approved in an informal meeting of high ranking clergy. Nevertheless in 1691 Malpighi was appointed at the Pope's Archiatra, that is chief physician, he moved to Rome and there died in 1694.

The first paper by Malpighi was published in 1661 and is a fundamental study on the structure and function of the lungs. In this study Malpighi, thanks to the large size of the Amphibians red-cells, was able to see the passage of the blood through the capillaries. He was thus able to provide the final proof of Harvey's theory. On the other side Malpighi, having seen the red-cells for the first time, thought that they were droplets of fat!

In his successive papers Malpighi studied practically everything that came at hand and he was possibly the first to try some histological techniques: such as fixation by cooking and maceration to clean skeletal structures.

He was able to see the pyramidal cells in the brain and thought that they were of glandular nature and produced the 'nervous fluid' that most people expected to flow in the nerves. In the kidney he described the 'Malpighian glomeruli', in the skin of Mammals the *stratum Malpighianum*. He studied the liver and the spleen both in healthy and diseased conditions. As usual he studied the embryology of chickens and first saw the pharyngeal slits, but obviously had no way of understanding what they possibly were.

Possibly the most famous of Malpighi's contributions is the *De Bombyce* (1669), the first detailed account of the anatomy of an Insect, until then a poorly understood subject. Many scholars were particularly impressed by the discovery of the insect tracheal system. Malpighi correctly understood the function of this apparatus, but went badly wrong with his morphological interpretation, as we shall presently see.

Indeed Malpighi's contributions to plant anatomy are as important as those on animal anatomy (*Anatomes plantarum*, 1675, and *Anatomes plantarum pars altera*, 1679). Here Malpighi studied a number of tissues and found that they were all made of '*Utriculi*', small vesicles, that is cells, or rather their lignified walls. As he had no general theory available for the interpretation of what he was seeing and could not study the internal structure of the cells, he concentrated his attention on the tracheal vessels, and thus he went completely astray.

He was struck by the superficial similarity between the strengthening rings of the tracheal vessels of plants, the tracheae that he had discovered in insects and those of the respiratory apparatus of terrestrial Vertebrates. As he was not aware that in the tracheae of plants flows a fluid, while in those of animals (which anyway have been independently evolved several times), air flows, Malpighi maintained that all these organs were 'corresponding' (at this stage it would be misleading to use the term 'homologous' as the corresponding concept did not exist at the time and, anyway, it would not precisely correspond with Malpighi's idea. Malpighi supposed that in the simpler organisms, such as plants and insects there was a tracheal system branching through the whole body, in the more and more perfect animals, the tracheal system became more and more concentrated in the lungs. Obviously Malpighi conceived this in terms of a *Scala Naturae* as conceived by Albertus Magnus and not at all in terms of evolution. That Malpighi's interpretation was all wrong does in no way detract from the enormous theoretical significance of this attempt, one of the very first and absolutely the first as far as microscopic structures are concerned, of a true attempt at comparative morphology.

Actually Malpighi tried to use the same approach in connection with several other structures of both plants and animals, always assuming that in the so called 'inferior' animals, one could find a simpler version of the structures occurring in the 'higher animals'. Although basically wrong, this assumption was adopted for a long time by comparative morphologists and led them to an impressive number of first class discoveries.

A last discovery by Malpighi worth mentioning is that, together with Vallisnieri they were able to show that also the Gall-wasps develop from eggs, thus completing the work of Redi (see further on) against spontaneous generation (*generatio aequivoca)*

A second great microscopist was Nehemiah Grew (1641-1712). Grew's father was a parliamentarian clergyman. He begun his university studies in Cambridge, but after the restoration of Charles II, his family left England and he took his degree in Leiden in 1671. Sometime later he returned to England and started practising in Coventry. Shortly thereafter Grew moved to London at the instance of that same Bishop Wilkins that we have met in connection with Ray. Wilkins was so anxious to have Grew in London that he raised 50 pounds among the Fellows of the Royal Society in order to ease the costs of the transfer. In fact Grew in order to earn a living, had to practice as a physician through his whole life. His scientific activities were to a considerable extent parallel with those of Malpighi and he was even more closely related with the early activities of the Royal Society, as he was one of its first members and after 1677 was its secretary. Grew was a most pious man and he had some trouble with his conscience before deciding to study botany, in the end he gave in to his true call considering that, after all, plants were creatures of God as were animals and thus deserved the same significance in the eyes of the Divinity.

As we said Grew was the first to introduce formally the term 'Comparative anatomy', though he used it first in a purely botanical context. Grew was essentially a botanist (though he made also a number of observations on both invertebrates and vertebrates, as, for instance in his *Comparative anatomy of stomachs and guts begun*, 1681) and may well be considered as the true founder of botanical morphology (The titles of Grew's publications are exceedingly long and so we shall follow the tradition of quoting them in a shortened equivalent: *Philosophical history of Plants*, 1672; 'Anatomy of plants', 1682). Obviously Grew could not fail to notice the cellular structure of vegetable tissues, which had been already seen by his friend Hooke. Noticing the similarity between the cellular structure of plants parenchyma (he did, indeed, resuscitate a term used by Erasistratus) and foams that often appear when boiling or fermenting different substances, he concluded that the cellular structures were sufficient proof that biological processes and especially growth were dependent on fermentation, just as maintained by Van Helmont, whose son was in London just at that time (see further on). Nehemiah Grew also co-operated with Boyle in some curious experiments

Also Robert Hooke (1635-1703), who had begun his scientific activities as a technical help for the Honourable Robert Boyle, and who, because of his extraordinary gifts had been appointed as the curator of the instruments of the Royal Society and its technical 'demonstrator', during his manifold activities as an inventor and an experimentalist, made several important contributions to microscopic investigations. He actually begun his microscopic investigations stimulated by his friendship with Christopher Wren (later knighted), and this probably prompted Hooke also to tackle problems of Geometry and Architecture. Wren's own biological experiments have an interest only in the fields of pathology and just as his work in Physics and Astronomy (he was actually professor of Astronomy at Oxford), have been obscured by his fame as possibly the foremost British architect of that age.

Hooke was an accomplished and most versatile scientist, but his difficult character and his readiness to engage in polemics made him clash with a several the major scientists of his age, Newton included, who was able to somewhat diminish Hooke's credit with later generations. Hooke was certainly the first to use the term 'cell' in connection with the structure of cork in his *Micrographia*, a miscellaneous collection of observations and theories ranging from a theory on light and colour, to experiments on combustion and respiration. There he describes and illustrates different vegetal tissues. While his drawings and descriptions are excellent, his observations are little more than a mere list of facts.

Two even more important microscopists are the Dutch Antoni van Leeuwenhoek (1632-1723) and Jan Swammerdam (1637-1680).

Van Leeuwenhoek is a strange type: (actually he, for unknown reasons changed his family name from Thomiszoon into van Leeuwenhoek (= corner of the Lion), which was the name of the place where his family had a house); he was almost totally devoid of academic training, and there is a persistent legend that he abandoned trade and that, in order to be free to pursue his beloved microscopic investigations, he had managed to obtain a little sinecure as a bailiff of the town's aldermen, and was later advanced to a little better administrative sinecure. In fact he was a dealer in fabrics and related commodities, and it was his trade that introduced him to the use of lenses. In fact he was a rather eminent citizen and in 1660 he was appointed as chamberlain of the City council, and in 1669 he became surveyor for the Court of Holland; in 1677 he was appointed Chief Warden of the city and in 1679 inspector of weights and measures of his town of Delft. He thus gained a substantial income, supplemented in old age by a special pension from the town! He had an uncanny ability for making lenses not only from glasses, but even from diamonds and was able to make some minute ones which had incredible magnifications: one of the surviving ones has a power of magnification of 270 diameters and a resolution of 1.4μ, but from his descriptions it is clear that he must have managed to make lenses with a power of 500X and a resolution of 1 μ !. He was also the first who tried to get a clearer view of the tissues by staining thin sections of muscle by an alcoholic solution of Saffron.

Having got in touch with the Royal Society by a letter from Reigner De Graaf to Henry Oldenburg, the fellows of the Society were so interested in his entirely novel observations, that they charged themselves with the translation of his letters from the Dutch, the only language known to van Leeuwenhoek, into Latin and English and of their publication in the Philosophical Transactions.

Van Leeuwenhoek spent his whole life looking into anything that either attracted his notice or that was mentioned to him. Such was the case of his famous investigations on the sperms: he became interested in them when he knew of the observations by a medical student from Arnhem[1], Johann Ham, in 1677, and always ignored that they had been previously observed by Nicolaus Hartsoeker in 1664.

Van Leeuwenhoek's observations are generally accurate and his drawings reasonably good. Being fully conscious of his cultural gaps, he never proposed new general theories, while he freely used his own evidence either to support someone's ideas or when they falsified other's peoples theories.

Among the vast assortment of observations by Leeuwenhoek, many do not concern biology at all; among those concerning biology the most notable are his observation on the capillary circulation and on the red-cells of the blood, on the structure of muscles, bones, teeth and he was even able to see bacteria. On fertilisation van Leeuwenhoek was a supporter of the so-called 'animalculist' theory, that is that he considered only the sperms to be responsible of the development of the new individual.

Contrary to Van Leeuwenhoek, Swammerdam (1637-1680) was a well educated scientist. Born in a well-to-do family, (his father was an apothecary with a keen interest in the natural sciences), Swammerdam studied medicine under the guidance of De la Boë (Sylvius), who was a good anatomist and physician and whose name is linked with the description of the 'sylvian scissure' of the brain. Sylvius moreover was an excellent and famous master. Swammerdam was a good friend of both De Graaf, Ruysch and Steno, of whom we shall have much to say later, and was especially close to the French botanist Melchisedec Thévenot. He was also a member of a rather transitory academy: the 'Private College' of Amsterdam, to which publications he, with Blasius, was the main contributor.

Swammerdam's personality was a complicated and melancholic one. Once graduated in medicine, he refused to practice, so that he quarrelled with his whole family.

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[1] Haller in one of his famous contributions to the history of biology made the mistake of crediting the discovery of sperms to a Dr Johann von Hamm of Danzig and this mistake has been commonly handled down from book to book until now.

After some years of destitution he inherited his father's estate and then begun quarrelling with his sister. His health was poor and he may have suffered from malaria, anyway he died when only 43. During his life he published but little: apart some short papers, his main contributions were an *Allgemeene Werhandeling von blioedloose*, republished in Latin in 1669 as *Historia insectorum generalis,* and the *Miraculum naturrae sive uteri muliebris fabrica* in 1672. In the latter he also describes the technique for injecting coloured waxes into the vessels in order to study their finer ramifications, and, finally, he published a biology of the Mayfly.

Swammerdan was a deeply religious, lonely soul with very few friends, and he was increasingly interested in mystic speculations. In 1675 Steno, while sending to Malpighi, his friend's drawings of the anatomy of a caterpillar, informed him that Swammerdam had destroyed the manuscript and recommended to Malpighi to pray for the soul of his friend, so that he could find his way. Later Swammerdam got in touch with a certain Antoine Bourignon, a mystic woman equally hated by Catholics and Protestants, whom he visited in Schleswig in 1677. Finally, Swammerdam, shortly before his death, bequeathed to his friend Thévenot his surviving manuscripts, providing the money for their publication. However Thévenot did nothing and sold them to Duvernay and when the latter died, they were acquired by Boerhaave, who finally edited them, 57 years after the death of their author.

The fact that Swammerdam was so deeply involved in mysticism should not be considered as surprising at all; his friend Steno did just the same, albeit in a very different direction. The 17th century was a time of lively, and often acrimonious debates on theology, 'scientific interpretations' of the Bible's prophecies tinged often with expectations of the millennium, the new kingdom etc.. Scientists were mostly deeply involved in such debates, as for instance Newton, who spent a good deal of his time in the exegesis and analysis of the Biblical texts and actually finally decided that the second advent would occur in 1948!

As we said Hermann Boerhaave, a famous physician and physiologist, bought the surviving manuscripts of Swammerdam and edited them both in Dutch as *Bijbel der Natuure* and in Latin as *Biblia Naturae*. In spite of the lapse of time between Swammerdam's investigations and their publication, the *Biblia* was a momentous publication, and deservedly so. Not only are both the descriptions and illustrations extremely accurate, but it still included a number of new and significant observations. Especially important were his studies on the metamorphosis of insects and amphibians and on the anatomy of several invertebrates, but there were also important applications of morphological evidence for classification.

Swammerdam was a convinced 'preformist' and, contrary to Leeuwenhoek, an 'ovist'; that is: he was convinced that inside the egg not only there was a fully structured new individual, but all future generations, encased one into the other, that necessarily, he thought, having been there since creation. This seems even stranger when one considers that he had correctly drawn and described the first stages of development of the

Amphibians! The reasons for this blatant contradiction will be discussed in the next chapter, as the debate on the reproduction and development of individuals was to be a prominent one and to last through the following century.

A last important Dutchman has to be remembered here: Reigner De Graaf (1641-1673) deserves to be remembered for a single publication. Actually De Graaf, whose father was a famous architect, had studied in Utrecht and Leiden, but, probably for religious reasons, as he was a Catholic, he went to graduate at the French University of Angers, taking with him his thesis, that he had prepared under the influence of Sylvius. This thesis, which won him considerable acclaim is extremely curious (*Disputatio medica de natura et usu succi pancreatici*): on one side for the first time and by an excellent technique, De Graaf was able to get from dogs, pure pancreatic juice and saliva and experiment with them in vitro, on the other, simply following his masters, he gave a completely wrong interpretation of the results, arguing, among other thing that the pancreatic secretion is strongly acid, whereas it is basic! On the other side, for reasons that escape me, almost all historians maintain that he discovered the follicles of the ovaries (now commonly known as follicles of Graaf) that he mistook for eggs. Now he did nothing of the sort. The ovaries of Birds had been known since antiquity and so were their very big follicles; and the correspondence of the mammalian ovaries with that of birds was, again an accepted fact, but, as it was assumed that it produced some sort of liquid secretion (remember the theory of Albertus Magnus about the function of the 'feminine sperm'), they were commonly called 'feminine testicles'. In 1668 De Graaf published in a single volume three treatises: *De virorum organis generationi inservientibus, De clisteribus et de usu siphonis in anatomia.* Actually it is only the first which interests us (the third suggests some improved tecniques for the injection of coloured fluids in vessels) . De Graaf discovered some small round bodies (De Graaf's follicles) in the ovary of rabbits, and he correctly equated these with the much larger ones of birds and, thus, was the first to formally call the mammalian ovary by this name. However he failed to see anything inside the follicles, whereas he saw both in the Fallopian tubes and in the uterus of the same rabbits some extremely small round bodies and these he mistook for eggs, while they are, in fact blastocysts (as we shall see the true mammalian, ovocyte 2, was discovered by Von Baer in 1837). In Mammals, as meiosis is completed after the entrance into the cell of the sperm, the true egg is an extremely ephemeral phase lasting just the time between the expulsion of the second polar cell and the fusion of the feminine pronucleus with the spermatic pronucleus, which had been kept waiting inside a vacuole of the ovocyte. De Graaf thence concluded that the liquid filling the follicle, after reaching the Fallopian tubes organised itself into the true egg.

The debate on spontaneous generation

Nowadays we are all familiar with the debate on the origin of life and we all take for granted that, while today the prerequisite conditions for the appearance of living organisms from lifeless molecules have long vanished, yet we have good reasons to believe that, in the very remote past, when physico-chemical conditions on the Earth's surface were quite different from nowadays, by some type of, probably, stochastic event some molecules joined in the proper way to begin some sort of elementary life and such organisms became capable of duplicating themselves. Such a belief requires pooling an enormous amount of evidence from the most diverse branches of science, from cosmology to molecular biology. Our ancestors, instead, (a) could not possibly conceive, even as a mere hypothesis, of a world quite different from the present one, and (b) whatever their individual religious beliefs, they were bound to two alternative hypotheses: either the world had always been, more or less, like the present one, or it had been created by God fairly recently (somewhere between 6,000 and 5,000 years ago for those who followed the Biblical chronology), and both animals and plants had been created soon after the creation of the world.

As it was far from obvious how many animals and plant could reproduce while they seemed to appear at least in some proper environments out of nothing, without any clear continuity with parents of any sort, it seemed reasonable to many to assume that they could be self-forming. In fact, but a proper analysis, would lead us far, both the theories and experiments of alchemists, seemed to prove that some substances, when subject to proper manipulations, 'died' and from their ashes or putrid remains, appeared new substances and always the same whenever the correct proceedings had been followed and such as, apparently, did not exist before. Finally some alchemists such as Paracelsus, claimed that it was possible to create living organisms in the laboratory (actually they were able to produce 'objects of such shapes, that looked a bit like organisms and thus thought that by some more effort and with best materials or techniques they would come to life). Just to take a well known personality, the philosopher Tommaso Campanella (1568-1639) in his *Il senso delle cose e la magia* (= *the meaning of things and magic*) while is very doubtful as to the possibility of fabricating the homunculus, is positive that spontaneous generation must occur in many animals.

Given 'the state of the art' it was reasonable that most scholars were more or less inclined to accept spontaneous generation, for some organisms at least.

Obviously the question had to be settled on experimental evidence. Now it was just in the transition between the 16th and the 17th century that experimentation became a standard practice in physics and somewhat later in biology (Alchemy was a 'trial and error art', rather than an experimental science until it was transformed into chemistry by the systematic usage of quantitative methods).

Indeed by the beginning of the 17th century experiments became a rule in problems of physiology, though they did not immediately find the support of any general theory of the method comparable with those that Galileo was providing for physics.

As noted in discussing the studies on circulation, all these had been made by typical experimental methods. As examples for the 17th century we may quote the studies on digestion and the function of the pancreatic secretion *in vitro* by De Graaf, the studies on the possible variation in volume of muscles during contraction by Swammerdam and by Glisson (1677), on the transformation of venous into arterial blood (as we now know, by oxygenation) by Carlo Fracassati (Bologna, 1630-1672) and, independently, by Richard Lower (1631-1691). (Lower studied the oxygenation of blood both in the lung and in vitro and, as a good Paracelsian, thought that the change in colour and qualities of the blood was the result of the action of 'the nitrous part' of air. He was probably under the influence of George Ent (1604-1689) who considered 'the nitrous part' of the blood the responsible for the preservation of life).

However, coming back to the problems of generation, William Harvey, in his study of the development of Mammals, had denied the possibility of spontaneous generation already at the beginning of the 17th century.

Both on the problem of spontaneous generation, as well as other subjects, the man who had a knack of designing experiments as simple, as elegant and as convincing as possible was Francesco Redi (1626-1698).

Redi had graduated in medicine in Pisa in 1646 and was a splendid example of the accomplished gentleman of his times. He was a learned linguist (and used of his knowledge to 'pull the leg' of his fellows the Academicians of the Crusca, by fabricating ancient texts such as a *Chronicle of Sandro Pipozzo* a suposedly 12th century manuscript that describes the invention of spectacles!), he was a good poet (two of his little poems are amongst the most delightful poetic jokes in Italian poetry), he was also a wise and careful physician and an accomplished fencer; as a man he was both generous and kind.

Redi begun his activities as a biologist by a study on the poison of vipers and on how it was injected, a subject to which he returned again and again and which was the occasion for several anatomical observations. Anyway his lasting claim to fame is his little treatise *Esperienze intorno alla generazione degli insetti, fatte da Francesco Redi Accademico della Crusca e da lui scritte in una lettera all'illustrissimo Signor Carlo Dati*, Florence 1668 (= *Experiments on the reproduction of insects made by Francesco Redi, Academician of the Crusca, and written by him in a letter to the most illustrious Gentleman Carlo Dati*). The most famous section in this booklet concern the reproduction of flies and big flies that develop in rotting meat. Briefly: Redi's experiments proved first that there is no connection between the kind of meat used and the kind of fly that developed; second, having noticed the eggs laid by the flies, he first stopped with paper and subsequently with muslin, so that air could freely pass, the jars where the meat was put to rot, thus proving that when the flies or their maggots were prevented from reaching the meat, the meat would rot all the same, but no flies would develop; third that on the lids of the stopped jars the flies deposited their eggs and from them developed maggots which desperately tried to get to the meat and eventually died. There was thus but one necessary conclusion: the maggots and the flies did not spontaneously develop from rotting meat (or from vegetables as Redi tried these as well), but came simply from the eggs laid by adult flies (in fact some large flies are viviparous and lay the maggots directly). Strictly speaking the hypothesis of Aristotle that maggots developed from the eggs of flies, but that the pupa spontaneously generated itself into the dying maggot would not be falsified by Redi's experiments. But Aristotle's theory was completely obsolete by the time, as insects metamorphosis had been well observed, so there was no need to bother about Aristotle's theory.

As was common at the time, Redi's booklet includes a number of other important items such as microscopic observations on insects and mites parasitic on the surfaces of plants and animals. The figures are quite good and include the first picture of a bird-louse (Mallophaga).

In spite of such conclusive experiments, Redi's conclusions were challenged by some, and especially by the already mentioned Father Athanasius Kircher (1601-1680), who was in may ways a good scholar, especially in pre-Leibniz combinatory mathematics and in geology[2]. Kircher made a number of criticisms of Redi's experiments and unfortunately for his reputation, he suggested a recipe for getting the spontaneous reproduction of frogs! This folly he compounded by arguing that the legs of frogs developed by the splitting of the tail of the tadpoles, an incredible error by an otherwise good microscopist ! Obviously Redi had no problem in testing the experiments suggested by Kircher and, naturally, no frogs were born and, and it was as easy for him to show that the tail of tadpoles had nothing to do with the development of legs of the frogs.

In fact Father Kircher always maintained the existence of spontaneous generation, as it was needed by an extremely interesting argument that he developed especially in his *Archa Noe.* He had made an analysis of the measurements of Noah's Ark as reported in the Bible and had found that it was impossible to squeeze into the Ark all the animals species.

Aquatic animals, obviously, need not be considered, but even so the Ark was far too small! So the Jesuit Father Kircher proposed a daring theory: he maintained that only 130 species of Mammals, about 150 of birds, and about 30 Reptiles had entered the Ark, and then from these few species all the living species had been evolved after the Flood by adaptation to local environments and by mechanisms that, to some extent anticipate those proposed by Buffon. Moreover, in order to explain the faunas of isles Father Kircher assumed that they had been peopled by a few animals reaching there on temporary sedimentary land bridges. The very first true evolutionary theory! Anyway we have not finished with Father Kircher as we shall see further on.

Finally Redi, as he was a careful scientist and had not been able to make any conclusive experiment on Gall-insects, considers that there may still be a possibility of sponteneous generation in the case of such animals.

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[2] Father Kircher was famous also in other fields: he proposed a wrong interpretation of Egyptian hieroglyphs, created the 'magic lantern': the distant ancestor of movies and television, was an enthusiast hermetist and kabbalist and, following in the steps of Paracelsus and Fludd, advocated the medical use of magnets and strogly advocated invisible 'worms' to be the cause of bubonic plague!

Another critic of Redi was the already mentioned Father Buonanni S.J. (1635-1725) who was a good naturalist and whose main merit is his improvement in 1691 of the type of microscope called 'del Tortona', the first instrument for observation by transmitted light. Buonanni eventually became the successor of Kircher at the Collegio Romano and salvaged most of the collections there. He published the first well illustrated monograph on Molluscs and was the first to describe pollination (which, obviously, he could not understand). Buonanni's objections to Redi were partly based on wrong experiments (he maintained that he had bred flies from meat mixed with dust), but mainly by philosophic-theological speculations. Buonanni was also grossly mistaken by maintaining the spontaneous generation of molluscs and generally in fossils, and criticising that Father Marsili (later bishop), who had discovered the eggs of snails and whom we have met previously as a would be reformer of the University of Bologna.

As we shall see the problem of spontaneous generation was revived by the discovery of 'infusorians' and again, by that of Bacteria (though some of them had been seen by Leeuwenhoek) and its theological implications underlie such theories as that of continuous creation by Alcide D'Orbigny, one of the best pupils of Cuvier.

Redi is also considered as one of the 'fathers' of modern parasitology, as in his book *Osservazioni intorno agli animali viventi, che si trovano negli animali viventi* (= *Observations on the living animals which are found inside other living animals*), he described, after a good deal of methodical researches on several different species, several new species of parasites. In fact several internal parasites, such as different species of intestinal worms, had been described by physicians of the late Antiquity, but unquestionably Redi's is the first to make a methodical investigation on this subject.

Redi published several more contributions on marine animals (he also discussed the function of the natatory vesicle), on earth-worms (a side show of his study of intestinal worms, and on drugs to eliminate them) and on the physiology of turtles. His contributions to the anatomy of molluscs were a great advance for his age, although they were soon made partly obsolete by the work of his contemporary Lister, who, however, acknowledges the value of Redi's contributions and even borrowed some of his figures.

Finally the name of Redi is linked, this time as the recipient of the 'letter' where, in 1687, Giovan Cosimo Bonomo (1663-1696) and Diacinto Cestoni (1637-1718) the Protospeziale (chief apothecary) of Grand-duke Cosimo III and a friend of Redi, gave the first adequate description of the mite *Sarcoptes scabiei* that causes scabies. Later Cestoni, in a letter to Vallisneri, claimed the discovery for himself alone, and both authors apparently ignored the fact that the mite had been known since the 10th century, had been described by Benedictus in 1508, mentioned by Moufet in 1634 and badly illustrated by Hauptmann in 1657. Anyway many did not believe that the mite was really responsible for the disease (and indeed at the time several different diseases were confused under the label 'Scabies'. The matter was finally settled by the Corsican F. Renucci in 1834.

In the late years of the 17th century began the debate on the real nature of reproduction. What, indeed, caused reproduction and how did it happen? The Greco-Roman tradition envisaged two possibilities: either extremely small particles became detached from every organ of the parents and concentrated either in the sperm of the male, or in the so-called feminine sperm, or in the egg and thence reorganised themselves to produce the embryo, or that the sperm gave, by its peculiar 'power' (*virtus*), the 'form', that is moulded the matter provided by the female, more or less in the image of the parent.

During the 17th century, partly because of the more or less general abandonment of all Aristotelian theories, partly because of religious preoccupations, scholars begun to talk of 'germs' which, when in a suitable environment, developed into a new individual. This posed a problem: were such germs contained inside the parents or were they diffused in the general environment. This last hypothesis was almost necessary in order to maintain the possibility of spontaneous generation and, as we have already mentioned, was championed by Perrault.

Physiology in the 17th century: The debate between 'Jatrochemists' and 'Jatromechanists'

We have seen how many anatomists following in the steps of Vesalius and Harvey were busily accruing the knowledge of anatomy that is prerequisite to the proposal of a new physiology. The discovery that the capillary network actually joined the arteries and veins (the *vasa per capillamenta resoluta* had been well known since antiquity, but they had been thought by Galen and his followers to be blind ended). In fact the passage of the blood from the arterioles to the veins through the capillaries was seen in the 17th century only in 'cold-blooded' vertebrates, it was only by the end of next century that Lazzaro Spallanzani was able to see it in the embryo of chickens.

Although several ancient philosophers had thought that there could have been a definite age when living organisms had first appeared and that they might have evolved from non-living, mineral substances, yet, apart from some rigorous atomists, there had always been a tendency either cleanly to divide the inorganic world from the living one, or to believe in a 'pneuma' more or less widespread and capable of animating and organising the living beings. It was simple common sense applied to the interpretation of the greatest mystery of Nature. Indeed, everybody knows that any object either stands still, or there must be something to move it, but for the air (that is 'pneuma'), as we actually notice when the wind starts, but we cannot see what is causing the air to move: it apparently is the only thing that moves by itself. Moreover in terrestrial vertebrates which were the animals most easily studied, so far as the animal breathed it was alive, after the last gasp it was dead. It appeared thus obvious that what kept the animal alive was the air moving in and out, which again was 'pneuma'.

If, then, it is air which keeps animals alive, it seemed reasonable to assume that it was also the cause of life. This was, broadly speaking the background of all the 17th century theories of life, with obvious specific connotations, usually according to religions.

Indeed some experiments by Hooke and by Boyle, appeared to prove precisely the point: if the thorax of a mammal was punctured so that air entered it, respiration ceased (the lung collapsed, but Hooke did not know it) and the heart ceased beating, but, if air was artificially pumped in and out, than the heart-beat was resumed. It appeared that it was not respiration itself, but merely the air going in and out that kept the animal alive.

The general theory of the four elements, moreover, assumed that each element had a spontaneous motion either downwards, the heavy ones, or upward, the light), if so, each one could be endowed with a particular power (*vis*)*;* thus a *vis vitalis* was credited to the pneuma.

The first people who tried to understand living beings as physical mechanisms (Leonardo being one of the very first) were either mathematicians or engineers, who could not fail to perceive the common similarity between human made machines and parts of organisms; moreover as soon as the need for 'measurements' became standard in physics, it was natural to try it on organisms.

A scientist who has been often underrated by historians of biology was Santoro Santorio (1561-1636) from a noble and influential family of Capodistria (now in Slovenia, but for centuries Venetian territory). He matriculated at the university of Padua at the early age of 14 and took his doctorate in medicine in 1582 when 21. He then served as family doctor with a noble Croatian family for some years and returned to Venice in 1599, he was then appointed as a professor in Padua, where he met and became a friend with a group of outstanding personalities: Galileo, Fabricius, Fra' Paolo Sarpi, Gianbattista Della Porta and several other physicians, artists, alchemists and religious thinkers.

Santorio was fascinated with mathematics and especially by measurements. While a resident of the Dalmatian coast he had experimented with instruments of his own design, measuring the speed of marine currents and of winds. Taking as a starting point Galen's theories as to how the unbalance of humours might develop and cause the 'discrasy' or disease, he found that 80,000 possible combinations could occur and, therefore that there should be as many different diseases! Thence Santorio invented that he called 'Static medicine'. His problem was to check Galen's theory that some of the food ingested was eliminated as *perspiratio insensibilis* (nowadays we use this term to signify the evaporation occurring through the skin and mucous membranes, without the appearance of sweat, but in Galen's understanding of it, it was a much broader concept covering the invisible loss of any kind of matter). Santorio, therefore decided to watch for a significant length of time the changes in weight of an individual. He did the experiment on himself. So, while he carefully weighed all the food and liquids that he ingested and all his excreta, he built a gigantic scale where he put even his furniture and lived on it for a while. In order to check for every possible source of error and ever passionate for measurements, Santorio modified Galileo's thermoscope (the invention of a true thermometer is credited with some reservations to the alchemist Robert Fludd, who claimed to have found its description in an ancient manuscript). The purpose of Santorio's improvements on the thermoscope was to check the variation in temperature of the individuals under experiment. Thus Santorio concluded after many experiments (including having the subject breathing upon the thermoscope under a sort of capuchon so as to be able to measure the temperature of the expired air), that correct measurements required that the thermoscope had to be applied for ten pulses of his 'pulsilogio' (an improved pendulum of his own device, which could be synchronised with he heart's beat). He even considered the relative humidity of the air, which he measured by a rudimentary hygroscope, again his own invention.

In 1614 he published his results: he was then able to show that a considerable amount of the food and liquid ingested is neither excreted nor is stored as an increase in the weight of the patient. Santorio thought that he had shown the significance of the *perspiratio insensibilis* for the preservation of the 'eucrasia' or optimum balance of functions. In fact he had accidentally discovered the study of metabolism and had also mounted a perfect experiment for this type of research. Santorio's work was translated into several languages and was generally applauded. The British Martin Lister (1639-1712) thought it as important as Harvey's work on circulation. Boerhaave (1668-1738) thought is was the most perfect example of medical research.

Santorio advanced also another brilliant idea: in order to explain how it happened that, as far as they were alive living beings do not rot and begin rotting as soon as they die, he supposed that this happened because the tissues were formed by very short lived elements which were continuously renewed!

The most typical of the scholars who tried to understand the mechanics of living beings was Giovanni Alfonso Borelli, born in 1608 an illegitimate child. Borelli was basically a mathematician (his *Euclides restitutus sive priscae geometriae elementa* is a significant text in geometric combinatorial, distantly in the wake of Lullus). Borelli studied in Naples with the mathematician Benedetto Castelli, later he became a professor, first in Messina and then in Pisa (1655-1668) arriving there at the same time as Malpighi, who was actually instrumental in getting Borelli interested in physiology. Borelli was also a fellow of the Accademia del Cimento. In 1668 he returned to Messina, but there he got involved in a plot to expel the Spaniards from Sicily. The plot was discovered and Borelli fled to Rome, where he was for a while a member of the Academy established by Queen Christina of Sweden, who had settled in Rome after her abdication. The queen even subsidised him for a while. Later, utterly destitute, he was the guest of the Calasantian friars and died in their convent in 1671. The Friars later published his famous book *De motu animalium* at their own expenses (1680-1681).

Borelli was a pure mathematician and engineer. He assumes as a starting point the idea that was commonly accepted in his times that when movement was required a nervous fluid was discharged in the muscle and there mixes with blood causing some sort of fermentation which makes the muscle to bulge and shorten. Taking this for granted, Borelli made an extensive study of the musculature and skeleton of a number of animals, and chiefly of man, interpreting them basically in terms of interplaying levers. It is probable that Borelli's manuscript was completed before the publication of Cartesius' studies of 1667 and, anyway, he apparently did not know of it.

René Descartes (Cartesius, 1596-1650) is unquestionably a great mathematician, a rather poor physicist and philosopher and, as far as biology was concerned he was almost always wrong. He systematically and obstinately refused either to understand or to accept anything that did not fit with his own ideas in mathematics, physics or philosophy; thus he never understood the work of Blaise Pascal, refused to admit the possibility of Torricelli's vacuum scorned Galileo's cynematics and judged Kepler's laws 'impossible'; did not believe that the heart was a muscle and that the mechanics of circulation were those discovered by Harvey and so on.

A country gentleman, Descartes had a good education and tried soldiering for a while. Being fully conscious that the advances in astronomy, physics and biology had made neoplatonism (which, anyway, he did not like) completely obsolete, he was afraid that Christian religion could collapse if the impersonal 'Laws of Nature' were to substitute for Divine Providence; thus he decided that it was necessary to create a new philosophy and a new science to solve the theological problems that most of his contemporaries were trying to sort out in the battlefield. Thus, as every college student knows, he began by assuming a total separation between thinking (*res cogitans*) and matter (*res extensa*). He imagined a corpuscolate *res extensa* (matter) subject only to the mechanical actions of such corpuscles which had a natural tendency to move in vortices, and which deviated and bounced as the result of collisions between the particles. It is basically a mixture in equal shares of epicurean physics, in which vortexes substitute for the fall of atoms, with or without *clinamen*, the influence of heat and a strong preoccupation with religion. Cartesius' problem was that it was necessary to join man and God, and God was pure *Res cogitans,* while man was the only being that united the two substances. By the way, as he believed that the pituitary occurred only in man, he located there the soul and, therefore, the power of *cogitatio.*

Thanks to his perfect and elegant Latin and French Cartesius rapidly became famous and, as we shall see, enormously influential even in biology. It is largely due to the counter influence of the equally strange Paracelsians and to English common sense if Mechanicism, as Cartesian patterned biology came to be known, did not succeed in throttling biology. Though Cartesius thought of himself as a good Catholic, at a certain point he thought it wise to move to Holland, which, after getting its independence recognised, had rapidly become a model affluent and tolerant country.

There he was reached by an invitation of Queen Christina to visit Stockholm, where he died of pneumonia.

Cartesius for years kept his *De homine* in a drawer although it had been planned as the necessary complement to the *Discours de la Méthode*. He was afraid of running into troubles with it, as the Catholic authorities did not entirely trust his orthodoxy, in spite of the valiant and successful efforts of Père Mersenne to clear him from the suspect of being a Rosicrucian (the Rosicrucians were a fictitious sect which did not actually exist, but whose supposed existence had been invented by a small group of reformed thinkers linked with neopythagoreanism and whose purpose was to promote a special, mathematically inspired reformation in the church). Cartesius had, indeed, been interested in the Rosicrucian manifestos, while he was soldiering in Germany, but had naturally failed to contact the non-extant brotherhood and that was all.

So the *De homine* was not published until 1667, seventeen years after the death of Cartesius, but was, nevertheless, quite influential.

The *De homine* is scientifically a total failure, pace such anti-vitalist historians of biology that try to excuse it on the factually correct argument that, although every single hypothesis on the functioning of every apparatus are much below the average level of research of the times of its compilation, yet the book was very influential.

Just to provide an example of Cartesius' fantasy, he imagines that animal spirits are fluids, which naturally obey the laws of the mechanics of fluids. The nerves are hollow and are partly filled by thin threads which on one side end in the walls of the cerebral ventricles (a traditional idea going back to the 14th century, when it was believed that each ventricle had a different function in the process of thinking), the other end of the thread being in the organ it served. These threads, move in response to the stimulations got by the peripherical sensory organs and pull on the walls of the ventricles and so open some small pores through which the nervous fluid flows into the nerves and so reaches the muscles and activates them. Much in the same manner, the animal spirits may cause some movements in the pineal gland, in which resides the Soul, *Ens rationale*, which is both immaterial and immortal. As a consequence of the bending of the pineal gland more or less to the left or to the right, greater of lesser amounts of fluids can flow and thus certain ideas free themselves, while other are inhibited!

The same Perrault, whom we mentioned for his morphologic investigations, made, entirely independently, an attempt to investigate the mechanics of organisms analogous to that of Borelli (*Essais de Physique*, 1680), but he gave due consideration to the sense organs. Perrault was a pupil of Gassendi and on one side rightly criticised Cartesius' tenet that animals were mere automata (a point made by Cartesius in order to save for man only the *res cogitans*, and that he could maintain as he was probably a bad horseman and did not like dogs), on the other hand Perrault made a mistake incredible for the age by holding that the contractile part of moving systems was not the 'meat', that is muscular tissues, but rather the tendons and the connective fasciae!

Two more great mathematicians tried to use mathematics for the study of muscular movement: George and Daniel Bernoulli.

Indeed Steno was basically right when he laughed at Cartesius and at the early Jatrophysicists 'to whom everything is crystal clear, just as they had been able to see by their own eyes the whole structure of such an admirable machine and had understood the secrets of the Great Artifex himself'.

Much more interesting is the attitude of Francis Glisson (1597-1677) whom we have already mentioned. He advocates mechanistic interpretations similar to those of Borelli, and it is curious that the two are strictly contemporaries. However, he introduces a new concept and one that was to become a keystone for the physiology of the next century, especially at the hands of Haller, this concept is 'irritability' and it is proposed as a property unique to living matter.

In the field of the physiology of plants, mechanistic physics inspire that of Edmé Mariotte (1620-1684), the one familiar to college students because of the 'law of Boyle-Mariotte'. He thought that lymphatic pressure was the basic force driving the growth of plants and that these synthesised their own food with the help of air, this last being a shrewd guess based on very rudimentary experiments (*Sur la Végétation des plantes*, 1676).

Basically the mechanistic school, that in purely medical matters was termed the 'jatromechanic school', criticised the kind of finalism of the Aristotelean tradition by substituting to the classic Anagke-Necessitas, a merely physical 'necessity'.

Parallel and opposed to the mechanistic school there developed another school, based on completely different premises: those termed in the medical media, 'of the iatrochemists'.

The leading figure among them was Jean-Baptiste van Helmont (1577-1644). Born in Brussels of a noble and rich feudal family, he began by studying philosophy, then switched to law and only as a third choice took up medicine and yet he was able to take his doctorate at the age of 22. Being himself reasonably affluent and having married a very rich woman, he was able do dedicate all his medical activity to the destitute and always refused payment for his services. At the same time he pursued his studies both alchemical and physiological. Apparently because of some of his medical ideas, by 1624 he was suspected by the Inquisition and he was tried in court (Catholic Belgium had remained under the Spanish crown after the Protestant Dutch provinces claimed independence). The trial dragged on for twenty years, actually until Van Helmont's death and he was jailed for almost two years (1634-1636). Obviously in order not to harm his chances in the trial, Van Helmont never published anything, but charged his son Franciscus Mercurius with editing his manuscripts, which Franciscus did in 1648 under the title *Ortus Medicinae,* a book which caused immediate sensation.

Van Helmont was a passionate Paracelsian and, as many adepts to the *Magnum opus,* he was to some extent a mystic and had occasional visions, finally there is no doubt that he was a man of absolute integrity. We have no room here to delve into the general theories, methods and results of the 17th century alchemists, but we should not forget that, given the theories of the time on the nature of matter, their studies appeared perfectly justified, so much so that none less than Isaac Newton spent much of his time in alchemical experiments (and acquired much of the knowledge that made him elegible to be Master of the Mint).

To be fair to Jean Baptiste van Helmont, we must briefly say something first of those of his ideas that we now label as 'crazy' and then of his sound experiments and discoveries and, as usual in this book, we shall make no reference to the purely medical aspects of his activities.

Van Helmont was always convinced of the common occurrence of spontaneous generation, but we shall probably never know how he could imagine that he had obtained the generation of mice from a mix of rags and bran! Much more credible is his claim that he had obtained alchemic gold. In fact alchemists never claimed that their 'gold' was the same as natural gold; they actually believed that not only gold but any of their products: alchemical mercury, sulphur, etc. were 'purer' than the natural ones, so we may well believe that he had obtained some sort of alloy that could pass as gold by the tests available at the time (in fact some medals are preserved in numismatic collections that we know from unquestionable documentary evidence to have been struck with 'alchemical gold', but these have never been properly analysed as to their composition). As far as biology is concerned, Van Helmont assumes that each individual has several 'archaei' which control the different functions and organs, such are the *archaeus faber* which is in control of generation, an *Archaeus insitus* which keeps man alive, an *Archaeus influus* in control of divine activities in man and so on. Each *archaeus* works in conjunction with two more entities, thus forming a multiple triadic system. These two entities are called 'gas' and 'Blas' (possibly from the German 'Blasen' = to blow), which is the originator of every movement and energy. There are universal 'Blas', but there is also an individual 'blas' which is placed near the stomach and directs the work of the different *archeai*. The second entity, *Gas*, is an air-like substance and that develops inside the organisms during any vital process. According the notes by Franciscus Mercurius, Gas should derive from the term Chaos, anyway the term had been used sporadically by Paracelsus as a synonym of air. In man only in addition to Blas, Gas and Archaei there is also an immortal *Intellectus* to whom obeys *ratio.* Telepathy is considered by van Helmondt as being due to a *virtus* that he considers to be a Blas.

As most good alchemists, van Helmondt put great store in fermentations and it is in this field that he made some of his major discoveries. Studying alcoholic fermentation he discovered that during this process is produced a special 'Gas', that he called *Gas sylvestre* and that the same Gas is produced when wood is burnt: he had discovered carbon dioxyde! Studying digestion he concluded that in the stomach begins an acid fermentation, though the acid is not the 'ferment' itself. The acid chyme passes then into the duodenum, where it becomes basic and is supplied by the bile of another ferment, again a considerable progress on previous ideas. Another classical experiment performed by van Helmont concerns the function of water in the growth of plants: he proved that a bush grown for 5 years in a pot had grown from 5 to 164 pounds, while the weight of the garden-mould in the pot had diminished by barely 3 ounces from the 200 weighted at the beginning of the experiment. This clearly showed that almost all of the plant's growth depended on the assimilation of the water which had been irrigated on the plant. Indeed a pioneering experiment in plant physiology.

The Paracelsian chemists were as many in number as their enemies the mechanists, moreover, although they shared a common faith in the possibility of explaining everything or almost everything in terms of fermentations, almost each one proposed some different kinds of it, and we shall mention but a few of them. We have already mentioned the ideas of De la Boe (Sylvius) on the significance of pancreatic juice. Many were British, and both Willis and Highmore, whom we have already mentioned followed the 'chemical' school. The same did the Danes Boch and Thomas Bartolin, a good many Germans and a fair number of Italians. The war of Bismuth, which raged between the school of Montpellier (chemists) and the Faculty of Paris (mechanist) went on for most of the century, the first extolling its therapeutic powers, while the Sorboniates (as Rableais had christened them), for once, were right in considering it a dangerous poison, though it small amounts it is really useful in a few diseases.

One important trend will be acknowledged as the century wanes: the need for precise measurements in experimental research.

Meantime in the field of Botanical physiology considerable advances were made in the understanding of sexuality in plants.

The fact that some plants have separate sexes was known to the Greeks, although in many instances the plants considered to be the male and female of a single species did, in fact, belong to different taxa. On the other hand many were the plants which were thought to reproduce by spontaneous generation and, anyway, were thought to be devoid of sex organs.

Pollination had been described in 1691 by Father Buonanni. He was precisely the one that we mentioned as a staunch supporter of spontaneous generation, anyway he did not appreciate the significance of his observations. Thus the credit for the discovery of the significance of pollination, goes to Rudolph Jakob Kammermeister (Camerarius, 1665-1721), then professor in Tübingen, who identified the pollen as the male fertilising element (we now know that this is not strictly true, as in fact the granule of pollen generates a rudimentary male gametophyte and the real germ cells which effect the fertilisation are produced by this rudimentary and microscopic plant). Camerarius identified also in the pistil the feminine receiving organ. He published his observations in the *De sexu plantarum epistula* (1694), but it took some time before the results of Camerarius were universally accepted. Still over fifty years afterwards, in the times of Linnaeus, some botanists denied a general significance to plant sexuality.

The beginnings of Paleontology

We have repeatedly mentioned the opinions on fossils of Ancient and Medieval scholars. For them the fundamental problem was to reconcile the geological evidence, and particularly the fossils with the concept of a universe created by an act of God.

While for such Greeks as Aristotle, who held that the universe is eternal and basically unchangeable, though several of them allowed for continuous and even drastic changes of the Earth surface, such as local emergence or submergence of lands, fossils were no problem (but in practice they ignored them, except for Theophrastus, who considers that, as the fossils belong to marine species, either they are petrified remains of animals carried inland by floods or, perhaps they come from eggs, equally carried by floods, but which could not completely develop out of their natural environment.

On the other side all myths of Creation, and not only the Biblical account, grants to the Earth at most a few thousands year of age. Thus it was simple for everyone believing in the truth of the account of the Biblical flood, to consider the fossils as the remains of organisms killed ad 'Noah's flood'. The only logical alternative to this theory, was supported by those scholars who believed in spontaneous generation, who maintained that the fossils were abortive attempts at generation.

In Italy since the 14th century, the theory that the fossils were a testimony of the Flood was largely prevalent among the scholars: people like Boccaccio, Ristoro d'Arezzo (1289-1332), Fracastoro (1517) and so on, considered the fossils as animal remains petrified by some mysterious local force, and the problem was just that of reconciling the idea of a rather recent Creation with the great changes needed to allow such fossils to be found hundreds of miles from the present seas. In Northern Europe the supporters of the hypothesis of incomplete development *in loco* were in the majority, though, for instance, Albert of Saxony († 1390) was a supporter of the 'Flood theory'.

Leonardo had indeed thought of a radical answer, holding that geological times were extremely long and that the sea floors themselves had gradually emerged.

Indeed in Italy the vast majority of fossil-bearing localities are comparatively recent and their fossils are rather similar to living animals.

We have also mentioned how some important scientists, such as Falloppio, resolutely denied the nature of biological remain to the fossils.

In the 17th century we have seen how Father Kircher, in order to explain insular faunas, had assumed temporary emergences and links between continents and islands.

An opponent of the biological origin of fossils was Stelluti, whom we have already mentioned. Stelluti in his brief *Trattato del legno fossile*, which he wrote in order to complete a study begun by Prince Cesi, and which is the very first monograph on fossil fuels, maintains, in a Paracelsian mood, that brown coal was a kind of earth, which, by the action of subterranean heat and sulphuric waters had been progressively modified so as to resemble wood. As it happens he was right as far as sulphur was concerned as all Italian fossil fuels are, unfortunately, rich in sulphur (but apparently Stelluti was thinking simultaneously of mineral sulphur and of the 'Paracelsian' sulphur and its supposed proprieties).

We have seen how in Germany and France the organic nature of fossils had been strongly advocated by Georg Bauer (= Agricola, 1494-1555), who, however thought that the shark teeth known as *glossopterae* were mere minerals, and by Bernard Palissy (1510-1589). Agricola is responsible for the new term *fossilis* which, however he derived from the Latin *fodere* = to dig, and applied to any product of excavation.

In the early 17th century Giulio Cesare Vanini (1585-1619) from Taurisano believed in the transformation of species. Very few of his works survive and these were published shortly before he was burnt as a heretic (*Anfiteatro dell'Eterna Provvidenza, divino-magico, Cristiano-fisico nonché astrologico cattolico = The amphitheatre of Eternal Providence, divine-magic, Christian-physic and also astrologic-Catholic*, *De admirabilis naturae arcanis = The wonderful mysteries of Nature*). There he extends to animals the idea, which was then commonly accepted for plants, that species could change their aspect and structure. Vanini's theories, anyway, had no relevance for successive developments.

The real founder of geo-palaeontological sciences is Steno, who also proposed some basic principles of method. His *Prodromus de solido intra solidus naturaliter contento* (= *Introduction on the solid which is naturally included in the interior of a solid*) was prepared during his last stay in Tuscany. It has a penetrating discussion of the alternative possible theories about the origin of the different kinds of fossils, and Steno was helped to reach the right conclusions by the peculiar situation of the Arno valley close to Florence, the nature of which as a former lacustrine basin is absolutely clear. So Steno was able to propose a convincing model of the formation of sediments and of the fossils thereof.

Steno also took advantage of his studies on recent sharks and proved that the *glossopterae*, which were also used by apothecaries for their concoctions, were fossilised teeth of sharks. There, however he arrived a good second as Fabio Colonna (1567-1650) had already arrived to the same results in his *De glossopteris* of 1616.

Another scholar who arrived to the same conclusions as Steno and that almost at the same time is Agostino Scilla (1639-1700), who provided excellent figures of fossils and stressed the significance of stratification.

Also Robert Hooke must be listed among the 'founding fathers' of palaeontology: he repeatedly made sound statements about fossils. However his main contribution *A Discourse of Earthquakes* though written between 1686 and 1689, was printed only in 1705: It includes a very good discussion and two excellent plates on Ammonites.

As we said the argument on the origin of fossils went through the whole century and still at the its close many still held by the idea that fossils were naturally generated inside the rocks like crystals. One such was the Welsh Edward Lhwyd (also written Lwyd, Lloyd and Lhuid, 1640-1709), who developed an idea by Libavius (1560-1616) and maintained that there was an *aura seminalis* being blown inland, this would be taken to Earth by rains and infiltrated into the rocks and that, where it found conditions partly suitable for development began to produce new organisms, which, however, could not complete their development. Similar ideas were proposed at the beginning of next century by the Swiss Karl Nicolaus Lang (1670-1741), who had studied in Bologna and Rome and both these scholars produced quite good geological correlations, as they thought that similar rocks would produce similar fossils! More or less the same ideas were maintained in Germany by Elias Camerarius (1672-1734), in England by Martin Lister (1638-1711), who produced the first idea of a true geologic map, in Italy by the chemist Giorgio Baglivi (1668-1707).

Leibniz, in his *Protogea*, partly published in 1683, but which was published complete only after Leibniz death, thinks that there were several 'floods', possibly occasioned by the collapse of immense caves full of water and that when such catastrophes did occur it was possible for species to undergo some changes. Then is his *Nouveaux Essais* he writes, while dealing with his *Law of continuity*: "It is possible that in some places, and occasionally, in the past, at present and in the future, animal species were much more apt to change than we can presently observe. Again I must say that our classifications are just provisional, and just agree with our present knowledge" and adds further on "Some people go so far with their daring speculations, that they say that in the time when Ocean covered the whole Earth, the present terrestrial animals lived in water, that later they became amphibians. Finally their descendants were no more able to live in their original home. But these thoughts run against the Sacred Scriptures and it would be sinful to stray from them". And in this last sentence he may just be thinking of Vanini's ideas.

Just as a matter of curiosity: the first reconstruction of a fossil skeleton is just to be found in a posthumous work by Leibniz and it reproduces a drawing of the skeleton of a 'unicorn' made by Otto von Guericke and based on the bones of a Mammuth.

The problem of contagion

The 17th century was a century of devastatingly epidemics. Apart from those that could be rated as 'the usual ones', practically every country was scourged two or, more commonly, three times by serious epidemics of bubonic plague. Wars certainly fostered it, but climatic deterioration also lent a hand, as did the by product of war and climatic deterioration: famine. Even some of the people mentioned in this chapter, such as De Graaf, died of the plague. Thus the problems of contagion were hotly debated and sanitary police measures were desperately attempted. Commonly the Authorities responsible for public health (in Italy each main town or state had a permanent *ad hoc* committee) tried to enforce quarantines, burning of goods and even houses of diseased people, isolation of patients and their families, control or closing of trade with infected areas. Indeed almost everyone was convinced that there was a material cause for the contagion. Many of the people mentioned in this chapter, as they were mostly physicians, wrote on this problem. Gradually the idea that contagious diseases were due to invisible little organisms, usually called 'worms' took a foothold. This was rooted in Fracastoro hypothesis of the *seminaria viva* and was being buttressed on one side by the discoveries concerning the large internal parasites (see Redi's, Vallisnieri's and others' contributions: if there were large parasites, why not also small?) on the other by the discovery of microscopic organisms (actually Leeuwenhoeck even saw some bacteria), which either could be transported by air, transmitted by contact or, why not, be self generated? The hypothesis was obviously controversial, but it was strongly advocated, for instance, by August Hauptman (1607-1674) who published his views in 1650, by Pierre Jean Fabre, a graduate of Montpellier (1588-1658), a medicine-alchemist, who, apart for his work on bubonic plague wrote only alchemic treatises where he even compared alchemic operations with the different sacraments of the Church) and by the ever present Father Athanasius Kircher in his *Scrutinio physico-medicum contagiosae luis, quae pestis dicitur* (= *Physico-medical discussion on the cantagious disease, which is called the plague*, 1658), where he maintains that either all or at least most contagious diseases begin by the 'putrefaction' of the blood or of some other humor of some individuals, this produces the birth of microscopic 'worms', that afterwards spread from one individual to another either by direct or by indirect contacts. Indeed Father Kircher is an excellent example of how a scientist may be quite wrong on one subject and on the right track on another.

Concluding remarks

Biological sciences did indeed achieve very brilliant results during the 17th century, but, as a whole their development was slower than that of astronomy or physics. Quantitative methods were being developed, but a real understanding of their results demanded an adequate corpuscular theory, which Cartesian mechanicism did not provide. The importance for the development of our discipline of the proof that vacuum exists, was also opening new avenues for research in biology thanks to Torricelli's vacuum, Von Guericke's and Boyle's-Hooke's pumps. However, especially the development of physiology was unquestionably hampered by the lack of an adequate understanding of chemistry.

CHAPTER IX

The 18th century before the french revolution

MAIN HISTORICAL EVENTS

1701-1713/14 war for the Spanish succession.
1737-1738 war for the Polish succession.
Halley 1656-1742, Vico 1668-1744, Berkeley 1685-1753, Franklin 1706-1790, Euler 1707-1783
1741-1748 war of Austrian succession.
Hume 1711-1776, Boskovič 1711-1787, Cavendish 1731-1810, Lagrange 1736-1813, de Coulomb 1736-1806, Kant 1724-1804
1756-1763 Seven Years war.
1768 beginning of the American Revolution.
1776 declaration of Independence of the United States.
1788 Louis XVI convenes the 'Etats Generaux'; they actually meet on May 5, 1789.
1792-1815 wars of the French revolution and of Napoleon's Empire.

Some general features of this period

During the 18th century the evolution of all sciences generally increased its tempo, and biology was no exception: both the number of scholars and the number of teaching and research facilities increased and thus also did the number of books and of journals published. It is also increasingly difficult to subdivide any account of such development under distinct headings, as most scholars deal with quite different subjects more or less at the same time. To compound the difficulty of a clear and orderly treatment of our subject, not only the different research activities of each scholar often overlap, but so does the progress of the different sciences and this influences the activities of the individual scholars.

From a merely historical standpoint we must also depart from a strict periodicity by centuries: such a scansion is always questionable, but the French revolution involved such an abrupt change in the social context within which all cultural and scientific activities were developing, that scientists working in the last decade of the century, appear indeed to be much closer to their successors of the next century than to their colleagues of just a few years before. Even such scholars as Lamarck, who had been educated and had developed a considerable amount of their studies during the 'Ancien Régime' are in fact closer to their successors than to their masters. We shall obviously consider all this in more detail in the next chapter, where we shall deal with the greatest biologists of the times of the Revolution and of the Empire. The curious coincidence that the death of Buffon, one of the leading figures of the 18th century biology occurred just a few month before the beginning of the sequence of events of the French revolution gives us a natural date for the close of this chapter: 1788-1789.

A first important characteristic of the period covered by this chapter is the way by which scholars tackle the problem of such evidence that appear to be contrasting with the traditional religious doctrines.

A considerable tolerance for religious differences can be seen to spread more or less throughout Europe. This may well be the result of disgust at the bloody religious strife that had beset the preceding century, but certainly during the 18th century the political influence of the Churches, be they Catholic of Protestant, is generally eroded by the, so called, 'jurisdictionalist' policy of almost all states, which tend to limit the privileges and influence of the Churches, both Catholic and Protestant, also in such aspects of life in which the churches jurisdiction had been traditionally prevalent. It must be noted, however, that France and Spain were slow to move in this direction and this was one of the reasons of the prevailing confused legislation of these countries, which worsened their budgetary situation and was one of the causes of the French revolution.

Scientists, however had to deal with their own religious problems. Whereas some of them, usually the more specialised ones, seem indifferent to the problems of possible conflicts between sciences and the traditional and basically literal interpretation of the Sacred Books, a few moved to openly atheistic and positivist positions, whereas most, as we shall see, adopted some sort of theism, which assumed a Wise Creator (The Great Watchmaker), who had generated a rational universe, which by its own rationality invited study. This was ruled by the Natural Law that God had granted and which, obviously by His own will, He was bound to follow. Quite a few clergymen, including several members of the Anglican High Church, as well as Catholic and Lutheran Bishops appear, in their writings, to follow a somewhat dualistic creed: on one side they venerate a God who is the creator and legislator of the universe, and who looks very much like Aristotle's 'unmoving motor', who allows or even demands as an act of veneration the empirical study of the laws and order of Nature; on the other Christ, who deals only with the problems of daily human life and human relationships. Thus the scholar usually does not worry about Christianity when at his study desk, but almost always is profoundly conscious of this Supreme Ens, who has so wonderfully ordered Nature.

For this purpose we must consider as significant the arguments between the Newtonian and the Leibnizian schools, that went on for almost the whole century, and which in the second half of the century was even tinged with politics. Both Newton and Leibniz were deeply religious men, albeit their faiths were very different. Their quarrel, as it is well known, sprung from their claims to the discovery of calculus (and the discovery of mathematical infinitesimals was to have a deep influence in biology on the debate on reproduction).

Newton had been a faithful Anglican[1] and his theories were well received by the Anglican Church. In France Newtonians were prevalent partly because of the influence of Montesquieu and of Voltaire, who were strongly Anglophile for political reasons, and partly because it was much easier to convert from Cartesian mechanicism to Newotnian models than from Descartes to Leibniz. In Germany and Italy predominates the influence of Leibniz; for instance some Italian Universities, such as Padua, repeatedly asked Leibniz for advice about the selection of their professors.

The different intellectual movements of the 18th century are often dubbed 'enlightenment', as their leading figures often claimed to live or to inaugurate 'the age of enlightenment'. However there were very different trends in the different countries and these were largely the result of the local political situations.

The 18th century was an age of great technical developments, of economic expansion and of cultural flourishing. A prime motor which activated the intellectual debate was the problem of the badly needed economic reforms. All States, sometimes for different reasons, had to deal with the problem of updating their economy and administration, including money and finances, to a rapidly evolving situation. As a result bookshelves were flooded by studies and proposals for the rationalisation of this or that aspect of trade and finances. Both the theoreticians of the traditional mercantile school, upholding rather closed markets, protected by strong tariffs, and the champions of the new liberal theories were aware of the need to dismantle the traditional economic organisation.

However everyone was aware that rationalisation in economy involved rationalisation in the governmental and administrative fields, in schooling and so on.

The result of the debate was that, whereas basically everyone in Europe praised the developments of the economy and of the liberties in England, what happened in practice was that the English model continued its regular development in England and to some extent in Holland, Sweden and Denmark. This was possible because there the parliamentary system was well established since the previous century. In England this had been due to the 'Bill of Rights' of 1685, the 'Glorious Revolution' of 1688, etc., and because the 18th century was a period of steady economic and colonial expansion (even the loss of the 13 American colonies was offset by the acquisition of Canada and the final supremacy in India).

In Prussia, Austria, and, on the Austrian example, in Tuscany there were energetic reforms, but they were joined with an increasing power of the sovereign. In France there was confusion without reform, and the well meaning efforts of Louis XVI were effectively obstructed by the blind obduracy of the provincial parliaments, dominated by the local (and often recently ennobled) lesser aristocracy and by the high clergy. In the minor states and especially in the Italian ones, the political weakness of

[1] It has been claimed (cpr. M. White) that Newton was secretly an Aryan, however, the evidence seems inconclusive.

small nations, continuously threatened by powerful neighbours, and who could not find ways to economic development and to redress their budgets, ended, somewhat like in France, in the relative failure of the attempts to reform, which were either blocked by conservatives, or because of failure in co-ordination. However the effort of both the German and Italian minor states to promote sciences and especially applied sciences was admirable: for instance the Republic of Venice funded the translation and distribution of the complete collections of the 'Philosophical Transactions' and of the 'Mémoires' of the French academy (over 140 volumes), and equally funded the organisation of over 30 local Academies mainly aimed to develop both research and education devoted to the modernisation of agriculture. Many German states did the same, while in Tuscany the more centralised 'Accademia dei Georgofili' was so successful that it is still extant and quite active. Any objective scholar has to admit that the European governments previous to the French revolution were certainly not democratic in any modern sense, but, apart from a few exception, they were certainly liberal, scientifically minded and well meaning, anything but the myopic conservatives as the French revolutionaries painted them.

Thus, while most of the scholars of the 'Age of enlightenment' were moderate conservatives and the advocates of parliamentary monarchy, in practice, both in France and Italy, they became more and more exasperated by the immobility and blunderings of governments and, therefore became, their more and more radical critics.

In Northern Europe most of the philosophers of this age were, as we said, some sort of Deists in the lines of English Deism, while in the Catholic countries, in which the Church still retained the control of pre-university schooling, they, as in the typical, albeit extreme, instance of Voltaire, tended towards an anti-clerical and anti-Christian tinge. Their writings were filled with praise to the Creator and to Divine Providence, while Jesus is left for the poor country parsons.

Thus the French and the French-inspired scholars were enthusiastically 'scientists' and incredibly sanguine with theories which, given the little evidence available, could well be excellent as working hypotheses, but that were far from having been sufficiently substantiated, though their authors believed them to be absolute truths and thus used as propaganda items.

The aggressive behaviour of the 'reformers' prompted a progressive reaction by the clergy. Whereas at the beginning of the century pope Lambertini was promoting research in his Bologna with the most liberal spirit, the official attitude of both the Catholic Church and the Reformed Churches was increasingly suspicious of novelties.

In this context it is significant to consider the development of the curricula in the high schools. I have had the opportunity to see only those of several Lombard schools run by different religious Orders. They all stress Natural Theology and Natural Rights, the curricula of mathematics and physics are both strong and extremely advanced, among modern philosophers there is a marked preference for Leibniz and they continuously stress the need to fight the 'errors of the fashionable ideas'!

Anyway, the anti-Christian attacks launched by the French extremists pushed both the Catholic and the Protestant churches into an extreme conservatism even in sciences, and we shall deal with it in the next chapters.

Another important factor in our history is the powerful development that geographical explorations had during the 18th century, and which was always accompanied by the zoological and botanical exploration of far away lands. This is just the development of the explorations of the previous centuries, but during this century it is to some extent ruled by the peculiar political and commercial rivalry between France and England, who were most of the time crossing swords in India, North America and on all the high seas, while the minor powers were trying to acquire some little spaces for themselves while avoiding provoking the 'big ones'.

Finally, before closing these general considerations, it is necessary to stress that as the number of scholars and of publications increases, so increases the difficulty of an objective choice of the scholars and publications worth quoting in a book like this. As, obviously, any scholar has access to different bibliographical facilities, any Italian researcher has no problem in consulting Italian authors, while, just to take an example at random, he is almost bound to miss some relevant Swedish work. It is thus almost unavoidable that some of my choices and quotations will appear to be subjective and, perhaps, objectionable.

Descriptive biology in the first half of the century

Antonio Vallisnieri senior (1661-1730) was born in Trasillico, a tiny village, which was the fief of the Vallisnieri, in the mountains of Garfagnana, then part of the Duchy of Modena (at the time the valley of Gerfagnana was divided like a chessboard between the Grand-duke of Tuscany, the Duke of Modena and the Republic of Lucca). The Vallisnieri were puny feudal lords, almost as poor as their vassals, who made a living from charcoal and chestnuts. Vallisnieri studied in Bologna with Malpighi, who had a high opinion of this youth, but, as it was forbidden to any subject of the Duke of Modena to study abroad (Bologna is 37 km from Modena), he graduated in medicine in the University of Reggio Emilia (later abolished). He worked as a general practitioner until he was appointed as a professor at the University of Padua in 1700 on the evidence of a single paper. Almost all his papers were published during the 30 years that he was in Padua.

Vallisnieri in his papers *Dialoghi sopra la curiosa origine di molti Insetti …* (*Dialogues on the curious origin of many insects …*) (1700) and *Esperienze ed osservazioni intorno all'origine, sviluppi e costumi di vari insetti, con altre spettanti alla naturale e medica storia* (*Experiences and observations on the origin, development and habits of different insects, together with others pertaining with natural and medical history*) (1713), was able to show that even the gall-insects, reproduce by eggs, while Redi, who had not been able to make any conclusive observation, had, with many reservations, allowed that they could be spontaneously generated. Vallisnieri reached the same conclusions for the internal parasites of animals (*Considerazioni ed esperienze intorno alla generazione de' vermi ordinari del corpo umano*, 1710, 1726 = *Considerations and experiments on the reproduction of the common worms of the human body*). This, however, left him in a quandary: as such worms cannot live outside the human body, they must have been created together with Adam, but how is it possible to reconcile the perfect life of Adam and Eve in the Eden with the fact that they must have been crammed with worms of all kinds: roundworms, tapeworms etc:? Vallisnieri argues that when Adam was living in Eden the worms were useful (we would say that they were symbionts) and that they became nasty parasites only later on. This was an obvious evolutionary assumption, but its significance escaped everyone, including Vallisnieri himself!

Some scholars have credited Vallisnieri with proto-evolutionist ideas because he often refers to the *scala naturae* and that he gave a basically correct assessment of fossils. But this is certainly mistaken. Vallisnieri's opinions on fossils were almost the standard ones in Italy at the time, while Vallisnieri was an absolute supporter of *preformism* and a *preformist* cannot conceive of an evolutionary process. People like Leibniz could conceive of important transmutations, but rule them out as contrary to the Scriptures; an heterodox protestant like Bonnet could conceive of a metaphysical design which materialised by successive abrupt transformations, but they were not committed like Vallisnieri senior to a coherent scientific theory which implied fixism. However both Vallisnieri senior and his son Antonio Vallisnieri junior (1708-1777), who held the chair of Natural sciences in Padova after his father's death, were strongly opposed to the theories which attributed the fossils to the Noachian Flood and always maintained their great antiquity. Their considerable influence was certainly helpful in the development in Italy of very advanced geo-palaeontologic theories, as we shall see further on.

Among the morphological papers of Vallisnieri senior, those on the Ostrich and on the Chameleon are particularly good. Even more significant were his studies on the reproduction of the plant that Linneus named after him: *Vallisneria.*

Curiously Vallisnieri took at face value a curious pamphlet by a 'Dalempatius', actually the anagram of 'Plantadius' (De Plantade, a good French botanist). 'Dalempatius' had figured some sperms with a little man inside and claimed to have seen this coming out of a sort of envelope of the sperm's head. The paper was a joke aimed to the 'spermatists' or 'animalculists', but Vallisnieri, who was a committed 'ovist', fell for it and seriously discussed such 'evidence' in his paper on reproduction.

Slightly junior than Vallisnieri is Réné-Antoine Ferchault, sieur (= lord) of Réaumur, of the Alpes and of the Bermondière (1683-1757), by far the most important French biologist before Buffon. Young Réné-Antoine, studied law in Paris, but as he was very rich, never practised it and, instead, devoted himself to Natural History in a very broad sense. His studies range from the manufacture of pins, anchors or the preservation of eggs, to studies on mineralogy and metallurgy totalling a grand total of 7 big volumes (a further one was published during the last century)! His practical contributions earned him a special pension from the king, and Réaumur took the necessary measures to guarantee that, at his death, this pension would be inherited by the Academy, and used to foster applied research.

One of the merits of Réaumur was to verify, either personally or by his collaborators, any potentially significant contribution. He thus became a sort of European 'referee' and, just to take two examples: the discoveries by Trembley on *Hydra* and of Bonnet on the parthenogenesis of Aphids, became instantly famous as soon as Réaumur validated them.

Here is just as an attempt to survey Réaumur's studies as a biologists: apart his verification of Trembley's studies on the regeneration of *Hydra*, he studied the regeneration of the legs in the crayfish, he described anew the anatomy of *Torpedo* (totally ignoring Lorenzini's contribution), the secretion of the shell by the Mollusc's mantle, the movements of several invertebrates and the secretion of purple and bioluminiscence. Finally, his most important contributions are, according most scholars, in the *Mémoires pour servir à l'histoire des Insectes*. Réaumur himself published six volumes (totalling over 4,000 pages) of this gigantic work, and another two volumes were published after his death from the notes that he had left. As it was usual at the time, this volumes deal also with several animals that presently we do not deem to be insects. Réaumur considers methodically not only the morphology, but also the development, the physiology and the behaviour of all the animals studied. For instance he was the first to use glass bee-hives to study the behaviour of Bees. He also made basic contributions to the study of parasitic insects.

Another important entomologist was Karl De Geer (1720-1778). He belonged to a rich and noble family of Dutch origin and, as it was traditional in his family, he studied in Holland. His improvements in the techniques of his iron producing firm made him the richest gentleman of Sweden and he was knighted a baron and Court Marshall. A true philanthropist, he improved the salaries of his workers and developed schools for their children. He was an amateur entomologist and took as a model Réaumur to the extent that he titled his works *Mémoires pour servir à l'histoire des Insectes*. The first volume was published in 1751 and another six were printed after 1771. As a whole he provides an excellent study of some 1500 species and here and there amended some mistakes by Réaumur himself.

His earliest contribution precedes the famous 10th edition of Linné's *Systema Naturae,* but, in fact, he never adopted the binomial Linnean nomenclature and his books had a limited circulation, so that his influence was much less than it deserved.

One who, instead, became famous by practically a single discovery was Abraham Trembley: a Swiss from Geneva and of distant French origin. Trembley was also a cousin of Bonnet, of whom we shall have much to say further on. He became the private tutor of the children of a British noble family who had settled in Holland. Thus he spent his leisure studying the fauna and flora of the local canals. Using a simple microscope, he made a number of observations, that he regularly reported to Reaumur. So he discovered the Green Hydra (*Hydra viridis*), that small Coelenterate, hardly one cm long and 1 mm thick, that all students learn as 'the example' for the Coelenterate polypoid morphology. He studied every aspect of the biology of *Hydra,* but his truly sensational discovery was that one could cut a Hydra to pieces and that each one of them was able to regenerate a complete, smaller, polyp, which later grows to normal size. Moreover he found that *Hydra* often reproduce by budding small hydras, which eventually become detached from its parent, and that, by careful, cuts one could obtain hydras with many crowns of tentacles and many mouths, and, finally, that if one carefully gets into the mouth of a hydra a hair with a knot at one end, then, by a gentle pull, it is possible to reverse the hydra, so that the inside becomes the outside of the animal. Such upturned animals, after a while start again to eat and live normally (in fact the entodermal cells migrate and the animal reorganises itself). Réaumur's tests of these results gave Trembley a great publicity, he passed shortly afterwards into the service of the Duke of Richmond, and with him travelled through Europe. Finally, in 1757, he returned to Geneva, married and dedicated himself to studies on education. He, nevertheless kept in touch with several naturalists (for instance he accompanied Spallanzani on several excursions).

The figures of Trembley's papers were drawn by another amateur naturalist, who belonged to one of the many Huguenot families that left France when Louis XIV revoked the Edict of Nantes. This was Pierre Lyonnet (1707-1789), by profession a lawyer who had such command of many languages that he was often employed as an interpreter or translator. Lyonnet's activities, include, apart from his original papers and the preparation of Trembley's figures, the development of an advanced simple microscope, which was largely used by a number of scientists, including Spallanzani, and his co-operation with Friederich Christian Lesser (1692-1754), whom he provided with both figures and comments for his *Insectotheologia* and *Testaceotheologia*. These are compilations, which belong to a widespread category of educational books which, while extremely up to date as far as information goes and equally well illustrated, were planned all over Europe by both Protestants and Catholics, to illustrate the marvels of the creation by the 'Great Watchmaker'.

The scientific contribution of Lyonnet consists of a single monograph: the *Traité anatomique de la Chenille qui ronge le bois du Saule* (1760), a monograph of the larva of *Cossus ligniperda,* an excellent work with splendid figures. This was to be completed by the study of the adult, but this was never achieved.

As we said the whole of the 18th century saw the publication of a number of 'popular' books of sciences which often include also some original observations. Even the great Treatise by Buffon belongs to this category.

Among these books the 'Insekten Belustigungen' (1746-1761) by August Johann Roesel von Rosenhof are generally considered to be especially valuable. Roesenhof was a well known painter of miniatures. Having once seen the famous paintings by Sibylla Merian (see chapter VIII), he thought that it could well be a profitable business to produce a series of illustrated albums on insects. Thus he begun to breed insects, other invertebrates and a few vertebrates, much to the annoyance, if reports are reliable, of his neighbours in Nürnberg. His booklets, which title literally means *Pastimes with insects*, are a good deal more important than they promise. They include a number of new observations, especially concerning the reproduction and development of Amphibians.

Another notable describer of small invertebrates is Otto Friederich Müller, (1730-1784) from Copenhagen. He had studied both theology and laws and worked both as a teacher and as archivist. However, he is chiefly remembered as the first to propose a classification of the infusorians and for his description of some bacteria.

There were several more scholars whose merits are more or less the same as those of the others named so far, but it is better to turn to the two leading figures of the 18th century biology: Linnaeus and Buffon.

Linnaeus and his pupils

The family of Linnaeus was originally one of farmers, but his grandfather had become a Lutheran parson and his father, Nils Ingemarsson was also a parson and took the surname Lind from that of a tree in his garden. Karl Lind was born in Råshult in Sweden in 1707. I have the impression that the Augustinian-Lutheran tradition of his family may well underlie some of Linnaeus' ideas on classification. When a famous and ageing man Linnaeus wrote four autobiographies (writing like Caesar in the third person). There is no doubt that Linnaeus' family was poor, but may not have been as destitute as Linnaeus describes it. As tradition goes Linnaeus' father was worried by the poor results that Karl was getting at school, and thought to make him an apprentice with a cobbler, but a Doctor Johan Rothman, who doubled as provincial physician and one of Karl's schoolteachers, persuaded him to send young Karl to the University to study medicine (at that time there were no matriculation test to pass and some people went to University when merely 13 or 14). So Linnaeus went first (1727) to Lund, where his qualities were immediately appreciated by one of the teachers, Stobaeus; and, next year, he went to Uppsala. As for the times, Linnaeus went to University later than most pupils; however he had been interested in plants since he was a boy and, as soon as he was at university he plunged into a furious activity. Leisure enough he had, since at that time the professors gave very few lectures and so the willing student had plenty of opportunity to study even when he duly attended such lectures that were actually given. The efficiency of the faculty was such that though they had a regular curriculum in medicine, young Linnaeus, in order to see an 'anatomy', had to travel to Stockholm! Just as in Lund, in Uppsala Linnaeus won the immediate recognition of the best professors. His earliest patron was Olaf Celsius, theologian, botanist and historian; Celsius got a small scholarship for Linnaeus, who, in return, collaborated in Celsius' *Hierobotanicon* (a book on plants mentioned in the Bible), published in 1745-47. Later he dedicated to Celsius an essay on the sex in plants, which was first circulated in manuscript, and later was printed twice. The second, expanded, edition was revised in co-operation with Wahblom in 1746 with the title *Sponsalia plantarum*. By then Linnaeus had already published the *Hortus Uplandicus* (1730) and a paper that was published under the name of Olof Rudbeck junior. Rudbeck was then an aged professor and had been exempted from lecturing; so he first paid Linnaeus some money for saving him the trouble of writing the paper and then proceeded to appoint Linnaeus as 'demonstrator' with a small salary and unloaded on the young botanist all his teaching duties. Linnaeus was an immediate success as a teacher and, in his new capacities, immediately set to work on the reorganisation of the botanical gardens and the collections, the former being at, the time, almost forlorn and the latter practically non extant, as such collections that had existed in the past, had been lost in the great fire that had ravaged the town some years before. In 1732 Linnaeus gave a first proof of his abilities as a 'persuader': he had planned an expedition to Lapland and he got the Royal Scientific Society to sponsor it to the extent that, according tradition, he left the Society with exactly 1 crown and 17 pence. Yet all the money that he could get was still less than the 2/3 of the total amount that he had estimated necessary for an expedition of two people, and so Linnaeus went alone. It was a hard job, but the results were excellent and Linnaeus spent the next two years studying his collections and, in order to square his budget, privately lectured on chemical analysis, which was then a new subject and thus it was fashionable. Actually Linnaeus began to study chemistry just in order to give his lectures. Meantime Linnaeus made some rather long stays in Falun (where he was courting the daughter of the local physician) and to lead a short expedition of a group of students to Dalecarlia. The costs of this expedition were partly borne by the students themselves and partly by a Mr. Sohlberg. Shortly after his return from Lapland Linnaeus was able to publish some preliminary results of his expedition (the *Florula laponica),* while the complete *Flora laponica* was published in 1737.

1735 was a turning point in Linnaeus' life: Mr. Sohlberg was persuaded by the joint efforts of Sohlberg's son Claes and of his friend Linnaeus to send them both for a study trip to Holland. In fact Sohlberg senior, in the end, provided much less money than the amount originally promised, but the two friends were able to leave all the same. In order to get his passport Linnaeus passed the only examination in his whole academic curriculum: Theology. Just before leaving he was also able to get the consent of his beloved's father for their engagement and it is often said that he also got some money for his trip from his future father-in-law. On the way to Holland Linnaeus and his friend stopped some time in Hamburg where Linnaeus was a social success as a scientist, until he ran into trouble with the Mayor and had to leave in a hurry (he had exposed as hoaxs some extraordinary specimens in the Mayor's collection). In Hamburg Linnaeus made one of the few social mistakes of his admirable career: he claimed to be in touch with the famous Boerhaave, who, at the time had never heard of him; a local gazette reported admiringly how much such a young scientist was valued by the great man and this went close to seriously damaging Linnaeus when in Holland.

Karl's and Claes' problem was to get a degree as promptly as possible. So, on arrival in Holland, they travelled to the small University of Hardervijk. At the time there was a saying:

'Hardervijk is en stad van negotie

Men verkoopt er bokkig, blaubessen en bullen van promotie!'

(= Hardervijk is a commercial town: there one can buy herrings, bilberries, and doctoral degrees!).

In fact Linnaeus arrived there on June 17th, registered at the University the 18th, passed some prescribed tests, had his thesis printed (he had already written it in Sweden and was titled *On a new hypothesis on the origin of intermittent fevers*) and got his doctorate the 23rd, being thus authorised to practice and teach medicine anywhere in the world!

More or less at the same time he met Gronovius, a reputed botanist and showed him his first version of the *Systema Naturae*: 12 folio pages of tables planned as a complement of the *Fundamenta botanica* to be published later. Gronovius was so enthusiast that he immediately persuaded an English amateur, a Mr. Isaak Lawson, to join him in paying the fees for their publication!

Linnaeus was obviously anxious to meet Boerhaave and thus travelled to Leiden, but when he knocked at the door of the great man, Boerhaave, who, just by chance, had read the Hamburger journal reporting Linnaeus claims, flatly refused to see him. However in a few days Linnaeus not only managed to meet the aged man, but Boerhaave immediately appreciated the outstanding qualities of the ambitious young man so much that henceforth he did his best to help him. Thus he recommended Linnaeus to the Amsterdam botanist J. Burman, who, in turn, got Linnaeus into the service of Georg Clifford. Clifford, as chairman of the Dutch Company for the East Indies was an immensely rich and powerful person and had assembled a splendid collection and turned his gardens into a regular botanical garden. Thus Linnaeus set to work to classify and put into order his master's collections. He thus published in 1737 the *Hortus Cliffordianus*; again in 1737 he published the *Genera plantarum* and in 1738 the *Classes plantarum*. So, when he travelled to Paris in 1738, Linnaeus was already famous. So much so that it was said that he arrived at the Jardin du Roi while Bernard de Jussieu was just showing the students some plants recently arrived from America. Linnaeus thus sat quietly among the students and when the specimens where shown him, he remarked: 'These plants look American!' and de Jussieu then exclaimed 'You must be Linnaeus!'.

Later in 1738 Linnaeus returned to Sweden, married, and, as he did not immediately find an appointment, began to practice in Stockholm. He was immediately elected to membership in the Swedish Academy (and as usual with him, he set out most effectively to improve its activities). He also became a friend of Count Karl G. Tessin, who was the leader in Parliament of the powerful 'Party of the Hats'. Count Tessin first secured for Linnaeus an appointment as consultant for the navy, shortly afterwards he got for Linnaeus also an appointment as court physician and, finally, in 1741 Linnaeus was appointed professor of Physics and Medicine in Uppsala, a chair that he promptly exchanged for that of Botany. Henceforth Linnaeus left Uppsala only for brief visits to the court or for short teaching tours. In 1761 he was knighted in the Order of the Polar Star and took the name von Linné. In 1774 he had a first stroke, followed by others and was completely disabled. Linnaeus died in 1778. It is noteworthy that Linnaeus trained as botanists not only his son Karl junior, but also at least one of his daughters, Elisabeth Christine, who in 1762, published a paper on the nocturnal phosphorescence of poppies.

Before being struck by apoplexy, Linnaeus had secured the chair of Botany for his son Karl (1741-1783). Thus when, at the death of Linnaeus, Sir Joseph Banks offered to buy his collections for 1,200 pounds, Karl junior refused to sell them. However, when Karl junior died, the widower of Linnaeus senior offered the whole library, the collections and the manuscripts to Banks. Banks, at the time had no ready money available and thus he called on J.E. Smith. Smith persuaded his father to pay the necessary 2,500 pounds, and thence, in 1788 founded the Linnean Society of London, who still owns all of Linnaus's collections and papers.

Linnaeus produced an immense amount of work: well over 180 books and important papers. Moreover he had a number of excellent pupils. Among his most important works one may list the *Methodus plantarum* (1737), *Preludia Sponsalium Plantarum seu Nuptiae arborum* (1740), *Flora Svecica* (1745), *Fauna Svecica* (1746), *Philosophia botanica* (1751), *Species Plantarum* (1753), *Plantae Hybridae* (1760), etc. A peculiar problem is posed by the *Amoenitates academicae*, 10 volumes published between 1748 and 1785. Here, besides some unquestionably Linnean papers, Linnaeus reprinted under his own name a number of doctoral dissertations by different, mostly indifferent, students. These, naturally, had been originally published under the student's name and there Linnaeus appeared only as the *promotor* of the thesis. The question is: 'whose really is the authorship of these papers?', the answer is that, with but a few exceptions where the student did actually contribute something, these theses were entirely written by Linnaeus, but they do create some problems in matters of priority and one wonders as to the reasons that prompted Linnaeus to write himself the theses for which he was the *promotor.*

Naturally the most famous of Linnaeus' books is the *Systema Naturae secundum classes, ordines, genera, species, cum caracteribus, differentiis, synonymis, locis.* Linnaeus himself published 12 editions of it (the 10th of 1758 being the starting point for Zoological nomenclature, the starting point for botanical nomenclature being the *Species Plantarum* of 1753), while the 13th was edited by J.F. Gmelin.

Actually Linnaeus went on improving and expanding his system through all of his life.

Linnaeus was a fanatic of systematics, so much so that, using exactly the same methods used in the *Systema Naturae*, he published a classification of diseases (*Clavis medicinae,* 1766), a botanical bibliography (*Bibliotheca botanica,* 1736) and persuaded a pupil to publish a similar classification of medical drugs.

As Linnaeus work was to have a lasting influence on the development of biology and as the International Rules of Nomenclature are still largely dependent on the *Species Plantarum* and on *Systema Naturae,* it is important to consider in some detail both the principles followed by Linnaeus and his results.

First of all we must stress that, given the age and the evidence available, Linnaeus showed an uncanny flair for the good classification. Naturally he made a number of mistakes, including some really gross ones. He is at his best with the higher plants and with Mammals, at his worst with invertebrates.

The theoretical background of Linnaeus classification is a healthy synthesis of all the work done by his forerunners, while his text is often weak. He usually does not explain his methods, he states some sets of rules and these are quite clear, while where he argues his stance he is often unclear, but his conclusions are trenchant. In his writings there is a lot of common sense and quite often he goes by the fundamental principle that there is a divine design which is manifest in the *Scala Naturae* and that the scholar endeavouring to get at it is in some way *praying*. Some scholars, such as Cain, have recently maintained that Linnaeus was purposely unclear as far as his basic principles were concerned because he was afraid that, had he stated clearly his principles, they would have been condemned by the powerful Lutheran clergy as they were, to some extent, a development of Hermetic-Rosicrucian sources. There is little doubt that, in his youth Linnaeus was greatly interested in Hermetic-neoplatonic Christian thinkers as well as in white magic. His library includes a Lullian apocryph and several texts of combinatory; however, as in his writings one does notice also clear influxes of Spinoza, to label Linnaeus as a hermetist may well be too extreme.

In practice he works typically by a synthesis of combinatory methods in Leibniz's tradition and a strictly divisory logic and Linnaeus may be considered to be definitely a 'quinarist', that is that he considered the number five of very special significance and that it underlays a number of biological phenomena (Quinarism had a strong revival, as we shall see, in early Victorian times). He thus followed and improved on methods that, originating with Cesalpino, whom he often quotes, through Ray and others had become widely agreed. By subdividing the flowers and the fruits into their component parts, he identified 26 characters, which he called *Litterae vegetabilium.* These, by their different combinations by *numerus, figura, situs, proportio* determine the systematic position of a plant. On one side this is a typical example of medieval combinatorial and, at the same time, a good ancestor for some modern cladistic practices! His conclusions are offered most simply and well organised, and this was one of the reasons for their immediate widespread acceptance.

Linnaeus has been classified by different authors either a prototype 'fixist', holding species to be unchangeable in time, or as a sort of proto-evolutionist.

We have seen how the first theory envisaging the changing and multiplication of species was suggested by Father Athanasius Kircher. In his early publications Linnaeus absolutely rejected any hypothesis of transformation of species and his sentence is often quoted *'Species tot sunt quot formas ab initio creavit infinitum Ens'* (= *Species are as many as many forms the Infinite Entity created in the beginning*). However, already in 1750, well in advance to his fundamental works, Linnaeus was thinking in terms of a most peculiar 'transformism', somewhat on the lines of that later adopted by Buffon.

Linnaeus was indeed greatly interested in hybridisation (*Plantae hybridae*, 1751) and in its consequences. Already Theophrastus had mentioned the possibility of hybrid plants and a belief that such hybrid might become true 'new species' had been fairly widespread, in spite of the known low fertility of such hybrids. Thus Linnaeus gradually evolved the idea that God may have originally created plants provided with only their generic, or even only 'ordinal' characters and that fertile hybrids were the source of all the species. Indeed he argued that Amoebas, with their indefinite shape, were a pure 'medullar' matter, and that 'species' had a composite origin by the action of the different kinds of 'cortical' matter. In 1762 (*Fundamenta fructificationis*), having seen a mutation, he complemented his argument on the species arising from the interaction of different kinds of medulla and cortex, by assuming also some sort of influence by local conditions. All this, while definitely envisaging the probability of transformation in plants (and also in animals, though these posed several difficulties), yet has very little to do with evolution as a historical process. During this later phase of Linnaeus' scientific evolution, he did again rely on combinatorial principles. He supposed that God had created possibly only the archetypes of the orders and that in plants and probably in animals (he makes a number of parallels between plants and insects), the male principle was the cortex, while the feminine principle was the medulla. Later God had developed all the thousands of species by hybridisation (*Plantae hybridae,* 1760), Thus, in each genus the feminine medulla does not change, while the male cortex depends from the genus with which the female element has crossed and that determines the species. Such hybridisations must have happened in a very distant past. These ideas can be traced to hints in Cesalpino, but it is typical of Linnaeus to have extended it even to animals, including man and in his *Clavis medicinae* (and we must always remember that whenever the word *Clavis* appears in the title of a book, one must expect to deal with some sort of natural magic), he classified even the diseases into 'cortical' and 'medullary'.

As far as plants are concerned, and we must always remember that Linnaeus was basically a botanist, he held that, in principle, one should base classification of a single set of characters of an essential apparatus and on its variations. So, considering its significance for the continuation of the species, he chose the reproductive apparatus. Even his very earliest books, are amusing for the verbal acrobatics he used in order to avoid terms which could shock anyone's sensibilities, and, at the same time he was almost lyric when comparing the different structures of the flowers to the curtains, the cushions and so forth of a bed. Yet, even some fifty years later, the good Goethe was recommending that the 'vulgar dogma of sexuality' of Linnaeus' writings should not be made known to ladies and children.

The double influence of his classificatory principles, stressing discontinuity, and of his cortex-medulla theory, which envisaged, in fact a continuum between the morphologies of the different organisms were the source of a number of interesting results: his formal systematic is linear and made of distinct species, but his morphology envisages a network of connections.

Linnaeus, moreover, considering the difficulties in the placing of the fungi, the observations by the Baron von Münchhausen (actually the serious brother of the protagonist of the famous 'adventures'), who had claimed that tiny animals could be born from fungi (actually they were the protozoans now known by the Linnean name *Chaos chaos* and which developed from cysts attached to the mushroom) and the infusorians, pooled the fungi, a number of infusorians, and the smaller Coelenterates, such as *Hydra*, corals and the like, into a third 'kingdom' intermediate between plants and animals.

Anyway, though he had officially accepted the sexual parts of flowers as the basis of systematics, Linnaeus was too good a systematist not to notice that by consistently using only the sexual parts of plants for their classification one reached a systematic arrangement that was often conflicting with groupings done on the evidence of other, equally important characters. Actually he stated explicitily that any classification based on a single set of characters is necessary for cataloguing purposes, but is, nevertheless, artificial. Thus he suggested that such non floral characters should be properly considered, but, as he himself says 'under the table', so as to make such escapades as inconspicuous as possible. A wise suggestion indeed even for the modern systematists when he is tempted to rigidly follow some system.

Curiously Linnaeus conceived of a sort of social hierarchy of plants, comparing them, according their size and general shape, with the human social classes. Instead, he conceived the animals, merely as mechanisms for the control of vegetation. Indeed it is often forgotten that it was Linnaeus who clearly formulated the concept of the food chains and of a dynamic ecological balance, first in *Oeconomia Naturae* (1749) and later in *Politia Naturae*.

His basic choices were to paralyse Linnaeus when he came to the classification of those plant that had no visible sexual organs, such as mosses, lichens, fungi etc. and thus he threw all of them into the waste basket of the *Cryptogamae*. Likewise with the animals, though he had to acknowledge here that he could not find any single basis for their classification, nevertheless, as far as the Vertebrates are concerned, Linnaeus' classification is remarkably good, but he did discard into the waste-bin of the *Vermes* all sorts of miscellaneous beings.

Possibly the most lasting benefit of Linnaeus' activities is binomial nomenclature. We have seen how the practice of binomial nomenclature had been already advocated before to Linnaeus. Linnaeus himself adopted it but gradually, and, even in the 10th edition of the *Systema naturae* it is not yet completely developed. In this, as in *Species plantarum* each species is described as follows: first comes a single word, for instance *indicus* followed by the generic name *Elephas*, to which immediately follows a diagnose, that is the list of relevant characters that identify it. This system was developed by Linnaeus largely because he considered that the Genus was really the most significant of the taxonomic categories. The practical advantages of a consistent binomial system were immediately perceived by the scientific community and, in spite of severe criticism by eminent scholars, such as, for instance Buffon, it was soon generally adopted. In spite of its inadequacies, it is indeed so practical that, with minor improvements, it is still and it will continue to be, the basis of all scientific nomenclature of living beings.

It is worth emphasising here a peripheral aspect of Linnean nomenclature. The whole description of any species is, in fact assumed to describe the essential true nature of the being so named and described. But, according the Bible, Adam was charged by God himself with naming the animals and thus acquiring power over them. Thus the systematist naming and describing the species knows the essence of each species just as a new Adam and it is clear from the writings of Linnaeus himself, that he considered the systematist, and especially himself, as such and having a special relationship with God himself.

Among the many improvement on systematics proposed by Linnaeus, one of the most brilliant and daring was the inclusion of Man, Chimpanzee, Orang and Gibbons in the same genus *Homo* and the inclusion of *Homo* with all the monkeys, Lemurs and Bats in an order Primates. Considering the poor knowledge that was available at the time about these animals this was a remarkable example of serendipity. It was also, just as naturally, immediately challenged from the most diverse quarters. Buffon was particularly incensed by Linnaeus betrayal 'of this truth, which is degrading for mankind'. Indeed Johan Friederich Gmelin (1748-1804)[2] as editor of the 13th , posthumous, edition of the *Systema*, promptly removed man to a separate kingdom!

Many critics have charged Linnaeus with having used almost exclusively external characters to describe the various species, but one should consider: (a) that his two basic books were planned to provide a practical handbook for the identification of animals and plants, and what best, for this purpose, than external characters? (b) that,

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[2] The Gmelin family produced three notable naturalists: Johan Georg (1709-1755), the above mentioned Johan Friederich, and Leopold (1788-1853).

at the time, most of the taxa listed by Linnaeus, were known only from rather superficial descriptions, nothing being known of their internal morphology; (c) that Linnaeus vas primarily a botanist and that in plants there are not many characters, other than those used by him which may be easily inspected. (d) that Linnaeus had in very many instances to rely on such evidence as was published and this was mostly concerned with the exterior aspects of the animals; thus, just in the case of his genus *Homo,* the anatomy of man was adequately known, but for the the Chimpanzee he could rely only on Tyson's description, for the Orang was available only a partial description of the skeleton and for Gibbons there were no anatomical descriptions at all.

It was only too natural for Linnaeus to make a number of mistakes and some were soon discovered, while others became apparent only a long time after his death. For instance, as he did not admit external fertilisation, Linnaeus maintained that female fishes ate the sperm and that the sperm reached the ovaries by way of the gut. Again: he held that the sperms were mere oily or salty particles, set in motion by the heat of the medium, and other such errors might easily be listed.

However his work was unquestionably a considerable advance on any comparable previous one; moreover its structure was such that additions and emendations could be easily fitted into it without completely upsetting its framework and that was one of the main reasons of its immediate success among Linnaeus' contemporaries.

Linnaeus has also been descried as an 'Essentialist' and on this point I think the problem much more complex than it appears to Linnaeus' critics. There is no question that Linnaeus used a strictly aristotelean system of definitions; but, within this framework, he assumes that all potentially observable features of a being, apart from those that appear to be strictly individual, concur to give the 'essence' characteristic of a class, only that, for the purposes for classification, it is possible to rely for each single instance, on one or a few characters as these will be sufficient, by using a strictly divisive logic, to identify them.

Such an approach, rather than from Aristotle, is inherited by Linnaeus from his much admired Cesalpino. Moreover, just following through Ray both Bishop Wilkins and Leibniz, Linnaeus aimed at a universal language, which he, indeed, achieved to a considerable extent, as the International Rules of Nomenclature largely follow his pattern. On the other side his procedures, being influenced by combinatorial methods, stand elegantly midway between classic renaissance combinatorial practices of Lullian origin and modern combinatorials of cladistic pattern.

Linnaeus made also some notable contributions to palaeontology and geology. Indeed he was the first to recognise Trilobites as Arthropods and he also envisaged very long geological times in order to account for the uplifting of sedimentary layers of the Scandinavian peninsula, which he considered as part of a more general tendency to an increase of dry lands and which, to him, could explain the geographical distribution of animals (*Oratio de telluris habitabili incremento*): Linnaeus supposed that the animals and plants had originally lived in the Earthly Paradise, a tropical island in an immense ocean, thence the uplifting of continental masses had allowed for the dispersal of the different living beings.

There is no doubt that Linnaeus was blind to some major advances of contemporary biology, thus he completely misunderstood the growing evidence concerning microscopic organisms, and went so far as to deny that sperms could be living things!

It is probably this attitude that made such a person as Spallanzani so critical of the whole production of Linnaeus. On the other hand Spallanzani, though he had some interest in marine faunas, was essentially an experimental biologist, uninterested in systematics and comparative morphology. Such reciprocal attitudes were, unfortunately, lasting ones in the further development of biology and are still very much with us.

It is more difficult to understand the hostile reception of Linnaeus science by Bonnet, as the religious and philosophical outlook of the two were rather akin. Perhaps Bonnet, who was a very careful reader, saw in Linnaeus' work a latent 'A-Christian' background.

Linnaeus was also severely criticised by 'litterati' such as Voltaire, Diderot, De la Mettrie, but as these authors were actually totally incapable of understanding anything of biology, their criticisms may be safely ignored.

Much more interesting are the criticisms levelled at Linnaeus by Buffon and the evolution of this scientist's ideas. Buffon's criticisms are many and sometimes sarcastic. They clearly show that Buffon was incapable to understand both the practical value of systematics and how the Linnean system had a strong potentiality to goad zoologists into tackling very basic issues. The only thing that Buffon did well understand was that the Linnean system, implicitly, involved the idea of transformation. An idea that Buffon himself expounded in his late years, when he, at least, agreed on the Linnean concept of genus.

As we said, once Linnaeus had settled in Uppsala, his travels were quite limited, but he was able to train a number of enthusiast pupils, ready to risk their own lives (and quite a few in fact died) in expeditions to collect specimens for the Master, who was singularly able to get them funds and shipping facilities.

Before dealing with the pupils, we must, however, mention Linnaeus school-fellow and friend Peter Artedi (1705-1735). Artedi left Sweden for England and Linnaeus later met him by chance in Holland a few months before Artedi drowned in an accident. Linnaeus thereafter edited Artedi's *Ichthyologia* from his late friend notes and in all his subsequent works, as far as fishes are concerned, he faithfully followed his friend's outlines, including leaving the Cetaceans among fishes!

Among Linnaeus pupils the following deserve to be remembered: Peter Forskål (1732-1763) a Finn, who first studied in Uppsala and later at Göttingen and, as he did not find a job in Sweden, entered into the Danish service and died during an expedition in the East; Fredrik Hasselqvist (1722-1752), who explored the Middle East and died in Smyrna; Pehr Löfling, who worked for the Spanish government, first in Spain and then in South America, where he died. Carl Peter Thunberg (1743-1828) who, after exploring Japan, became professor of botany in Uppsala. Tänström who died in an island off the coast of Indo-China. The Finn Pehr Kalm (1716-1779), who first explored North America and later became professor of economy in Åbo; Pehr Osbeck (1723-1805) who was sent to China; Anders Sparrmann (1748-1820), sent to South Africa. Finally we have the greater two: Daniel Solander (1736-1782) and Johan Christian Fabricius (1745-1810).

Johan Christian Fabricius, a Dane, was one of the foremost entomologists of the 18th century. While Linnaeus considered as basic for classification the wings, Fabricius advocated the mouth-parts (Aristotle had considered both). Fabricius studies, quite apart from the description of a number of taxa, were most influential on the development of Insect systematic for a long time.

Solander's story is interesting: Linnaeus had formed such a good opinion of him that it is said that he planned to give him his elder daughter as a wife. There is an often repeated legend that Joseph Banks (1743-1820), when about to sail with Captain Cook and the *Endeavour* (1768-1771), requested Linnaeus to send him a good naturalist to help in what was to be the first scientific exploration of Australia, New Zealand, etc. and that Linnaeus sent Solander. According this legend, when they came back, Solander did not send a single specimen to Linnaeus, hoping to lure him to England to study the incredible collections made during the expedition. The story adds that Linnaeus was incensed and Solander never went back to Sweden and became the librarian of Sir Joseph. The truth is that Solander went to England with the heartiest recommendations by Linnaeus, in 1760 on request of Peter Collinson and John Ellis for the purpose of studying Collinson's collections. Once in England Solander was soon well received among both scientists and people of the upper classes. Linnaeus continued to promote the advancement of Solander and in 1761 proposed him for the chair of Botany in St. Petersburg and next year his own chair in Uppsala upon his retirement. Solander declined both and decided to stay in England, where his friends first secured him a minor appointment with the staff of the fledgling British Museum. Solander did, indeed, sail with the *Endeavour*, but that was in 1768, when he had been in England for almost eight years; nor wes he on Banks' staff, though Banks had obliged to obtain leave of absence for him from the British Museum. It was on their return that Solander finally became the collaborator of Banks.

Chronologically the last pupil of Linnaeus was Erik Acharius (1757-1819), a botanist who worked on Lichens.

Banks and British explorations

Joseph Banks, later knighted Sir Joseph, because of his close association with Solander, deserves being mentioned here. He was a very rich man by birth, but, as a wise and extraordinarily efficient administrator, was able to raise his income from about 6,000 pounds a year (in times when an average family could live in comfort with some 200) to some 30,000 and was the most efficient economic adviser of King George III. He was a considerate and generous gentleman equally fair with his friends, staff and tenants. His scientific value is difficult to assess as, having formed such a close association with Solander, when this last died, failing health and the many commitments prevented Banks from completing some apparently very important works. There is no doubt that Banks was a very competent botanist and an all round naturalist and collector, who had visited Iceland, Newfoundland and the surrounding region before sailing in the *Endeavour* with Captain Cook, during which exploration he developed his friendship and co-operation with Solander. Their collections were incredibly rich and included hundreds of new genera of both plants and animals and their study was not yet completed when Banks died. However the scientific importance of Banks lies not with his personal scientific achievements, which were not outstanding, but in the work he did during his many years as President of the Royal Society. He not only rescued the Society from a period of decay, due to the rather amateurish selection of fellows, but was a powerful influence in its development as an institution for the promotion of all sciences, in the development of Kew gardens into an institution which was soon to equal and even outdo the Jardin of Paris, and in the early development of the British Museum. In many ways his influence may be compared with that of Buffon: the gifted amateur and the great promotor of sciences. The basic difference being that while Buffon was a master of scientific prose, Banks' grammar and orthography were always rather shocking.

During the whole of the century the British, just like the French, the Dutch and, on a lesser scale the minor powers, sent a number of expeditions to map new regions and to explore their resources. Such expeditions were normally staffed with scientists and draftsmen charged to record all possible aspects of the natural history of the different countries. However, while the British Navy made an outstanding work at mapping and the astronomical observations were equally good, apart from the two expeditions where Banks was engaged as a gentleman-adventurer, that is as a sort of volunteer-paying guest, the British great naturalist-explorers were left for the next century.

Buffon and his school

Georges Louis Leclerc, later count of Buffon, was born in Montbard, in Bourgogne in 1707. His great-great-grandfather was a barber-surgeon, his great-grandfather was a physician, his grand-father a judge and his father was a high officer of the provincial administration, who had married an extremely rich woman, and Georges completed the family's social climb by entering the nobility. He died in 1788, on the eve of the French revolution, which was to kill his only son, who was beheaded in 1793.

Buffon, still as Leclerc, began his university studies first in the faculty of Laws, thence in Medicine at Angers, but had to cut them short in a hurry, having killed an officer in a duel. Duels were obviously prohibited, but if the culprits could keep at large for some time, they could not be prosecuted. Thus young Georges joined with Lord Kingston, a good amateur naturalist and his mentor, who was equally a naturalist and who were making the traditional 'grand tour'. Travelling through France and Italy in such company, Georges became interested in biology. He thence went to England and was promptly enrolled into the Royal Society, although he had not yet made any original research. At that time the Society was always ready to enrol a young gentlemen of promise. Having thus spent the time necessary to escape a trial, Buffon returned to France, and got busy publishing both translations of scientific works and original papers in engineering and technology. He was thus received as 'élève' in the French Academy of Sciences. During the following years he became a very successful industrialist, as he developed his proprieties in Montbard into a flourishing factory by using at its best the wood from his forests and some iron ores that had previously been regarded as poor. At the same time he successfully developed his connections with the upper classes and the court in Paris. Thus, in 1739, when barely 32, he succeeded in being appointed 'intendent', that is director, of the Jardin du Roy and the 'Cabinet' of natural history collections. By the way, in getting this appointment, he bypassed Maupertuis, who was much more qualified.

Henceforth Buffon acquired his ennoblement as a count and spent all his time caring for his own business and for his beloved Jardin. He was extremely successful in both activities. Under his direction both the Jardin and the Cabinet developed into the best such institutions in Europe both in the quality of the collections and equipment and in that of the staff. Buffon loved his Jardin and his Cabinet so much that he did not bother about the means to get what he wanted for them. The Jardin itself was extended to almost three times its original surface. So, when he decided that he needed to extend the Garden to the Seine, so as to be able to pump water from the river, he got the areas that he needed, in spite of the fact that the coveted area was a built one and belonged to the Abbey of St. Victor and therefore their sale was illegal. Nevertheless by a complicated legal arrangement involving an exchange of lands, he got the property and then proceeded to have all the buildings declared illegal and pulled down.

True enough, when Buffon died it was discovered that the whole institution was dangerously sailing on the immense debt of 606,026 livres, about one third being advances made by Buffon himself, probably in order to secure for his son his appointment, which entitled the director to the fabulous yearly salary of 12,000 livres and his lodging, which Buffon himself had embellished into a splendid residence. Buffon's son, an army officer, however, did not press his claims and the appointment went to the Marquis De la Billarderie.

Buffon was much admired by his contemporaries (Rousseau, in spite of his egalitarian tendencies, writes that when he went to see him, he stooped to kiss the lintel of his door!) and lived a quiet existance, though he was deeply affected by the death of his beloved wife and by the behaviour of his daughter-in-law, who left her husband and became the paramour of the Duke of Orleans (later Philippe Égalité who was also beheaded by Robespierre and company). Buffon died in 1788, just in time to be spared seeing the catastrophe which was shortly to wipe away his world.

As soon as he became director of the Jardin and of the collections and having an excellent staff he began to develop his grand projects. The staff he found was very good, but Buffon proved to be an excellent judge of people and always filled the vacancies with precisely the kind of people he needed. His first recruit was Daubenton, a notable scientist and who was to play a leading part in the turmoils of the Revolution.

So Buffon began to work on his major opus: the *Histoire Naturelle génerale et particulière avec la description du Cabinet du Roy*, that he conceived as a sort of updated Aldrovandi or Gesner. The first three volumes were published in 1749, the last (from 36 to 44) were posthumous and edited by Count De Lacépède, one of the hand picked collaborators of Buffon.

The work was richly illustrated and met with a considerable editorial success and abridged editions were printed until the end of the 19th century. There is no doubt that Buffon was an influential scientist among his contemporaries, but even greater was his success with the general public interested in Natural History

As we said, when Buffon took charge of the Jardin he found there several first class naturalists, but he went on recruiting the best as it appears from the following, incomplete, list: the mineralogist Hauy, the botasnist Desfontaines, the Abbée Bexon, count De Lacépède, Lamarck, Étiènne Geoffroy Saint Hilaire and L.J. Marie Daubenton. Buffon was also adept at recruiting into his staff also willing visitors, such as abbot Needham, whom we shall repeatedly quote.

As a scientist Buffon basically developed the main guidelines of the investigations made at the Jardin and kept under close control their development, while he left all the actual investigations to his staff. Meanwhile he was largely directly responsible for the writing and publication of volumes and papers, as, indeed, he is an acknowledged master of scientific French. His methods are aptly illustrated in a figure of the *Histoire Naturelle*, where one sees Buffon comfortably sitting in an armchair, and talking to Needham, while two unidentified members of his staff are busy, one with a dissection and the other is working with the microscope (actually the figure aimed to underline that scientific progress required both the gathering of macro and microscopic evidence and its elaboration into theory by appropriate discussion).

The first volumes of Buffon's book were quite successful, though, after a first flurry of sales, the editor went bankrupt and in 1764 Buffon had to borrow the enormous amount of 179,000 livres to purchase the remaining unsold copies and save them from destruction.

Buffon was busy with his treatise for well over 40 years and, naturally, his ideas changed meantime, as we shall especially see when dealing with Buffon as a transformist.

There are three subjects that deserve special discussion: the first are Buffon's ideas on the origin and development of Earth, the second is the order by which the evidence is organized (Buffon deliberately shunned formal systematics); the third are Buffon's ideas on the origin of life.

As for the origin of Earth, Buffon believes that it is a chunk of material which became detached from the Sun because of a planetary impact and that later became gradually cool. As for the Earth cooling Buffon made an elegant as well as absurd experiment: he had made in his factory several globes of different substances and of known volume, he then made them red-hot and measured the time needed by the different globes to cool; he thence assumed an average value as a unit for cooling and multiplied it by the volume of the Earth, thus getting an age of 74,832 years! Quite apart our present knowledge on the temperature of the Sun, on the intrinsic heat of Earth, sidereal temperature, and consequently the thermic gradient, and so on, as it is obvious that he could not take into account factors which were still unknown, yet he discounted also factors that he should have considered, such as the significance for cooling of the ratio between volume and surface and others. One wonders whether the whole 'experiment' was an elaborate hoax played upon unsophisticated readers in order to smuggle in the idea that the age of the Earth was vastly greater than the traditional one. Indeed, even if Buffon figures are ridiculous by comparison with modern estimates, at the times it was a sensational departure from the biblical account (bishop Usher had fixed the creation of Earth at October 26, 4004 bC and Newton, after a lengthy study, was basically in agreement). Buffon's dating was a shocking novelty to propose, but it appears from his notes that this figure was an arbitrary compromise in order to test the reactions of the readers, while he thought as more probable some 120,000 or even over 1,000,000,000 years.

As expected, the novelty caused a flurry, and, after three years of discussions, the theological faculty of the Sorbonne, raised a number of objections.

As he pointedly stated with friends, in order to avoid troubles, Buffon wrote a letter of apology and included in the fourth volume of the Histoire Naturelle all the statements needed to pacify the Sorboniates, and obviously did not change his ideas and re-stated them some twenty years later, in 1773, in a conference held in Dijon and published under the title *Histoire de la Terre*. Probably Buffon re-advanced his ideas because, meantime (1755) Kant had published his theory (later familiar as theory of Kant-Laplace) that the solar system had originated from a spiral nebula, and Kant 'was in the news'. Moreover Buffon suggested that the new story of the Earth should be divided into seven periods to be considered as equivalents to the seven days of Genesis. The Sorboniates were incensed and Buffon went on a long holiday in Montbard while Abbot Bexon was preparing an answer to the Sorbonne and the right moves were made in Versailles. The end was that the king ordered the Sorbonne to leave alone one of the most famous scientists of France.

By his theory of the Earth, Buffon believed he had a framework, within which he could explain the degenerative 'transformations' that he had gradually granted that could occur to the organisms. In fact all of Buffon's theories are grossly wrong, but had the great merit of bringing the attention of the vast cultivated media on the debate of the reliability of the Bible's account. Doubts were, so to say, 'in the air' and, actually, especially in Italy, geologists and palaeontologists were openly debating the problem, but it is entirely to Buffon's credit to have embodied them into a general theory and to have stimulated a wide awareness of them.

It may be of some interest to insert here a digression, in order to show the different intellectual climates of France and Italy. While in France the first serious attempt to make a geological map was done by Guettards and Lavoisier shortly before the Revolution (and its outbreak cut it short both by stopping its funding by the government and beheading Lavoisier) several governments of the Italian states, within the framework of their attemps to revive their economies, had sponsored a number of geological surveys and thus in 1721 Vallisnieri senior proposed a general geology of Italy, described the famous fossilliferous layers of Mount Bolca and maintained that Italy must have been repeatedly and for long times under the sea in the past: and that this had nothing to do with the Flood. In 1740 Lazzaro Moro, in his book *De' Crostacei e del'altri Marini corpi che si truovano su' monti* (= *On the Crustaceans and other marine beings that may be found on the mountains*), on the evidence of the appearance of a new volcanic islet near Santorini, advanced a general theory on the volcanic origin of dry lands. In 1751-1752 Targioni maintained in his *Viaggi in Toscana* (= *Tuscan travels*) the general significance of erosion in determining the landscape and criticised Buffon. Giovanni Arduino (1714-1795) in 1759 on the evidence of the structures of the rocks of Venetian territories, proposed a general classification by age into 'Primary', 'Secondary' and 'Tertiary' (and quite correctly included into these last the Bolca Lagerstatt). In 1771 Galeazzi, a professor in Bologna, had shown that many seashells from the Pliocene strata near Bologna, had affinities with seashells from the Indian ocean, and showed that the sediments had been formed in tropical waters.

Serafino Volta described 123 species of fishes from Bolca and argued that 12 of them had no living representatives.

All these ideas, probably because they were not framed into any general theory as the French ones, were freely debated and no theological argument arose. The only one who may have had some trouble was the Paduan abbot Alberto Fortis (1741-1803), who had claimed an immense antiquity for the Earth and that there was no evidence for Noah's Flood (and later in Napoleon's times maintained the origin of man from an ape), and who thus failed to get Vallisnieri's junior chair when this last died.

Anyway, while Buffon's theories had a crucial impact on both the scientific world and the educated media, the Italian debates are but an erudite curiosity.

Coming back to Buffon, he long assumed that the only real entity is the species, this being characterised by the indefinite sexual reproduction among its members. Indeed Buffon made a number of experiments in hybridisation among closely related species. His first experiments appeared to prove that hybrids, when they could be born at all, were always sterile. Later he found that occasionally hybrids had limited reproductive capacities. We shall come back to these results. The fact was that Buffon as a systematist was as prisoner as Linnaeus (and as many others before and after) of a concept of ultimate Platonic origin, but that had been incorporated by Aristotle: the 'species' was considered at the same time both as the pool of the individuals which were assumed to belong to it, and as well as the pool of all characters defining the species itself, once all mere individual variations were discounted. All these authors failed to appreciate that the concept of 'population' (to which the concept of 'reproductive community' does usually apply) is logically separate from that of 'species'. This important distinction had been clearly identified by medieval nominalists and terminists, but by the 18th century all of medieval logic was either scorned or at least undervalued even in schools run by ecclesiastics, as here we see that the curricula pointedly recommend to eclude from the courses 'excessive scholastic subtilities'.

Assuming, as he did to begin with, that species were entirely separate entities devoid of any real connections, Buffon was justified in listing them in his volumes according any arbitrary principle. He decided for the utility and interest that any one of them had for mankind. This was also a good commercial approach: then, as today, all zoological books coming in instalments (as the Histoire Naturelle) begin with big Mammals.

However, when Réaumur died, though he had bequeathed all his immense collections to the Academie Royale, Buffon got an order from the king and, against the will, the collections went to the Cabinet.

Réaumur's collections included a splendid collection of birds, which had been already studied by M.J. Brisson (1723-1806), who from these specimens had been able to describe in his *Ornithologie* (1760) three times as many species of birds than those listed in the *Systema Naturae*. Faced with the problem of an immense number of species, new ones being continuously discovered, and with the results of his later experiments in hybridisation, Buffon begun to doubt the absolute fixity of species, and, in his late years he became a decided advocate of a limited transformism.

Both during his early 'fixist' phase and in his later 'transformist' one, Buffon thought that, particularly at high temperatures, such as he supposed should have obtained in the past, spontaneous generation could produce even very large animals and plants, while the cooling of the environment had put an end to the possibility of spontaneous generation of large animals, and many of them had migrated into the tropics. Anyway, during his 'transformist' phase, Buffon supposed that only a limited number of species had been produced by spontaneous generation, one for each genus (a category that he was now forced to admit). Some very 'noble' genera, such as the

Lion or the Elephant had remained unchanged and were entirely isolated, those who were somewhat less 'noble' had evolved by 'degenerative transformations', presumably resulting from hybridisation (thus the Ass was considered to be a degenerated Horse), and such possibilities for transformation were the more capable to produce a large number of species, the smaller and more prolific the animal, this being the case for birds and rodents. This curious and rather snobbish idea of an evolution from the more perfect to the more 'degenerate' will be further discussed later on, just as Buffon's ideas on spontaneous generation and reproduction will be considered comparatively with those of other scholars. We may just note here to consider that they appear as psychologically a bit peculiar considering that Buffon himself was a 'new' noble and the result of a steady social climb by his family.

Buffon's collaborators

We said that Buffon was able to develop his main treatise, because of the technical and scientific support of his staff.

We shall begin by the De Jussieu tribe, all of them being botanists: The first was Antoine De Jussieu (1687-1758), who had been recruited for the Jardin by Fagon. He was not one of Buffon's men, but, having died in 1758, he cooperated with Buffon for several years. Antoine was mainly concerned with practical issues; for instance he was the one who introduced coffee in the Antillean plantations, whence it spread through South America.

Joseph De Jussieu is the mystery man of the family: after having co-operated with his brother, he went to South America with a big geodetic expedition; when this sailed back to France, Joseph and another naturalist remained to continue their researches. From time to time crates full of precious materials continued to reach the museum. After a few years Joseph's fellow repatriated alone. The crates with the collections, always unaccompanied by any personal information, continued to arrive for some time more and finally stopped.

By far more important is Bernard (1699-1777), who, strangely, always refused promotion and published very little. However he developed new ideas on the systematic of plants and embodied them into the arrangement of the garden of the Trianon and meanwhile he passed them to his nephew Antoine-Laurent (1748-1836), who elaborated and codified them in his *Genera plantarum secundum ordines naturales disposita* (1789). This book has always been hailed as an enormous advance on the Linnean classification, as the De Jussieu were able to consider an important range of characters in the different structures of plants. The last of the tribe was, in the next century, Adrien, who did nothing notable apart sitting for 30 years on a chair of the Museum.

Another botanist of some significance who worked at the Jardin both under the rule of Buffon and afterwards was R.L. Desfontaines (1750-1833). He begun as curator of the herbaria, which he entirely reorganised. He had an extremely important part, in co-operation with Daubenton, in the rescue of the Museum from the danger of suppression during the confused times of the Revolution. His main scientific achievement was the discovery that, even when seeds are not available, it is always possible to distinguish a monocotyledonous from a dicotyledonous plant, on the evidence of leaves (with parallel nervatures in monocotyledonous and branching in dicotyledonous plants) and of the stem (hollow in monocotyledonous and solid in dicotyledonous plants).

Our next character is not very famous, but he had an immense influence: L.-J.-Marie Daubenton, was a true discovery of Buffon. Daubenton was a young physician from the same town as Buffon and a friend of the family.

Buffon called him to Paris and he slowly rose in responsibilites, until he became the director in the hectic times of the Revolution and he was the true saviour of the Cabinet du Roy, in imminent danger of suppression, by making it into the Muséum National d'Histoire Naturelle, one of the great research centres of the world.

Buffon, to begin with, charged Daubenton with the whole anatomical sections of the first volumes of the Histoire Naturelle (sections that were deleted in the following editions as they had no interest for the readers). Daubenton thus performed accurate dissections of dozens of species which had never been studied before, so that it was said 'Daubenton does not even know how many discoveries he has made!'. He acquired a less outstanding merit, but a really important one, when Buffon charged him of the complete reorganisation of the collections in the new building that had been built to house them. For years Daubenton worked to improve on the order, rationalisation and increase of the collections.

Daubenton was also a good judge of men and so he was largely responsible for the recruitment of Lamarck, duly appreciated the qualities of young Étiènne Geoffroy St. Hilaire, so that, on his advice, he recruited Cuvier.

Next in significance to Daubenton, is Bernard-Germain-Étiènne de la Ville, count of Lacépède (1756-1825). The count of Lacépède had been originally a cello soloist, a conductor and a composer, who was praised by none the less than old Gluck; natural sciences being then barely a hobby for him. Thus since 1780 Lacépède, besides being assiduous in the social pastimes of the upper classes and of theatres, began to attend also the Cabinet. However a big quarrel which arose during the rehearsal of his second opera, the contemporary sudden death of Daubenton junior, who left a vacancy at the Jardin and the flair for good human material of old Buffon, recruited Lacépède into the establishment of the museum, that he left only once, during 'la Terreur' when the çi-devant count had to disappear, helped by Geoffroy St. Hilaire, to save his neck. He came back in 1795, when a special chair in Ichthyology and Herpetology was established for him. When Daubenton died Lacépède became the director of the Museum. Under Napoleon he was president of the Senate, minister of State, great chancellor of the Légion d'honneur. When Napoleon fell, acknowledging his qualities, his honesty and the unselfish way he had always worked in his different appointments, he was made a Peer of France by Louis XVIII. De Lacépède is a pure systematist, a describer of species who has left a lasting trace both in herpetology and ichthyology. All the same it must be granted that his *Histoire Naturelle des Poissons*, which he wrote when in hiding in the period of the 'Terreur' is unfortunately rich in gross mistakes.

The other biologists who worked with Buffon at the Jardin and the Cabinet do not deserve mention in such a brief book as this.

We must, instead, mention some explorers, as we did with Linnaeus pupils. Some were actually recruited by Buffon, others were entirely independent.

We shall first mention J. Houtou de la Billardière (1755-1834) (not to be confused either with Count de la Billarderie, who tried to get Buffon's place, or his cousin the Marquis de la Billarderie, who for some months succeeded Buffon). De la Billardière studied the Syrian flora around 1786.

Philibert Commerson (1727-1767), begun his scientific activities by a study on the Mediterranean fishes for which he was commissioned by the Queen of Sweden. The results were so good that Linnaeus himself personally introduced him to the Queen. In 1767 he joined De Bougainville who was sailing for his famous voyage aroung the world. He thus reached Mauritius, which, at the time, was a French possession. There he cooperated with the French governor De Poivre, himself a good botanist, and sent to Paris some big collections. Commerson died in Mauritius.

The real scientific significance of Michael Adanson (1727-1806) was not acknowledged either during his life or for over a century afterwards. He was an encyclopaedic scholar, but his main contribution are in Botany. He was sent to explore Senegal (1749-1754) and there he studied every possible aspect of the country. He made splendid collections and, back in France he published a number of papers on the fauna, flore, ethnography and linguistics of the country. In 1763 he published his main contribution: *Familles des Plantes* which, though difficult to use, is otherwise scientifically extremely advanced. He later thought of a general encyclopaedia of all sciences which he planned to have 177 volumes. As the Academie des Sciences refused the project, Adanson spent the next 30 years of his life trying to write it himself.

An important French explorer who had no connections with Buffon was Pierre Sonnerat (1749-1814); he made important collections in different regions of Asia and Africa.

Before we close this section we must remember, however, that, though Buffon's work was undoubtedly the most original and influential one, similar projects were implemented by other groups in France. These, however were at a disadvantage as they had no opportunity to develop with the same facilities available in such an institution as the Jardin and the Cabinet. Anyway we should mention the *Tableau encyclopédique et methodique des trois règnes de la Nature*, 166 volumes published around 1790 and which were the work of serious naturalists. Among them deserves a mention Joseph P. Bonneterre (1752-1804) who wrote many sections of the 14 volumes of zoology. He was a good systematist and he described several new species of Vertebrates.

Marine biology

Marine biology was a branch of biological sciences which during the 18th century begun to develop as a well defined specialisation; its cradle was in the Mediterranean.

We have already mentioned the early studies by Commerson, we shall now discuss about that Count Luigi Ferdinando Marsili, whom we mentioned when describing the birth of Benedictine Academy in Bologna. Count Marsili (1658-1730) belonged to that rare breed of cultivated, intelligent and curious soldiers, rather more explorers than mere soldiers, such as several were produced by the British imperial armies of the 19th century. The sort of people who took advantage of any opportunity in their service in far away countries to satisfy their scientific curiosities.

After a first trip to the Middle East, when he gathered the materials for his first book *Osservazioni intorno al Bosforo tracio ovvero canale di Costantinopoli* (= *Observations on the Thracian Bosphorus, that is the Costantinople's channel*), he fought against the Turks in Hungary and Southern Austria. He was wounded, made a prisoner and a slave. Having been redeemed in 1685, resumed his service and, because of his heroism and new wounds, he was made a colonel. Later he acted a diplomatic role during the negotiations for the peace of Carlowitz (1699). As a general he fought in the Spanish succession war. Since 1715 he gained admittance to the Académie des Sciences and, havig been enlisted some years previously, he was formally received as a fellow of the Royal Society in 1722. After leaving active service, he published several interesting works and others were published after his death. The *Osservazioni intorno al Bosforo Tracio* … is probably the first systematic account of all aspects of a marine environment: there Marsili deals with marine currents, bottoms, coasts, different fishes and molluscs etc. In 1714 Marsili published a *Dissertatio de generatione fungorum* (with comments by Lancisi), where he identified the fungine mycelium in advance on Pier Antonio Micheli (1679-1737), but he still maintained spontaneous generation for fungi as he had not been able to identify the spores, which were discovered by Micheli just a few years afterwards.

In 1725 Marsili published a *Histoire physique de la mer*, a general treatise on the sea, finally in 1726 he published his *Descriptio geografica Danubii Pannonico-Mysici,* where he summarises all the observations, geographic, geological, palaeontological, etc. that he had made while campaigning against the Turks. At his death Marsili left a number of unpublished papers, one being ready for the press: *Osservazioni fisiche intorno al lago di Garda, detto anticamente Benaco*, which is a complete treatise of limnology both for its lay out and its thoroughness; unfortunately it was printed only in 1930!

In 1706 Marsili published a description of the white *flowers* of coral, which open in still waters and which was a first contribution towards understanding the true nature of these organisms, which were variously considered: rocks, rocky plants, organisms intermediate between plants and animals. Jean Antoine Payssonel (1694-1756) resumed these researches and in 1723 he understood that they were animals and compared them with sea-anemones; however Réaumur discouraged him from publishing his results. It was only after Trembley's discoveries, that Réaumur asked Guettard and Bernard de Jussieu, to resume Payssonel studies and in 1742 finally stated that they were animals.

Still in the 18th century three more Italians deserve mention: Giuseppe Olivi (1769-1795), Giuseppe Saverio Poli (1746-1825), and Filippo Cavolini (1756-1810).

Olivi was born in Chioggia, near Venice. He published a number of papers on agriculture, botany, mineralogy and chemistry, but his best contribution is the *Zoologia Adriatica, ossia catalogo ragionato degli animali del golfo e della laguna di Venezia* (1792) (= *Adriatic zoology, that is a reasoned catalogue of the animals which occur in the Venetian gulf and lagoon*). This booklet was acknowledged as a turning point in Mediterranean zoology, but it also included a most important 'Saggio sulla proporzionalità trovata nell'accrescimento dei Granchi, delle conchiglie e dei pesci' (= Essay on the proportions found in the growth of crabs, seashells and fishes), which is the very first mention of what we now call allometric growths, which are quite important in morphology and which we shall deal with in some more detail writing of Thompson and Schiaparelli.

Poli is a familiar name to college students in zoology, because of 'Poli's vesicles' in the Echinoderms. Poli was born in Molfetta and then worked in Naples as a physician at the 'Ospedale degli incurabili' (= Hospital for hopeless cases) and as a teacher at the local medical school. He published contributions on the most diverse subjects, but he is mainly remembered for his studies on the Molluscs and Echinoderms of South Italian seas (*Testacea utriusque Sicilae eorumque historia et anatome*) which is notable not only as a faunal survey, but chiefly for the amount of new anatomical details that he investigated in animals whose morphology had been almost completely ignored.

Cavolini was both a botanist and a zoologist and in 1808 was appointed as professor of 'General theory of Natural history' at the University of Naples. He did most of his work at home in Posillipo. His main contributions were on the reproduction of mushrooms (1778), on the biology and systematics of several marine animals, particularly Coelenterates (*Memorie per servire alla storia de' Polipi marini*, 1785 = *Memories for the history of marine polyps*), researches on the reproduction in fishes and crabs (1787) and a classic work on the reproduction of marine flowering plants of the genus *Zostera.*

The debate on spontaneous generation

While, after the studies of Redi, Vallisnieri, Swammerdam and Réaumur, no one doubted any more that all such animals which could be seen by the naked eye, just as in the higher plants, there is always biological continuity from one generation to the next one, but did the assumption hold for such microscopic organisms as Leeuwenhoek had described from infusions and that, consequently, were currently called 'infusorians'? The debate went on for most of the century. The great mathematician and physicist Pierre-Louis Moreau de Maupertuis (1698-1759) was among the first to open it. He aimed to provide a general theist framework for all sciences and was a rigid supporter of Newtonian corpuscularism, finally he wanted to bring everything under the mantle of Leibniz's monads. Thus he invented 'organic molecules' which had an elementary psyche which made them active, just as gravity activated the particles of non living matter, and that was much alike the *vis viva* that Leibniz had supposed for his monads.

At the beginning of the century the spontaneous generation of 'infusorians' was a rather common belief, and this was temporarily strengthened by the studies of the British abbot John Turbeville Needham (1713-1785).

Since 1710 the French L. Joblot (1645-1723) had shown that contact with air was prerequisite if infusorians were to develop at all. He had made basically the same experiments of Redi: after boiling his infusions, he had sealed some of the containers and had seen that infusorians developed only in the containers that had remained open (later Gay-Lussac showed the function of oxygen for allowing the development of most organisms). John Turbeville Needham (1713-1781) was born from a noble Catholic English family and, probably because of his religion, studied in an English college in the Flanders. He was ordained a priest in 1738 and thereafter returned to England and was later received into the Royal Society. He begun his biological studies in 1743 and these included the discovery of the first Nematodes parasitic on plants (a Tylenchid in smutty wheat) and experiments on the regeneration in starfishes. These were further developed and were published in 1745 under the title *An account of some new microscopical discoveries*, which include a number of different observations, including studies on the sexual development in the cuttlefish. He then extended his researches to the 'infusorians' and in 1750 published the *Nouvelles observations microscopiques Avec des decouvertes interessantes sur la composition et la décomposition des corps organisés* which actually include the description of the supposed spontaneous generation of infusorians and that sparked the classical researches of Spallanzani and the ensuing debate. As this title is almost a perfect translation of the book published in 1745, most scholars, including myself, failed to compare the two publications and thought that actually the book of 1750 was the translation of that of 1745. Needham had made infusions of both vegetable and meats by heating them short of boiling and, having sealed them, nevertheless infusorians had appeared after some time. In fact

Needham, by using such temperatures that would unfailingly kill any visible animal, thought to have killed as well all the infusorians and their 'germs', having thereafter prevented contamination by sealing the containers, he thought to have proved spontaneous generation. Moreover Needham thought that he had seen the development of living infusorians from the tiny blobs resulting from the decay of used materials. Thereafter Needham went to Paris, where he co-operated sometime with Buffon, who was then busy preparing the first volumes of the *Histoire Naturelle*, which were printed in 1749. Buffon incorporated there Needham's data, as they fitted with his own ideas on the origin of life.

Buffon had basically adopted Maupertuis' hypothesis: he supposed that, especially by the action of heat during the first phases of Earth's existence, extremely small 'organic molecules' had been generated, these had, however their different structural specificity and were endowed with a 'penetration force' analogous with gravity. The only difference between Maupertuis' and Buffon's molecules was that those of Maupertuis had an elementary will, while those of Buffon had merely material powers. According to Buffon's hypothesis the organic molecules were eternal and, by their specificity, nourished the growing organs by fitting, once absorbed by the organism, into such appropriate organ which had the proper 'internal mould' and so developed the whole organism. When the organism died the 'molecules' became free and, unless immediately absorbed by an appropriate organism, dispersed into the environment, where they moved at random, waiting for being reused. Should they, while floating in the environment, happen to meet with other appropriate molecules, they could spontaneously associate and originate the infusorians. These, as a consequence, were regarded as sort of intermediate beings between true organisms and simple aggregation of molecules with just a few characters of a living being.

Some historians have claimed for Buffon's ideas an Aristotelean ascent, buth this is incorrect: they are much more like the ideas of Anaxagoras who, as we should remember, thought that within any body there were the 'spermata' of all substances and that, for instance during the digestion of bread, the organism selected and incorporated the spermata of meat that were in the bread.

Buffon believed that the sperms were just his supposed 'molecules' or monads, and thought that he had seen similar corpuscles in the liquid inside the ovary's follicles. The molecules of Buffon were not only just what was needed to explain growth, but one could also easily imagine that, as soon as the animal had reached its complete development, it could store them in the sperms and eggs, to be mixed at fertilisation; they were thus immediately available for the 'internal mould' of the female and could thus produce new organisms. These would be different according the receiving mould, of the amount of each kind of 'molecules' received at the beginning and during subsequent development. All this nicely fitted into the epigenist views of Buffon, who held that embryos were formed and subsequently grew by aggregation of unorganised particles. Thus Needham's findings apparently precisely fitted into Buffon's theories and

Buffon became their strenuous advocate. The reader may be surprised at the extraordinarily sanguine attitudes of most scholars of this age when theorising on the few facts available. Indeed this is typical of the age of enlightenment, and is not at all limited to biologists: philosophers, and especially the French ones, were much worst.

Spallanzani

Some years after the publications by Needham and Buffon, Lazzaro Spallanzani began his research activities. Spallanzani proved to be one of the most brilliant experimenters in the whole history of biology. His impact on the developments of biology was great indeed even if, as we shall see, some of the theoretical premises for his experiments and their general lay out were suggested to him by his friend Charles Bonnet.

Spallanzani was born in Scandiano, not far from Reggio Emilia, in 1729; he studied first in Reggio, then moved to Bologna, where his famous cousin Laura Bassi was teaching first physics and later philosophy.

It is here appropriate to remember that women, as we said in a previous chapter, were not barred from higher studies and even chairs before the French revolution, though admittedly women had always been rare at universities. However they were not so rare if our Francesco Redi wrote in one of his little poems:

Per le scuole oggidì vanno in persona
Dame di Salamanca e di Sorbona

Which may be translated: Nowadays damsels from Salamanca (the most famous Spanish University) and of Sorbonne attend the High Schools.

Indeed precisely in Bologna, at the same time when there was teaching the mathematician and physicist Laura Bassi, the less known Anna Morandi Manzolini was professor of Anatomy and the wife of Galvani was the chief co-operator in his experiments.

While in Bologna, Spallanzani was encouraged by Vallisnieri junior to leave Law, the carreer intended for him by his parents, and study Sciences. Spallanzani took the minor orders which, though not making him a priest, entitled him later to a titulary abbotcy and to its revenues. Spallanzani was first professor of philosophy in Reggio Emilia, and then of physics at the University of Modena and of mathematics and Greek at the still existing San Carlo college there (1760). Finally he was appointed at the University of Pavia, where he stayed until his death in 1799.

The University of Pavia, after a long cultural slumber, similar to that of many other such institutions in Europe in the late 17th and early 18th century, was being revived by the energetic prompting of the Austrian government, which had superseded the Spanish one in the duchy of Milan. Thus a notable group of scholars was assembled in Pavia, the most important being, obviously, count Alessandro Volta, but notable scientists were also the anatomist Antonio Scarpa, the mathematician Lorenzo Mascheroni, the chemist and pharmacologist Valentino Brugnatelli, the biologist Giovanni Antonio Scopoli (1723-1788), this being a notable botanist, ornithologist and a pioneer in the study of both cave's fauna and flora, the mathematicians Gregorio Fontana and Serafino Volta, whom we already remembered as a paleontologist. With most of them Spallanzani quarelled to the extent that Scopoli, Fontana and Serafino Volta officially charged him with theft of specimens from the Museum. Spallanzani, who was a hard and proud man[3], who never hid his contempt for his minor colleagues, counter-charged his enemies and especially Serafino Volta, whom he charged with appropriation and Scopoli whom he nicknamed in some publications by the name of a supposed worm that Scopoli had described and that was nothing but a fragment of the trachea of a bird! There was an official inquest and it was found that while Volta had broken some crystals to give their fragments to friends, Spallanzani had, indeed, brought home some specimens, but he was able to argue that this had been done for study purposes and, while he was fully cleared, Scopoli and Fontana got a reprimand and Serafino Volta was removed.

That Spallanzani ranks among the greatest experimenters and that he produced basic contributions in many fields is unquestionable, but we shall see that occasionally his natural dogmatism led him entirely astray. Equally, while he worked hard to enrich the Museum in Pavia, yet he never understood the significance of descriptive biology as a premise to good systematics and morphology; he never understood the significance of Linnaeus' work, which he ridiculed and occasionally severely criticised, sometimes correctly but also, in as many instances, quite wrongly.

In fact Spallanzani was always basically interested only in the function and mechanisms of the phenomena he was investigating.

Most of the main work of Spallanzani fall under four headings: reproduction, circulation, digestion and respiration, plus the work on the hearing in bats, and it will be expedient to consider them under these separate headings. Yet the first publication by Spallanzani was a philological criticism of the Italian translation by Salvini of the Iliad[4]. This was followed by one on fountains, which he dedicated to Vallisnieri junior, and one on the causes of rebounding, that he dedicated to his cousin Laura Bassi.

Spallanzani had read Needham's papers when still in Reggio and began by checking them. He then completed his work in Modena. This was his first contribution to biology and together with the following papers on the same subject it still remains, perhaps, his major title to glory, though certainly not the only one.

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[3] As an example of the quarrelsome personality of Spallanzani we may also quote his open and advertised contempt for Vallisnieri junior, a minor scholar, but who had the gift of selecting and encouraging promising scientists, including Spallanzani himself!

[4] Spallanzani never lost his antiquarian interests, as he later proved during his travel to Turkey and the Black Sea when he, among other things, investigated and discussed the possible location of Troy.

Spallanzani, as any Italian scholar of the times, was familiar with French, but published all his works in Italian and these rank, together with Galileo's and Redi's scripts, amongst the most perfect examples of Italian scientific prose.

Spallanzani's international renown was due to his friends: Needham himself and Bonnet who translated into French his most important papers.

Coming back to the problem of spontaneous generation, Spallanzani was not convinced by Needham's experiments and much less by Buffon's fantasies. So he precisely duplicated Needham experiments and then proceeded to make them more rigorous by better techniques of sterilisation (by the way this term is a good example of the persistence of ancient concepts: to sterilise literally means to make something incapable of generating, with obvious reference to the ability of the culture medium to produce living beings!). After about two years of experiments he published his results in the celebrated *Saggio di osservazioni microscopiche concernenti il sistema della generazione de' Signori Needham e Buffon* (1765) (= *An essay of microscopical observations concerning the system of generation [supposed] by Messers Needham and Buffon*), which ranks among the most significant papers published in the 18th century.

As soon as he begun to study 'infusorians' Spallanzani was fully convinced of their animal nature because of how they moved and fed and thus he ruled our their intermediate condition supposed by Buffon. Indeed Spallanzani saw what we now call Brownian movement (from the name of Robert Brown, 1773-1858, who gave a full account of it) and remarked on the differences between the swimming of microscopic animals and the passive Brownian movement of non living particles. Moreover Spallanzani failed to find any evidence of the formation of infusorians as described by the English abbot, but, instead, he saw the 'cysts' (that he called 'little eggs', which was a justified interpretation by the biology of the time, but which paved the way to his worst theoretical mistakes). Spallanzani clearly saw how the first infusorians creep out of the cysts and thus had an additional reason to reject Needham-Buffon's theories. The crux of Spallanzani's experiments was, however that, if Needham's experiments were precisely duplicated, Needham's results did, indeed, occur, but that should the infusions be actually boiled and securely sealed from any dust, then no infusorians appeared.

So far so good, but Needham, who, in the meantime had become a close friend of Spallanzani, translated Spallanzani's paper, but added several notes pointing the criticisms that he thought could be made to the Italian's conclusions. Basically, argued Needham, it could be supposed that the higher temperatures employed and the sealing techniques could well have sufficiently modified the intrinsic qualities of the culture medium as to make it unsuitable to produce the organisms (precisely the objections made a century later in the discussion on the origin of Bacteria).

Thus Spallanzani, who, in the meantime, had moved from Modena to Pavia, exhaustively answered Needham's remarks in the first volume of the *Opuscoli di fisica animale e vegetabile* (1776) and by a set of new accurate experiments and observations completely won his point: he did, indeed show that even prolonged boiling of the medium did not impair the possibility for infusorians to develop in it; that, even assuming that there occurred a 'loss of elasticity of the air', this supposed event has no influence of the reproduction and growth of these animals, etc.

It was during his studies on infusorians that he begun his studies on different resistance to heat and to low temperatures as well as to desiccation of eggs, seeds, cysts and some complete animals (Rotifers, Nematodes, Tardigrada, this last a phylum discovered by Spallanzani himself) in comparison with the tolerance to variation of the same factors by the active or vegetating corresponding beings. He thus verified that resistance, especially to high temperatures was always notably higher in seeds, eggs and cysts than in the active phases. He also remarked that dry heat was much better tolerated than the same under damp conditions.

He made a special study of 'resuscitating' animals (we now call this phenomenon cryptobiosis, but to Spallanzani the animals were truly dead and resuscitating). In fact the first observations of this phenomenon had been made by Leeuwenhoek on Rotifers, and had been studied, again on Rotifers, by Needham and on Nematodes by abbot Felice Fontana in Florence (Fontana delayed publication of his results, but had shown his results to Bonnet in 1775). Even undergraduates are familiar with the fact that these animals can last for years in almost completely dehydrated conditions and that, when dehydrated they withstand both extremely high and extremely low temperature (in recent years some have been found to withstand temperatures close to that of liquid Nitrogen). Spallanzani's studies on resuscitating animals occasioned an episode that throws much light of the character of the different people involved. Spallanzani, in spite of being a lay abbot, was especially anxious to get from Voltaire an appreciation of his results as this would have gained European fame to his discoveries, but was also a good friend of Bonnet, who had been fighting a years long battle against Voltaire and his followers in defence of a deist interpretation of modern biology of a protestant pattern. Spallanzani mailed to Bonnet several copies of his works asking Bonnet to forward some of them to scholars that he listed, among them Voltaire. Bonnet obliged and Spallanzani was duly praised by both. However Voltaire decided to publish his letter to Spallanzani, but manipulated it by inserting a poisonous attack on Bonnet's *Palingénésie Philosophique*. Spallanzani was able to clarify the matter with Bonnet and their friendship did not suffer, but he did not protest with Voltaire: such a promoter was too good to risk breaking his connection with him!

Most Infusorians are, in fact, Protozoans and, studying them, he noticed that their body may variously divide. Some he calls viviparous and split either longitudinally or transversely originating two or more animals, who than grow back to the same size of the undivided cell; other he calls oviparous as he saw that they produced little outgrowths which thence detached themselved, fell to the bottom of the container and could produce new little animals similar to their parent (these being either buds or cysts). Spallanzani also maintained that infusorians are hermaphrodites as, even if bred in isolation they nevertheless reproduce. Nowadays we know that some protozoans, being unicellular may be, in some sense, considered as such, but that the rotifers studied by Spallanzani are really parhenogenetic females. Spallanzani' descriptions are quite good, but both kinds of reproduction in the Protozoans had been already seen by Trembley (1741) and by Horace Benedict De Saussure (1740-1799). De Saussure was born of a noble Huguenot family, who had emigrated to Geneve, and is mainly remembered as a geologist, metereologist, politician and as a pioneer of high altitude studies; he is not to be confounded with his son Nicolas-Theodore (1767-1845), a notable chemist and plant physiologist; both of them were both good friends and occasional fellow travellers with Spallanzani. On the other side conjugation in Protozoans had been seen by Leeuwenhoek (1695) and was better described by the Dane O.F. Müller shortly afterwards (1786).

Anyway, so far as there was no 'cellular theory' available, it was impossible to give a correct interpretation of these facts. In fact the first true advances in this field were made by Balbiani (1851) and by Maupas (1888-1889) and it still is the object of much research, using sophisticated histological, histochemical and genetic techniques.

Spallanzani was led entirely astray as far as the problem of fertilisation was concerned just by his observations of reproduction by cysts and gems: he considered these little round bodies as eggs and this strengthened his 'ovist' persuasions.

During his researches Spallanzani also noticed and briefly described the first cell structures, as he noticed both the cytostoma and pulsating vacuoles of Ciliates.

Both the rigorous experiments and the results of Spallanzani were welcomed with great interest by the scientific community and cultivated media. Thus Voltaire thought to use them for his mill in an attack on Needham, where he qualified him as a Jesuit, which was entirely false! Voltaire was enthusiastic about Spallanzani's results as there he saw some sort of final evidence against Leibniz's monads, and Leibniz was his 'bête noire' (he ridiculed him as Pangloss in *Candide*), and the living molecules of Maupertuis, thus helping in his fight against such trends as were later named 'vitalist'. However, Voltaire missed entirely the fact that they could just as well be used against the 'mechanistsic' theories of Buffon.

The debate on reproduction

The problems of the reproduction of Metazoan and of higher plants are closely interwoven with that of spontaneous generation.

As far as plants are concerned, the total inadequacy of the microscopes then available for the observation of some critical evidence, practically resulted, as plant sexuality was generally acknowledged, in the, wrong, identification of pollen grains with the semen or even with the sperms themselves. As for the generalised acceptance of plant's sexuality, this was largely due to the work of Sébastien Vaillant (1669-1722), who was working at the Jardin du Roy in the early years of the 18th century. Sexual characters were, obviously, currently used in systematics, within the patterns of Linnaeus and the De Jussieu. Considerable advances were thus made in the understanding of fertilisation in plants.

Joseph Gottlieb Kölreuter (1733-1806) had taken his medical degree in Tübingen, had a chair for a while in St. Petersbourg, later he became director of the botanical gardens in Carlsruhe. He pioneered the experimental studies on hybrids and we shall discuss his work further on; but he is mainly remembered for his studies on pollination by wind and by insects (as usual already at that time it was rare for the well known pioneer not to have at least one obscure forerunner or contemporary, thus results strictly comparable with those of Kölreuter, were published in 1767-1768 by a Father Filippo Arena S.J in an extremely rare book by an incredibly long title).

Kölreuter's results were further extended especially by Christian Conrad Sprengel (1750-1816). Sprengel, who was apparently a misanthropist always quarreling with most people, was for a while director of a school at Spandau, but he was fired on the charge of having forsaken his duty to the school in favour of his botanical researches. He then retired to Berlin, where he lived such a secluded life as to be considered seriously unbalanced. His main work is *Das entdeckte Geheimnis der Natur im Bau und in der Befruchtung der Blumen* (= *The mystery of Nature revealed in the structure and in the fertilisation of flowers*, 1793), which is still considered as a classic for the knowledge of the mechanisms of pollination in plants. The significance of Insects in the pollination of many plants and on the different adaptations of many flowers in order to attract them were well described by Sprengel. Moreover he described the features common to plants pollinated by winds and of these which prevent self pollination. The term 'Dichogamous' was his creation and is still used.

Some aspects of Kölreuter's experiments are significant to illustrate the general trend to very gradual evolution of scientific ideas. Kölreuter made a number of experiments in hybridisation and his results, in some ways anticipate some of Mendel's. Nevertheless he thought that by using a mixture of pollens of two different plants, he could get semi-hybrids. Moreover he was convinced that the changes that he obtained by hybridisation were akin to alchemic transformations and attributed to pollen a 'sulphurean' and to the feminine element a 'mercurial' nature.

It must be said that both Kölreuter's and Sprengel's results were received with considerable scepticism, to the extent that still in the years 1820's and 1830's researchers in Germany were busy satisfying themselves as to the reliability of Kölreuter's results and it was Darwin who was the first to really appreciate the value of Sprengel's researches. Curiously much attention was immediately paid to the parallel work by the British T.A. Knight *Experiments on the fertilisation of vegetables* (1799), which is altogether much less exhaustive.

Again this subject is instructive in showing how a great man may be blinded to evidence by his own scientific bias: Spallanzani made some experiments on plants aimed to show that his extreme 'ovistic' views were correct and that plants may fructify also without pollination. Some of these experiments ware definitely wrong, while he entirely misunderstood the significance of other delicate and correctly executed experiments. Such misunderstanding may be only partly excused by the fact that he lacked some criteria which were not available in his times, but it must be acknowledged that basically he was put on the wrong track by his own pre-conceived ideas.

Taken as a whole one may say that the works of these authors: Kölreuter, Sprengel and Knight had esatablished the main lines of the basic facts of fertilisation in the phanerogamous plants. Moreover the hybridisation experiments of Kölreuter had established beyond possible doubt the respective functions of males and females in reproduction. However it must be noted that Kölreuter was prompted into his experiments by his belief that species should be recognised on the evidence of the sterility of hybrids, much on the lines advocated by Ray. He was undoubtedly lucky in his choice of his experimental materials, as the possibility of fertile hybrids is much greater in plants than in animals, where it is, nevertheless, occasionally possible. Anyway his results allowed him to maintain that in hybrids between 'good' species fertility was at least considerably reduced and very often there was complete sterility. He also was the first to notice that, which was later known as 'First law of Mendel', that is the uniformity of first generation hybrids, and, though the numerical relations discovered by Mendel, completely escaped him, he also noticed the tendency in generations following the first one, for the separation of characters. Nevertheless Kölreuter can not be considered as a pioneer geneticist, as he never though of such queries that are at the basis for Mendel's experiments. His results and those of his continuators during the first half of the following century did, however, pave the way for Mendel's famous experiments.

As far as reproduction in animals was concerned, we must return to the end of the 16th century: such evidence as was available suggested that both in animals and plants the same basic mechanisms were involved, even in such species where either the egg or the seed had not been discovered. The eggs had thus been a focus of attention, although no one ever suspected that usually what were deemed to be the eggs were in fact complex structures, where the real egg-cell was contained. However the situation had become more obscure by the discovery of sperms (1677), thus, as we shall now see, the scientific world, tiny as it still was, became divided between a majority of 'ovists', who held that the sperms either had no function at all or that they were mere 'activators' of the egg, and a minority of 'spermatists' or 'animalculists' who held by the sperm, thinking it the essential element in reproduction. Moreover, and this was even more hotly debated, a central point was that of the mechanisms by which development occurred and a complete new organism was formed from more or less undifferentiated matter.

Remembering that even if cells had been seen (Protozoans and the cell-walls of plants) there was no idea of their significance, so long as Aristotelian theories of generation were the only available there was no problem as to the father's function: people thought that the Eidos (which may be freely translated as 'body-plan' or 'information', which literally means that which brings in the form) was carried by the semen and worked by giving form to undifferentiated materials supplied either by the egg, by menstrual blood of by the feminine semen: It was equally common sense to allow that the feminine 'raw materials' available might have some say in the actual details of the final product.

However in 1626 Giuseppe degli Aromatari had claimed that the embryo existed in the bird's egg before incubation begun, which was plainly correct as he had described the germinal disk, and Henry Power in 1664 had claimed that in the chicken embryo the heart both occurs and pulses since the second day of incubation.

Malpighi had dealt with the problem in two contributions: *De ovo incubato observationes* (1689) and *De formatione pulli in ovo* (1673). He confirmed the existence of the embryo in the egg before incubation (as we now know, in fertilised eggs of birds the segmentation begins before deposition). However, although the fact had been correctly observed, yet it was the prime cause of research taking a mistaken path and of a lengthy debate, that between 'preformationists' and 'epigenesists'. Malpighi hypothesis was that organs were actually existing in the egg, not as miniature adult organs, but as filaments or *stamina* each one capable of growth and differentiation, each into one given organ (and actually, if you substitute 'cell' for *stamen*, this is precisely what occurs in Nematodes and some other animals already at very early stages of segmentation and more or less later for all animals).

Approximately at the same time, Swammerdam, on the evidence of his observations on the development of tadpoles (1666) and of the pupae of butterflies (that he, following Aristotle, believed to be true eggs) (1669) suggested in his *Miraculum Naturae* (1672) a daring theory, which he needed in order to come to terms with Lutheran and Calvinist theology. As a conclusion to his study of insects and especially of butterflies, Swammerdam maintained that in olometabolic insects there is neither a true metamorphosis, nor the generation of a new individual as was believed by Aristotle; the adult insects is already contained in the pupa and in the caterpillar, but that it cannot be seen as it is masked by the tissues of the caterpillar itself, and that we must think that it already existed in the egg. As usual with ancient authors, Swammerdam was right and wrong at the same time: he was right in that we now know that in the caterpillar some organs of the adult occur and function, such as the true legs, and merely change their shape and proportions during pupation, while others occur as 'imaginal disks', packages of cells hidden among other tissues and that develop during pupation, while most of the caterpillar's tissues are metabolised to feed the new developing organs. This much Swammerdam had been able to see by his accurate dissections, though he could not see the imaginal disks in the caterpillar. However he was wrong in that he, by studying larval stages, was not studying the embryos of the animals. We now know that development is entirely different during the true embryonic stages.

A logical consequence of the denial of spontaneous generation and of the development of new individuals from an undifferentiated matter, and the one consequence that, as we shall see, mattered most with Swammerdam and others, was that one could well suppose that not only all the organs somehow already existed in the egg, but the egg itself could contain, one inside the other, miniatures of all successive generations. This particular consequence of preformism was formalised under the name of 'Embiotement' much later, but it was quite clear to Swammerdam and, to him it provided the explanation of how the 'Original Sin' had been transmitted: by supposing that Eve, since her creation, had contained all the successive generations, her sin had contaminated all of them at the same time!

But such a theory appeared to be not only theologically satisfactory, it allowed also for a purely mechanistic and Cartesian theory of the universe. Indeed, if the germs of all generations had been created by a single act of God and at the same time, everything could be explained in terms of normal development: Leibniz was quite clear on it: '… moreover as to the formation of plants and animals, there is no miracle in it, but for the first principle of these things. The organism of animals is a mechanism which presumes a Divine pre-formation, everything that follows is purely natural and mechanic'.

As we have repeatedly said, naturalists began to have the theoretical instruments for the understanding of what really happened during development only after the publication of Schleiden's and Schwann's papers (1838, 1839), which stand as the first successful proposal of a general cellular theory. By the end of the 17th century they were hardly better equipped than Aristotle to guess at some explanations as to what happened during embryonic development.

By agreeing to 'preformation' because of empirical evidence, philosophical and theological reasons, the next question was naturally: 'then: where are the germs?'. Two alternative answers were provided: 'panspermy' and 'emboitement'. By the first the 'germs' of all species were diffused in infinite numbers everywhere and they simply awaited a favourable opportunity to develop; by the second hypothesis each individual had within himself the germs of the next generation issuing from him and inside such germs were those of again the next generation, and so on. A few thought *ad infinitum*, most, who believed, according the Bible, that the world had been created but a few thousands years ago ad was not to last long, were content with just some thousands of generations, the necessary number of generations to get to the 'End of the Times'. Both theories had the same assumed logic: on one side the recent discovery of calculus was thought to provide the theoretical basis for arguing that, just as there were mathematical infinitesimals, there could be organic ones, while as microscopic observations had revealed microscopic organisms of great complexity, there could well be even more minute complicated organisms: the pre-formed germs.

Pre-formation appeared as a scientifically better grounded theory with respect to epigenesis. Indeed it did not need obscure factors, such a *vis formativa,* which appeared to be mere verbal tricks aiming to mask ignorance of real causes. As, while witnessing the eclipse of Cartesianism, the 18th century saw the triumph of Newtonian physics and the birth of quantitative chemistry, thus the intellectual media were hardly favourable to accept any more ill defined factors to steer such complex phenomena as fertilisation and embryonic development.

Among the most prominent pre-formists were Malpighi, Swammerdam and Vallisnieri and, later, Haller, Spallanzani and Bonnet.

However the pre-formist party was soon split by the discovery made by L. Hamm in 1677 and confirmed by Leeuwenhoek of the existence in the semen of 'minute animals similar to infusorians': the sperms. Thus some maintained that these were the chief agents in reproduction and, presumably the pre-formed embryo was to be found into them, while others championed the egg.

As examples of the fantasy of late 17th century authors, one still often finds reproduced the figures by Hartsoeker (1694), and Dalempatius (1699), who both figure a tiny human foetus crammed in the head of the sperm. However Hartsoeker plainly states that his figure is a mere hypothesis, and Dalempatius' paper – as we have already said — is a joke, published under an anagram of the Latinized name (Plantadius) of the author, the French botanist De Plantade. He, in his paper claimed that he had seen the minute foetus coming out of his spermatic envelopes, and Vallisnieri, who thought the paper a serious one, spent some pages airing his criticisms!

Several ovists, on the other side went so far as to consider the sperms not only devoid of any function in reproduction, but argued that they were, in fact, parasites!

Among the most prominent ovists we must first consider Bonnet.

Charles Bonnet was born in 1720 in Geneva from a Protestant family of French origin, who had left France when the Huguenots were persecuted. He studied law, but soon preferred natural sciences and, on the example of Réaumur, he undertook the study of insects. His first, and practically unique experimental discovery was the parthenogenesis of Plant-lice (Aphids), which Réaumur immediately verified and hailed as a major discovery. Soon afterwards Bonnet begun to loose his sight, He then left zoology for botany, but, as his sight powers were steadily worsening, he thence undertook purely theoretical works.

While still a student and contrary to the prevailing trends in Geneva, he was fascinated by Augustinian Neoplatonism as this appeared suitable to merge in his views with his commitment to the basic tenets of Leibniz's philosophy. Bonnet's theoretical syntheses (*Considérations sur les corps organisés*, 1761; *Contemplation de la Nature* 1764, which was later translated into Italian by Spallanzani; *Palingénésie philosophique*, 1770) had a considerable influence on Cuvier.

When Bonnet begun his studies on Plant-lice, it was already well known that in most species apparently there occurred only females (it was later found that most species of plant-lice have a cyclic parthenogenesis, there being several generations of females, which every so many generations produced both male and females, which regularly copulated, thus reopening the cycle). By growing females in isolation since their birth, Bonnet was able to show that these virgin females did normally reproduce for several successive generations. The fact that eggs could develop without fertilisation appeared to Bonnet and many others as the best evidence for the ovistic theory. Gradually Bonnet committed himself to a most extreme pre-formism and it was he who completely formalised the theory of the 'emboitement des germes' We have already mentioned the essential of this theory and need not to repeat it. It must be stressed, however, that Bonnet never dreamed that such 'emboitement' was infinite, neither he absolutely denied changes. As we said most preformationists thought the Earth to be between 4,000 and 6,000 years old. Bonnet's ideas were complex and occasionally obscure. Basically he agrees with the Augustinian theory of creation, however he dissents from Augustine in believing that everything did not exist only *potentialiter* in the Aristotelian sense, but *in actu* in the shape of infinitely small germs, housed one within the other. Bonnet may have been inspired ultimateley by the anti-Augustinian theses maintained by Malebranche (1638-1715) and by Cardinal De Polignac in his *Antilucretius*. Such germs, as conceived by Bonnet, however, were supposed to be of two sorts: 'repetitive' and 'of improvement'. Thus there was a series of hundreds or thousands generations, each one identical with the preceding one, but when 'global revolutions' occurred, and Bonnet judged that at least three and probably more had already occurred, the 'germs of improvement' would begin a new series of improved and more complicated beings. Bonnet's 'Révolutions du Globe' are much like the successive 'ends of the times' which in many a religion close one cycle in the history of the Earth and open a new one. Such were envisaged, to remain in a Christian tradition, by the prophecies of Saint Paul or the famous and influential medieval ones of Abbot Gioacchino da Fiore. Thus Bonnet expected that by the last global revolution each kind of being would have reached its own perfect stage. In the meantime the 'germs of restitution' of dead organisms migrate as guest into both organic and inorganic structures waiting for the Final Judgement. When this will come, while the 'restitution Germs' of human beings would originate a new humankind so much better than the present one that it would immediately move into a better world, all the other organisms would improve too, so that Elephants and monkeys would improve their intelligence to the point that they would have their own Leibnizs and Newtons, Beavers would produce engineers comparable with Vauban and even plants would at least be capable of walking! Strange as this theory does seem to us, no less than Cuvier thought it 'Admirable!' and, as we shall see, was greatly influenced by it.

Coming back to less theoretical or metaphysical aspects of the debate, the first name deserving quoting is again Spallanzani, who wrongly thought that his experiments on frogs had finally proved his 'ovistic' persuasion. Spallanzani's results were most welcome by Bonnet, who, on purely theoretical grounds had ruled out any possibility that the 'little worms' of the semen could have anything to do with reproduction. Bonnet's main argument was, curiously, an economic one: as the number of offsprings generated is always comparatively low, should these derive from the sperms there would be an incredible wastage of individuals, a thing that neither the Eternal Father nor the Laws of Nature could possibly wish. When it was objected that, indeed some animals reproduced by the thousands, Bonnet ignored the objection. Truly it must be said that Bonnet's argument was used also against the panspermy hypothesis. Anyway Spallanzani had some reservations on his friend's theory of 'emboitement'.

We said that Spallanzani thought that some of his experiments supported his 'ovist' persuasion, let us see what he did, as, in fact his experiments were perfectly conceived and it was just by repeating them more accurately that he was proved wrong! Spallanzani argued that in the Amphibians the tadpole was not born 'from the egg', but that it is the entire egg which becomes the tadpole. Indeed, in the species studied by him segmentation is total and the yolk-rich cells end up inside the embryo, thus Spallanzani argued that the egg was potentially a tadpole and, as such, that it could develop without participation from the sperm. He, by fitting male frogs with sort of pantaloons, was able to collect their sperm. He then proceeded to prove that the smell of semen, to which had often been attributed the function to quicken the egg, had no action whatsoever, while development followed any, even minimal, contact with the semen. He, then proceeded to filter the sperm itself and found that the diluted and filtered sperm lost its ability to fertilise the eggs the more thoroughly was filtered, while, if the filters themselves where then rinsed, the water where they where rinsed was capable of fertilising eggs and, by some strange calculations, assumed that the size of the fertilizing particle had to be in the proportion to the egg of one to one billion, which happens to be approximately correct in Anurans. However, for once, he did not make a microscopic examination of the water resulting from his rinsings and continued to flatly deny the function of sperms, which he considered to be parasites. It was just by correctly repeating this experiment that the function of the sperms was verified by Jean-Louis Prévost and Jean-Baptiste-André Dumas. Finally, in order to provide final proof that the sperms had no function in reproduction, he put his eggs into an extremely diluted solution of semen, so diluted that he thought that no sperm had remained, and saw the fertilisation occurred all the same. So, as the reader will notice, Spallanzani's experiments were well planned, but inadequately performed. One is tempted to suggest that Spallanzani's preconceived ideas for once prevented him from seeing what was under his nose.

As a by-product of his researches on semen Spallanzani was able to perform the first artificial inseminations in Mammals (Dogs) and, when his friend Bonnet learned of this achievement, he was not only enthusiast, but, in a letter to Spallanzani, mentioned the interesting results that in the future could accrue from implementing it in humans!

Preformation, even when conceived not as the pre-existence of the different organs, but simply of their primordia, met with considerable difficulties. A first problem was: how does it happen, supposing that the embryo exists inside the egg, that the individual born from it has both paternal and maternal features? To this other objections could also be added. The opposite theory, epigenesis, could easily answer the problem, but had few supporters during the 18th century, as, in order to explain the development of embryos, it required the existence of such entities as 'vital forces' and 'informative' powers and thus, to belong to an obsolete tradition, with numbers of not-testable hypotheses. It was, indeed 'less parsimonious' than the preformist one. Nowadays, when so many scholars stick by the 'principle of parsimony', the memory of the colossal mistakes that 18th scholars made by the application of this principle, should teach a sobering lesson.

Again it was the principle of parsimony that was used when it was argued that preformism was supported by the fact that it was easy to see in the seed the miniature of the future plant, as the seed was, wrongly, believed to correspond to the egg.

Among the rather few epigenesists, one may mention Felice Fontana, who made a very pertinent criticism to Spallanzani's claims, but there is little doubt that the best was Caspar Friedrich Wolff, who made an all out attack on preformation. Wolff was born in Berlin, the son of a tailor in 1733. He studied medicine and philosophy in Halle and graduated in 1759 presenting a thesis, *Theoria generationis*, which is almost his only title to fame. His stand was not welcome in Germany and, thus, he accepted an invitation by Catherine II of Russia (who was a German princess) and got a chair in St. Petersburg, where he died in 1794. The work of Wolff was appreciated even by preformationists like Bonnet, and was just the best that epigenesists could then produce, but it was generally disregarded by his contemporaries, though much later it has been considered as the true beginning of modern embryology. Wolff was an excellent observer and was able to show that the organs of both plants (leaves, roots, etc.) and of the chicken, and especially the gut, cannot be seen in the earliest stages of development, and that microscopy shows that they develop from undifferentiated materials (Wolff had no idea of cells and thus he thought of molecules).Wolff theory is largely a vindication of ancient theories, but he supports them by accurate observations. He maintains that there must be a *vis essentialis* an essential force which first directs development and thence the maintainance of the adult individual. Apart from this 'force' Wolff, tries, nevertheless to provide an interpretation of development as mechanistic as possible. An interesting point and one which is usually overlooked is that in the *Theoria generationis*, Wolff suggests as an hypothesis that all the flower's parts, petals, sepals, etc. are modified leaves. This is precisely the 'floral theory' which was later and independently proposed by Goethe. Anyway there are several aspects of Wolff which may qualify him as a forerunner of the 'Naturohilosopie'. Later in his life (1768) Wolff published several observations on the development of the gut in the chicken embryo.

Regeneration

The problem of the regeneration of mutilated parts was obviously connected with that of embryogenesis. The problem had been raised by Trembley's experiments on the regeneration in *Hydra*. It was soon found that it was possible to cut earthworms into several pieces and that each one of them regenerated the missing organs. Thus not only several individual could be produced from a single one, but that the experiment could be easily repeated a number of successive times (later it was found that multiple segmentation is a common system of reproduction in several Annelids). The problem was the occasion for passionate debates and immediately prompted a number of investigations. Actually the first researches by Needham (1745) were on regeneration and their results prompted him to his later researches on spontaneous generation.

Spallanzani did show that in Salamanders and Newts it was possible to obtain the regeneration of the tail, of legs and even of the mandible; he did also show that in snails, if their heads were appropriately cut a new head could be completely regenerated (actually it was later found that it was necessary not to remove or damage some nervous ganglions). Spallanzani published these results in a paper titled *Prodromo di un'opera da imprimersi sulle riproduzioni animali* (1768) (= *Introduction to a book to be published on animal reproduction*), but the announced treatise never materialised.

Spallanzani's results made a sensation and that was especially so with his experiments of beheading snails. Voltaire himself repeated the experiment and got a few successes. The Académie des Sciences appointed a special committee to check the results (Turgot, Lavoisier, Tenon and Herissant) and these properly confirmed the Italian's results.

It was obvious that the problems of reproduction by budding and of regeneration, were highly interesting both to scholars and laymen as it implied some basic problems of a very general nature: Bonnet was keenly aware that it was incredible that the buds which formed on the surface of *Hydra* or in the scars of the mutilated salamanders of his friend Spallanzani could each contain a pre-formed miniature spare *Hydra*, or a leg or a mandible.

The flurry for regeneration, or, in modern terms for experimental morphology, died out rather quickly, possibly as scholars became aware of the difficulty of framing these phenomena into any theory different from the otherwise hated epigenesis. Thus Spallanzani himself abandoned these researches and it was over one century later that they were effectively resumed.

The moral of the story is clearly that when some evidence is found that requires a complete revision of all the current theories, the probability is rather that the subject is either dropped or ignored than that it really promotes further research and questioning of established beliefs.

The physiology of the 18th century

We have seen that, during the 17th century different authors had tried to improve on the understanding of the various bodily functions either by the interpretation of the available evidence in the framework of the mechanistic physics or, alternatively, by the principles of a 'Chemistry' which was still, by and large, based on the traditional alchemy of the Renaissance.

Descartes (Cartesius) had proposed a sort of rigid code to which the methods and scope of any naturalistic endeavour should conform. We have also seen how 'iatro-mechanistic' scholars, like Borelli and Santorio, often quite independently from the Cartesian rules, had, in practice, implemented such a program.

Harvey, by his falsification of Galen's theory of circulation, had practically exploded traditional physiology, so that a complete reassessment was needed. However, even forgetting the many conservatives who were in principle against all changes and reform, even scholars quite willing to change, were in a quandary as to how to begin: the new evidence clearly negated Galen's physiology, but until the 18th century a new comprehensive framework proved impossible.

The new physiology is largely the result of the work of a few notable scholars, the three greatest ones being, by common consent, Hales, Haller and Spallanzani and, because of his contribution to a basic problem, that of respiration, the chemist Lavoisier. Naturally these outstanding scholars were not alone and important contributions were provided throughout the 18th century by several other notable scholars.

Of these last, also because he is commonly mentioned in the books on history of biology, we shall mention first the Dutch Hermann Boerhaave, born in 1668 to a country parson and himself a professor in the University of Leiden. He was perhaps the most famous physician of his age and earned an immense amount of money, which he mainly spent in humanitarian and cultural endeavours. He died in 1738.

There is no doubt that Boerhaave was a great teacher: a number of the major physicians and scholars of his age attended his lectures, both the more theoretical ones, such as those on botany, and the clinical ones, and all his pupils had for him unlimited praise. His keen eye for spotting the promising youth was equally notable and we have already seen how he quickly changed into full appreciation his justified prejudice towards the young Linnaeus, who had falsely claimed to be in touch with him. Linnaeus was indeed perhaps the last promising youth whose advancement he promoted. While he was an excellent practitioner, he was a competent and sensible, but not outstanding biologist.

His main contribution was a textbook (*Institutiones medicae,* 1708) which was often reprinted and translated into several languages. This was almost the standard textbook in Physiology until the publication of Haller's treatise.

The *Institutiones* is a model textbook: the orderly sequence of arguments and the clear text are admirable. Digestion, circulation, respiration and reproduction are well described and the different interpretations of the evidence are most objectively debated both from the standpoint of a mechanistic and of a chemical interpretation. The contribution of Boerhaave himself is, however, a sensible attempt towards a synthesis of the two approaches with a slight penchant for the mechanistic interpretation.

The Rev. Stephen Hales was born in Beckesbury, in England in 1677 and studied in Cambridge when Newton was teaching there. He was later the parson of Teddington in Middlesex, and there he died in 1765. Already when a student he became interested in botany, but he began to study problems of plant physiology comparatively late.

Hales was ever fascinated by problems of mechanics of fluids and his early studies, published under the title 'Haemastaticks' concern the problem of the precise measuring of blood pressure in different animals (he began with the horse) and he soon noticed how pressure changes in different circumstances and according the physiologic and psychic conditions of the animal. He was also able to show how it is different in the arteries and veins and during the systole and diastole. He studied the differences in pressure in large and small Mammals, whether relaxed or excited and so on.

He was soon elected a member of the Royal Society (not a very difficult thing at the time, when almost any gentleman curious of philosophy and sciences was deemed to be eligible), but he was also elected to the Academie des Sciences. Later on he abandoned his studies of animal physiology for plant's and in 1727 he published his results as *Vegetable staticks*.

Hales was able to measure the amount of water absorbed by the roots and that lost through the leaves, thus measuring what botanists now call 'transpiration'. Among his most notable feats is the measure of the speed by which lymph climbs along the stem. He thus proved that this is a function of the amount of water absorbed by the roots and of its loss through the leaves. He was even able to measure the pressure of the lymph in the stem.

Hales made also the first advances in the study of photosynthesis as he showed how, contrary to the thesis of Van Helmont, the growth of a plant does not depend only from the amount of water absorbed by the roots, but that also air supplies some materials to the plant. However, the true significance of this discovery was not appreciated at the time.

Just as many other scientists, but also prompted by his religious charity, he studied a number of practical devices. He devised some ventilators to improve the conditions inside ships and jails (but the Admiralty was very slow in adopting Hales devices' on His Majesty's Ships). He studied also the possibility of distillation of seawater and problems of storage and preservation of foods and devices for the cleaning of ports, etc. Finally he devised an instrument like a barometer, to be used to measure the depths at sea by the measurement of the pressure.

Basically Hales aimed to study the physical aspects of both animal and plant physiology, taking advantage of his excellent training in physics.

Hales became particularly well known in Germany, as his books were translated into German by Christian Wolff (1679-1754), a good mathematician, an average botanist and who is mainly remembered for his popularisation of Leibniz's philosophy.

At the same time Sarrabat de la Baisse (this is the name which he is usually quoted but it appears that this is not correct) (1698-1737?) studied the movement of fluids inside plants by using coloured solutions: he put plants, either entire or parts of them in the solution and measured the amount and speed of absorption of the coloured fluids ('Dissertation sur la circulation de la sève dans les plantes', 1733). Other investigators of the same kind of problems who made important contributions were Georg Christian Reichel (1727-1771) from Lipsia and Sir John Hill (1716-1775) in England.

Baron Albrecht von Haller, the son of a magistrate, was born in Bern in 1707 and was a most precocious genius: at ten he already knew Greek and Hebrew and was writing dictionaries. At fifteen he had already written some poems and tragedies. He took his doctorate in medicine at 19 defending a thesis in Anatomy, while he had already studied with both Albinus and Boerhaave. Then he made further studies in anatomy in London and Paris and in mathematics in Basel with no one less than one of the Bernoullis. Having come back to his native Bern, he started as a practitioner, but with little success, while he became soon famous as a botanist, a poet and a mountain climber. In 1736, when not yet 30 he was appointed as professor of medicine in Göttingen, a recently established university whose chancellor was the Baron (Freiherr) Otto of Münchhausen, the serious brother of the famous Munchausen (as it is usually Anglicised) whose fantastic adventures are still a classic of German literature for children!

In Göttingen he carried out an incredible amount of work, but in 1753 he went back to Bern, where he immediately became one of the town's leading personalities, so that he was even employed as a diplomat. In spite of his many commitments, he continued to work and publish on botany, anatomy, physiology, philosophy and literature both in prose and poetry. There is still some doubt as to the precise amount of his publications. He wrote over 1200 articles in a journal of Göttingen. Haller himself in his *Bibliotheca Anatomica*, which we shall discuss further on, lists 195 anatomical papers by himself. His biographer Snebier lists 576 titles as important, while Haser considers as especially significant 2 encyclopaedic books, 4 books of anatomy, 12 of physiology, 7 of botany, 5 of bibliography, 1 of poems, 4 historical novels and 2 books of theology; a good record if ever there was one.

Haller, besides being considered as one of the founders of physiology in a modern sense, must rate also as one of the first historians of science. He produced four monumental critical bibliographies, which are still much used: *Bibliotheca botanica* (1771-1772), *Bibliotheca anatomica* (1774-1777). *Bibliotheca chirurgica* (1775), *Bibliotheca medicinae practicae* (1776-1779). To this immense amount of work one must add a vast correspondence with the most important scholars of his age. A fair assessment of this immense work is that Haller produced much good work, but no great discovery. His great influence on contemporary and later scientists is basically due to his remarkable ability of synthesis, so that he produced a number of treatises that immediately became standard reference and study texts for the whole of Europe.

In his later age Haller suffered from melancholy and depression of a religious kind. It is indeed probable that his immense powers of work during his youth and maturity was the result of a long moderate ‘manic’ phase in basically manic-depressive syndrome. So during his depressive phase he even regretted the animals that he had so brilliantly used for his studies. Haller died in 1777, when just 70.

As we said, Haller was especially influential in the development of physiology, which he calls *animata anatome* by his outstanding treatises rather than because of his discoveries, which, however contributed much new and relevant evidence to biology. His treatise in eight volumes *Elementa physiologiae* (1759-1766) soon substituted as a basic textbook for the books of Boerhaave.

The main original contributions by Haller concern the mechanics of respiration, the physiology of blood-vessels and the embryology of bones (but on this subject some very important contributions were due approximately at the same time to the British John Belchier (1706-1785) and to the French Louis Duhamel du Monceau (1700-1782), who both took advantage of the the peculiarity of developing bones to fix on themselves the red colour obtained from the roots of Madder (*Rubia tinctorum*).

Haller criticised any purely mechanistic interpretation of physiological processes and thus he underlined the significance of digestive juices and especially of bile. As an embryologist Haller, in his early works was an epigenesist, but later he became an extreme preformationist of the ovist brand and was a harsh critic of Wolff. He was unquestionably a good human anatomist and made much use and recommended the usage of microscopes in embryological studies.

By far the most influential part of Haller’s physiology was his study of the basic properties of living matter and he effectively underlined the irritability of muscular fibres (which had been discovered by Glisson) and the fact that sensitivity is a property found only in the nervous tissue. He supports these contention by the evidence of 567 papers, 190 written by himself.

As Haller was not aware of the existence of smooth musculature in many organs, he argued that not only muscles but also the heart and the gut could contract when irritated and relax when irritation ceased. According to the mechanistic theory of Descartes, of Borelli and, to some extent of Boerhaave, the contraction of the muscle is a dilatation due to penetration into it of nervous fluid; yet both Steno and Swammerdam had shown by elegant experiments that such a theory was untenable. Haller proposed that irritability (a term that he took from Glisson), was a basic propriety of several living structures and that it was characterised by a reaction (movement) out of proportion with the intensity of the stimulus. Haller thus distinguishes between the ‘muscular force’, which is inherent to muscles, and a ‘nervous’ force which comes from outside the muscle and is transmitted through the nerves. Also the nervous force is independent from will and may operate even after death (and on this see further on the origins of electrophysiology). These forces, as postulated by Haller, are very different from the forces which contract or expand inorganic matter and some tissues both alive and dead, when environmental conditions of humidity, pressure, etc., change. Haller maintains that such parts of the body that show irritability are insensitive, that is they have no sensation, while sensations move along the nerves and end up in the brain.

The physiology of Haller, and especially his experiments and interpretations had a great influence on all the subsequent developments of biology. It seemed to many that especially the muscular force set organisms quite apart from inorganic matter, as this proved that in organisms there was some sort of mechanism entirely different from those found in inorganic matter. These differences were undervalued by mechanistic philosophers and overstressed by 'vitalist' scholars. In fact it was necessary to wait for molecular biology and electron microscopy in order to begin to have some clear ideas as to the origin and essential nature of life.

Haller was then the supreme theorist and, indeed, he was a singularly well balanced theorist in a century, that of 'enlightenment', which abounded with theorists who believed they knew the final answer for all problems, this being, I believe, the greatest fault in an otherwise splendid century.

Still in connection with Hallerian physiology, it is worth remembering Theophile Bordeau (1722-1776), the son of a physician and a physician himself, who studied in Montpellier and showed how also the gland's secretion is controlled by nerves. His observations are remarkable, though his interpretation of them happens to be entirely wrong.

Circulation

As we have already mentioned Hales' contributions, we may here just mention those of Spallanzani and of such evidence that was obtained by the improvements of the methods for injecting the vessels.

Spallanzani published but two papers on circulation, the most important being an early one (*Dei fenomeni della circolazione osservata nel giro universale dei vasi*, 1773). Indeed some good observations had been done by Cowper in 1702 by the study of circulation in the mesenteries, but Spallanzani, again using his preferred victims, the salamanders and the newts, and, later, the traditional chicken embryo, was able to follow the details of the passage of blood from the arteries to the veins through the capillaries and, at the same time, to provide an improved account of the movements of the heart.

Such was the intrinsic merit of the observations and the brilliance of the descriptions that Haller thought it fair to dedicate him the first volume of the *Elementa Physiologiae* and Bonnet was thoroughly enthusiastic about them.

While Spallanzani has the merit of having provided the final observations in the living animals, several scholars share in the merit of perfecting the methods for injecting coloured materials into the vessels, so that it became possible to follow in details the whole network of vessels. Undoubtedly the most important of them was Johann Nathaniel Lieberkühn (1711-1756), who wrote about several of the most important discoveries of this period in the field of microscopic anatomy. Of lesser significance are Petrus Simon Rouhault, who, in 1716, was the first to use coloured jellies for injections, Samuel Thomas Sömmering (1755-1837) and Ignaz Döllinger (1740-1799) who both improved this method.

Digestion

At the beginning of the 18th century there were three main theories contending for the best explanation of the physiology of digestion: iatro-mechanists argued that digestion was basically the result of mechanic grinding of the food (the most notable advocate of this theory had been Borelli), and it was followed to some extent by Boerhaave (who allowed for a partial fermentation). The second theory, which was advocated by iatro-chemists maintained that digestion was a sort of fermentation and it was maintained by people like Van Helmont, Boyle, Pringle, Macbride. The third and last one was advocating a modernised version of the Galenic theory of the dissolving principles, and thought digestion to be the result of the action of gastric juices and was proposed by Vallisnieri, Viridet, etc.).

The mechanistic theory was rooted in the old observations by the members of the Accademia del Cimento, that had recognised that the gizzard of many birds has a powerful musculature and is capable of crushing and mill many objects; moreover grain-eating birds and birds of prey, ate small pebbles, which could grind the food just as teeth.

Vallisnieri senior, however had argued that such mechanic function, though real was an accessory one and that digestion was basically the result of the action of gastric juices.

Réaumur had tried to solve the problem by experiment: he had fed some birds with perforated metal tubes filled with different foods, so as to let the digestive juices to freely enter it. Thus the food could not be affected by the mechanic actions of the gizzard. His results, however were far from clear, as it appeared later, because of a wrong choice in the kind of birds used.

Spallanzani started by repeating the experiments of the academicians of the Cimento, but, as it was his habit, he varied them in different ways and repeated them on different species of poultry. He thus verified the ability of such animals to grind extremely hard and spiky objects without any damage to the surface of the gizzard. Moreover he was able to show that, while the presence of grit (gastrolyths) was undoubtedly helpful, food was equally ground if the animals were prevented from eating grit. He also repeated Réaumur's experiments of feeding the animals with perforated metal tubes and thus was able to show that Vallisnieri was wrong when assuming that the crumbling of food in the gizzard was due to the gastric juices, but he also found why Réaumur's experiments were inconclusive: he found that the gastric juices were capable of digesting meat independently of any grinding action, but that cereal seeds, unless ground, could not be digested by the juices.

Spallanzani thence performed the first *in vitro* digestions. He begun by using the juices that he collected from the stomach of just killed turkeys, but that being both a scarcely productive method and a costly one, he turned to small sponges that he pushed into the stomach of crows and thence took out and squeezed. Thus he found that different foods dissolved in the gastric juices much more quickly that when left in water, which partly vindicated Vallisnieri's theses. Spallanzani did also follow the different stages of stomach digestion by feeding his crows with food put into small glass tubes that he recovered at different times. He then turned to mammals, himself included, but soon found that to force himself to vomit at regular intervals after feeding was a bit too unpleasant and thus abandoned the experiments.

Spallanzani's experiments were, as a whole, reasonably exhaustive, but he made an unavoidable mistake when working on mammals: he used, in order to have pure gastric juice, to take it from animals which had not yet fed and thus he could not get the hydrochloric acid, and never suspected its existence. Moreover he was deceived by his results in birds into assuming that the whole digestion occurred in the stomach and thought that the intestine had a merely absorbing function.

Respiration

As we have seen in the foregoing chapters respiration had always been a key topic of biological debate and its close link with circulation had been at least guessed since antiquity. Researches had been always mainly concerned with Mammals, not only because of the central position of Man in a science that was developing mainly as subservient to medicine, but also because of the fact that respiration is easier to study in terrestrial animals.

On the other side it was clearly impossible to propose a correct interpretation of all the evidence available until the true composition of air had been established and at least the basic facts of the oxidation processes discovered.

The reader will certainly recall the significance that since antiquity had been attributed to *Pneuma* or *spiritus.* and we will remember how J.B. van Helmont (1577-1644) had described how during many organic processes: fermentations, burning of coals, etc., there forms an air-like substance which is incapable of keeping a flame burning and which causes the asphyxiation of animals. The same fluid is naturally present in certain places, such as at the Spa mineral waters, in the 'cave of the dog' near Naples, etc. Van Helmont had called this substance *Gas sylvestre* and it is precisely

Carbon dioxide. Naturally we cannot summarise here the developments of chemistry during the 17th and 18th century, but we must mention that practically all the scholars who studied the problems of the nature of air and of those processes that we call oxidations, also experimented on animals and gathered some relevant evidence.

In 1676 Edmé Mariotte (1620-1684) a member of the Académie, whom any college student remembers as one of the culprits of the discovery of the 'Law of Boyle and Mariotte', maintained, on the evidence of somewhat crude experiments, that plants synthesised their growing matter with the help of air.

In practice, by the end of the 17th century it was generally known that burning, calcination, fermentation and respiration had something in common. Moreover and that since Vesalius' times, when artificial respiration had been already attempted, some experiments had provided interesting evidence. The Bolognese professor Fracassati in 1665 had noticed that venous blood, if put in contact with air, even *in vitro* becomes bright red like arterial blood. Four years later R. Lower (1669) using artificial ventilation of the lungs, a technique commonly used by Robert Hooke, was able to show that the blood flowing from the lungs to the heart is arterial blood; thus it necessarily followed that the change of the blood from venous to arterial must occur in the lungs and not in the heart, as it had been commonly been maintained until then. Moreover it was known that it is air that is responsible for the change. Indeed, that the change depends from the air was proved by the fact that as soon as respiration stops, so stops the production of arterial blood.

Borelli, in 1680, had acknowledged this function of the air and had, rightly, maintained that air does not enter the blood through minute pores, but that it becomes dissolved into the thin layer of liquid that soaks the surface of the lung alveoli and that it is absorbed into the blood as dissolved air.

Sir John Mayow (1646-1679) prepared what he called 'Nytro-aerial spirit', which was in fact oxygen. Mayow thought that his 'nitro-aerial spirit' was the substance theoretically postulated by the Polish diplomat and alchemist Sendivogius (1556 or 1566-1636 or 1646) in his *Novum lumen chymicum* (a book much studied by Newton). Sendivogius, starting from some ideas of Paracelsus and, most probably, of Alexander Seton (+1604) had assumed the existence in the air of a 'nitro-aerial spirit' which was needed for burning and that he described as having all the basic characters later found of oxygen. Not only did Sir Mayow prepare pure oxygen, but he proved its function in keeping the animals alive, in making the blood bright red etc. However, the theoretical basis for a modern view of air and respiration were still lacking (and, as we shall see, not even Lavoisier fully realized them, as he assumed that in the reactions other fluids were also participating such as a supposed 'caloric' (= heat) and possibly light, which he conceived as being both corpuscular substances).

The understanding of the chemistry of oxidations was reached by a quite devious path: the temporary triumph of the 'phlogiston theory', which was repeatedly amended until it was finally abandoned, except by Lamarck, at the beginning of the 19th century.

The 'phlogiston theory' originates from the ancient idea that fire is an element. Johann Johachim Becher (1635-1682) in his *Physica subterranea* refused the Paracelsian hypothesis of the three principles (salt, mercury and sulfur = principle of fire) and substituted Air, Water and Earth, but his Air is barely a principle causing activity wich causes different combinations of the other two, moreover he distinguished among three different kinds of 'earths', which to some extent correspond with the principles of Paracelsus. These are *Terra lapidea* which may be fused and can turn into glass, *Terra pinguis* which is fat, oily, can burn and has sulphuric qualities, and *Terra fluida* or *mercirieaxis* (which is something as mysterious as the *Aqua sicca* = dry water, of some of the alchemist recipes by Newton). The next step was by Georg Ernst Stahl (1660-1734), who graduated in medicine in Vienna in 1684, was a professor in Halle from 1694 to 1716 and was afterwards court physician in Berlin. Stahl was, like van Helmont, both an excellent experimenter and a mystic. He reprinted Becher's book and thence re-elaborated Becher's ideas in his *Fundamenta chimiae* (1723) which develops the first version of the 'phlogiston theory'. According it the Phlogiston is a substance which basically corresponds with fire and heat, is contained in different amounts in all bodies and, when anything burns the phlogiston leaves the burning object and either disperses into the environment or is transferred into some other substance involved in the reaction. In a sense it is precisely the reverse of our present ideas: we think that during oxidation oxygen gets fixed to the oxidised substance, while according Stahl it was the phlogiston that was being lost. It was immediately appreciated that the theory, as such, was not entirely satisfactory, though it provided an apparently satisfactory explanation for many phenomena, which were left unexplained by the previous theories.

In order to appreciate the full value, at the time, of the phlogistic theory, one must just think that such a chemist as Furcroy (1755-1809) followed it for a while. Furcroy was one of the major scholars who worked at the Jardin du Roy. He held Lavoisier in high estimate, even when, having sided with the Jacobins, was his political opponent. Well: Furcroy was for quite some time absolutely uncertain whether to side with the phlogiston theory or with the new chemistry of Lavoisier. Curiously Furcroy together with Daubenton had a great part in saving the Jardin and the Museum during the Revolution. Just as many other members of the Academie, he had been savagely criticised by Marat, who was furious with the Academie which had snobbed his would be scientific papers. However Furcroy, whether for real convinctions or by simple opportunism, sided with the radicals and Marat became friendly, so that Furcroy became a member of the Convenction and chance dictated that he went to a meeting for the first time just after the murder of Marat and set in his seat. Once a member of that legislative body, Furcroy used of his abilities in support of the Museum and more generally for sciences and made a successful political career with all the successive French governments.

Coming back to our narrative on the phlogiston, the Dutch physician and naturalist Jan Ingerhouser or Ingenhousz (1730-1799), born in Breda, who studied with

Albinus and was famous as an excellent variolizer, had discovered that plants could 'purify' air when in the light and 'damage' it when shaded or at night (he was obviously speaking in terms of 'de-phlogisticiated' air), and that only the green leaves had this power. He also proved that plants absorbed CO_2 (*gas sylvestre*), though that this was possible only for green plant was proved by H.J. Dutroche (1776-1847) in 1837.

One of the foremost advocates of the phlogistic theory was none the less than Joseph Priestley (1733-1804). Priestley had prepared pure oxygen (De-phlogisticated air) by the photosynthesis of plant kept under water, and made a number of experiments on the physiology of respiration in both plants and animals.

On the evidence of his experiments Pristley argued that during respiration animals lost the phlogiston that they had assumed with food (that food containe phlogiston was apparently proven by the fact that all foods can burn). They can thus eliminate phlogiston until the surrounding air is saturated. When all the dephlogisticated air in the environment has been saturated by phlogiston, the animal, incapable of further elimination begins to suffocate and eventually dies. Pristley thought that plants, with the help of light absorb phlogiston and thus purify the air.

It was Antoine Laurent Lavoisier (1743-1794) who first falsified the whole phlogistic theory and who established some of the basic facts of respiration. Lavoisier was a chemist (everyone remembers Lavoisier's principle of conservation of energy and matter, but he made also some pioneer work in geology) and, by 1775 he took as a starting point the fact that oxidation of metals involved an increase in weight and that the hypothesis which had been advanced that phlogiston had a negative weight was clearly untenable. Thus he argued that it was impossible that the process involved the loss of phlogiston, but that it necessarily required the acquisition of something. He then proceeded to show that what was acquired was precisedly Priestley's 'de-phlogisticated air' and Lavoisier proposed to call it 'acidifying principle' or 'oxygen' (= oxide generator). Furthermore Lavoisier proved that respiration is a process of oxidation and that the air polluted by respiration has the same composition as that of an environment where a metal has been calcinated, both having lost a certain amount of oxygen. However in the air polluted by respiration Lavoisier found what he called 'aeriform calcic acid', that is the same as the 'fixed air' of Black. This last gas could be eliminated by the presence of a caustic base and only then the remaining air has the same qualities as that remaining after calcination of a metal. Such remnant could not sustain life and, therefore Lavoisier called it 'Azoth'.

Thus Lavouisier proved that air is a mixture of oxygen, azoth and 'fixed air', this last being, as he soon found, a combination of oxygen and carbon.

By then Lavoisier had shown that respiration can be compared with the process of burning and that this may explain the origin of animal heat. This last was indeed the explanation that was jointly proposed by Lavoisier and Lagrange in a paper of 1780. Yet they still thought that the process involved the dispersal of an additional substance, which they called 'caloric', that is heat, and which strongly resembles the old phlogiston.

'Heat' was supposed to be a fluid with such a low density as being impossible to measure. They also thought that this combustion occurred in the lungs, that it heated the blood in the lung's alveoli and that the heated blood carried its heat to di different organs.

In 1785 Lavoisier, by measuring the amount of oxygen assumed and of 'fixed air' lost during respiration, found that some oxygen apparently disappears.

A few years before Lord Cavendish (the great chemist and a most perfect example of the eccentric nobleman) had established the composition of water and had discovered hydrogen. Thus Lavoisier concluded that the small amount of oxygen which is not eliminated as carbonic anhydride is combined in the body to produce water.

So far Lavoisier had reasoned as a chemist and Lagrange as a mathematician, and they were basically right, but Lavoisier was mistaken in a paper published with the physiologist Séguin (1790) where they assumed that the oxidation of both carbon and hydrogen occurred in the lungs and that it was due to the the oxidation of a 'hydrocarbonic fluid', supposed to be produced by the bronchioli. They indeed completely overlooked what was already known of the effects of air on the blood and discounted the possibility that blood acted as a transporter of oxygen. It is quite possible that they purposedly overlooked such evidence: the hypothesis that oxidations accurred in the lungs was simple, while that of transport by the blood, resuscitated the ancient Greek hypothesis of the transportation of 'pneuma' by the blood: the evidence was there, but there was no theory capable to explain them.

Lavoisier was a very rich man and had been one of the last 'Fermiers generaux' of the king, a top position in the financial administration of the state. He was thus beheaded in 1794 without having the opportunity to pursue his studies (his widower later married the famous American scientist and inventor Benjamin Thompson, count Rumford).

It was thus left to Giuseppe Luigi Lagrange (1736-1813) to determine the true function of blood in respiration.

Lagrange was born in Turin, but he was of French origin: his grandfather had left the service of Louis XIV for that of the king of Sardinia) and is usually remembered as one of the greatest of mathematicians. Soon after the death of Lavoisier, he realized that the hypothesis proposed by Lavoisier and himself a few years before was untenable. Indeed, already in 1791 a pupil of Lagrange, Hassenfratz had announced that Lagrange had noted that the temperature in the lungs is too low to justify the hypothesis that the respiratory combustion could happen there and that it was more reasonable to think that the oxygen in the lungs became dissolved into the blood and that, thus carried through the body, was burned in the different organs, where occurred the true respiration.

Also Spallanzani made some really important contribution in a posthumous paper, published in 1803. By a series of experiments both on terrestrial and aquatic animals he proved that oxygen was, indeed necessary for life and that it is just absorbed through the different organs of respiration (lungs, gills, tracheae, skin) and that, carried by blood, is thence used by the different organs. Moreover he proved that the link existing between respiration and production of Carbon dioxide, is entirely indirect. In fact snails placed into an artificial atmosphere of azoth or hydrogen or in boiled water, which had thus been deprived of dissolved air continue, until they die, to produce the same amount of carbon dioxide as in a normal atmosphere. He thus proved that, while oxygen is indeed necessary for all vital processes, these may go on some time even when there is no imput of oxigen from outside, clearly using up what little reserves there are in the tissues.

Meantime the Swiss chemist Nicholas Theodore de Saussure (1763-1845), by a series of elegant quantitative experiments clarified many aspects of plant respiration.

The physiology of the nervous system

While interest for the nervous system had always been a lively one, yet up to the middle of the 18th century little of interest was added to the old lore.

The first person deserving mention is a rather extraordinary personality: Emmanuel Swedberg (who became Swedenborg when he was knighted) (1688-1772) was the son of the Lutheran bishop of Uppsala. Nowadays he is chiefly remembered as a mystic who founded a religion which still has a small following in Northern Europe. However he turned to mysticism late in his life. During his youth he was a military engineer during the last campaigns of the warlike king Charles XII, then he became a brilliant engineer of mines and made significant contributions in theoretical physics and in anatomy. He was also an active member of the Swedish equivalent of the House of Lords. Suddenly in 1744-1745 his religious calling was revealed to him in a vision.

From our standpoint Swedenborg deserves mention as one of the earliest scholars who correctly localised in the brain cortex the main higher functions of the brain, such as ideation. Until then, as the reader will recall these had been commonly believed to be located in the walls of the ventriculi. Moreover Swedenborg believed that the pyramid cells, which had been described by Malpighi, reached, by extremely thin branches both the cortex and the various organs and that through them flowed a *fluidum spirituosum* which was responsible for the functioning of the nervous system. Later Swedenborg developed on this groundwork a complicated theory with a strong mystic tinge.

Much of the century was enlivened by the debate whether or not there was animal electricity and whether the 'nervous fluid' was just animal electricity. Such debate was always to take into proper account the evolution of theories on electricity as well as the development of electrical equipment.

We have mentioned how during the 16th and 17th centuries there were proposals to identify magnetic forces and gravity, or rather 'attraction' and later to identify magnetism with electricity. These were natural developments of the Cartesian principles which aimed to find a single cause for all movements. Once abandoned shocks and elasticity, it was plainly plausible to come to consider such forces that, whatever their nature, were capable of produce movements. The discovery that electrical shocks produce muscular contractions followed immediately the discovery of Leiden's jar (1746) and the fact that simultaneously with the spark in a Leiden's bottle joined with a muscle there was a sudden contraction was immediately applied in therapeutics. This was especially common for paralysis or pareses, but enthusiasts like Marat, when he was practising as a physician before he turned into the murderous fanatic of 'La terreur', used it to cure an endless lists of diseases.

At the University of Bologna there was a group of scholars deeply interested in the new Hallerian physiology. At the time it was commonly supposed that 'Nerveous fluid' once formed in the brain, could freely flow in the nerves and thus reach a speed sufficient to shock the target muscle and cause its contraction.

Marcantonio Caldani (1725-1813) made himself a spokesman for Haller's theories during the Carnival anatomy of 1760 and his lecture met with such a disapproval from the majority of the faculty (and with enthusiastic support from the minority), that shortly afterwards Caldani moved from Bologna to Padua. Anyway both Haller and Caldani believed the nervous fluid to be something different from electricity, as they both believed that animals could not confine the electric fluid into the nerves. Another Bolognese 'Benidictine', Tommaso Laghi (1709-1764) in 1757 maintained that nerves must have been sheathed by an isolating membrane, a true anticipation of Schwann's sheath which was actually discovered eighty years later. Laghi had been helped in his researches by Galeazzi, who was the father in law of Galvani and a supporter of Caldani.

Luigi Galvani (born in Bologna in 1737 and died there in 1798) was professor of anatomy and surgery there. He begun the series of his publications by submitting in 1773 to the Benedictine Academy a thoroughly Hallerian paper on irritability; moreover Galvani, at this stage of his researches, followed the identification by Laghi of the nerveous fluid with electricity. This was apparently supported by the familiar fact that Torpedoes (and other electric fishes) were able to store electricity. The electric organs of torpedoes are, in fact, modified muscles, and each one of them consists of a pile of superimposed discs. These organs were studied by Alessandro Volta during his preparatory studies for 'Volta's pile'.

Galvani began his experiments on the muscular contractions of frogs in 1780, just when his colleague, the physicist Veratti (the husband of Laura Bassi, whom we mentioned as professor of Physics and philosophy and a cousin of Spallanzani) was at work on electric phenomena in the atmosphere. Galvani's studies engaged him for ten years in systematic experimentations, which he alternated with studies on the gases which could be obtained form animal tissues and, as a physician, trying to combine the two sets of experiments and their results into a single useful theory.

He kept delaying the publication of his results, but he communicated them to a number of colleagues, so that they were largely known in Italy and also abroad.

At least during the early phases of his studies his main assistant was his wife, who appears to have been the one who called her husband's attention to the fact that frog's muscles reacted to the spark even when not directly connected to the Leiden's jar. With the help of his collaborators, and especially of his wife and of his nephew Camillo, Galvani made a systematic study of the problem, including the effects of atmospheric electricity. In 1786 he found that the muscles of a freshly dead frog contract when one completes a circuit between the muscle and its nerve by a bimetallic arch, and argued that the muscle, like a Leiden's jar is a store for electricity and that this is positive inside the muscle and negative at its surface. Therefore when the two are connected there follow the contraction. However count Alessandro Volta himself (1745-1827) pointed out that it is the bimetallic arch itself that is the source of electricity (and this was the starting point that led him to the discovery of the voltaic pile). Henceforth, while Volta continued his experiments, in spite of his wife's complaints that he was using silver and copper coins to build his prototype piles, and became one of the fathers of modern physics; Galvani repeated his experiences with a monometallic arch and again got his muscle contractions, and finally was able to get a slight contraction by closing the circuit by touching the isolated muscle on a glass dish with a cut surface of a nerve, thus thinking that he had found a final test for his theories.

In the meantime news of Galvani's discoveries had become known all over Europe and Baron Alexander von Humbolt (1788) confirmed them. Finally in 1790 Galvani, morally shattered by the death of his wife, decided to abandon scientific research and published in four sections all his results.

Anyway the study of animal electricity was hampered by the lack of precise measurements until in 1825-27 Nobili's galvanometer was built. The debate on the homology-analogy of galvanic and voltaic forces was practically settled by Matteucci in 1838-1840.

A side line of the debate on the nature of nervous fluid was the quarrel on 'Mesmerism', scientifically of marginal interest, but rather interesting as it involved some major personalities.

The distant origins of mesmerism are very ancient. We may remember the medieval debate on planetary influences and on 'sympathies' and this had led to the problem of the nature of such influences and of the working of the nervous system.

The paracelsian Sebastian Wirdig (1613-1687) was perhaps the first to suggest a link between 'sympathies' and magnetism. Van Helmont was then maintaining that a *spiritus* was an extremely 'thin' and volatile substance (everyone knew that if you breath 'wine spirit' or alcohol for enough time its effects are just the same as if you drink it). Animal spirit was joined with the immaterial soul and controlled the sensibility and vital processes. Thus Wirdig argued that strong spirits could dominate, by a sort of fascination, the weak ones. Similar thoughts had been stated by Robert Fludd

(1574-1637), who thought that man had two poles and an equator, like the compass' needle (an idea which made a come-back in the early 19th century in Germany) and that, again like magnetic needles that, when close enough, influence each other, when two people meet there is a magnetic interaction. Our old friend Father Athanasius Kircher (1601-1680), being a Jesuit, could not agree on such an identification, and argued that the magnetic forces of minerals and those of living beings were of an essentially different nature. In 1679 the Scot William Maxwell, advocated ideas that were rooted in an extremely archaic tradition: the vital principle came from the Sun and was transmitted to living beings by the movement of the tides. He wrote a three volumes treaty, *De medicina magnetica libri III*, where he listed all the cases which had benefitted from treatment with a magnetic water of his own invention. Boerhaave supposed that an extremely fine fluid was produced in the brain and that its motions caused nervous reactions. Friedrich Hoffman (1666-1742) developed Leibniz's ideas (Leibniz's authority and his monads had a pervasive effect in all fields of biology) and thought that the nervous fluid was an ether occurring in the brain, in the blood and in the lymph and that caused movements; its particles or monads had each its own 'Bewegungstrieb', a motor program similar to that supposed by Malpighi. This is roughly the pedigree of the assumed relationship electricity-magnetism-nervous fluid.

On the other side, lode-stones had been sporadically used by physicians since antiquity. To keep to European authors, the Byzantine Aetius from Amida, recommended its use in some pains, Marsilio Ficino, Pietro Pomponazzi and Girolamo Cardano employed lode-stones, and their systematic application for diseases linked with the influence of Mars and a few others celestial bodies was strongly advocated by Paracelsus. The debate on magnetism and the medical usage of lode-stones went on through the 17th and 18th centuries, just as the debate on critical days and their possible connection with astronomical events. All this shows how the Mesmer 'scandal' was linked with the ever present and multi-faced Paracelsism.

Franz Anton Mesmer (1734-1815) was of rather poor family, but his brilliant genius gained him the support of the bishop of Constanz, who paid the fees for his training at the universities of Dillingen and Ingolstadt; there he graduated in philosophy and thence passed to Vienna, where he graduated in medicine in 1766 with a self revaling thesis titled *De planetarum influxu*. In 1768 he married a very rich widow, Marie Anne von Posch, and henceforth practiced medicine only for charity and became a musical sponsor (Mozart's early little piece 'Bastien und Bastienne' was played for the first time in Mesmer's home), Mesmer himself being a good player with the glass armonica. In 1774 he tried for the first time the lodestone on a neuropatic patient and obtained, as it happens with this sort of patients, an immediate success. Others followed and in 1775 Mesmer published an account of these first cases and tried to explain them by a rather confused hypothesis, where traditional Paracelsian theories on microcosm and macrocosm mixed with the identification of electric and nervous fluids. Mesmer's activities were immediately boycotted by the Viennese medical body, even well beyond the verge of honesty. The same attitude was later that of the Parisian medical faculty. There is no doubt that both Mesmer and his pupil Deslon (1750-1786) were absolutely honest and that both tried hard to have their methods and results objectively assessed by competent and unbiased judges. Various committees were appointed , but only two people appear to have had an understanding of the problem: one was Antoin-Laurent de Jussieu, the botanist, who, though opposing the views of Mesmer and thinking that a physical explanations could possibly be found, yet considered most evidence to be genuine and so refused to sign the negative report of his committee. The other was Jean-Sylvain Bailly (1736-1793), a brilliant astronomer, who had advocated a curious theory on the location and age of the 'lost continent' Atlantis and then chief of the police, that in a secret report to King Louis XVI made extremely pertinent comments, one could say in a psychanalytic framework, on the relationship that grew between the patient and the physician and on the peculiar features of the therapeutic crises. Bailly fully subscribed the fears that Deslon himself had reported to the police, on the dangers of prevarication by unscrupulous doctors. Several people, including Benjamin Franklin, who was then ambassador in Paris for the future United States, thought that Mesmer's results were due to suggestion. Mesmer, indeed, had inadvertedly stumbled into group psychotherapeutic techniques and that is the only reason for which he deserves a small place in the history of the physiology of the nervous system. Growing hostilities from the medical establishent and the tensions forecasting the revolution made Mesmer quit Paris in 1785 and, in the end, he settled in a Swiss village, where he continued to practice for love. In spite of accusations of chicanery, there is no doubt that he was honest. He could not appreciate that what he was doing was a systematic practice of hypnosis and that some of his successes were probably due to improvement of the functioning of the immune system as a consequence of diminishing stress in neurotic patients. All taken Mesmer was an infortunate and misguided pioneer of the physiopathology of the nervous system.

As we are dealing here with aspects of research which are difficult to place, let us mention here Spallanzani's experiments on the bat's sonar. With the proviso that 'sonar' occurs only in Microchiroptera and that Spallanzani's animals were precisely members of this sub-order, Spallanzani, by a series of elegant experiments proved that bats can fly and catch their preys even in complete darkness and that, even if blinded, can avoid extremely delicate obstacles, such as weighted threads hanging from the roof of a room. After his early experiments Spallanzani thought that bats had some new sense organ, which he could not identify. Later, after Jaurine had shown that the bats lose their capacities if either the nose or the ears were choked, Spallanzani repeated and improved the experiments and concluded that the bats could do such performances because of their hearing.

Spallanzani made also a number of other miscellaneous and brilliant experiments on light perception by different animals, on the electric organ of Torpedoes, on hybernation and on the catadromic migrations of Eels, etc.

Morphology

The term 'Morphology' was proposed by Goethe towards the end of the 18th century in order to signify something more that the mere anatomy, the precise description and the medico-functional interpretation of the evidence, that had been the aim of the anatomists of the first half of the 18th century and of the preceding ones. 'Morphology' had 'philosophical' implications: it asked questions about the significance of each structure in the general framwork of Nature and of its laws.

However, the transition from pure anatomy to morphology was naturally a gradual one and the first half of the 18th century numbered several distinguished anatomists and, especially as far as human anatomy is concerned there was a steady progress, although still in the pattern of that of the foregoing century.

We shall here name but a few ones, who were, perhaps not better than others, but that for some reason were more famous.

A first example is Joseph Guchard Duvernay (1648-1730). He belonged to a most ancient and noble family and was anatomist at the Jardin. Having studied in Avignon, he became an Academician in 1674, when barely 26. His fame is due to his qualities as a teacher and as, being the teacher of the Dauphin, succeeded in making anatomy 'à la môde' at court. When elderly and ailing, he became passionate for the study of terrestrial Molluscs. He made some advances in the anatomy of the circulatory system of lower Vertebrates.

Bernhard Siegfried Weiss is the already quoted Albinus (1697-1770) of Frankfurt on Oder. He was appointed as professor at Leiden when barely 24, and enjoyed a vast repute among his contemporaries. He was a man of vast culture, an excellent teacher and a student of the history of anatomy. His special field of enquiry was the development of the skeleton and published a very accurate and superbly illustrated descriptive treatise titled *Tabulae sceleti et musculorum corporis humani* (1747). As for his colleague Boerhaave, a good deal of his renown is a reflection of that of his many famous pupils.

One of them was Johann Nathanael Lieberkühn (1711-1756), whom we have already briefly mentioned, he was born in Berlin, the town where he also settled as a practitioner after graduation and where he continued his studies of microscopic anatomy. His *Dissertatio anatomico-physiologica de fabrica et actione villorum et intestini tenuium hominis* (1745), includes the description of the crypts in the mucosae of the tenuis that still bear his name (though hunters for priorities have a good case for claiming their discovery for Domenico Maria Gusmano Galeazzi of Bologna, 1686-1775). As we have already said Lieberkühn considerably improved the technique of injection in the thin vessels, which later allowed for many of the detailed studies of the circulatory and lymphatic systems.

Having mentioned the technical improvements suggested by Lieberkühn, it is proper to mention here other technical advances in microscopic anatomy. Spallanzani had suggested some stains suitable to make the interpretation of microscopic preparations easier, but really significant advances were suggested by Philippe Pinel (1755-1826), who is mainly known for his contributions to the improvement of phsychiatric therapy and for his monumental, and failed, attempt to a general classification of all diseases on the Linnean patterns.

Other improvements were suggested by Marie-François-Xavier Bichat (1771-1802), an anatomist who first proposed a general classification of tissues in his *Anatomie generale* published in 1801. Curiously Bichat is considered as the founder or as one of the founders of histology, while he always fiercily opposed the usage of microscopes! He was the first to classify the tissues in a way resembling the present one and thought that they were all formed by a meshwork of tiny membranes of different natures and that the macroscopic aspect of the different tissues depended on the different proportions of these membranes. Having assumed that all organisms were always made by different arrangements of but a few kinds of tissues which were the same in all animals (which is not quite true), he provided a useful tool in the development of comparative anatomy. On the other side, as we shall better appreciate in the next chapter, he had a great influence on young Cuvier, especially as Bichat paid great attention to what he called the 'economy' of organisms, a concept used also by Goethe, and from which derives the concept of 'balancement des organes' of Cuvier, Geoffroy and, with a somewhat different connotation, of Goethe.

Bichat was an all out vitalist and absolutely contrasted the non living world, where the laws of chemistry and physics obtain, and that of living matter. He defined life as a perpetual fight against death and that in life because of a peculiar quality or vital force, phenomena develop differently fron those of the inorganic world. Using a much later terminology, we can say that Bichat was the first to notice that organisms had a negative entropy, while in non living system entropy is necessarily positive.

Another scholar who made significant advances in microscopic techniques was Felice Fontana (1702-1805), a complex personality (and a difficult character) who studied the most diverse biological and non-biological subjects. As a hystologist he devised some of the earliest staining methods and thus provided a clearer description of striated muscles, and was able to see the nucleus, at least in some cells; he also saw both the axon and the myelinic sheath of nervous cells. Fontana was also charged by the Grand-duke of Tuscany to organise in Florence a Natural History Museum on the pattern of the Parisian one and this he did, but with some quite original features, such as the large collection of wax models of anatomy, a special feature of Florence and Bologna.

A most notable anatomist was Peter Camper (1722-1789), born in Leiden, but professor in Amsterdam, Groningen and Franeker. He delved in a number of subjects, ranging from surgery to gynecology and veterinary medicine. He made important contributions to human anatomy, especially of the skull and may be considered as being a pioneer of physical anthropology, where he introduced the measurement of the facial angle known as angle of Camper and which has been used in the classification of human races. Camper made also some important studies on apes and especially on the Orang Utang. He studied also the anatomy of many other animals such as elephants, rhinoceros, reindeer, etc.) He thought that he had discovered that the main bones of birds are pneumatic, that is they are hollow and that branchings of the air-sacs enter them, a fact that had been really discovered by emperor Frederic II of Swabia. Finally, he made comparative studies of the hearing apparatus of cetaceans, reptiles and fishes. Camper, like Tyson, may be considered as to some extent a forerunner of comparative anatomy, just as his contemporary Daubenton. However, as he did not have the necessary theoretical background, he is rather a pure describer.

John Hunter (1728-1793) was a Scot, his family was in rather poor circumstances and he did not follow a regular curriculum of studies. At twenty he went to London, where his senior brother, William (1718-1783) was a well known physician, surgeon and anatomist. John began by working as the sector for his brother and later he became his collaborator. He also thought of taking a degree, but after two months in Oxford, left as he thought that he was losing his time. For a couple of years he was a military surgeon, which gained hin a considerable experience. With time John Hunter became a famous surgeon, anatomist and teacher, though his cultural limitations always hampered him when lecturing.

As an anatomist he dealed indifferently in human normal and pathologic anatomy as well as in animal anatomy. He was a fanatic collector and spent all the money he earned for the increase of his museum. Thus he assembled an enormous collection of preparations, which, after his death and much debate, was bought by the State for 75,000 pounds and this was estimated to be just one fourth of the real value of the collection. It is obviously impossible to transform this estimate in today's money, but as these were gold sovereigns, it would certainly be an enormous amount. The Hunterian Museum was managed by the Royal College of Surgeons and was severely damaged during the Second World War. It has been rebuilt, partly with salvaged materials and partly with new preparations. John Hunter was a member of the Royal Society and several of his papers were published in the Philosophical Transactions. From the standpoint of pure biology his most important contribution is 'Observations on certain parts of animal oecomomy' (1786). This paper had a great influence on Bichat and, through Bichat, on Cuvier. Hunter had moreover a great influence on whole generations of youngsters both directly and through his museum and indirectly through the great Sir Richard Owen, one of the greatest of the British comparative anatomists, and who was for some years the director of the Hunterian Museum before being appointed as director of the British Museum.

Peter Simon Pallas (1745-1855) from Berlin, is commonly remembered as an explorer in the service of the Russian government, but he was also an anatomist and systematist of value. Pallas' activities as an explorer concern mainly Siberia and Crimea and from there he described many new species. Later he returned to Berlin.

Pallas always endeavoured to study the anatomy and biology of the species he was collecting. Quite apart from his contributions to the systematics and anatomy of Mammals, his *Spicilegia zoologica* (1767-1780) is an important collection of monographs on various invertebrates and especially on intestinal worms. He was, indeed, the first to suspect that the *Echinococcus* cysts, which had been known since antiquity, were parasites. In the *Spicilegia* Pallas severely criticised Linnaeus who had pooled in his *Vermes* a moot ensemble of the most different animals. On the other side Pallas was badly conservative when he maintained that a moot ensemble of fixed animals (Sponges, Coelenterates, etc.), that is the traditional 'zoophytes', were really transitional forms between the plants and true animals.

We shall not mention here a number of excellent human anatomists who also made some occasionals contributions to biology in broader sense.

An exception may be made for Felix Vicq d'Azyr, who is commonly considered as a forerunner of Cuvier. He was born in Normandy in 1748, and was a practitioner who also gave free courses in anatomy. He became personal physician to Queen Marie Antoinette. When the Revolution came, having been forced to watch the feast of the 'Goddes Reason', he caught a pneumonia which killed him and probably saved him from being beheaded after one of those farcical trials which, in Paris only, ended in over 1300 executions, including that of Lavoisier.

Vicq d'Azyr was too busy with his practice to make any really important discovery, but his abilities as a teacher made him influential, as was Daubenton, in persuading people that both zoology and medicine ought of be grounded on sound foundations of comparative anatomy and physiology. He wrote for the *Encyclopedie Methodique* an important *Discours préliminaire du système anatomique*, and three important treatises: *Traité d'Anatomie et Physiologie* (1786), *Système anatomique des Quadrupèdes* (1786) and *Sur l'analogie qui existe entre les membres inférieurs et supérieurs chez l'homme et chez les animaux* (1792). In them Vicq d'Azyr advocated the thesis, which was later championed by Geoffroy St. Hilaire, that there is a single organisational plan for all Metazoans.

Goethe

Another person whose manifold activities made him influential beyond the intrinsic merit of his personal researches is the great German poet Johan Wolfgang von Goethe (1749-1832). Goethe stated some general principles that are of great importance in comparative anatomy and, in a way, also in the development of evolutionary studies. In spite of the large number of studies on this man who was a poet, a novelist, a politician and courtier, a physicist, zoologist and botanist, there is still no comprehensive account on the reciprocal influences that all these activities had on each other. Moreover, for many different reasons, Goethe was for over fifty years a most influential power in the whole of the German intellectual life, thus shaping to some extent both the sciences and the philosophy of his country. Finally an incredible number of outstanding personalities got appointments in the German academic world by Goethe's recommendation and then, from their chairs, moulded whole generations of students.

Goethe was in some way a forerunner of the German 'Naturphilosophie' and had a great appreciation for young Schelling, so that he largely used of some of Schelling's works when writing the second part of 'Faust'. However this was after he had done all his main morphologic studies.

Goethe worked on 'Faust' for many years and may have never thought it as really finished; the poem-drama is in a sense Goethe's spiritual authobiography or, at least tells us much about the attitude of Goethe towards the Nature that he was studying.

It is clear from the poem that Goethe had a deep understanding of the German Renaissance thought and was quite familiar with the Paracelsian alchemists (in his youth he had been greatly interested both in alchemy and astrology). Indeed, Goethe is always stressing that in Nature there are trascendent values, and especially aesthetic ones.

On the other side, in spite of his passion for the Classic world, Goethe always sees it through the deforming spectacles of the Florentine humanists plus Schelling's ideas. His religious attitude is close to that of Spinoza and is a deism in which Christian salvation has no room (Faust's salvation is not due to repentance), but he sees macro- and microcosm always striving towards a perfection which is essentially renaissance neoplatonism. The world is seen as a dynamic self realisation which requires a material universe which in turn is nothing but the materialisation and realisation in infinite varieties of a limited number of archetypes. This is clearly a transformist attitude, but not an evolutionist one, as time has little or no room in it.

Goethe's transformist views began to develop during his trip to Italy. In fact the first instance of Goethe's intuition is dated by him 27 September 1786, following his visit of the botanical gardens in Padua and is repeated several times during the later phases of his journey and particularly on the occasion of his visit to the botanical gardens of Palermo. It took Goethe about two years to change his intuition into a fully developed theory.

I must say that I think it most peculiar that a man such as Goethe, when in Bologna in the autumn 1786, comments on the beauty of the University's buildings, but he makes no mention of the eminent scholars, such as Galvani, that were then active there.

We have said that Goethe introduced the word 'Morphology' in science. To Goethe 'Morphology' meant both natural and trascendent order. Indeed Mephistopheles, the trickster devil of Medieval literature, is the maker of the apparent chaos which masks the inborn order of nature and troubles its development.

Linnaeus had thought to be able to see the Order of Divine Design by the identification of a few, essential, features. Goethe maintains that what is needed is the contemplation of a global essentiality, which discovery requires the study of every structure and that the essential plan of all apparatuses may be studied by the comparative method. Thus far Goethe is close to Aristotle, but he also maintains, and on this the Stagirite would have had some queries, that for everything there must be an archetype, for instance an 'Urpflantze', an 'original plant' from which all plants derive (not by temporal evolution). This is basically the concept of a 'Bauplan' which proved to be such an essential tool in evolutionary morphology.

This was unquestionably the great intuition of Goethe, but the idea that there should be a general common basic structure for all plants and for all animals, had been in some way 'in the air' and was independently developed by Cuvier (whom Goethe appreciated, but could not suffer) and by Geoffroy St. Hilaire (whom he unconditionally admired). This gradually led to the identification of the many phyla currently recognized. Goethe maintains that morphological studies must be comparative, and that by the implementation of comparative principles we may identify the ideal prototype, whose individual materialisations are minor or major varieties of it. Thus orders, families, species and individuals may be recognized. It is, anyway, absolutely clear, as proved by the correspondence between Goethe and Schiller, that Goethe never thought of evolution as a historical process.

We said that Goethe was certainly a highly qualified (for the times) scholar of late Medieval literature and that may have been a reason for his collaboration with Johan Kaspar Lawater (1741-1801), an occultist and student of the peculiar pseudo-science of physiognomy (*Physiognomische fragmente*, 1775-78).

Thus his ideas in morphology may also be rooted in distant Augustinian traditions of the necessary perfection of creation; but he employed them pragmatically. Later and repeatedly, for instance recently by many orthodox cladists, there has been a tendency to formalize rigidly the basic principles of morphology, albeit on very confused theoretical backgrounds. In other thinkers, such as in recent years in the theological-evoutionary ideas of Father Teihard du Chardin, its neoplatonic aspects come into the fore.

Anyway the concept of 'plan of organisation' or 'Bauplan' of Gegenbaur finally became well formalized, as far as its methodological aspects are concerned, and is still quite useful. For instance it is easy to think of a Vertebrate basic plan which may be found in all Vertebrates, a different one for Arthropods, and so on; each 'plan' finds its material expression in living and past beings as topologic varietes (see further on on Schiaparelli and Thompson).

One important result of this concept has been that it made obsolete the traditionsl reference to human anatomy and it soon led to the realisation that there were several, quite different 'basic plans' among animals.

However it was also soon realized that, useful as it is, the 'bauplan' concept can often, and especially for fixed and parasitic organisms, be used only for certain of their developmental stages or only at some stages of their cycle.

Coming back to Goethe and his ideas, it must be acknowledged that quite often either his theories are strongly tinged with mystic-poetic trends and lack the logical rigour usual in modern scientific research or that he propounded ideas similar to those already advanced by other scholars, but which gained a permanent influence due to the art and authority of the Poet. Goethe made a number of special contributions to biolgy as his broad scientific interests were quite alive until his death and he was able to pursue them even when quite busy with his literay and political activities.

Chronologically Goethe's first contribution to morphology concerned the 'intermaxillary' (now 'premaxillary' bone) in man. He though that this was a discovery and as such he communicated it to Camper in 1784. In fact the occurrence of this bone in man was already known to Vesalius. Camper, wisely, while praising Goethe's remarks on some animals where the bone really had not yet been found, made no comment about man and Goethe, for the time being, did not publish anything. In 1790 Goethe published his famous *Essay on the metamorphosis of plants* where, with adequate observations and interpretations, he maintained that all the various parts of the flower: petals, sepals, etc. and even the seed cotyledons were modified leaves. Goethe argued that this proved that every individual may be considered as the result of many variations of but a few variously modified basic elements. This is often true, but it cannot certainly be generalised. However for Goethe this theory was significant also in order to assume that in all natural beings there is a strong spiritual drive to perfection, a belief of clearly neoplatonic origin.

Goethe further developed these ideas in his *Preliminary scheme of an introduction to comparative anatomy* (1795) and in *Formation and tranformation in living beings* (1807). Naturally Goethe extended his ideas to animals and, perhaps, its most famous application is the 'vertebral theory of the skull'.

There is a legend that Goethe had an intuition of it when, strolling on the seashore of the Lido of Venice, he picked up the skull of a sheep. However Goethe makes no mention of the episode in the *Voyage in Italy* and Ocken, himself a great admirer of Goethe, had published a closely similar theory six years before Goethe's publication and he says that he had talked of it to Goethe (a fact that the poet denies). The theory, let us call it 'Ocken-Goethe's theory', states that the skull is made up by modified and ingrained vertebrae (probably three of them). This theory prompted a number of important researches. Though we now well know that only the occipital region of the skull is actually a specialized section of the segmental skeleton, while all the rest belongs to the visceral and neuroectodermal skeleton.

Another principle dear to Goethe was the 'Law of compensation' or of 'balance of organs'. This holds that if an organ is modified or a new one is added, some other parts of the body must correspondingly be modified or reduced. This principle had been already stated by Aristotle, and, at the same time as by Goethe, it was upheld by Bichat, Cuvier and Geoffroy St. Hilaire as 'Loi du balancement des organes' and with this name became very popular.

Such few or obscure historical aspects that may be found in Goethe's thought, are unquestionably due to the influence of the historian and philosopher Johann Gottfried Herder, an intimate friend of the poet, who had secured for him the appointment as Chaplain at the court of Weimar. He, just at the time when Goethe was busy with his morphologic research, published a lengthy book (*Ideas for a philosophy of the history of mankind*, 1784-91) where he maintained that different organisms appeared successively in time, but not by their transformation of one into another. Anyway it appears that Goethe was not greatly interested in this aspect of Herder's theories.

A contemporary of Goethe was Johann Friederich Blumenbach (1752-1840), born in Gotha and a professor of anatomy in Göttingen. He was so famous as to be nicknamed '*Magister Germaniae*'. His importance is much less than that of Goethe, but he was certainly an excellent scholar and his great academic authority enhanced the study of morphology in Germany and gave it a pattern of accurate precision, thoroughness and method so that, even when, as it happened more than once, some fantastic theories became fashionable, German papers kept a deserved reputation for reliability at least as far as facts were concerned. The most original part of Blumenbach's work concerns anthropology and may be considered as a devolopment of Camper's. By his studies on human skulls (*Collectionis suae craniorum decades*, 1790) he established the fundamentals for a positive research on physical characters in the human species, though basically grounded on craniology. He advanced the first precise classification of human races (*Ueber die naturlichen Verschiedenheiten in Menschegeschlechte*, 1798). Though Blumenbach wrote a lot and published papers on botany, comparative anatomy, zoology and palaeontology, he contributed little new. To his credit must be said that he maintained that there had been at least two major phases of extinction in animal history and openly declared that Genesis could not be taken as God's revelation. At the time that was very much a minority stand as most people did not admit of extinctions. The argument of these people was that to admit extinctions was tantamount as either admitting that creation had been imperfect or, had creation been perfect, extinctions would have made it imperfect, in either case casting doubts on the perfectness of creation and, hence on the perfection of God himself. However Blumenbach stand was rather obscure: he admitted a progression of organisms with time towards more perfect standards. This is not a truly evolutionary stand, but comes closer to the idea of succesive creations of archetypes, each one having thence had a limited radiation and differentiation within the boundaries of its own possibilities.

Vitalism and Mechanism

As we have already said, Descartes had been preoccupied with certain tendencies of his contemporary sciences (to which he was himself contributing) that worried him both as a philosopher and as a Christian. Such preoccupations were common enough among both major and minor scientists of that age. We have seen that Descartes went for a radical dichotomy between the physical World, the body of man included, and the thinking soul. Such a radical solution was accepted by only a few scholars, but it can be taken as the opening move into the long quarrel between 'Mechanists' more or less close to Cartesian stances and 'Vitalists' who broadly maintained that the living world worked by laws that were more or less different from those applying to merely physical world. Generally speaking and within a varied range of positions, there was a general trend towards vitalism by scholars with a greater interest in alchemical studies.

All through the 17th and early 18th century scholars tended to uphold either position in more and more extreme way and the debate took also political turns which were promptly exploited by professional politicians.

All attempts by biologists of Cartesian affiliations to provide a purely mechanistic explanation of all biological phenomena had clearly failed, but, nevertheless, they had provided evidence that at least some facts could be described completely as purely physical phenomena. Likewise also the possibility of explaining all in terms of chemistry appeared less and less likely. If none of the two schools could win the day there were but two possibilities: either to find a compromise between the two approaches, or consider the existence of some 'vital force' or 'Spirit' and this immediately involved the problem, at least for mankind, of its relationship with the 'Soul' of traditional religion.

One quite popular possible solution was to assume that all phenomena of the life of an organism are of either physical or chemical nature, but that they are directed or supervised by a vital principle or soul and we have already met with these theories when dealing with the physiology of reproduction, of respiration and of body heat, the traditional strongpoints of the old Stoic's theory of the individual 'pneuma'.

We shall now deal with such scholars that either did not contribute any new evidence to science or contributed them in a strictly medical field, while actively interested in the more general aspects of the debate.

We have already mentioned Bichat's ideas. In Germany considerable importance had Friederich Hoffmann. He was born in Halle in 1660, studied in Jena and was soon called to teach in the university which had been established recently in his native town. He became famous as a physician and a supporter of iatro-chemical theories. Hoffmann died in 1742.

Hoffman made a bold attempt to conflate a physico-chemical foundation with his deep religious faith in an immortal and rational soul. In his books (*Fundamenta medicinae,* 1703; *Medicina rationalis,* 1739) he considered life as a basically mechanic fact: the body is like a machine, which is moved by a *spiritus animalis* occurring in the blood and which is secreted by the brain. This is a material substance obtained from air: something extremely subtle, ethereal and capable to keep the vital mechanism going. The reader will immediately see that it is a concept quite close to the theories of the second Stoa and to the theories of several Paracelsians.

Man and only man, is provided with an immortal soul which, directs the *spiritus animalis* and thus guides the movements and reactions of the body. Thus we are able to think, understand, act. As the soul is not the direct cause of vital phenomena, both physiology and medicine must be based on purely mechanistic basis: matter and movement. Hoffman has some difficulties in explaining the *modus operandi* of soul as he is both convinced of the possibility of a complete iatro-physical explanation of all vital phenomena, while in man the soul is somehow able to guide his conscious actions. As for animals he does not worry, as a follower of Descartes for him they are pure machines.

We have already mentioned his colleague Georg Ernst Stahl, who took a completely opposite stance. Stahl was born in Ansbach in Bavaria from a Protestant family. He was a fellow pupil with Hoffman in Jena. Hoffmann himself was instrumental in his appointment to the chair of theoretical medicine in Halle. However their friendship soon deteriorated into open hostility until Stahl, who had a foreboding and impatient character was exasperated, left Halle and became court physician in Berlin and died there in 1734.

As we have seen Stahl not only reprinted and commented on some Hermetic-alchemic books, but, like Hoffmann published a number of diffuse and occasionally obscure papers. We have already discussed Stahl theory of phlogiston; his basic principles of biology are expounded in his *Theoria medica vera* published in 1737, after his death. There he maintains a theory of the functions of the body, both normal and pathologic, that is quite contrary to any mechanistic view. In the first chapter of his *Theoria medica vera* he opposes to the concept body = machine, that of 'organism'. For Stahl bodily phenomena are not ruled by fixed physical laws but by the soul. The body is made for the soul, which is the cause of life: all and every organic function, nutrition, respiration, circulation, secretion, movement, sensibility, are ruled by the soul. Disease, that is malfunctioning of these functions are also dependent on the soul and therapy must thus be aimed, by the administration of adequate and bland medicines, to cure the malfunctioning of the soul. Such extreme animistic concept did not gain much support, though similar, less radical ideas were quite common at the time.

The most interesting developments of vitalism occurred in the medical school of Montpellier, which throughout the 18th century went on waging its traditional feud with the mummified conservatism of the Sorbonne.

Among the champions for Montpellier, Paul Barthez (1734-1806) was one of the foremost advocates for vitalism. He opposed both mechanism and the extreme animism of Stahl and advanced a theory (which was basically the same as Hoffmann's): he maintained that besides material body and the thinking soul there must be a special principle, to whom the unique proprieties of vital phenomena are due. Barthez is not clear whether he considered this principle as a material substance or not.

Mechanistic theories, which were obviously capable of incorporating also the new chemistry, were especially popular amongst the media of the French 'enlightenment' and in those related with it. Thus we shall deal with this school of thought and we shall spend on the work of de La Mettrie more space than it deserves from a purely scientific standpoint as, while this was minimal, it was hailed as important both in his own times and later.

Julien Offray de La Mettrie was born in St. Malo in 1709 from a rich bougeois family. His family planned for him a career in the clergy, and thus he studied theology in Paris, but, as it often happens to whom either by character or by reaction to bad schooling, leaves this road, he completely abandoned Christianity, went over to medical studies and graduated in Rheims. Thence he went to Leiden to study with Boerhaave, who certainly had a great influence on him. De La Mettrie translated into French some books of Boerhaave and this made him unpopular with the medical faculty in Paris. This last, at the time, was an extraordinary museum piece of cultural immobilism. De La Mettrie was appointed as military physician to the King's Gards. It was at this time that he wrote the *Histoire Naturelle de l'Ame* (1745). This book scandalized the Catholic circles and he was denounced. So de La Mettrie fled to Holland. In Leiden he published as an anonymous (but as it was then usual everyone knew the author) his most famous book *L'homme machine* (1748), where he took the most extreme mechanistic stance. This was too much even for a protestant country, and de La Mettrie had to decamp. Frederik II of Prussia, who, half by convinctions and half by political opportunity, was supporting any sufficiently unconventional philosopher, invited him in Berlin as 'reader' to the King. There de La Mettrie was made a member of the Prussian Academy and was allowed to practice medicine. He died but three years later. During his stay in Berlin La Mettrie published two works to complement *L'homme machine*: *Les animaux plus que machines* and *L'homme plante* which have been generally overlooked, while they are really essential complements to La Mettrie's theories.

In the *Histoire naturelle de l'âme* de La Mettrie takes as a starting point the fact that there is no agreement on what the Soul is, therefore, he argues that any scientific of philosophic investigation must start from the study of physic world and, in the case of mankind, from the study of his structure and functions by strictly empirical methods. He than proceeds to a methodical attack on all traditional views and concludes for a rigorous materialism. Unfortunately, just as it is usual with the French philosophers of the 'enlightenment', his arguments are anything but rigorous themselves. However it must be mentioned, even as a curiosity, that de La Mettrie, considering that the Orang Utan has features very similar to those of men, suggest the possibility to teach Orangs to 'speak' by using the gestual language for deaf-mutes that had just been created. De La Mettrie does not explain why he supposed that gestural language should have functioned, but, as a matter of fact, during these last years it was just this way that it became possible to communicate with Chimpanzees and Gorillas. So this remains about the only lucky guess of de La Mettrie.

By the other three books, de La Mettrie attempts, in the framework of a comparative analysis, a synthesis of all the mechanistic hypotheses proposed to his days. He thus argues that, given that there is a spiritual life in man, this must exist in various amounts in all living organisms, and, possibly, it may occur even in non living matter, reviving here Leibniz's idea of the 'sleeping monads'. He thence proceeds to build a rigorous mechanistic system on Cartesian lines, but without a spiritual soul to interfere in its systematic organisation. To de La Mettrie even thoughts must be material, but must be very small as, otherwise, their great number could not fit into the brain.

One must concede that his is a complete and organic system, as it includes even ethics, but practically it turns out to be simply a refurbished Lucretius, seasoned in Leibnitzian sauce and definitely a truly amateurish effort.

Possibly the only point in the *Homme machine* which should have deserved attention and that was completely overlooked, is a passage where he points how isolated parts of an animal may continue to show some living activities even when the individual as such is dead. De La Mettrie correctly concludes that life is inherent with the structure of every part of the organism, and then goes wild and argues that this being a merely physical phenomenon, the soul is a material mechanism of conscience, an idea that, at the time, lacked any possible shred of supporting evidence.

As for the origin of life de La Mettrie holds by the common hypothesis of an fortunate aggregation of eternal organic monads and that, if at present the spontaneous generation of men and elephants is no more possible, that is because the Earth is old and 'tired'!

The whole of de La Mettrie's work is not to be taken seriously and in every detail it adds not a single piece of new evidence. Its only importance being that it is the first attempt in modern Europe to propose a complete mechanistic and materialistic system, covering even the origin of life. Obviously de La Mettrie's books on the natural history of Man made a sensation both in France and in Germany as there anything appearing new was taken most seriously and so they were commonly discussed, just as de La Mettrie's books on ethics, and had a lasting influence.

The next systematic defence of materialism appeared as the *Système de la Nature, ou des lois du monde physique et du monde moral* by Baron Paul-Henri-Dietrich von Holbach (1723-1789) a German of French origins and usually resident in Paris, who published it in 1770, anonymous, and with false name of the editor and of place of press (Mirbaud and London). D'Holbach, as the French called him, is also known for having substituted for D'Alembert when this last retired from the direction of the Encyclopédie Methodique, a direction that he had shared with Diderot. Von Holbach's theories touch biology but marginally and merely stress materialistic aspects of ancient Greek theories. Von Holbach is openly a transformist, but as he gives little if any justification to its ideas, they are irrelevant in the study of evolutionary theories, though they must have been known to Lamarck.

These materialistic trends were practically stressing the powers of Ananke, the impassive and aimless 'necessity' of Greek mythology, whom we have often mentioned. As such they were fiercely attacked both by such leaders of typical 'enlightenment' as Voltaire, as by 'proto-romantics, like Goethe, two opposite groups who shared two features: they were both not real rationalists as they were true optimists.

The supporters of the more rigorous mechanistic and materialistic hypotheses did usually associate with the more extreme within those intellectual trends which are usually grouped under the label 'enlightenment'. These 18th century thinkers were all sure that they were led by the 'lights' of reason, in contrast with the past 'obscure' ages.

Forerunners of evolutionary ideas

We have already mentioned the curious transformist ideas of Bonnet, which Cuvier judged 'Un tableau admirable' (= A marvellous painting) and we have seen how both Linnaeus and even more precisely Buffon considered as probable that both plants and animals might have passed through limited changes with respect with their archetypal ancestors. It was anyway dubious whether these changes would have been occurring in actual, presently existing animals, or in their 'generalised archetypes', be they living or not. Even more general views on transformism were advanced by Goethe and by several other scholars of the 18th century. Though none of them ever advanced any organic evolutionary hypothesis, some of them do deserve mention as they were familiar to Lamarck, when he first advanced his own true evolutionary theory. It is, therefore worth while considering whether any of them may have had some influence on Lamarck himself.

As I have already mentioned von Holbach, the first to consider is the French Benoit de Maillet (1656-1738); he wrote a curious book, which was published posthumously in 1749 and titled *Telliamed* [the anagram of his surname], *ou entretiens d'un philosophe indien aveq un missionaire français sur la diminution de la mer*, in it de Maillet by taking considerable liberties with ancient myths, tried to show that several of them and some of the philosophers' hypotheses may be compatible with the biblical account. He then proceeds to edit the Greek myths and maintains that originally waters covered all the world and there appeared the first animals, thence, as waters retired the lower animals had to metamorphose and became either terrestrial of flying animals. Even Man is supposed to derive from a 'Homme marin', a sea-man somewhat similar to mermaids. De Maillet's book does not have any pretension to be a scientific treatise: it is a well written pleasing fantasy.

The ideas of Pierre Louis Moreau de Maupertuis, one of the leading thinkers of the French 'enlightenment' are clearly much more important. Maupertuis was born in St. Malo in 1698 and died in Basel in 1759, and is mainly remembered for his contributions to mathematics, astronomy and geodesy. Frederick II appointed him as chairman to the Prussian Academy of Sciences (1746). As we said speaking of de La Mettrie, Frederick, who was effectively interested in the promotion of sciences, hoped, by recruiting into his Academy as many brilliant brains as possible, to promote the development of sciences in Prussia. On the other side, as he was almost permanently either at war or close to it with France, he wished to protect as many famous Frenchmen as possible who could be considered as hostile to the French king.

Maupertuis published two works that concern us here: *Vénus physique, contenent deux dissertations l'une sur l'origine de l'homme et des animaux et l'autre sur l'origine des Noirs*. Published anonymous at Den Hague in 1745; et *Système de la Nature, essai sur la formation des corps organisés* published in 1751.

Maupertuis is openly a transformist, was much interested in problems of heredity and paid attention to the genealogies of people suffering from polydactily, just as he made experiments with dogs. As usual in his age he envisaged intersterility or sterility of the hybrids as the decisive fact to tell apart taxa, but he considered that such sterility was presumably attained only gradually in the course of generations. He attributed heredity and variation to solid corpuscles who were each one responsible for the transmission of a given character. He also envisaged the fact that while his corpuscles transmitting the different characters could cause random variability, the environmental factors were directing the overall changes: such views were extraordinarily advanced for the age and may safely be called proto-Darwinian,

Dénis Diderot (1713-1784) is always remembered in the histories of evolutionary thought as the author of a *Pensées sur l'interprétation de la Nature* (1754) and of a pamphlet titled *Rêve de D'Alembert* (= *The dream of D'Alembert*) written in 1769, but actually published in 1830. Both are unquestionably outspoken praises of transformist ideas and the second one envisages even the tranformation of D'Alembert himself! But the *Rêve*, at least, is just a 'divertissement', a joke and almost a satyr against his former colleague as director of the *Encyclopédie* and the other is an extremely poor thing. If you consider Empedocles as an evolutionist, then Diderot too is evolutionist. Simply Diderot is such an established monument in the history of the French 'pensée' of the 18th century that he must be taken seriously even when joking. Otherwise his books, though openly advocating transformism do not provide either new evidence not new theoretical ideas of value and are irrilevant in the history of evolutionary studies.

While Maupertuis and Diderot (if we choose to count him) discuss biological and evolutionary problems in the light of a mechanistic philosophy, Jean Baptiste Robinet, from Rennes (1735-1820), is decidedly an evolutionist, but he maintained a definitely teleological thesis of frankly Leibnitzian pattern. This has, indeed some affinities with the stance of Lamarck and might also be considered as a sort of bridge towards some post-Darwinian evolutionary theories, such as hologenism, that assume that in Nature there is an inborn tendency to improvement, which in Robinet reaches its goal in Mankind (with a hope that the process may not stop there and that eventually it will produce something better), finalism is pervasive in Robinet's works *De la Nature* (1761-1766) and *Considérations philosophiques sur la gradation naturelle des formes de l'être, ou essai de la nature qui apprends à former l'homme* (1768). It is quite possible that Robinet's essays were influential in shaping the naive attempts by Bernardin de Saint Pierre that we shall mention in the next chapter.

The ancient idea of a natural stairway climbing from minerals to plants, to animals and finally to Man and that we have often mentioned, is still entirely predominant both in the thought of would-be evolutionists as of the staunch fixists..

The discovery of vaccination

We have still to deal with Edward Jenner (1749-1823). Young Jenner begun his career as a preparator in the service of Sir Joseph Banks, to whom he had been recommended by John Hunter as being especially gifted for any work requiring fine and precise techniques.

Jenner's work was strictly of medical interest, but as its developments were of major interest in biology, he certainly deserves a paragraph in any history of biology. In fact Jenner is now remembered only because he introduced vaccination against smallpox.

Smallpox epidemics had been a scourge since antiquity and in the 18th century (and later) still exacted a heavy toll both in lifes and in permanent disfigurement. As it was generally known that the infection left, for those who recovered, a permanent immunity, in different Mediterranean countries, and especially in Italy and Turkey, where the practice may have originated, variolation was fairly commonly practised. This was simply the inoculation of a tiny amount of pox exudate from a recovering patient with the aim of producing the disease in an attenuated form. It was obviously a dangerous procedure, as, whereas usually quite effective, it was plainly liable to produce the death of sensitive subjects and could, eventually, become the source of a new epidemic.

The practice had been introduced in England by Lady Mary Wortley Montagu (1689-1762) who, when in Costantinople as the wife of the British ambassador to the Sultan's court, had her children so inoculated.

Jenner had gained a renown by his particular skill in the inoculation and he noticed that Cow-pox, while usually producing in man an very slight disease, could produce just the same amount of immunity as variolation.

At the time it was a momentous discovery for public health, but of little or no scientific significance. However it was the first step into the whole field of immunology.

CHAPTER X

From the beginning of the french revolution to the publication of the *Origin of species*

SYNOPSIS OF MAIN HISTORICAL EVENTS AND OF THE MOST IMPORTANT SCIENTISTS AND SCIENTIFIC DISCOVERIES

1789 opening session of the *États Générals*, beginning of the French revolution.
1792 beginning of the wars of the Revolution.
1793 King Louis XVI and the queen are deposed and beheaded, beginning of 'the Terror'.
1796 first successes of Napoleon, who thus begins his ascent.
1796 Laplace publishes his *Système du monde*
1797-1815 Napoleonic wars.
1814 restoration of the constitutional monarchy in France.
1815 Napoleon's 100 days and battle of Waterloo.
1814-1815 Congress of Vienna.
Rumford's experiments on heat and work (1798), Laplace publishes the *Mécanique Celeste* (1799-1805); Herschel discovers infrared rays (1800) and the movement of the solar system through the Galaxy (1806); Young formulates the undulatory theory of light (1801); Wollaston discovers the absorbtion spectres of Sun light (1802); Michael Faraday (1791-1867); laws of Dalton and of Gay Lussac (1808); Molecular theory of Avogadro (1811); Fraunhofer studies the absorbtion lines in spectres (1814) and discovers the spectres of the stars (1823)
1821 first period of constitutional revolutions in Europe, they are promptly crushed.
1826 Ohm's law, Lobachewsky's geometry.
1830 fall of Charles X of France, Louis Philippe becomes constitutional king.
1831 Belgian revolution against the Dutch and liberal revolutions in Italy and Poland.
1848-49 Liberal revolutions in France, Germany, Austria, Hungary and Italy, first war of Independence of Italy.
1849 Fizeau measures the speed of light, 1850, Calusius formulates the second law of thermodynamics
1853 Crimean war.
1854 von Helmholtz formulates the hypothesis of the contraction of the Sun as a source of energy
1857 Cipoy's mutiny in Northern India, proclamation of the British Empire of India.
1858 Cannizzaro's law
1859 Second war for the independence of Italy.
1860 Piedmont annexes most of the Italian States; establishment of the Italian kingdom.

Some general features of this age

We have closed the foregoing chapter approximately by the beginning of the French revolution. Obviously we had to follow some of the scholars who straddled the two centuries well beyond this limit, just as we left for this chapter some important biologists who began their activities well before the Revolution, but whose main studies were published later.

It is probable that, had the great revolution never happened, the development of sciences would not have changed much, but there is no doubt that the personal story of several important figures was deeply affected by the revolution and that both their scientific activities and their cultural attitudes were to some extent shaped by the political convulsions of their age.

Only now we begin to give a fair assessment of the events of the French revolution, and, curiously, even those who lived through its tumults and were swept into the Napoleonic adventure, rarely gave an objective judgement of the events they lived through.

The call for the 'États Générals' was a last attempt by the King Louis XVI, who was loyally supported by the queen, Marie Antoinette, to achieve at least such limited reforms as were needed to patch up the state budget and to begin moving towards such organic reforms as were commonly advocated, but that were systematically obstructed by both the majority of the lesser nobility (quite often rather poor or recently ennobled) and by a most inefficient civil service. Thus for some time the king was commonly hailed as the champion of reforms. According none the less than the Duke of Taillerand, later 'Prince' of Benevento, who lived all these events from a prominent position, the ineptitude of the king and of some of his advisers, the sudden death of the Count of Mirabeau, some vacillations and delays by Marquis de La Fayette, were responsible for the loss of several occasions when it would have been possible to steer the situation on the same path as the British 'Glorious revolution'. The ensuing chaos, increased by popular riots in Paris and a few other towns were the economic crisis and penury were notable, paved the way for radicals of both sides. The *Declaration of the rights of Man and of Citizen* soon became a dead piece of paper, riots and massacres spread (though it is now generally acknowledged that the republicans managed to kill many times more suspected royalists than the republicans who were killed by the royalists). Naturally there followed a reaction which soon gave way to Napoleon's dictatorship, masked by the make-believe show of a parliamentary system.

By the end of the revolution and of Napoleon's rule, all the European countries were sadly impoverished, illiteracy had consistently increased and public health deteriorated! There is no doubt that, on the material side, the Revolution was a sad affair for everyone. Even on account of tolerance and of liberties, the opposed extreme attitudes which developed everywhere during the first years of the Revolution and the uninterrupted series of wars, mutual invasions, massacres and loots which plagued the twenty-five years following 1789 are a black page in the history of European civilisation, just on par with the religious wars of the 16th-17th century, and unquestionably delayed the liberal development of continental Europe. Such was the fear and hate, that for over twenty years had obsessed all European conservatives, that in the after-math of their final victory they strove for a most obtuse reaction. One might almost say that the practical inheritances of the revolutionary years were compulsory military service and the development of police establishments and ever more centralised governments.

On the other side, first in France and thence wherever the French armies went, there remained a dream: that such ideals that the French proclaimed, but that they were careful not to implement, would have materialised if, in the place of the Jacobins and of Napoleon, there had been a sufficient number of Washingtons.

The blind obstinacy of almost all the restoration governments and the silly persecutions that they made against all who had served under the French had the result that almost all the best people were driven into the different secret societies which were planning for the liberal revolutions and, in the cases of Italy, Poland, Hungary, etc. of nationality causes.

The churches, and especially the Catholic Church, which had greatly suffered during the revolutionary and post-revolutionary period (the Pope himself had been abducted and made a prisoner in France) were lured into an intransigent conservatism which alienated a number of scholars. Thus, and not only in Italy, to be liberal was almost a synonym to be anti-Church and the split between members of the liberal aristocracy and bourgeoisie and their reactionary counterparts was still felt almost at a clan level as late as the period between the two World Wars. The different families read different newspapers, were not members of the same clubs and seldom intermarried!

In spite of all that, the development of sciences went on untroubled and at a steady pace until the crisis of 1848. Between 1848 and 1859, at least in Italy, in the Universities everyone had one eye on studies and one on politics and that, joined with economic difficulties, did hamper research. Italy, during the 'Risorgimento', had some really notable biologists, but undoubtedly they were neither as famous nor as influential as their forerunners.

Finally, in 1858, at the famous meeting at the Linnean Society, exploded the Darwinian crisis, which radically changed the outlooks of biology.

When we consider the organisational framework within which sciences were developing, the first half of the 19th century was, again, an age of transition. The reform of the universities, which had began during the 18th century was practically achieved, and meantime the Academies, also for economic reasons, were ceasing to be the principal motor of research: their journals were more and more publishing papers prepared by the staff of university's and museum's laboratories and within such facilities. Thus research becomes again mainly the activity of professionals.

An increasing number of universities and museums also developed in the Americas parallel with the economic development and with that of independence from European powers.

The growing complication of research and the successful examples of the Muséum d'Histoire Naturelle, of the British Museum and of the Royal Society, prompted an increasing number of institutions to supply the 'Professors' with some collaborators, be they technical preparers, and this was not a novel thing, but mainly of junior researchers, either labelled as 'demonstrators', 'junior lecturers' or as 'assistants', who, while waiting for a promotion to a full chair, were learning their trade under the guidance of the 'boss'. This was nothing new, but formerly such pupils often had no salaries and either had their own private or family means, or had to be sponsored by some benefactor (who, occasionally was the master himself). Now they all begin to get a regular, albeit small salary.

Finally, and this is significant for our purposes, mainly between 1830 and 1860 almost everywhere develops a split between the faculty of medicine and that of 'Natural Philosophy' or 'Sciences'. This, in a way, closes a circle: the faculty of Arts, after having been either preparatory for the medical faculty or even having been entirely incorporated into the medical curriculum, comes back on its own and becomes entirely parallel to the other faculties.

Obviously, throughout the period hospitals multiplied and some great biologists were actually pursuing their researches either in old or new hospitals, but at the same time when new chairs were established, the 'Institutes' came into their own. This last was, so to say, a process of growth: by the 18th century the colleges preparatory for university studies had provided themselves with equipment and facilities in order to give the students the prerequisite practical exercises, while the academies had 'cabinets' and there were the astronomical observatories. Otherwise, usually the professor made his researches, and often much of his teaching, at home. These arrangements could not work any more in the next century, given the low salaries of the professors (the 19th century is everywhere a century of underpaid professors, and therefore they could not afford homes large enough and costly instruments). So the universities began to meet the costs.

A final consideration: the 19th century was a century of explosive development of periodicals and serials. They came in all types and suitable for all types of purses. Scientific novelties, especially when joined with some more or less adventurous exploration, made sensation.

At the same time the increasing impact of chemistry, engineering and generally of applied sciences gave to scholars of scientific disciplines the sort of social standing that was formerly only of successful physicians, writers and lawyers.

The first half of the 19th century was an enthusiastic age both for moral ideals and for sciences: old standards appeared to be obsolete, and the new ones were continually superseded. Thus, and for a number of reasons, side by side with the increasing specialisation of the scholars, biology and more generally natural sciences grew more and more apart from philosophy, with deleterious effects on both, at least on the purely theoretical aspects. Sciences, apparently, suffered less as, by the (rather fictitious) implementation of a more or less Baconian combination of experiments and induction, they collected more and more spectacular achievements, which successfully masked the very poor philosophical background of most scholars. The philosophy of the scholars in humanities was even poorer: indeed it is sufficient to consider that such a man as Hegel was increasingly considered as a great philosopher (Bertrand Russel has very aptly said of Hegel "This illustrates an important truth, namely, that the worse your logic, the more interesting the consequences to which it gives rise.").

The 'big three' of the Muséum

Lamarck

Jean-Baptiste-Pierre-Antoine de Monet, Chevalier de Lamarck (more precisely La Marck), was born in Bazentin in Piccardy in 1744, the eleventh son in a family of small feudal lords. Naturally, as it was the custom in all noble families who could not afford to split a small heritage, his destiny was priesthood. But his was a family of soldiers and his senior brother was killed in battle almost at the same time as his father died and Lamarck, when barely 17, promptly left his ecclesiastical training to join the army in Germany. He enlisted in the army of Marshal the Duke of Soubise and almost immediately his bravery in battle earned him a commission. However, when his regiment took the winter quarters in Monaco, he fell seriously ill with a cervical adenitis and was thence dismissed with a small pension. He underwent surgery in Paris and spent a long convalescence in dire need. It was then that he became passionate for sciences and especially for botany. For the next ten years, in order just to barely earn his living, he did this and that: he was for a time a clerk in a bank in Paris, a free lance journalist etc. In the meantime he began to study medicine and attended the courses of Bernard De Jussieu; moreover he published several botanical papers and finally produced the *Flore Française*, where he first introduced dicotomic keys for the identification of plants. Anyone with but a little experience in identifying specimens after the descriptions given by the different authors knows how long and tedious is the job. The system created by Lamarck was extremely simple and is based on a series of alternatives: for instance a given group, say a family is divided into two by the presence/absence of a single character, preferably an easy one to check, so you have 'Plants with A – plants without A', then in, say, the group 'Plants without A' you make a further division: 'Plants with B – plants without B» and so on until you reach to the level of the species. This way routine identifications become both easy and quick and the new system was hailed as a great bonus by the scientific community. It was, moreover a practical device derived from the traditional binary logics of classification as they were used, *e.g.* by Ray. De Jussieu had recommended Lamarck to Buffon, and the great man was prompt to perceive the value of the young scientist. On recommendation of Buffon, Lamarck, in 1779, was made a fellow of the Académie des Sciences and in 1781-82 Buffon charged Lamarck to accompany his son in a long trip through

Europe (as we previously said, Buffon had planned to make his son his successor at the Jardin and may have hoped that, under the tutelage of Lamarck, Buffon junior might have learnt botany). However, the goodwill of Buffon was not sufficient for the practical purposes of getting Lamarck into a permanent appointment. This came, with the very small salary of 'demonstrator', under the brief rule of the Marquis De la Billarderie, who succeeded Buffon in 1788. Small as it was, that salary solved the more pressing needs of a big family of seven (Lamarck actually married four times). Yet this was soon at risk: shortly after the opening session of the États Générals, the assembly began to screen the administrative muddle that had brought France on the verge of bankruptcy. Thus the funds for the Jardin were drastically reduced and not only the famous salary of the Director (Intendent) was cut from 12,000 to 8,000 livres, but several positions were scheduled to be cut, including that of Lamarck. However he was not dismissed: chaos was daily growing, De la Billarderie resigned and was substituted by the equally incompetent novelist Jacques-Henri Bernardin de Saint Pierre, who had no sympathy for Lamarck, but simply delayed the implementation of the cuts.

Finally, when the 'Terreur' began, Count De Lacépède, who held the chair of invertebrates, thought advisable to make himself scarce in order not to be beheaded. Thence Daubenton and Lakanal (1762-1845), who had jointly taken over from Bernardin de St. Pierre and who, with Lakanal and Fourcroy as members of the assembly pulling the right political strings, decided on a temporary reorganisation of what staff was left and to recruit new ones (De Lacépède was not the only one to go into hiding) and they appointed Lamarck to one of the chairs which had been made by the splitting of that abandoned by Lacépède (the other went to Geoffroy St. Hilaire). Thus Lamarck, by now 49, had to begin to study a field that was new for him and which was very poorly developed at the time.

When the 'Terreur' finished with the execution of its chief promoter, Robespierre, and Lacépède reappeared, Daubenton was able to create a brand new chair of Herpetology and Ichthyology for him, and so to leave Lamarck in his appointment. Later on, in 1810, when Étiènne Geoffroy St. Hilaire was appointed to the chair in Zoology in the University, he, with his customary kindness, offered Lamarck to renounce his appointment in the latter's favour. Lamarck, who, in spite of his republican feelings, was an accomplished gentleman in the style of the Ancien Régime, kindness for kindness, refused. Thus he remained at the invertebrate section of the Museum.

His eyesight had began to trouble Lamarck since the '90s and, having become entirely blind, he resigned his post in 1819 and died in 1829. Even after his retirement and completely blind Lamarck, with the help of his two daughters, continued to work to the end.

Nowadays Lamarck is mainly remembered as a zoologist, but he never completely abandoned his botanical studies and, also to make some money, made several forays in fields that he did not master at all. Thus besides his works on botany and zoology, he tried physics and chemistry. Thus he published in 1797-99 the *Mémoires de Physique et d'Histoire naturelle, établis sur des bases de raisonnement indépendentes de toute théorie*, followed by the *Récherches sur les causes des principaux faits physiques*, published in 1793, but written around 1780. In these curious books he strove to falsify the new chemistry of Lavoisier (*Réfutation de la théorie pneumatique*) which had been, in the meantime, adopted by his colleague Fourcroy, and he proposed a 'pyrotic' theory, which is practically a variety of Stahl's phlogistic one. In the first of these works Lamarck denied the existence of oxygen, while later, though admitting its existence, denied its importance both in burning and other oxidations and strongly advocated the ancient four elements plus his 'pyrotic principle' which is practically the same as phlogiston.

Lamarck had always been interested in meteorology and, possibly for economic reasons, between 1802 and 1810, when Napoleon personally vetoed their continuation (possibly at the instigation of Laplace), Lamarck published a *Meteorological annual*, which was based on a sort of statistical evaluation of the climatic variations and rhythms, and advanced a number of forecasts as to when to expect rains and good weather, storms and tempests, frost and thaw and so on. The *Meteorological annuals* have always been considered as a bad stain on the scientific coat of arms of Lamarck. Undoubtedly, just as his other forays out of Zoology and Botany, they make a painful impression, however, it must be said for Lamarck that he may well have been the first to suggest that a statistical analysis of past meteorological data might provide evidence for weather forecasts. Anyway the almanacs were economically quite profitable: the author was a well known scientist and so, while Lamarck honestly stressed that his forecasts were mere probabilities, the general public took them as scientifically reliable documents.

When Lamarck was appointed to invertebrate zoology, he immediately found that, as it had been maintained by Pallas and Fabricius, the Linnean classification which divided the animal kingdom in but six classes: Mammals, Birds, Reptiles-Amphibians, Fishes, Insects and Worms, was untenable. So, going back to Aristotle, he began by the separation between Vertebrates and Invertebrates.

As proved by his manuscript notes for his introductory lectures, up to 1799 Lamarck did not believe in evolution, but, almost suddenly, in 1800 he became a convinced evolutionist. He was then 55 and it appears that his conversion was due to his study on Molluscs. Lamarck began to study Molluscs when his friend Bruguière, who had been in charge of the collections, died. Lamarck was immediately struck by the series of fossil shells, which plainly appeared to illustrate the transition from one species to another. Lamarck's theory is hinted in a preliminary lecture delivered on 21 Floreal, year VIII of the Republic (1800) and in the introduction of his booklet *Système des Animaux sans vertèbres* published shortly afterwards, and is fully developed in the *Phylosophie zoologique* of 1809, and his is the first organised evolutionary theory. He then embodied his views using it as the framework for his *Histoire naturelle des*

Animaux sans vertèbres, which first volume was published in 1815, when Lamarck was 71, and the seventh and last in 1822.

Lamarck's *magnum opus*, together with the contemporary work of Cuvier, were the starting points for all later revisions of the systematic of the Invertebrates.

Lamarck arrived at his evolutionary theory starting from two premises, one correct and one false. The false one is his firm belief, traceable to Buffon's and to D'Holbach's influences, in spontaneous generation, which he continued to believe in spite of all contrary evidence (just as he did with chemistry and as other eminent scientists did even much later), and which implied that organisms form by the influence of local conditions. The correct premise was that the Muséum's collections of shells allowed for their arrangement into different morphologic series showing the transition from one morphology to another and that such series were also in accordance with their succession in the strata. This evidence was bolstered by Lamarck's belief (which was rapidly spreading through the scientific media), that the duration of geological times was very much longer than it had been considered in the past. He, indeed, exclaims "Oh! How great is the antiquity of the terrestrial globe and how small the ideas of such that credit this globe with an existence of little more of six thousands years from its origin until our present days".

Starting on these premises and having a sound knowledge of anatomy, Lamarck found, just as Cuvier was doing next door at the Muséum, that all organisms may be allotted to a small number of large 'natural masses' (basically he divided animals between metameric and non-metameric), which will include both living and fossil organisms. Each one of these can be arranged into one or more series, which resemble those of the traditional *scala naturae.*

He thus in his early writings on evolution, assumed that, both the continuities within each 'mass' and the discontinuities between the 'masses' must be explained by assuming that each one had independently evolved from a different infusorian, these having been generated by spontaneous generation. It must be added that in his successive publications Lamarck gradually evolved his phylogenies to the point that finally (in 1820) he envisaged a single common source for all animals (much to the delight of Geoffroy).

As for the causes that make the organisms to evolve and perfect themselves Lamarck assumes that there are both internal and external causes. The internal factor is presumed to be the innate energy of the 'internal fluids', which pressure caves into the tissues and remoulds them; however, the movements of the internal fluids are prompted by the functional requirements of the organism itself. Thus each organism moulds the useful characters, while the useless organs, lacking a stimulus to develop, necessarily become reduced and eventually lost. Thus, supposing that all organisms had always lived under uniform circumstances, all of them would have evolved because of their innate tendency to perfection, but that would have been a totally homogeneous evolution and presently all the organisms would be identical. But, as local conditions are never the same and continuously change from place to place, the tendency to development must have been retarded, stimulated or steered in infinite ways through the times and this explains the incredible variety of nature.

Lamarck holds that any change occasioned by local conditions may be inherited by the progeny provided that such change has occurred in both the parents. Thus Lamarck is quite clear that acquired characters may be inherited, but is totally obscure as to its mechanisms.

To sum up Lamarck's theory of evolution has some unique features. Though mainly a follower of D'Holbach, Lamarck is not a total materialist. That he often mentions the 'Supreme Author' of everything may well be irrelevant and due to the need to avoid problems with Napoleon, who having forced his peace and his coronation upon the prisoner Pope, was adamant that his scientists should avoid to create any problem with the Church. Nevertheless Lamarck asks himself the question: "Which original cause may have planned the early infusorians with a tendency to perfect themselves and evolve into ever more complicated organisms?". His reply is complex: he holds that nature evolved by strictly deterministic mechanisms, but all material events are ruled by the laws given by God in the beginning. With but minor changes it is the same answer that had been proposed by such a pious man as Bonnet a few years before, and it is a reply that is again like that of late alchemists (and we may well remember the strict alchemic origin of Lamarck's ideas in his chemico-physical writings) who, with Van Helmont, believed Nature to be 'the order of God', and goes back to a classic Lullist and finally Neoplatonic origin.

Lamarck, contrary to Darwin, does not care much to find evidence to support his theory. When he indulges in arguing his points, his are often *petitiones principii,* such as 'the mole is blind because she lives in the dark' and as such are practically worthless, as Cuvier could easily prove and as even Geoffroy had to admit.

Lamarck's theory looks superficially very attractive, yet very few scientists bought it and Lamarck himself and the few followers he had were seen askance by the academic media However, Lamarckian ideas had a considerable success with the general public and were promoted in quite a few successful books of popular science.

As we shall further delve into the scientific debate within the Muséum, we must always remember that while Lamarck was considerably older than either Geoffroy or Cuvier, all his zoological work and his theories were developed just in the same years as those of his colleagues. It appears, indeed, that Lamarck was rather a solitary soul in the establishment of the Muséum, yet his dealings with his colleagues were always quite correct and that both his systematic and his evolutionary theories were developed just while both Cuvier and Geoffroy St. Hilaire were working and lecturing in the same laboratories. It is thus reasonable to assume that the three discussed together their work. This may well explain how, while they hold by quite different general theories, their systematic were largely compatible. Cuvier and Lamarck did, indeed criticise each other's theories, but they avoided personalities to the point that they discuss their colleague's theories without mentioning names!

There is a legend that Cuvier gave a scathing judgement of Lamarck, though dressed nicely and in true academic prose, when speaking at Lamarck's funerals. Actually Cuvier did not attend the funeral, the only colleagues present being Geoffroy and Latreille and actually the funeral speech was given by Geoffroy. Cuvier's text is authentic, but was written later for a commemoration at the Académie and actually published only in 1835, after Cuvier's death. It is, indeed, obviously severely critic of Lamarck's evolutionary ideas but it is not entirely unfair.

Étiènne Geoffroy Saint-Hilaire

Étiènne-François Geoffroy St. Hilaire was born in 1772 from a distinguished family, which we have already mentioned and which had recently added to its surname the 'St. Hilaire' suggesting noble associations (Étiènne Geoffroy senior (1632-1731) had been a professor at the Collège de France, Claude-Joseph Geoffroy (1685-1752) had been a distinguished chemist attached to the Cabinet of the Jardin and was instrumental in clearing chemistry of its esoteric superstructures. Other, unrelated Saint-Hilaire, were contemporary with Étienne: the botanists J.H. Jaume Saint Hilaire and A.-F.-C. Prouvençal Saint Hilaire. Étiènne's father was the local magistrate in Etampes. His family planned for Étiènne a career in the clergy and he received the minor orders at twelve and was appointed as a canon at fifteen. However the impending revolution made a change advisable, and he began his medical studies. Thus he got in touch with Abbot Haüy, who was then laying the foundations of crystallography, and, at the risk of his own life, saved him during the 'massacres of September', when the Parisian mob murdered a number of people from both the aristocracy and the clergy. This gained him the unconditional support of Daubenton, who got Bernardin de St. Pierre to nominate him to a minor appointment at the Muséum and, later, to share with Lamarck the duties of Count De Lacépède, who had had to make himself scarce. Shortly afterwards, when the whole staff was reorganised, both Geoffroy and Lamarck were appointed to a chair, Geoffroy at 21 and Lamarck at 49! When Robespierre was executed and the 'Terreur' finished in September '93, De Lacépède reappeared and, as we have seen, he got the chair of Lower Vertebrates, Geoffroy keeping Mammals and Birds and Lamarck the Invertebrates. The invitation to Cuvier to come to Paris was largely due to Geoffroy's support and the two began a close cooperation and a friendship which was to survive to some extent the bitter scientific quarrels of later years.

In fact, already in 1796, Geoffroy took up a previous suggestion by Vicq d'Azyr to look for a common plan in all animals, and, in a paper on Lemurs, argues for a basic structural identity of the skull of all Vertebrates. In 1798 Geoffroy was able to join the scientific staff assembled on Napoleon's orders, to follow his expedition into Egypt, whilst bad health prevented Cuvier.

During his three years in Africa, Geoffroy did an enormous amount of research work and assembled a great collection. His investigations allowed him to become a specialist on crocodiles and African fishes (including the description of *Polypterus*) and to formulate the idea of the 'balancement des organes', that he was later to use systematically, just as did Cuvier, but with quite different results.

When the French army, abandoned by Napoleon, surrendered to the British, Geoffroy was able to lead all his colleagues to threaten the destruction of the collections rather than their surrender, as it was stipulated in the capitulation terms and got such terms amended. Later, when the French invaded Spain and Portugal, he was sent there, charged to bring to Paris any materials of interest. Rather than complying to orders which were repulsive to him, he organised a series of mutually advantageous exchanges with the local institutions and helped them in the reorganisation of their collections, so that, after Waterloo, Portugal was the only country that did not lay claims on France for the returning of collections.

Gradually, as we shall see, his differences with Cuvier increased and there were ever stronger disputes among them to the end of Cuvier's life, but their personal ties still held to a considerable extent. Geoffroy may have resented the appointment, in 1803, of Frederic Cuvier (1773-1838) as superintendent of the zoo. The establishment of a regular zoo had been advocated in principle by Bernardin de St. Pierre during his brief tenure as head of the Jardin, but it had been practically established and almost single handed managed by Geoffroy since 1793. Frederic was George's brother and, by all means the 'minor' Cuvier; but, nevertheless, he made some useful contributions to the taxonomy of Mammals. Anyway, as Frederic was to some extent subordinate to Geoffroy, the two were able to co-operate until the famous quarrel on the fossil crocodiles between Georges and Geoffroy broke their ties until the death of Georges Cuvier. Thereafter the two were again able to co-operate from 1824 onwards in the publication of the *Histoire Naturelle des Mammifères*, which was the continuation of a previous work by Georges Cuvier et Lacépède.

The anatomical and especially the teratological lines of investigation of Étiènne Geoffroy were successfully pursued by his son Isidore, as we shall see in the next chapter.

Étiènne Geoffroy gradually became a true evolutionist, but that he was only since began his argument with Cuvier on the fossil crocodiles. Previously he had rather been a transformist on Goethe's lines, though he claimed to be developing Buffon's ideas. He had a great consideration for Lamarck, but never entirely shared his evolutionary theory. Unquestionably he is the first modern author who asked the question "How did life begin?".

As Geoffroy's ideas are often not very clear and, anyway, they evolved with time, let us see to outline their development.

We have seen that, already when in Egypt, Geoffroy had thought of the 'law of the organ's balance', which assumed that, whenever an organ increased either in size or in complexity, another one had to give way and become either reduced or lost. This obviously gave a precise significance to the study of rudimentary organs. His second main idea, which, soundly applied in his works on the skull of vertebrates, produced excellent results, but that, because of Geoffroy's determination to give it universal application, also led him to incredible mistakes, was the principle of unity of plan.

More an more, with the passing of years, Geoffroy tried to prove that all animals are built on the same basic plan. By 1812 Geoffroy had established that all Vertebrates are organised according a single basic morphology. This being unquestionably true, Cuvier was full of praise for his colleague.

The trouble began in 1824, when Geoffroy published a paper where, together with valuable observations on the anatomy of Arthropods, advocated a preposterous correspondence between the sclerites of the segments of the body of Arthropods and vertebral pieces. He claimed that the arthropod was an animal living inside its vertebral column, while vertebrates lived outside it! Cuvier was nonplussed, yet, for the time being, he kept his criticism private.

Between 1820 and 1830 Geoffroy, who was a convinced epigenist, became more and more convinced of the significance of embryonic stages for the understanding of affinities, and dedicated himself both to its study and, at the same time as Meckel, to the study of abnormalities, and thus he and his friend the anatomist Étiènne Serres succeeded, by holing, shaking, varnishing or waxing eggs at different stages of incubation, to cause abnormalities that, at least, proved the point that the embryo is not rigidly pre-formed in the egg. Both Geoffroy and Serres, at this point, argued that the embryo, during its development had to pass through stages corresponding with those of less advanced organisms.

Meantime the embryological experiments of Geoffroy had, somehow, thoroughly alarmed Cuvier, who had previously stated that, if Geoffroy succeeded in modifying a single species, one of the basic principles of geology would crumble. Thus, for once in his life, Cuvier abandoned his rigid morality and, by using surreptitiously of his influence with the ministry of interior, tried to get the police to stop Geoffroy's experiments! Indeed Cuvier had no qualms when crashing the scientific reputation and possibly the perspectives of advancement of a poor soul dissenting from him, but this he did by openly using of his scientific prestige and of sound arguments.

Meantime Cuvier had published, in the second edition of the *Récherches sur les ossements fossiles* a new account of Mesozoic crocodiles found at Caen, Le Havre and Honfleur, and which had been poorly described by Foujas. There he ranged them in the, to him subgenus *Gavialis*, but acknowledged that they were different species, and underlined the many differences between the fossils among themselves and with the living Gavial. Foujas, however, had noticed some of these differences and had attributed them to some evolution in a Lamarckian sense. Thus Cuvier felt obliged to stress that these were extinct species and to insist that there could not be any sort of evolution.

Geoffroy was not convinced and re-examined the material, found some factual mistakes in Cuvier's description, and his results are summarised in the sub-title of his resulting paper: "On their natural affinities, from which results the need of a different generic allotment, *Gavialis, Teleosaurus* and *Steneosaurus*; and on the problem if the Gavials (*Gavialis)* now living in the oriental parts of Asia, are descended, by an uninterrupted lineage, from antediluvian Gavials, that is from the fossil Gavials, so called crocodiles of Caen (*Teleosaurus)* and of Havre et Honfleur (*Steneosaurus*)".

This paper is indeed the first that may be considered as a paper in evolutionary palaeontology. By it Geoffroy declares his conversion to true evolutionary ideas and at the same time defies a basic idea of Cuvier.

One of the critical remarks of Geoffroy concerns the osseous palate of *Teleosaurus*, which is a typical Mesosuchian, and therefore does not include the pterygoids. Geoffroy considers it of mammalian type, which technically is, and places this genus as possibly close to the ancestry of Mammals (it is actually a marine crocodile and the character that Geoffroy considers as 'advanced' is actually a comparatively primitive one in crocodiles).

Geoffroy, at this stage, for the first time openly praised Lamarck for his evolutionary ideas, but, nevertheless he did not accept the idea of the influence of environment as Lamarck did. His evolutionism is influenced by his embryological studies and he sees it rather as the deployment through the ages of a plan like the development of the embryo is in each generation; if environmental factors have an influence, which he does not deny, that must affect the embryo. Thus evolution is seen as the accretion of embryological changes.

At this point (1829) two students of Cephalopods, Laurencet and Meyranx, sent to the Academy a paper where, among other things, they suggested the possibility of comparing the organisational plan of cuttlefish to that of a vertebrate with the body bent so that the two abdominal surfaces merge and the anus comes to lie below the mouth.

It is possible that Cuvier, who, as secretary of the Academy, saw all the incoming papers, did not like it. Anyway, as a reply as to publication was delayed, the two authors applied for what we would now call 'peer review', which was entrusted to Geoffroy and Latreille. As the authors quoted his ideas on connections and their hypotheses fitted with his pet ideas, Geoffroy wrote an enthusiastic referee, signed also by Latreille, a pupil and good friend of Lamarck, who, however, promptly dissociated himself in a letter to Cuvier. Also Meyranx was scared by the well founded fear that Geoffroy's enthusiasm may lead him into trouble with the great Baron and, in a letter to Cuvier, took cover saying that Geoffroy in his comments had gone well beyond their intentions. Thus the pair immediately vanished from the debate. The first comments by Cuvier aimed to play down Geoffroy's ideas as 'weak analogies' which, as such, he might even appreciate. Thus were fired the first shots of a typical academic battle in the classical style of something like: "Mon illustre confrère, pour lequel j'ai la plus haute considération, et qui presqe toutjours se trompe …" (My renowned colleague, for whom I have the utmost consideration, and who is almost always wrong …). Geoffroy replied and the battle went on for a few meetings of the Academy; the cuttlefish and the two authors of the paper which originated the debate immediately vanished and their place was taken by a variety of arguments about the hyoid bone, general principles etc.

Even newspapers took sides, the old Goethe, who, though granting that Cuvier was "A Napoleon of intelligence", naturally sided with Geoffroy and dedicated to him his last paper. Eckermann relates in his *Meetings with Goethe* how, when he went to see the old poet on August 2, shortly after the July days, when a brief revolution had chased Charles X and substituted him by Louis Philippe, Goethe hailed him exclaiming: "What do you think of the great news? The volcano is aflame!". Eckermann thought that he was speaking of the political events and made some sad remarks on the family of Charles X and some forecast on future developments, but the poet cut him short: "We do not understand each other: I mean the public deflagration that happened at the Académie because of the debate between Cuvier and Geoffroy St. Hilaire, which is so important for Science!". Actually, as shown by a letter of Goethe to Soret, he quite correctly, considered the real quarrel as really beginning at the Academy's meeting of August 10.

Goethe, who died a few months later, actually wrote a curious essay, that is his last work: *Über der Spiraltendenz der Vegetation* where he praises Geoffroy's ideas,

However both scientists were surprised by the 'fracas' and when Cuvier, who had lost a daughter two years before, went to visit Geoffroy, who, just at the time, had lost one of his own daughters, the two agreed to drop the argument.

However this was not for long, as the crocodiles resurfaced next autumn with two papers by Geoffroy which are outspokenly evolutionist and where he tried to join the evidence of the fossils with that of embryology and teratology and even got into palaeoecology, as he suggested that the explication of the evolution of Mesozoic crocodiles into the living ones might have been occasioned by the fact that the Mesozoic atmosphere was presumably poorer in oxygen, than the later one!

From now on, and until the death of Cuvier, the shots fall on the students: Ampère, a friend of Geoffroy went to listen to the lectures where Cuvier attacked Geoffroy, then went to Geoffroy and, after consultation, gave his own counter-lectures in his course, but Frederic Cuvier went to Ampère's lectures and reported to his big Brother, and so on until Cuvier's death.

The later years of Geoffroy saw the old man more and more isolated, so much that the Académie published only the titles of his later papers; then he became blind, retired from his chair and died in 1844, aged 72.

Geoffroy went on from generalisation to generalisation: he thought that his 'law of the attraction of itself by itself' could explain Siamese twins, bilateral symmetry and, perhaps, a total view of the Universe!

Contrary to Cuvier, who never discussed homology versus analogy, but usually employed this instrument for correct analyses, Geoffroy was the first to introduce these two terms, and in fact Owen precisely says that he took these terms and their basic implications from Geoffroy. However, with his passion for general, universal laws and connections, Geoffroy often and grossly failed precisely on these issues and thus his potential influence on future developments of biology was considerably limited.

Georges Cuvier

Georges (more precisely Georges-Leopold-Chrétien-Frédéric-Dagobert) Cuvier was born in Montbéliard in 1769. He was a Frenchman of the Franche-Comté, which, at the time, was a dependency of the Grand-dukedom of Würtenberg. He belonged to a French protestant family of limited means. It appears that he was greatly influenced by his mother, a very pious woman, who assiduously cared for his education and instruction. Given the very limited possibilities of his family, Georges endeavoured to get some scholarships. He failed at the University of Tübingen, but he got a scholarship for a college in Stuttgart: the Caroline Academy, on a personal recommendation of the Duke of Würtenberg. There he followed the curriculum in administration (which included a good deal of natural sciences) and must have made good his time there as throughout his life Cuvier proved an excellent administrator. At the Caroline Academy he had as teachers in sciences, the botanist Kerner and Karl Friedrich Kielmeyer (1765-1844), a good biologist, who was later appointed to the University of Tübingen and who belonged to that trend that was shortly to develop into the 'Naturphilosophie' of Schelling. Cuvier got from Kielmeyer his interest for anatomy and zoology and remained in constant touch with his old master.

Having graduated brilliantly in 1788 Cuvier returned home, but as money was scarce (his father had been an officer in the French Army and had but a small pension), he looked for a job. There Cuvier was lucky, as he became a tutor in the house of the Count d'Ericy, himself a Protestant, who lived in his castle near Fécamp, in Normandy and there he spent quietly the next seven years, the most hectic of the Revolution, spending his free time studying the anatomy and systematic of the local animals, which, as Fécamp was a fishermen port, were mainly marine.

In 1794 abbot Teissier, an agronomist and a member of the Académie, who had taken shelter in Fécamp in order to avoid the attentions of the Jacobins, noted his qualities and recommended him to his friends at the Muséum: Daubenton, Jussieu, Geoffroy St. Hilaire, Parmentier. Having received such an enthusiastic letter, Geoffroy, in agreement with Daubenton, asked some manuscripts from Cuvier and found that Teissier had been just right in his appreciation of the young scholar. So Cuvier was invited to Paris. In 1795 Cuvier left Normandy and, shortly after his arrival in Paris, he was appointed as professor to the École centrale du Pantheon. Shortly afterwards he entered the Academy. Later, in 1802, the aged Metrud retired, leaving for Cuvier the chair of animal anatomy (comparative anatomy) at the Muséum, a chair that Cuvier held until his death. Thus, not yet thirty three years old he became the colleague of people already famous like Lamarck, Daubenton, De Lacépède. During his first period in Paris, Cuvier established a close friendship with Étiènne Geoffroy

St. Hilaire, with whom he began also a close scientific co-operation, so that within a few month they published a joint paper. Cuvier was also a born teacher and his lectures were immediately successful.

Georges Cuvier was an extraordinary worker and organiser, and he always did everything by the most methodical approach.

Cuvier made a surprisingly amount of work and it was practically always work of the highest quality. Besides teaching and doing his research work in anatomy, palaeontology and history of biology, he managed several administrative charges: he was twice in Italy and once in Holland, officially to inspect their Universities and Academies, but with instructions by Napoleon to ransack them to the profit of Parisian institutions and, in fact, though he also arranged some exchanges, he carried out his task with a much more heavy hand than his colleague Geoffroy did in Spain and Portugal.

Through his life Cuvier made a true collection of appointments and promotions: with Napoleon he became Chairman of the Institut de France, General inspector for public education and Minister for non Catholic Cults (a position that he kept under the Restoration), with Louis XVIII he was made a Member of the State Council, Chairman of the committee for internal affairs and a Baron, with Charles X, Chancellor of the University, Great Officer of the Legion d'honneur, Director of the interior ministry for the affairs of non Catholic Cults; Louis Philippe made him a Peer of France. The only two appointments that he refused were that of 'Intendent' of the Jardin and Muséum (Buffon's appointment), which was mooted to be revived just for Cuvier and that he probably refused out of consideration for his senior colleague Lacépède) and Minister of Interior. Cuvier passed triumphant from the service of Napoleon to that of Louis XVIII and of Charles X and got his last promotion by Louis Philippe shortly before his death in 1832. The extraordinary thing is that he did quite honestly and successfully in all his multifarious activities and meantime his scientific activities went on unhampered! True he was able to get a first class staff of collaborators (de Blainville, with whom he eventually quarrelled, Duméril, Duvernoy junior, Alexandre Brongniart, Valenciennes, Rousseau, Laurillard), but he also used of his immense influence to saturate all possible vacancies with his pupils. The result was that while French research kept a high standard, yet it was the last country in Europe to accept evolutionary theories.

Cuvier died after a disease of but a few days in 1832.

For Cuvier classification was a central problem, just as it had been for Linnaeus, but he was the first, in parallel with Lamarck and with far greater consistency, to deal with it using comparative anatomy.

There is no doubt that, on his arrival in Paris, he received a lasting influence by Bichat's theories and by Daubenton's methods both in anatomy and in the passion for a didactic arrangement of collections.

From Bichat, who had got it from John Hunter, Cuvier adopted the concept of 'economy of the organism' = co-operation or interaction of organs, an idea that both in Cuvier and Geoffroy is framed as 'Balancement des Organes' and which we have already considered. Again from Bichat he received and elaborated vitalism, that is to conceive life as resistance to death, which is the consequence of the free play of physico-chemical forces when their action is not victoriously fought by the 'vital force'. Moreover it is assumed that there are two level of life: organic life, that occurs both in plants and animals, and animal life, which is peculiar to animals.

The great asset of Cuvier was that he had a clear perception of the concept of 'organisation plan', on which he for some time co-operated with Geoffroy, and which was simultaneously developed by his colleague Lamarck and in Germany by Goethe.

When Cuvier began to give his courses, comparative anatomy was still a moot assemblage of more or less vague ideas and there is no question that it was Cuvier who made it into an organised and basic discipline. He did it to the very limit where could reach a single man provided with great learning and intuition joined with the ability of accurate observation.

His lectures were collected and edited by two of his best pupils, Constant Duméril and Georges Duvernoy, and published under the title *Leçons d'Anatomie Comparée* (5 volumes, 1800-1805). Cuvier's comparative anatomy begins by the accurate description of anatomy itself, but gives ample room to physiological consideration, as recommended by Haller, as well as, following Bichat's recommendations, he methodically discusses the interrelations which occur between organs in the same animal and the comparisons possible with other kinds of animals. Following this thorough consideration of the evidence, the significance of both differences and similarities is finally assessed.

Given Cuvier's religious beliefs, he was in a scientific impasse: his own studies on fossils and geology forced him to admit for far longer times than those quoted in the Bible, but yet he had to save the essentials of Genesis by denying evolution and we have seen how this led to the growing estrangement with Geoffroy and to his strong reactions to every hint to evolution. Moreover, his stubbornness led him to his most absurd investigations: in order to show that there was no evolution, he studied some mummies of Egyptian Sacred Ibis *(Threskiornis aethiopicus),* compared them with living specimens and, obviously, did not find any difference, so that he, triumphantly, concluded that he had disproved the possibility of evolution, a conclusion that could be easily countered by Lamarck as, by this latter's theory, evolution had to be adaptive, so, as no environmental factors had changed in Egypt since the times of the Pharaos, no change in the Ibis was to be expected!

It is to the great credit of Cuvier that he recognised that it was not possible to frame a single basic organisation for all animals. Thus he proposed to consider four basic plans in the organisation of animals that he called 'Embranchements', a curious choice, as it gives at first sight, the impression of an underlying idea of a branching genealogy. Apparently his innate conservatism considered that, as the terms of 'genus' and 'family' were already well established, it was a practical proposal to refer to larger assemblages as to a 'clan' made of many families. There is no evidence, but it may not be ruled out that the images of the Lullian trees, where 'species' are figured as the leaves, may have had some influence in the choice. Anyway the term was soon abandoned, first in favour of 'type' (suggested by De Blainville) and later, when evolutionary theories became dominant, by the now familiar 'phylum'.

The 'embranchements' advocated by Cuvier were the following ones: (a) Vertebrates, with the classes Fishes (inclusive of Agnatha and Chondrichthyes), Amphibians (inclusive of Reptiles), Birds and Mammals (which had been kept separate by Linnaeus) and, so far Cuvier's classification is identical with that proposed by Lamarck in the same years), the basic common characters for all Vertebrates being metamery, that is a segmentation of the body, mainly shown by the vertebral column and by the muscles, nerves and vessels connected with it, a ventral hearth and a nervous system entirely dorsal with respect to the gut. (b) Molluscs: non metameric animals usually provided with a calcareous shell and which nervous system is basically formed by a circum-oesophageal cingulum and a visceral loop. (c) Articulates, that is Insects, Crustaceans and annelid 'worms', which all have a clear external metameric structure and which nervous system has a group of supra-oesophageal ganglia, joined to a ventral gangliar chain by circum-oesophageal loops. (d) Radiates, which organs are, at least apparently, arranged as rays or spikes around a centre (jelly-fishes, sea-stars, sea-urchins).

Clearly such a classification did not fit for a number of animals, such as, for instance flatworms, which were poorly distributed here and there.

Cuvier's reformation of systematic on the evidence of comparative anatomy was published in Paris as *Sur un rapprochement à établir entre les différentes classes des animaux* in 1812, to be followed in 1817 by Cuvier's basic treatise *Le règne animal distribué d'après son organisation* where he completely abandons the Linnean and Buffonian tradition of using external characters, in favour of an anatomical approach, at least as far as available evidence allowed.

Still in the tradition of the Muséun, Georges Cuvier had care to develop the, so called 'collection des vélines', a series of excellent, coloured drawings of all the animals in the collections, anatomical preparations included. This collection, quite apart from its relevant artistic value, is especially significant as it generally allows for the precise identification the specimens studied by Cuvier or by his collaborators and, sometimes is the only evidence for materials since disappeared.

Cuvier's work as a systematist, and this was always his main concern, is notable not because he used really new criteria, but because, while his forerunners had used them occasionally or in a rather haphazard way, he was absolutely consistent. Cuvier was a most clear and methodical scholar and we may, at least briefly, summarise some of his basic criteria.

First animals are divided according their symmetry: in some there are at least two plans of symmetry, for instance in the Coelenterata the number of such plans is usually even: 4, 6, 8, etc.), in Echinoderms, such as star-fishes, it is usually odd (most commonly 5). As opposed to radial symmetry there is a bilateral symmetry, where the plan of symmetry divides the body so that one half of it is the mirror image of the other (as a matter of fact it was soon found that things were different from what Cuvier had assumed: a number of Coelenterates are, in fact, bilaterally symmetrical, and Echinoderms are almost never really radiates, moreover in several animals symmetry undergoes radical changes during development).

Metamery was another of the key criteria used by Cuvier. In a metameric animal the body is formed by a number of segments, more of less similar among themselves in their basic features, and ranged along the main axis of the body. Such are the Annelida (as, for instance, an earthworm), Arthropods (as a centipede) or Vertebrates. Again later research proved that Cuvier was partly wrong.

Animals such as slugs or snails, instead are non-metameric.

An extremely important concept in morphology is that of homology, which gained additional weight when it was incorporated with the evolutionary theory. However, Cuvier did not state the principle in any clear way, this was largely left for Owen to do, but used it intuitively in a topological way. In this sense organs may be said to be homologous when, independently whether they are used for the same or for different functions, they are built of the same organisational plan, for instance the fore appendage of man, of a horse, of a bat of a bird or of a frog, a lizard or a tortoise, are all homologous as they are all build by three basic segments: the arm, the forearm and the hand and, typically the same bones can be identified: humerus, ulna, radius, carpals, metacarpals and phalanxes. Each of these parts may be variously developed, there may be losses (for instance in the number of fingers), fusions and so on, but these may be recognised by a sufficiently accurate study. Instead the wings of a bird and of an insect are barely analogous: their function is the same, but their organisation is completely at variance. We shall, anyway, have to come back on this subject.

Other concepts that were largely used by Cuvier and, as we have seen, by Geoffroy, was that of the 'Balancement des organes' and that, already hinted by Aristotle, of 'correlation', by which certain morphological features are constantly associated. As we shall see this was a pivotal concept in Cuvier's studies on fossils, and can be illustrated by a joke which was told of Cuvier himself. It said that one day Cuvier was standing at his desk (it is known that Cuvier always wrote standing at high desks), when there was a sudden sort of thunderbolt and the devil appeared and exclaimed: "Cuvier, I have come to eat you!" Cuvier calmly examines the devil and "Impossible sir – replies – you have horns and cloven hoofs; you are necessarily vegetarian!" The devil, frustrated, vanished.

Indeed it was by using a balanced mixture of these criteria that Cuvier succeeded both in associating more or less isolated bones of some animals or to reconstruct the essential of many characters of several fossil animals on the evidence of but a few bones (and naturally went badly wrong when he had to deal with such incongruous animals as the Calicotheres!).

Cuvier's works were such that even scholars dissenting from his main views, took them as models for their descriptions.

As we said Georges Cuvier stands also as a founder of modern descriptive palaeontology.

Just to mention the main works of Cuvier in the field of a systematic zoology based on morphology, we may mention first his monumental *Histoire naturelle des Poissons*, which he undertook in co-operation with Valenciennes, 22 volumes, seven of them having been completed during Cuvier's lifetime, while the others, as far as osseous fishes are concerned were completed by Valenciennes in 1849, largely using notes left by Cuvier and where as many as 5,000 species are described; two further volumes on Chondroichthyes were written by Duméril. While both those completed during the lifetime of Cuvier and those completed by Valenciennes are quite good, those by Duméril are a poor thing.

Cuvier began his study of fossil as soon as he arrived in Paris by that of some elephant bones that had been acquired by Buffon and that Daubenton had but superficially examined. Cuvier was immediately able to show that these belonged to species different from both the living ones. At the time Paris was in the midst of a feverishly building activity and the Myocene gypsy layers of the then suburban areas were intensively exploited. These layers are rich in fossil vertebrates and Cuvier was soon busy with their systematic exploration. Most of it he did in co-operation with Alexandre Brongniart (beginning in 1776 and up to the end of the 19th century there were always some Brongniarts at the Muséum, and this is sometimes confusing). It became soon clear that, while these were mammalian bones, they were different from all other Mammals known. Moreover. Cuvier soon realised that these were stratified layers, each characterised by a different fauna and separated by thin layers either formed by volcanic ashes or by alluvial materials.

Thus the methodical comparison of the bones that he was collecting with those of living animals, allowed for the re-assemblage of several skeletons and, in many instances for the proposed reconstruction of missing parts which later finds usually proved to be correct (and this made Cuvier most famous and popular), this generally stimulated an interest in the collection of fossil bones, to the great benefit of museums, that of Paris first.

The studies on the Parisian region were also one of the foundations of French stratigraphy, which, until then had lagged behind both British and Italian studies. As we said in the previous chapter the first serious and brilliant attempts to a modern approach to Geology are due, in France to the joint efforts of Jean-Étiènne Guettard and Antoine-Laurent Lavoisier!

Cuvier's geological and palaeontological studies, however, were instrumental in determining his firm opposition to evolutionary theories and for his 'catastrophic' theories. As such they deserve some discussion. We have seen that Lamarck was studying fossil Molluscs, Cuvier, instead worked basically on terrestrial vertebrates. In

France terrestrial vertebrates were, during the Eocene-Pliocene period, in a sort of zoo-geographic trap, a sort of 'cul-de-sac', but Cuvier could not possibly imagine it. Very little evolution occurred there and, instead, there was a succession of faunal invasions coming from Asia and Africa. Moreover, especially during the Myocene, central France was the theatre of intense tectonic and volcanic activities. Thus Cuvier was correct when stating that 'his' fossils did not show any evolutionary continuity between one layer and the next one, while there were often clear evidences of important cataclysms.

Thus he added his palaeontological evidence, his morphological findings showing the complete discontinuity between the different 'embranchements', Bonnet's ideas that we have examined in the previous chapter, and his faith in Divine Creation and concluded that there were but two possibilities (i) a single creation, followed by local extinctions caused by cataclysms and re-populations by animals coming from some region that had escaped the cataclysm, or (ii) repeated creations by which God periodically, re-populated the Earth. The hypothesis of repeated creations is often labelled as the 'Cuvierian hypothesis', but, in fact, Cuvier himself preferred the first alternative and the hypothesis of successive creations was advocated mainly by his pupil Alcyde D'Orbigny. On the other side, in order to conflate his religious creed and his geological and palaeontological discoveries, Cuvier just needed to consider the Noachian Flood as the last of a series.

When we consider how, in the second half of the century evolutionary theories were politically used as an instrument to discredit the Bible tradition and both the Christian and Jewish faith, it is easy to understand how, while Lamarck and Cuvier might to some extent agree on classification, Cuvier, with his Calvinistic faith and his passion for precision even in the minute details, had absolutely to oppose the theories of Lamarck, a muddling evolutionist and follower of atheist 'philosophes' such as Diderot and D'Holbach, just as he could not swallow the often rather fantastic ideas of Geoffroy.

Actually the first paper by Cuvier on fossils was published in 1798 (*Sur les ossements qui se trouvent dans les gypses de Montmartre*) and thus it precedes by two years the earliest evolutionary hints in Lamarck. There followed a number of short papers, which were later collected under the title *Recherches sur les ossements fossiles* (4 volumes in the 1814 edition, grown to 5 in that of 1825). The 'Recherches' are the first almost organic approach to the study of fossil Vertebrates, and is the first work in which the principles of comparative morphology are systematically used for the interpretation of fossil materials. The preface to the 'Recherches' is titled *Discours sur les révolutions de la surface du globe* (*A discourse on the revolutions on the surface of the Globe*) and was later published as a separate volume. This partly incorporates suggestions from Bonnet and it is the monument to the basic mistake of all the scientific career of Cuvier: to have generalised an interpretation of the available evidence that was, on the purely local and temporary scale, basically correct!

Goethe, indeed, gave of catastrophism a short, but apt definition: "Diese vermaldeite Polsterkammer der Wissenschaffen!" (= "This cursed padded cell (for lunatics) of Natural history!").

Just to round up Cuvier's personality one has to mention his contributions to the history of sciences, such as the *Rapport historique sur les progrès des sciences naturelles depuis 1789 jusqu'à nos jours* (1810), which is part of the historical reviews which Napoleon had ordered from the secretaries of the various sections of the Institut de France, and his lectures, which were assembled after his death under the title *Histoire des sciences naturelles* (1841-1843). There is no doubt that he was convinced of the significance for any naturalist of a good scholarship in the history of sciences.

Cuvier also advocated to keep a sober judgement of the evidence and avoid all sanguine flights of fantasy (but for those that he himself did) and such "metaphysic subtilities" that Buffon had labelled as "brilliant chimères" and to which were liable to indulge both his old friend Geoffroy or his colleague Lamarck. However, Cuvier's outlook on biology is merely that of a descriptive science, very much like that of Linnaeus or Buffon. Nature and science are like two pictures, the second just trying to faithfully copy the first.

In Nature, as the creation of the Most perfect Creator, everything must be coherent, linked into a harmonic design and as such it must be portrayed by science. If this is not yet plain, that is because our understanding is equal only to describe a few features of this immense and sublime picture of natural beings.

Following Buffon and even more Daubenton, Cuvier holds it impossible for sciences to reach certainties, thus we must be content with such relative certainties as well made observations allow. Therefore one must strictly stick to establishing facts. The program of science is therefore merely that of an objective description, rather than indulge in developing non-testable theories. Stick to facts is the test of being wise. This being a disputable point we shall further discuss it at the end of this book.

The systematic criticism by Cuvier of the evolutionary theories of both Lamarck and Geoffroy is thus rooted in his principles to avoid all sanguine and daring conclusions. He was undoubtedly largely right, but, given the evidence available in his own time, while much of his criticism of his colleagues' theories was well grounded, yet facts largely proved that to maintain creationism was equally disputable. Lamarck had clearly said that, in order to explain facts there were two alternative possibilities: either the creationist one or the evolutionary one and that he believed in the second as a subjective choice. Had Cuvier been really as objective as he claimed, he should have acknowledged that, on the evidence then available, an objective choice was impossible.

A conclusion on the «three greats»

There is no question that seldom, if ever, in the history of biology three such outstanding personalities happened to work in the same institution, literally 'door to door'. It is equally plain that their genius were different and perfectly complementary. Had they been able to work as a team their results would have been immense, but the deep rooted prejudices of each one of them actually estranged them and faulted their ability of objective and constructive criticism and eventually led each one of them to maintain gross, blatant mistakes.

«Naturphilosophie» and evolutionary theories

A complicated mixture of good observational evidence and quite often of wild speculation are the legacy of a philosophic-naturalistic movement, to some extent linked also with the 'romantic' trend in arts, that, while it was mainly developed in Germany, yet it recruited a following almost everywhere during the first half of the 19th century. This is generally known as «Naturphilosophie» and is credited with Kantian origins. This is true only with the qualification that it may derive from the early phase of Kant's philosophical developments which, through Wolff (1679-1774) derives from Leibniz[1]. Morevoer some of the main advocates of Naturphilosophie (Schelling, von Baader, Hegel) acknowledged a considerable influence on their thoughts of the thoughts of Jacob Böhme (1575-1624), the so-called cobbler-philosopher, who was close to Paracelsism, which he developed into a mystic philosophy with archaic gnostic and Manichean features.

Thus all of them had to stress teleology. By their preliminary decision to find at all cost unity, order and purpose in the development of natural phenomena, they had no problem with accepting evolutionary theories. Thus German naturalists of the first half of the 19th century were mostly evolutionists, but a sort of evolutionists that were mainly concerned with the organism's internal drive to evolution; and who, eventually, saw evolution as the orderly development of a God's conceived plan.

They could thus easily accept the morphological basic concepts of Goethe, the precise methods of Cuvier , but not the idea of an inborn trend to perfection steered by environmental conditions as advocated by Lamarck. One can, perhaps, see their distant progeny in the 20th century in Daniele Rosa, Father Teilhard du Chardin or Willi Hennig, just to mention some.

Friedrich Schelling (1775-1854) was the true organiser of Naturphilosophie (he was, by the way, also the author of a study on Giordano Bruno and of another on Greek mythology, this last being used by Goethe when preparing one act of his Faust). He was the son of a parson and had began theological studies before turning to phi-

[1] I cannot judge whether Kant's thoughts on biological problems had any influence. Kant dealt repeatedly and in depth on logical problems pertaining to biological sciences. This he did especially in *Metaphysische Anfanfgsgründen der Naturvissenschaft* and in *Kritik der Urteilskraft*, which are usually considered among the most difficult Kantian works. Anyway Kant was clearly conscious of the peculiar theoretical difficulties that biological sciences have, with respect to sciences which are more liable to complete experimental tests and mathematical treatment.

losophy. He belonged for a while to that extraordinary group of thinkers and artists who, largely through Goethe's good offices, lived and worked between Jena and Weimar through their most active years: Herder, Schiller, Fichte, Schelling, Hegel, the Humboldt brothers, the Schlegel brothers, Hölderlin.

Shelling's main works from our standpoint, are the *Von der Weltseele* (*On the soul of the World*) of 1798 and *Erster Entwurf eines Systems der Naturphilosophie* (*First draft of a system of Nature philosophy*, 1799), which are purely theoretical works. Later in his life he moved through different universities and finally to Berlin, but by then, perhaps because of the death of his first wife, he had nothing more to say, but to, more or less acrimoniously, uphold his old ideas.

Several advocates of Naturphilosophie acknowledged themselves as neoplatonists. Such was Jacob Friederich Fries (1773-1843), who had began his studies in a Moravian seminar, and who advocated a correspondence between the macro- and the microcosm; and such was Franz Xavier von Baader (1765-1841), who, among other things, published in 1798 a typical *Ueber der Pythagorisce Quadrat* (*On the Pythagoric tetraktis*).

Seen through modern eyes one has almost the sensation that the many German biologists who followed this 'philosophy' were somewhat schizophrenic: on one side they produced accurate and even brilliant analytical studies, on the other, where general theories were concerned, they advocated a variety of providential evolutionism, a myth of the evolution of the World towards a planned perfect harmony, a faith for which no rational justification is needed.

Modern biologists generally decry 'Naturphilosophie' and the biology it produced. While I am not persuaded by most of their arguments, yet I certainly do not agree with something that I regard as a degeneration of a mythic instrument that had been most significant in Classic culture and that, once its Hellenistic superstructures be removed, might have had and, perhaps, still have a significant function.

Most historians of Science have assessed the theoretical speculations of people like Oken, von Esenbeck, etc. by the standards of later theories and more often than not by the standards of inductivist positivism of late 19th century or of its later derivatives, such as 'Dialectic materialism' (which, by assuming the myth of a fatal progress of humanity is just as rational as the neoplatonist assumed drive towards 'the One', and that, anyway, is a development of Hegel's theories, which are just rooted in Naturphilosophie!).

Clearly, given such standards for assessment, the theories of the romantic proto-evolutionists, must be judged to be thoroughly groundless. However, the serious scholar should study them within their historical context.

We said that Naturphilosopie is rooted in the religious sides of the thought of Leibiz and Spinoza, which, in turn, were rooted in the Christian and Jewish neoplatonism, as well as, occasionally in the mystic views of Böhme. Thus the romantic biologists had a complete faith in a 'Nature' with divine connotations (They, obviously did not offer any justification for their faith; faith does not need justification). But in their Divine Nature mankind had a central position in the system of the Universe (the problems of Godliness was central for most thinkers of that age: for instance in the French debates which developed into the official revolutionary cult of 'Goddess Reason'). Goddess 'Nature', which existed as the Creation itself, had to be one and rational (and so far this was pure pantheism and nothing new), but, as these thinkers appropriated the sanguine attitude of the 18th century rationalists, had also to be rather simple! If a joke is allowed, they had a sort of recipe: "take 1/3 Lucretius, 1/3 Spinoza, 1/3 Pithagoric numerology, fill to level with a bit of Wolff and a bit of Plato, season by Leibniz optimism and some generic determinism; serve hot". As I said, this is a joke which approximately describes the attitudes of many scholars. However some further consideration will show how their basic ideas were bound to produce a lot of sound evidence and paradoxical results.

Indeed these scholars saw the problem of distinguishing between 'affinities? and 'analogies', albeit they often went for very peculiar 'analogies'. By using and perfecting Cuvier's criteria, these scholars grouped organisms by 'affinities', that is by groups that, on structural plan, could be considered to be homogeneous (and in this fields, the best results were those by the British Richard Owen). However, following Linnaeus, they thought that each group assembled on the evidence of 'affinity' had some sort of connection with other groups, equally based on 'affinities' and considered such connected groups to be 'analogous'. They concluded that, in systematics, both 'affinity' and 'analogy' had to be considered and here they usually went wildly wrong. Again following Cuvier, they often based 'analogies' on physiological or ecological evidence. Thus Penguins were hold to have affinities with Ducks and Puffins (they all have webbed feet; the Great Auk (*Pinguinus impennis*), which was just about to become extinct, has a superficial similarity with Penguins), but they were also 'analogous' with other air-breathing marine animals like Cetaceans! Hawks might be grouped by 'affinity' with both Parrots and Doves, but were 'analogues' of carnivore Mammals! Moreover the joint implementation of the principles of 'affinity' and of 'analogy' allowed for ordering the organisms in co-ordinated groupings and, assuming a natural order, there must be a mathematical law ruling it. At this point scholars felt free to indulge in combinatorials and numerology of mixed and distant origins: Pythagoric, cabbalistic, Lullian.

Different authors tried with different numbers, the most successful being the entomologist W.S. MacLeay (1792-1865), who advocated 'quinarism': Species should be grouped by groups of five, and be ordered by affinity as the points of a regular pentagon (a well known magic figure of most ancient pedigree) inscribed in a circle. Analogous circles should be arranged so as to be tangent ('osculating', that is kissing) each other in the points of maximum analogy and so as to cover the maximum surface (a homage to Leibniz principles). The result being an orderly mosaic. This system had for a time a number of followers, including none less than Thomas Huxley, before be became 'Darwin's bulldog'. People following these techniques (today we would feed them as algorithms in some computer program) thought that their results showed the natural order of things and proved that the world was logically ordered by Providence.

The result of these speculations was that a number of scholars produced highly valuable studies on particular problems and wildly fantastic general theories.

In the previous chapter we mentioned Johann Friederich Blumenbach (1752-1840), just slightly junior to Goethe, and who may be considered as a forerunner of the biologists followers of Naturphilosophie.

Lorentz Oken (more precisely Okenfuss) (1779-1851) was born in Bohlsbach bei Offenburg, was one of the most noteworthy representative of this trend. Although from a poor farmer's family, he was able to graduate in medicine in 1807 and for a while was interested in military problems because of the Napoleonic wars. He was later appointed as a professor in Jena, where he published his *Vertebral theory of the skull* (see also the paragraphs on Goethe). He generalised his theory to the extent that he assumed that in the original 'Bauplan' of Vertebrates each vertebra must have had a pair of ribs and appendages and that in the head these were corresponding with the buccal and branchial arches, while each segment of the trunk should have had a pair of appendages. The funny thing is that though the theory has been proved basically wrong (most of the skull and the buccal-branchial skeleton derive from the neural crests and have nothing to do with the segmental skeleton) and in terrestrial vertebrates only the occipital region may be assimilated to modified vertebrae, yet in fishes a variable number of vertebrae is usually incorporated in the skull and among the fossil Acanthodians, the most primitive species do have a series of paired fins, and in living animals at least part of both the paired and unpaired fins derive from embryonic materials belonging to series of successive segments.

While in Jena, Oken proclaimed himself a neoplatonist and set much store on a confused theory of his holding a central function to the 0 (zero), to the serial additivity of characters and to the position of man as a microcosm synthesising the macrocosm of the whole living world.

Being an outspoken liberal Oken had to leave Jena for political reasons and moved to München and thence, in 1832, to Zurich, where he spent the remaining of his life. In 1816 he founded a journal, *Isis*, which was very relevant in the cultural evolution of the time and where a number of typically romantic biological papers were published. Finally he organised the first scientific congresses in history.

Not considering the many papers of a more special scope, Oken's main works were his *Lehrbuch der Naturphilosophie* (1809) and his *Naturgeschichte für alles Stände* (= *Natural History made intelligible for everyone*, 1833-1842), which was instrumental in the diffusion of a general awareness of the significance of a naturalistic culture.

As we said an objective judgement of Oken and of other romantic zoologists is quite difficult. Their insistence in looking for the one basic structural plan, led them often completely astray, but as often it was quite fruitful, as, for instance, allowing for the recognition of the various parts of the head and of the buccal apparatus of Arthropods with other serial structures of the rest of the body. On the other hand how shall we judge Oken's general criteria for a 'natural' classification? He considers that systematics must be based on the assumption of the addition of different basic organs and functions, which can be harmonised by a special brand of 'quinarism'.

As Oken actually changed his classifications from time to time, we shall here summarise the basic alternatives he considered.

(A) Mankind is the summit of nature and there are four basic functional levels corresponding with the four elements of ancient chemistry: Nutrition, corresponding with Earth; Digestion, corresponding with Water; Respiration, corresponding with air; motion, corresponding with fire: Thus the animal kingdom must necessarily materially correspond with the progressive addition of the various activities of human organs. Moreover as man has five senses there must be five and no more than five classes, of animals: these are the 'Dermatozoa', where touching is prevalent = Invertebrata, 'Glossozoans' (where taste is prevalent, and these are the fishes), Rhinozoans (where smell prevails, these being the Reptiles); Otozoans (prevalence of hearing, the Birds) and, finally Ophtalmozoans, where all senses are perfect and sight prevails, and these are Mammals).

(B) 1) – Animals may be divided into two 'provinces': Vertebrates and invertebrates; 2) – each 'province' is divided into four 'circles', corresponding with the elements of the basic organs of each one, the circles corresponding precisely with Cuvier's 'embranchements', 3) – each 'circle' is divided into three classes, apart for the last that has four, the fourth class of the fourth circle being divided on the principle of the five senses; 4) – each class has a number of orders equal to the number of the 'circles' + the numbers of those of a lover level; 5) – each class has a number of families equal to that of the classes of its own circle + that of the classes of the lower circles.

It is remarkable that, apart from his systematic, one result of Oken's beliefs in progressive evolution and addittivity of characters, made Oken a strenuous supporter of the theory which was later called 'recapitulation theory' or 'Haeckel's theory', as he maintained that, with passing times, in all animals, the 'inferior' features were pushed back and concentrated into the embryonic development, and thus that the embryos passed through the morphologic-functional stages of the adults of their remote ancestors!

Oken advanced also a theory on the origin of life, which was later revived by Haeckel: he assumed that there has been a primordial mucus (Urschleim) that, like planets, naturally took the shape of minute globes, these, on the seashore, aggregated to form infusorians (Urthiere).

Christian Gottfried Daniel Nees von Esenbeck (1776-1858) was professor of botany in Bonn, he was a good systematist and is usually remembered as one of the forerunners of the 'Cellular theory'. However he advocated some quite strange general theses: he had adopted Schelling's theory of 'polarity', and thus held that mushrooms correspond with the North and the other plants with South, that animals correspond with midnight and Mankind with midday.

Similar odd theories were advocated by many other German naturalists, who, otherwise, made considerable contributions to biology. Thus, as an example, Carl Gustav Carus (1789-1869), professor in Dresden, a good amateur painter and a friend of Goethe, by whom undoubtedly he was influenced, was a first class anatomist and deserves to be remembered also as a pioneer in the field of psychology and of what we now call psychosomatic syndromes. Nonetheless he propounded the most strange theses on the connections between the structure of the hands and moral features!

The influence of the Naturphilosophie lasted well beyond its official dismissal, for instance an influential philosopher like E. Hartmann developed what he called 'Trascendental realism', which he expounded in his *Philosophy of the inconscious* of 1869 and his *Theory of Categories* of 1896, very much under the influence of Schelling.

Franz Joseph Gall (1758-1828) is a scholar whom we might well have mentioned in the previous chapter, but, all taken, he is closer to the intellectual trends that we are considering here. Gall was the first to maintain that different functions of the brain were precisely located in different places of the brain itself. His proposed locations are rather fantastic and his claim to be able to determine the development of each area from the external morphology of the skull entirely groundless. His 'phrenology' (a term that he never employed) became immensely popular and quack doctors were still practising it on market-places in the times of Mark Twain. Nevertheless Gall was an excellent anatomist and was a pioneer in the study of the main connections in the central nervous system. He may also be considered, for what it is worth, the man who provided the earliest seminal ideas in criminal anthropology.

The Vestiges of Creation

In France the evolutionary theory of Lamarck was buried by the authority of Cuvier and the influence of his pupils, and for the same reasons Geoffroy's transformism had no room. Yet Lamarck's ideas continued to be more or less commonly quoted in scientific popular literature. In Italy, which, at the time, may well be considered as a cultural province of France, but four people who had actually worked in Paris, advocated transformist ideas, and got almost no audience: Giosuè Sangiovanni, a Neapolitan, F.A. Bonelli, a Piedmontese, abbot Alberto Fortis, whom we have already mentioned, and, finally Giovanni Rasori (1766-1837), a romantic madcap, the upholder of fantastic and really dangerous therapeutic theories and a good patriot, conspirator and, for a brief time during the French occupation, the chancellor of the University of Pavia.

In England almost no one cared for evolutionary ideas (though, as we shall see in the next chapter, Darwin began working of his ideas as soon as back from the *Beagle* voyage (1837). Nevertheless in 1813 three members of the Royal Society, Wells, J.C. Prichard and Lawrence, independently advanced some ideas on natural selection (and were promptly silenced) and in 1831, the very year when Darwin sailed with the 'Beagle', the obscure Scot botanist Patrick Matthew published, as an appendix to a book 'On naval timber', a few pages advocating evolution and which prompted Darwin, when he read it after the publication of his own book, to write to Wallace 'He gives most clearly, but very briefly … our views on natural selection.' and 'most expressly and clearly anticipated my views.' Matthew's book was totally ignored.

The one, important, exception to this lack of interest was a book: *Vestiges of Creation* published anonymously in 1844 by Robert Chambers, who had made a name for himself as an author of popular science. Chambers' is a rather poor work, but it met with immediate success and called for furious criticism from most of the scientific establishment, including both Lyell and Huxley, who just a few years later were to become the main advocates of Darwin's ideas. Probably the main merit of Chambers' book was that its critics offered Darwin the opportunity to review in advance most of the criticisms which could be proposed to his own *Origin of species*.

The development of microscopes

We have seen the development of microscopy in the 1600th and the comparative stasis of these studies during the following century. The immense importance that microscopic studies acquired in the 19th century justify a digression on the technical development of this device.

By the end of the 17th century there were two types of microscopes: simple microscopes and compound microscopes. Simple microscopes were just simple lenses mounted on appropriate gadgets both to make the best of the light and of the possible manipulations of the objects. The simplest laws of optics dictated the maximum magnifications possible and, as we have seen in the previous chapters, only workmen of superlative abilities could approach these. Compound microscopes, obviously usually allowed for higher magnifications, but at a heavy cost: image definition. Indeed, first: images were distorted close to the limits of the visible field; second: because of refraction, images were confused and surrounded by coloured halos. The first problem could be eliminated by the adoption of diafragms which eliminated the periphery of the field, but the loss of luminosity and the restricted field were serious inconveniences for the scholar. As for aberrations the situation appeared desperate. Newton and Huyghens had clarified the basic aspects of the problems, but Newton himself considered the problem hopeless. However in 1733 Sir Chester Moor Hall (1704-1771) had the idea to solve the problem by joining lenses made of glasses having different refraction index. He thus built an achromatic objective for telescopes by joining a lens made of «flint» glass with one made of «crown». Such objectives were fairly successful in astronomy, but in no other field of science.

Euler (Leonhard Euler, 1707-1783) elaborated between 1768 and 1771, the theoretical groundwork on which an achromatic microscope might be built. A first prototype was submitted in Leiden a few years later. The first achromatic lenses for microscopes were actually made by Franz Ulrich Theodor Aepinus (1724-1802), a Balt and a Russian State Counsellor, in 1784, and by François Gerardzoon Beeldsnyder (1755-1808) a colonel in the Dutch cavalry, in 1791. However the first microscopes capable of a good power of resolution were made only around 1807 and, as far as lenses were concerned, the best were the Dutch ones. Many improvements were soon introduced and, among those who greatly contributed to them we must certainly remember Giovan Battista Amici (1786-1863) who was first professor of Mathematics at the University of Modena (1815) and later moved to become astronomer at the Museum of the Specola in Florence and, at the same time, professor of astronomy in Pisa. It is sometimes related that Amici introduced the first «homogeneous immersion» objectives (which allow for much greater magnifications than the ordinary ones), but that is not true. Immersion objectives are those in which, during observation, the frontal lens is immersed into a liquid (usually cedar oil in modern objectives) which has a refraction index close to that of optic glasses. In fact Amici contributed basic improvements in the illumination systems (Amici's lens) and made some preliminary studies for the construction of 'water immersion objectives', which are excellent for the study of living protozoans, but which are exceedingly difficult to build and to employ, as the frontal lens is less than one millimetre across and their field is extremely narrow. These, anyway were first made by the British John Dolland in 1844. The «homogeneous immersion objective», the one using cedar oil, was first built only in 1870.

We shall not follow later developments, but it will be apparent to the reader how the enormous developments of microscopic anatomy during the 19th century were just made possible by the technical advances that, at last, allowed for a clear vision.

Anatomy and Embryology

The improvements in the methods of comparative anatomy, mainly due to George Cuvier, made morphology a powerful instrument of investigation not only for descriptive purposes or for systematic, but also to investigate physiology and adaptations to environment.

Among Cuvier's pupils we may begin by Johann Friederich Meckel (1781-1833) from Halle. Both the father and grandfather of Meckel had been professors of Anatomy there and had given significant contributions. Young Meckel studied with Cuvier for some years and, when barely 25, got a chair of Anatomy in his native town, where he remained until his death. His works cover several aspects of the anatomy both of Vertebrates and invertebrates, and especially notable are his studies on the morphology and development of the skeleton (every student of anatomy knows «Meckel's cartilage» or «mandibular cartilage»). His researches are amongst the most complete and important done in the first half of the 19th century. In his time special attention was given to his monograph on the Platypus, which had been but recently discovered and which position was most controversial. However much more important was his paper of 1809 where he showed that Cuvier's «Radiata» included two entirely different kinds of animals: Coelenterates and Echinoderms (actually this was commonly accepted only after 1848, when Leuckart confirmed Meckel's findings). The anatomical works of Meckel are in all ways model ones and prove the immense anatomical culture of their author. His general interpretations may be found in his treatise *System der vergleichenden Anatomie* (= *A system of Comparative anatomy*) (1821 and later): He has the unquestionable merit of being openly evolutionist, but the evolutionary mechanisms that he advocates are to some extent borrowed from the ideas of Geoffroy St. Hilaire, whom he had, naturally, well known in Paris, some hints from the Naturphilosophie and some from Lamarck, whom he equally knew, including the possibility of spontaneous generation. Meckel has not only the merit of having advocated the significance of comparative embryology, but he was also one of the first (1821) to maintain the principle that animals, during their development pass through morphological developmental stages comparable with the adult ones of other taxa related to them: the multi-parental 'fundamental biogenetic law' of Haeckel.

Meckel produced also some work, again under the inspiration of Étienne Geoffroy St. Hilaire (1805-1861), on the significance of monsters; which, however, was the true specialisation of Isidore Geoffroy St. Hilaire, Étiènne's son.

Ideas similar to those advocated by Meckel were maintained in France by another pupil of Geoffroy: Antoine Étiènne Renaud Augustin Serrès (1787-1868), who became professor of Comparative anatomy at the Muséum d'Histoire Naturelle, and that has already been mentioned as a supporter of Geoffroy.

Another great representative of comparative anatomy, whose general ideas may be derived from Cuvier's, is Richard Owen (1804-1868). He was born in Lancaster and was first a medical practitioner. Later he became a first class anatomist and was for some time director of the Hunterian Museum, where he succeeded his father-in-law. Finally he became director of the British Museum of Natural History. As such he was the prime responsible for the transformation of the Natural History Museum from an appendix of the British Museum (Arts and Library), then already rich with important collections, but a rather inorganic and haphazard establishment, into one of the major scientific institutions in the world. This he achieved by making it the central archives of the immense and capillary network of explorations that British scientists, military, missionaries and even tradesmen were building during this phase of fast expansion of the British Empire and which insured a constant flow of materials. Under his direction the British Museum became a basic reference for the biologists of the whole world (as the parallel development of Kew Gardens was for botanists).

Owen was the only British naturalist who had shown, in 1844, some appreciation for Chambers' book, which, as we said, was openly evolutionist, just as he appreciated the first Darwin-Wallace's communication to the Linnean Society. Soon, however, apparently being afraid of the potential danger that Darwinian theories posed to classic deism, refused the new theory in spite of being a outstanding comparative anatomist and a good palaeontologist. He, as far as methods stuck by Cuvier's, and these he developed and perfected, so that, even avoiding evolutionary concepts, he was the one who gave the better definitions of the criteria for homology, which he superbly employed in his papers.

As far as purely theoretic aspects are considered, he kept to the concept of archetypes of which the different species are varieties and completely adopted Oken's theory of vertebrate segmentation. Owen did not deny evolution, but only as the development of preordained project.

His contributions to both comparative anatomy, zoology and palaeontology were many and varied. and ranged both over Vertebrates and invertebrates. Owen became particularly interested in fossils when, on the return of the *Beagle*, Darwin handled him his collections of South American fossils, but his outstanding contributions in this field were the first descriptions of Mesozoic Mammals, from Lower Cretaceous English sites, and the studies on the Therapsids, the reptilian ancestors of mammals, which were reaching the British Museum from South Africa.

Few Italian anatomists deserve mention in this period, the best who contributed to problems of general biological interest being Luigi Rolando (1773-1835), who was professor in Turin and, while the French occupation of Piedmont lasted, in Sardinia. He made important studies on the anatomy of the brain (1809) and of the medulla (1824) both on man and on other animals, and Bartolomeo Panizza (1782-1867), who was professor in Pavia since 1817 and made notable comparative studies on the lymphatic system, particularly of Reptiles (1833), and on cranial nerves. His main merit is to have been the first to precisely locate the higher visual functions in what are now known as the optic lobes.

A peculiar story is that of Mauro Rusconi of Pavia (1776-1849). Though graduated as an M.D., he never attained a position better that 'demonstrator'. He worked for a while in Paris with Cuvier and, on his return, failing to get a satisfactory appointment, he retired and lived of a small rent that he had. His publications are few, and have been largely ignored outside Italy, although they earned him the gold medal of the Institut de France in 1831. Apart from the first description of a neotenic Amphibian (*Proteus anguineus*) which he published in co-operation with P.Configliachi, Rusconi chief merits are the demonstration that in Salamanders the separation between arterial and venous blood is not complete and his *Développément de la Grenouille*

(1826), where he was the first to precisely describe the segmentation of the typical Amphibian egg. Rusconi called 'framboise' (= strawberry) what we now call *morula* and, in spite of extremely poor instruments, was able to describe the formation of the *sulcus falciformis* and of the blastopore. As at the time there was not yet a cell theory available, Rusconi thought the blastomeres to be some sort of elementary molecules by which organisms were built.

When the Austrians recaptured Pavia at the end of the Italian first war for independence, Rusconi fled to Piemonte, where he died shortly afterwards.

By far the most important embryologist of this age was Karl Ernst Ritter von Baer, Edler von Huthorn[2] (to use his complete name and titles). Von Baer was born in 1792 in Estonia from a noble Balt family (Balts were most of the nobility of the Baltic states Estonia, Latvia and Lithuania; they were families of German origins and language, who had settled in the Baltic states after their capture by the knightly 'Order of the Sward' and 'Teutonic order' in Medieval times, and had formed the ruling class of the Grand-duchy of Lithuania, which had gradually extended until it comprised even the Ucrainian Kiew. Later, when Grand-Duchy had merged with Poland they had become Poles and, when Poland was partitioned, had finally become Russians, though still keeping their German language and tradition). Thus, when young Karl was of age, he was sent to study first in the university of Dorpat (now Tartu, and which had been opened just six year before) and later in Vienna and Würzburg. There he met with Ignaz Dollinger (1770-1841), a pupil of Schelling, who was especially interested in embryology. In 1817 von Baer was appointed as professor in Königsberg and in 1834 he moved to a chair of the Academy of St. Petersburg. His activities during his Russian sojourn were manifold: ethnography, anthropology, embryology, etc. Finally he retired and went back to Dorpat, where he died.

We may recall how in the 17th century Reigner de Graaf had described the mammalian ovaric follicle and the occurrence in the *tubae* of the blastocysts, which he had misjudged as eggs. These had been again studied by Cruikshank in the Fallopian tubae of the rabbit. It may be said that von Baer started where de Graaf had left. First he, studying the ovary and genital tracts of the dog, confirmed de Graaf observations, but, upon reflection, he considered that the bodies found in the tubae had the same size as the first stages of development that he found after their implantation in the uterine walls; he then made a better study of the development of ovary's follicles and was able to see the egg-cell inside them. Von Baer published his results in a small tract: *De ovi mammalium et hominis genesis* (1827) (in fact what he actually saw was the ovocyte 2).

It is interesting to remark that von Baer's work was almost contemporary with the first development of the cellular theory, while a correct interpretation of the process of fertilisation came much later.

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[2] Which may be translated 'Knight of Baer, noble (lord) of Huthorn'.

Von Baer's major work *Über Entwicklungsgeschichte der Thiere* was never finished, two volumes were published in 1828 and 1837, but the third one had to wait many years before being edited from von Baer's notes.

There von Baer took his lead from his work on the chicken's development and from careful consideration of all published evidence on all vertebrates, to attempt a general theory of development. His personal contributions of new evidence were the discovery of the dorsal chord in the chicken's embryo and how it was substituted by the centres of the vertebral column, his confirmation of the branchial arches and clefts, which had just been described by Rathke, the description of the development of the amnios, and other.

As far as the theoretical interpretation of the evidence was concerned, he, while using methods that were becoming standards ones, used them with such methodicity and precision that his embryological work may well compare with that of Cuvier on adults, and his book may be taken as the foundation of modern embryology.

Von Baer systematically stressed and practised the need for comparison with other organisms in order to gain a correct understanding of any particular morphology, and he also gave due attention both to the adult conditions occurring in other species and to that occurring in the species studied.

It was within this framework that von Baer, also on evidence supplied by his friend and pupil Pander, proposed the first formulation of that which was later called 'theory of embryonic or germinal layers'. Von Baer remarked that the zygote divides rapidly until it turns into an embryo where one may distinguish four layers of cells: first only one exterior and one interior, but these soon delaminate to produce two other intermediate ones and from these develop the various organs.

Later, in 1845, R. Remak argued that the primitive layers may well be considered as being only three: ectoderm, (the external one), mesoderm (the middle one) and entoderm (the inner one). By confirming and extending von Baer's results, Remak maintained that the ectoderm produces the epithelia of the outside of the body and the nervous system, the entoderm produces the epitlelia of the gut and of its glands and, in Vertebrates, the dorsal chord, while the mesoderm produces all the rest of the body. The theory of germinal layers was later generalised for all metazoans and it is still with us, although with considerable qualifications, as, even in Vertebrates, just for instance, a good deal of the head's skeleton, and the bony scales of the body derive from ultimately ectodermal cells. The theory is also unfit to account for the development of a number of invertebrates. Anyway in its times and for at least a whole century, it proved of great heuristic value, especially when it was embodied into the evolutionary theory.

Von Baer had also an important part in the development of what Haeckel pompously later called the 'fundamental biogenetic law', commonly called 'recapitulation'. Several authors previous to von Baer had remarked how in the embryos of 'higher' animals there was transitory evidence of organs that are permanently and fully developed in animals with a 'lower' organisation. Among them we have already mentioned Oken, Meckel and Serrès. Von Baer emphasised how corresponding stages of development in Vertebrates may be morphologically much closer than is the morphology of the adults (and that is generally true), and, moreover argued that the more different are the adults, the sooner differences appear during development.

Von Baer never accepted Darwin's theory and, both in his observations and interpretation, he took an extremely teleologic view of 'transformism', as evolutionism was then called. Indeed he firmly believed in an innate tendency of all organisms to perfection, which we have seen to be of distant neoplatonic origin and of recent Lamarckian parentage, and, moreover, assumed a rigorous teleology, a true detailed program for the development of the universe, that fitted with the theories of Schelling and of the other 'philosophers of Nature' and that ultimately depended on St. Augustine. Anyway Darwinian evolutionists were promptly able to incorporate von Baer's evidence and its general framework into their own theory and use it in phylogenetic reconstructions.

A good friend and close collaborator of von Baer was Henrich Pander, from a rich Balt family of Riga (1794-1865). As all Balts did, he went to complete his studies in Germany at Würtzburg. On the advice of von Baer he made some investigations on the development of the chicken, which he published in 1817. There, apart from other significant observation, he developed a first draft of that 'theory of embryonic layers»'that was later elaborated by his friend and mentor. Later Pander went back to Russia and there he completely abandoned experimental research, and turned unto geology and invertebrate palaeontology. His most remarkable studies concern Trilobites, where he discovered the special sense organs still named 'Pander's organs' and which are amongst the very few special sense organs known in invertebrate fossils.

As it often happens with new theories, people soon asked themselves whether the 'cell-layer's theory' could apply also to invertebrates and in 1849 Thomas Huxley (whom we shall mention again as 'Darwin's bulldog') thought that in jelly-fishes the outer epithelium could be considered as the homologue with the ectoderm and the gastral epithelium with the entoderm of more complex animals. Typical jelly-fishes have no true connective tissues between their outer and gastral epithelia and Huxley argued that this was a critical character for the assessment of their systematic position.

A contemporary of von Baer, who eventually took over his chair in Konigsberg, when the latter left for Russia, was Martin Heinrich Rathke, from Danzig (1793-1860). Rathke was the first to describe the branchial clefts in Birds and Mammals (1829) which previously had been but vaguely observed and that later von Baer was able to properly appreciate in the framework of his general theories.

In order to give an adequate appreciation of Rathke's achievements one must consider the technical difficulties that he faced. Methods for the fixation, staining and sectioning of the material were absolutely in their infancy, improved microscopes, which gave images not blurred by diffraction and spherical aberrations, were just appearing, serial sectioning and the construction of tri-dimensional models were unthinkable. Nevertheless Rathke was able to study the development of the branchial clefts and arches and their later regression in Amniotes. He studied the development of lungs. His studies on the genital apparatus allowed for the understanding of the 'bodies of Wolff' (the pronephros) which usually vanish when the mesonephros and metanephros develop. He gave an account of the development of 'Wolff's duct' which, during later embryonic development is incorporated into the reproductive apparatus. Finally he described the hypophysial pouch (Rathke's pouch), the ectodermal pouch at the bottom of the stomodeal depression that contacts with the mesencephalic infundibulum and which apical portion later becomes the anterior or glandular hypophisis.

Rathke was not only a brilliant describer. He took little notice of the more theoretical (and doubtful) aspects of the biological debates of his times, and concentrated on the study of metamorphosis, especially of amphibians. There he paid special attention to the reduction and eventual disappearance of organs, such as the tail of tadpoles, which he called 'rückschreitende Metamorphose', and which occur when the animal drastically changes his ecological requirements, as when turning from a purely aquatic to a basically terrestrial habitat.

Rathke made also a number of investigations on marine animals (Molluscs, Crustaceans) and was the first to establish the Chordate affinities of Amphioxus, which till then had been considered as a Mollusc.

This last discovery is almost contemporary with the studies (1835-38) by Johannes Müller on the Lampreys and other Cyclostomes and which proved that these were not true fishes, but rather primitive Vertebrates.

General biology: the development of histology and of the cell theory

A basic advance of biology in the first half of the 19th century was the development of the 'cell theory'.

During the previous centuries biologists had mooted the problem whether there was a complete separation between the living and non living beings and whether there was a basic common structure in all organisms and, finally, what was the precise significance of reproduction. We have seen how many scholars advocated, on purely theoretical arguments, that there must exist some sort of elementary units from which organisms were assembled. Many authors, moreover, had argued that such 'monads»'had to be eternal. We shall now see how the concept of 'cell' was developed.

The term "cellula" (a small cell or room) had been first used by Robert Hooke, who had examined to the structure of cork by the microscope, and later had considered other plant tissues. His, however, were mere descriptions. In 1672 Marcello Malpighi and Nehemiah Grew had maintained that at least some parts of the plants were made up of minute elementary units, which had been variously named: 'utriculi', 'sacculi', 'vesicles'. Later new hypotheses were advanced, always because of insufficient evidence, due to inadequate instruments. We have mentioned Haller's hypothesis of elementary 'fibres' (1757), the 'convoluted cylinders' of Felice Fontana (1781) and so on. With the beginning of the new century hypotheses became more and more numerous and gradually approached the classical theory. In 1802 Sprengel re-introduced the term *cellula* (= cell) and the already mentioned Nees von Esenbeck, in 1820, stated that all vegetable tissues were made of cells. Similar ideas were maintained by Pierre Jean-François Turpin (1775-1840); Turpin, born of a poor family, was first a soldier, then an explorer and a systematist. Between 1820 and 1824 he advanced some pioneer ideas on plant cellular structure and on yeasts. Another pioneer of the cell theory was Franz Julius Ferdinand Meyen, (1804-1840); he was the son of a magistrate and had also been a soldier, thence he was sent by Humbolt to explore and collect in South America and, since 1834, became a university professor. Meyen in 1830 advanced a cellular theory for plants.

François-Vincent Raspail (1794-1878) was an extraordinary 'personage': of poor family was originally meant to be a priest, instead he became a chronic revolutionary, a sociologist, physician and a notable chemist. His ideas on cells, advanced in 1833, were severely and unfairly criticised by Schleiden.

Milne-Edwards, whom we shall consider further on, thought of 'globules'. Charles François Brisseau de Mirbel (1774-1854), the son of a magistrate, entered to the Museum in 1798 in order to avoid recruitment into the army. He was the first French plant microscopist and a pioneer hystochemists; he considered the plant as a 'collective being'. Lorenz Oken thought that organisms were to be considered as colonies of 'infusorians' and that the inner content of each 'infusorian' was an 'Urschleim' a 'primordial mucus'. Finally René Joachim-Henri Dutrochet (1776-1847) from a rich and noble family, and who was basically interested in animal and plant physiology, physics and medicine and is mainly remembered for his discovery that mushrooms are the reproductive bodies growing from the diffuse mycelium and for his works on the respiration of plants, argued (on scanty evidence) that the organs of animals were made of agglomerated utricles.

Thus, by the third decade of 1800 the idea that plants, at least, were basically made of microscopic living units was fairly widespread. Anyway traditionally the merit for an organic cellular theory is credited to Mathias-Jacob Schleiden and Theodor Schwann.

Schleiden was born in 1804 in Hamburg, his father being a well known physician. He studied in Heidelberg and was for a short period a lawyer. He got into a depressive crisis, attempted suicide, but, having, by great chance, failed, he began studying natural sciences and graduated in Sciences and Philosophy. He was thence appointed as associate professor of botany in Jena in 1839, and there he stayed until 1862. Having been invited to Russia, he was but briefly there, thence returned to Germany and settled in Dresden, where he died in 1881. His lasting fame is linked to two publications: the first (1838), a short paper of 32 pages is titled *Beiträge zur Phytogenesis* (*A contribution to phytogenesis*) and is a study of the embryonic sac of phanerogamous plants and there he stated the basic idea of a cellular theory. He compared the tissues of plants with colonies of Coelenterates, each cell corresponds to a polyp. He also suggested that the nucleus (which existence had been conclusively proven by Brown in the epidermis of Orchids in 1831) was in a way the germ of a cell. The second is a book (*Grundzüge der wissenschaftlichen Botanik* (Foundations of Botanical Science) where Schleiden elaborated and completed his theory. Schleiden and his followers were set against the ramblings of the 'Naturphilosophie' and aimed to reduce everything to corpuscular physics. Thus the central tenet of the theory was that the minute 'grains' that they could see in the 'protoplasm' did coalesce and thus formed the nucleus; this acquired a membrane, and subsequently grew until the nuclear membrane became the membrane of the next doughter cell. Such aggregations and the new formation of nuclei-cells spontaneously occurred in organic fluids. It is clear that the cell theory of Schleiden (and of Schwann) was something entirely different from the cell theory with which we are now familiar and which was evolved within the next few years.

Theodor Schwann was born in 1810 in a village near Düsseldorf, the son of a bookseller, He was first a student with Johan Müller in Würzburg and later his assistant in Berlin (Müller we have already mentioned and we shall give him adequate space further on). He later got a chair in Louvain and still later in Liège. He died in Köln in 1882. Anyway most of the basic work of Schwann was made during the five years that he worked with Müller in Berlin. The scientific production of Schwann was rich and varied: structure of nervous fibres, proprieties of pepsyn, respiration in the Chicken's embryo, studies of fermentations, so called spontaneous generations. As Schwann himself related, the idea for his basic theory sprung form an occasional conversation with his friend Schleiden, who told him of his current researches and hypotheses on the nucleus of vegetable cells. Schwann thence remembered that he had recently seen a similar structure in the notochord of the tail of tadpoles (a particularly suitable tissue, as it consists of big, vacuolated cells with a strong outer membrane and a large nucleus. Schleiden immediately confirmed Schwann's observations (October 1838) and so Schwann published next year his paper *Mikroscopische Untersuchungen über die Übereinstimmung in der Struktur und Nachstum der Tiere und Pflanzen* (= *Microscopic researches on the analogy in structure between animals and plants*).

Schwann was fully aware of the potential great significance of his theory, however flawed as it was by such errors as believing the nucleus to be a purely transitory feature and that a cell might form by spontaneous aggregation of parts within an organic liquid, as it was a key for the undertaking of a systematic study of tissues. Indeed Schwann did propose a new classification of tissues, different from that proposed by Bichat and based on the supposed relationships of the cells among themselves. Quite obviously Schwann's classification was soon subject to continuous changes in order to cope with the flood of new evidence which began by the '40th of the century, as soon as adequate methods for the preparation and staining of slides became available.

As for the character of the two authors and friends they were quite different: while Schwann was ever careful to quote both the evidence and the hypotheses of previous authors, Schleiden completely omits any mention of them in any of his publications.

The cell theory was enthusiastically welcome everywhere and within some thirty years, an image of the cell structure was completed which lasted without major amendments until the advent of the electron microscope. It is also clear that Schleiden's contributions to the development of the theory are comparatively minor ones, as his ideas were not much more advanced that those that, as far as plants were concerned, had already been debated for years.

We may just add that already by 1844 Kölliker, by his studies on embryonic development in Cephalopods, was able to prove the reproduction by division of cells, though, also because he was not aware of Rusconi's work (Rusconi, not thinking in terms of cells could not give a correct interpretation of his precise observations), he did not entirely rule out the possibility of spontaneous generation of cells. So it was Remak, in 1852, who finally established that all cells were the result of the division of a previous cell and that the division itself was preceded by the division of the nucleus.

Thus the cell theory, while giving new contents to the concept of tissues, had a great influence in unifying the concept of living beings, and, by, in some ways, reviving the old idea of 'living monads', offered new grounds for the debates between vitalists and mechanists, who were both able to recruit the new evidence and the new theories in the service of their opposed basic theories.

In addition to the just related results, the cellular theory soon allowed to understand correctly many basic issues in embryology as, by identifying the egg with a cell, allowed for a clear interpretation of the recent discoveries by von Baers and the other embryologists.

Nevertheless, as the middle years of the 19th century were dominated by mechanists, it was not used in order to interpreter the basic facts of reproduction.

A few more names must be quoted before we close this section.

The first is the already mentioned Giovan Battista Amici: he made a number of important new observation, such as the confirmation of plasmatic currents in the cells of aquatic plants (1815), which had been seen by Bonaventura Corti (1729-1813) in 1774, and, most important, the discovery of the pollen tubule, which, growing out of the grain of pollen represent the male sporophyte and which, reaching to the ovule, allows the spermatic nucleus to reach and fertilise the ovum. This was a capital discovery, that neither Amici nor anyone else, at the time, was able to explain.

Equally important in the history of histology were Johannes E. Purkinje (more correctly Jan Evangelista Purkyne) (1787-1869) and Jacob Henle (1809-1885).

Purkinje was born in Lobkowitz in Bohemia and graduated in Prag. His thesis on sight (vision) got him the sympathy of Goethe, who later procured him an appointment as professor of physiology in Breslau (1823), Thence he returned to Prag (1850), but, already 63, by that time he had abandoned active research. Purkinje was basically an observer and an accurate describer. His main contributions concern the cells of the nervous tissue: he described the cilindraxis of neurons, gave accurate description of the histology of the cerebellum etc., and described some important details in the structure of the epithelium of the skin. He also made some good observations in embryology and physiology, the most remarkable being the ciliary movement in some vertebrate epithelia (1841), a discovery that was independently repeated by von Siebold in 1861.

Jacob Henle (1809-1884) was born of a well-to-do bourgeois family in Fürth and, as his scientific activities developed without break from 1830 onwards, many of them fell well within the temporal boundaries of the next chapter. However, as many of his main contributions were published before 1860, they will be dealt with here.

Henle is usually listed as the best pupil of Müller, whom he met for the first time while a student in Bonn. It was during the same years that he entered a liberal student's association. From Bonn Henle went to Heidelberg, where he graduated as a M.D. He met again with Müller on the occasion of a trip to Paris and thence in Berlin, where Henle had gone to pass his 'state examinations'. There he remained with Müller and decided for a scientific career. His first attempt to qualify as a 'Privatdozent' was blocked by the authorities, because of his past as a liberal student, and, to remove the obstruction was necessary the intervention of old Baron Von Humbolt. In Berlin Henle remained but two years, but these were extremely fruitful. Then he went first to the University of Zürich, from there he moved to Heidelberg and finally to Göttingen, where he remained until his death.

Henle was, indeed essentially a pathologist, but all his activities as a pathologist are so strictly interwoven with his researches as an anatomist and a theoretical biologist that he deserves a great place also as an all-round biologist.

As a morphologists he must be remembered as one of the main histologists and microscopic anatomists of the century. Not only he introduced a number of technical improvements, but his systematic investigations on the structure of different organs made a lasting contribution both to the understanding of their structure and functioning; thus one may just remember his studies on the structure of the kidney.

In 1840 Henle published a basic study: *Von den Miasmen und Contagien und von den Miasmatisch-contagiösen Krankheiten* (= *On myasms and contagions and on the myasmatic-contagious diseases*) which is both a keystone in the history of medicine and in that of biology, as Henle, after a careful analysis of all known evidence and especially of that resulting fron Schwann's studies on fermentations and those of Bassi on the pahtogenesis and epidemiology of the 'mal del calcino' (= muscardine) of silkworms, concluded: "The materials which case contagions are not only organic, buy alive and have their own life which, in relation to the sick body, is a parasitic one". And he was indeed able to show the occurrence of many types of micro-organisms in many pathologic products.

Microbiology

We thus come to the developments of microbiology. Nowadays we think of microbiology as being basically the study of procariotic organisms and, possibly of Viruses. However, at the beginning of the 19th century technical problems with microscopes made microbiology largely the study of 'infusorians' *sensu lato.*

Truly bacteria had been sporadically been observed since Leuvenhoeck, but the inadequacy of microscopes prevented any systematic research.

Thus, in the first years of the 19th century, while several authors made contributions on the distribution and morphology of such protozoans and protophyta that have a mineralized skeleton (especially on Foraminiferans and Radiolarians), the most important contributions were by Félix Dujardin (1801-1862) and Christian Gottfried Ehrenberg (1795-1876).

Dujardin's main contributions were an adequate description and interpretation of the Amoebae (which had been seen and figured almost a century before by Roesel von Rosenhof) and to have stressed how all protozoans must have a basically uniform inner structure, that he called 'sarcode', which corresponds with what Purkinje called 'protoplasm' and that is, by and large, what we now call cytoplasm.

Ehrenberg, who was born in Leipzig, but who mainly worked in Berlin, where he also collaborated with von Humboldt, laid the first foundations of a systematic of Protozoans and, given the times, gave excellent descriptions of several of them. However, as he had seen something of some protozonan's organelles and had wrongly interpreted some results of his *in vivo* staining, on one side he correctly interpreted protozoans as functionally complete organisms, on the other side he claimed to be able to see in their body the different apparatuses: digestive, nervous, reproductive (which he identified with the nucleus). As his book *Die Infusiontierchen als volkommene Organismen* (*The little animals of the infusions as complete organisms*) was published in 1838, the same year as the cell theory, he may be partly excused. Moreover he had the merit to have recognised that many organisms that had been grouped, because of their size, with the protozoans, did not belong there.

Parallel with the study of microscopic organism was revived the idea that at least some diseases might be due to the infection by parasitic micro-organisms, an idea which, as we saw, had its main advocate in Henle. The first to revive Fracastoro's theory of the *contagium vivum* were Isaac-Bénédict Prévost, who, in 1807 proved that the 'bunt', or 'smut' of wheat was caused by a microscopic fungus, that he succeeded to grow, and Agostino Bassi (1773-1856). Bassi was born at Mairago, near

Lodi, and graduated in laws in Pavia. When the French under Napoleon invaded the duchy of Milan, he got involved in political and administrative commitments, but health problems forced him to forfeit them. He thus retired to an estate of his and tried to improve it. As he was a good amateur naturalist, he made a number of practical agronomic and zoo-technical researches. Among them, as at the time the production of silk was of great significance for the budget of Lombard farms and it was frequently ruined by epidemics of what was called 'Muscardine', and was locally called 'mal del calcino', he began its study in 1807 and continued his researches for twenty years. When he finished them, he tried first to keep the results to himself and thus to square his budget, but as he failed to make his discoveries a paying proposition, he published them in 1835. Curiously, much later, Pasteur, who completely ignored the studies of Bassi, investigated a closely related problem and got the same results as Bassi. Bassi had, indeed, concluded that the disease was caused by a microscopic organism (in fact it is a mycosis), which spores may disperse through air and which Bassi succeeded in growing *in vitro*. Bassi's work won considerable approvals, but was soon entirely forgot. We have already mentioned how Henle, instead, made use of it.

Reproduction

We have mentioned how, while other fields of biology were fast evolving, the study of reproduction lagged. To a considerable extent this was the result of the progress of chemistry and of the chemists invasion of biology. This was not novel, when we remember how the 17th century alchemists battled with mechanists and surviving Galenists, and did indeed make significant contributions, and how, again, chemists like Lavoisier, paved the way to basic progress in physiology. However a number of chemists promptly tried to explain everything in terms of their still rather rudimentary science. Thus they proposed fantastic chemical models also for reproduction and even for the transmission of hereditary characters, advocating molecular or ionic turbulence, contact and catalysis etc. As an example we may quote Theodor Ludwig Wilhelm Bischoff (1807-1882), a pupil of J. Müller and Nägeli and a leading chemist of the times, but who, as a biologist, studied ovulation, fertilisation, embryology and, later in his life, brain structure and anthropology. He, in 1847, wrote: "the seminal fluid acts by contact by means of a catalytic force. That is that it is a special form of matter characterised by an intrinsic movement which is transferred to the egg … and there it determines the same or a similar organisation of the atoms.", that is precisely Aristotle's 'Eidos' clad into empty meaning chemical rags.

Anyway such was the influence of chemists, that excellent and crucial observations went unheeded.

In 1824 the physiologist Jean Louis Prévost (1790-1850) and Jean Baptiste-André

Dumas (1800-1884), by correctly repeating the failed experiment of Spallanzani to fit trousers on the frogs, proved that fertilisation was due only to the sperms. Dumas, who was basically a chemist, later made important studies on the animal and vegetal metabolism, which infuriated von Liebig.

The presence of the sperm inside the egg shortly after fertilisation was proved in the rabbit by M. Barry in 1843 and by George Newport (1803-1854) in the frog in 1851 (Newport was a physiologist and a surgeon, but also a notable entomologist and made important studies on the physiology of invertebrates). The fusion of the male and female pronuclei was described by Warnek in 1850, and completely ignored, so that it was independently rediscovered by Bütschli in 1874!

The whole process of fertilisation and even the penetration of the sperm into the egg of a fresh-water alga were described by Nathaniel Pringsheim (1823-1894). He was born in Upper Silesia, was a student of plant physiology, briefly a professor in Jena, and later gave private courses in Berlin). In 1856, he maintained that the fertilisation consisted in the fusion of two cells! Yet also these observations were practically ignored, so that the real progress in the understanding of fertilisation will be dealt with in the next chapter.

Physiology

We have repeatedly seen how, since antiquity, inquires were done into the chemical phenomena occurring in organisms. Such 'chemistry' as existed was, however too rudimentary to allow for any really significant progress. The new chemical theories evolved in the late 18th and early 19th century finally allowed scholars to tackle the problems of the chemical structures of organisms and of the processes of life.

The first to be mentioned in this connection is Jöns Jakob Berzelius (1779-1848) a Swede from a rather poor family, who became professor at the Karolinska Institut of Stockholm and obtained a number of academic honours. Among his many contributions to chemistry, for us are significant his *Lessons of animal chemistry* (1806-1808), which provided the first information as to the chemical composition of several animal structures. Being a very clear minded scholar, Berzelius was well aware that such preliminary investigations as his did not allow for any general, sweeping, conclusion. However the general impression from his writings is that his general attitude was a strong deistic background tinged by the French encyclopaedist's materialism.

During the earliest phases of chemical investigations, the scholars were struck by the fact that such compounds that they could identify in the living bodies, were usually quite different from those found in the inorganic world. Thus the announcement in 1828 that a pupil of Berzelius, Friedrich Wöhler (1800-1882), a German from Frankfurt am Mein, had synthesised in laboratory urea was unanimously considered a momentous advance. Wöhler was an extremely gifted and versatile chemist and has to his credit the discovery of the elements Aluminium and Beryllium; he made as well some pioneer work on the metabolism of several substances.

The synthesis of an organic compound, which was soon followed by others, was a serious set back for all such theorists that had postulated that organic syntheses had to be implemented by a *vis vitalis*, a mysterious 'vital force'.

Among the chemists that dealt with the chemistry of organisms during the first half of 1800, the first place goes undoubtedly to the German Justus von Liebig (1803-1873), born in Darmstatd from a merchant family. He studied in Paris with Gay-Lussac and was later a professor in Heidelberg, Giessen and, finally in München.

We owe to Liebig such basic concepts as that of 'limiting factor' in ecology, that is that factor (be it the availability in the environment of an element or of a compound or a physical or biological factor) whose amount, by itself, determines the maximum syntheses that the members of a given species in a given environment may produce, even when all other materials needed for their life are available in the greatest abundance. Such limiting factors, therefore, determine the maximum possible biological success, in terms of growth, survival and reproductive capacity, that a species has in a given environment.

Also to Liebig we owe a clear concept of metabolism. Anyway the overall significance of his work is well stated in the title of his book published in 1842: *Die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie* (*Organic chemistry and its use in physiology and pathology*).

The philosophical-scientific evolution of Liebig is complex and we cannot adequately discuss it here. Basically he was on one side linked with Schelling's theo-teleologism, on the other he was an extremist chemist: to him every phenomenon must be linked with a chemical reaction and this depends on the degree of agitation of either atoms or ions (that he had discovered). Thus, late in his life, he obstinately denied any significance to the fermentation phenomena that Pasteur was studying. Pasteur himself thus says how, after some of his basic studies on fermentations, he went to visit Liebig in his laboratory: "The tall, very old man, wearing a long dress, did kindly receive me — but when Pasteur tried to talk on fermentations — without loosing any of his kindness, he refused any discussion, saying that he felt slightly ill." Finally, in a period when it was fashionable to qualify oneself as a Baconian inductivist, he (1863) flatly refused inductivism!

While the methods of modern chemistry were thus applied to biology, other experiments were going on more traditional lines.

We must thus remember François Magendie (1785-1855), born in Bordeaux, his father being a surgeon, and who was a professor at the Collège de France. Magendie was an excellent experimenter on animals and did much to develop techniques which had been first hinted by Galenus. In a way he is a follower of Spallanzani. He was an inveterate polemist and this may even have helped him to attract a number of pupils, but he had the bad habit to dismiss other people's merit. Thus he was entirely wrong in his polemics with Charles Bell (1774-1842), who, indeed, had been the first to show the different function of the dorsal and ventral roots of the spinal nerves of Vertebrates: the ventral ones carrying only motor fibres and the dorsal one only sensory fibres (though this is entirely true only for Tetrapods and several fishes). In spite of his bad character, yet Magendie was a dedicated physician, who did not hesitate to risk his own life when tending patients of all sorts during epidemics.

Perhaps the greatest among the physiologists that we are considering was Johannes Peter Müller (1801-1858), whom we have repeatedly mentioned, and this because of the variety of problems that he studied, the significance of his results and, last but not least, the large number of first class pupils that he trained: Schwann, Henle, Remak, Kölliker, Virchow, Du Bois Raymond, Helmoltz, etc. Müller came from a well-to-do artisan family. He studied in Berlin and in 1830 he became a professor in Bonn, but soon was called back in Berlin (by the way the reader is invited to check how many of the brilliant scholars of this age got their chairs when quite young). His main interests were with fishes and other marine organisms and he often visited both the Baltic and Mediterranean shores.

Müller was, to begin with, closely linked with the Berlin teachings of Schelling, the most orthodox variety of 'Naturphilosophie', and was not only interested in philosophy, but also in mystics (in 1826 he even published a book on apparitions). Later he moved away from the positions of the more orthodox 'philosophers of Nature', but he still remained a strict 'vitalist' and always maintained that the structure of natural forms was not a product of chance, but of the creative spirit of God.

Among the many contributions by Müller, we shall pick a few significant examples.

He produced an excellent treatise on the systematic of fishes (*Natürliche System der Fische*, 1844), his systematic closely approaching the prevalent modern ones (apart from some cladistic proposals) and we have already mentioned his contributions to the morphology of Cyclostomes and of various invertebrates.

His is the, so called, 'law of the specificity of reaction', as he proved that each tissue or organ, when stimulated by any kind of stimulus, either does not react at all or its reaction is independent of the quality of the stimulus and is, instead that typical for which the organ is specialised. Thus a gland can only secrete or a muscle contract. Müller, indeed, summarised his thought in his *Handbuch der physiologie der Mensches* (1834-1840) ('A handbook of the physiology of Man'), which is a typical German 'Handbuch', as it takes several volumes and, rather than a physiology of Man is a complete and integrated treatise of comparative anatomy and physiology! When one consider both that this monumental work was produced by a single man and its date, there is no doubt that Müller must be listed amongst the greats of biology, even allowing that his rigidly vitalistic ideas led him to advocate some theses that, by the time, were already obsolete. Thus, for instance, and in the teeth of Spallanzani's experiments, he maintained that infusorians might appear by spontaneous generation or, considering that the nervous impulses were due to the 'vital force', he stated that its speed of transmission could not be measured (it was measured shortly afterwards by von Helmoltz, who was his own pupil!). Finally we my recall how we have repeatedly quoted Müller for having rediscovered the placentation of some sharks, etc.

Descriptive zoology

The synthesis between comparative anatomy and systematic zoology after the pattern established by Cuvier developed through the pre-Darwinian '800 with excellent results, while the improved quality of microscopes and the development of microscopic techniques allowed for much better investigations on the structure of both animals and plants and on the moot assemblage of microscopic organisms.

Obviously, moreover, the constant development of explorations supplied a constant flood of new evidence that had to be integrated with the previous one.

We shall now mention some of the more typical representatives of this branch of research.

Among the direct pupils of Cuvier, the first who deserves mention is Henry Ducrotay de Blainville (1777-1850), born in Normandy from an ancient noble family. He began his activities as a zoologists with Cuvier rather late, when he was about thirty, after an adventurous youth: having interrupted his studies because of the revolution, he later studied music and arts and, meantime squandered most of his fortune. A passionate character, suddenly abandoned it all, and as furiously began to study medicine and zoology. Full of character, he rather soon had serious differences with his master (De Blainville's scathing judgement of Cuvier's character was published only after his death), but his ties with the Muséum were not cut. He thus first got an appointment as professor in the faculty of sciences of the Sorbonne, and in 1830 was finally appointed at the Muséum, where later succeeded in becoming the successor of Cuvier himself. An absolute social reactionary, and a practising Catholic, nevertheless, since 1813, became also a friend of the pioneer socialist count Saint Simon and supported a mixture of monarchic absolutism, socialism and piety. De Bainville's contributions, both in theoretical biology and in systematics are varied. Broadly speaking, he is a follower of Cuvier both in the basic ideas and in his methods. As his master he was a faithful follower of Bichat as far as the fine structure of organisms was concerned and followed his classification of tissues. However he did not follow Cuvier in systematics: he was rather close to the ideas of Geoffroy in assuming that, by the Divine plan, species might vary, but not truly evolve and, reviving the idea of a *scala naturae*, he assumed that the discontinuities among the 'embranchements' that Cuvier had stressed, were due to the extinction of intermediate organisms. On the other side he was a strict creationist, who went even further than St. Augustine, as he maintained that all species had been created *in actu* and simultanously by God.

Another collaborator of Cuvier who had considerable importance was André Marie Constant Duméril (1774-1860), a good herpetologist, apart from his contribution as editor of the *Leçons d'anatomie comparée* of his master.

Pierre André Latreille (1762-1833), was a pupil and a good friend of Lamarck and actually was a few years senior to Cuvier. He took the place of Lamarck, when this last retired. He was an entomologist who actually owed his life to his passion for insects: he, during the 'Terreur', he had been included in a batch of people due for execution, but was noted while, calmly waiting in his cell, was contemplating a beetle, questioned he explained that it was a rare species. Such detachment and scientific knowledge, was luckily reported, made the due impression and both Latreille and the beetle (first the beetle, which, by a curious chance, is reported to have been a burying beetle) were rescued at the very last moment. Latreille either described or completed the descriptions of a good many species. Moreover his ideas as to which characters were significant and how they should be studied had a lasting effect on systematic entomology. Anyway Latreille was lucky: as the 'rules of nomenclature' forcibly link the name of the describer to the name of the species described, so that the two should be always quoted together. the result is that that scholars who, by working in a big institution constantly supplied with new materials, described and named very many species, even if simply painstaking workers who contributed few new ideas, became much more familiar than possibly better ones that worked on morphology, physiology etc, all branches where results become soon obsolete, but had not so many 'new species' to their credit.

Also worth mentioning are Victor Audouin (1798-1840) who was also in the staff of the Muséum. He made the first real systematic studies on the anatomy of Arthropods. Also deserving a mention is Antoine Dugès (1797-1838), who was rather close to the ideas of Geoffroy and who studied the Comparative anatomy of both vertebrates and invertebrates.

As we said Cuvier had both an overwhelming personality and a consummate political ability, his pupils thus learnt both commitment and exactitude in their work, but they were, at the same time, selected by their devotion to the principles of the big boss, who compensated their devotion by fitting them in all the French chairs available, and, as scientific personalities tend to reproduce themselves, he stamped the French zoology with a sort of 'pattern', which lasted almost over a century: a model in many ways, but also a paradigm of backwardness as far as evolutionary studies were concerned, when these became the leading field in biology.

There are other really notable zoologists that continued Cuvier's pattern: Milne Edwards, De Quatrefages, Lacaze-Duthiers, but as they mainly operated in the times of the 'Darwinian revolution' they shall be discussed in the next chapter.

Leaving France for Italy, not much can be said: Italian zoologists were few and culturally much linked with France. Worth mentioning are the faunal studies began by Filippo Cavolini (1756-1810) and continued by Stefano Delle Chiaie (Teano, 1794-

Naples, 1860) between 1830 and 1844, who both described animals from the then extremely rich fauna of the Gulf of Naples.

As we have seen discussing other aspects of biology, the influx of the Parisian school was notable also on the German biologists. Two of them are especially notable for the improvements that they introduced on Cuvier's systematics: von Siebold and Leuckart.

Theodor von Siebold (1804-1886) was born in Würzburg, the son of a professor of medicine and obstetrics; for some years he practised medicine, while privately doing research (he regularly received some advice from Von Baer), later he held chairs in various universities, finally settling as professor of Zoology and comparative anatomy in München. His main contribution to systematic zoology was given within the framework of a basic treatise in Comparative anatomy, which he prepared in co-operation with Hermann Friederich Stannius, from Hamburg (1808-1883), who was professor in Rostock (but who spent his last twenty years in a lunatic's asylum). In their treatise, while Stannius dealt with Vertebrates, von Siebold covered the invertebrates. The 'Stannius and Siebold' is the first and is a perfect example of that series of German treatises, each one larger than the preceding one, that are a true mine of information always accompanied by a most serious critical analysis.

In his treatise von Siebold, as it was high time, correctly formalised a division of some of Cuvier's types. Cuvier's 'Articulata' were divided between the Arthropoda, characterised by a comparatively rigid exoskeleton and with a metamery at least partly eteronomous (that is with some segments specialised with respect to others) and legs formed by distinct articles, and 'Worms', to whom von Siebold suggested to add a number of animals that either had been discovered since Cuvier's original proposals or that Cuvier had included in his 'Radiata'. This was at the same time a step forwards, as the Arthropods were, at last, clearly defined, and one backwards, by lumping with the Annelids a number of heterogeneous beings. Von Siebold fitted the remaining 'Radiata' within the ancient and now revived 'Zoophyta' (Animals-plants), but he ruled out of the Radiata all the unicellular animals, for which he established the 'Protozoa', again a step backwards and one forwards.

Speaking of Protozoans, though the actual researches belong into the second half of the century, it is worth mentioning here that von Siebold amended the worst errors of Ehrenberg, recognised them as unicellular animals and, therefore denied Ehrenberg's interpretation of their organelles, and, also on some evidence gathered by Nägeli, commented on the fact that many unicellular organisms were actually photosynthetic.

Development and the discovery of life cycles

By the beginning of the 19th century the metamorphoses of Insects and of Amphibians where well known, at least so far as they can be studied by the naked eye, and the reproduction of animals which go through complex cycles, as well as the real facts of the reproduction of plants, began to be appreciated precisely at this time. As many such animals are parasites, we may preface this section by mentioning Carl Asmund Rudolphi (1775-1832), born in Stockholm, but professor of Anatomy in Berlin, who dedicated himself to the study of parasites, and especially to the internal ones. In his *Entozoorum synopsis* (*A synopsis of internal animals*) he described a number of previously unknown species, but, against the opinion of his predecessor Pallas and without even considering the classical views of Vallisnieri, he still admitted their spontaneous generation!

Then this still confused field was tackled by the already mentioned Th. von Siebold. He had the very great merit to prove that in many cases, animals which were morphologically entirely different, and that had been thought to belong to different species, were in fact different developmental phases of the complex cycle of a single species, and that they were often the hosts of different kinds of animals.

For this purpose it was crucial his demonstration that the 'cerebral coenurus' that in the sheep causes the disease known as 'staggers', is a phase in the cycle of a small tape-worm which final (rather than adult) stage lives in the gut of dogs and other carnivores. Likewise von Siebold proved that the *Echinococcus*, which in herbivores, including man, develops into enormous cysts, often located in the liver, is in its final stage, again, a minute tape-worm which may live in great numbers in the gut of carnivores like dogs, cats, wolves etc. without causing any trouble. Von Siebold also discovered the cystic stage of other tape-worms, which final hosts are carnivores.

The discovery of the cycles of several tape-worms, as we shall see, was contemporary with the discovery of the cycles in some marine animals, and paved the way for a large number of studies both theoretical and, because of its obvious practical importance, of even more focused on the study of parasites of man and the domestic animals. This practical interests had as its counterpart the emphasis in textbooks on Cestodes and Trematodes parasitic on Man and a few Mammals and even today students have little if any idea of the evolutionary complexities of these groups, which often are parasites of invertebrates, of marine animals etc. or of related and theoretically extremely interesting animals such as Monogeneans, etc.

As we said the discovery of the complex life cycles of some marine animals occurred in the same years that von Siebold was studying internal parasites.

Adalbert von Chamisso (1785-1838) is a colourful character. He was from a French noble family who had settled in Germany to escape the 'Terror' and he was really Count Louis-Charles-Adélaide de Chamisso. Von Chamisso was a fully trained naturalist and, basically, a botanist, who regarded himself as an amateur poet and novelist, while he is nowadays mainly remembered as the author of *The story of Peter Schlemhil, the man who sold his shade*, a short novel which is still considered a masterpiece of German literature, and for his Lieders which music was composed by Schumann! Between 1815 and 1818 he joined into the circumnavigation of the Russian ship *Rjurik* and described the alternate generation of the Salps (planctonic tunicates). He saw that solitary individuals of this group reproduced asexually by budding and thus originated chains of individuals, these, by sexual reproduction originated new solitary individuals, which, in turn reproduced by budding. Obviously von Chamisso could not study the intimate mechanisms of the cycle and simply described the alternation of solitary and chained individuals.

Such first discovery was soon followed by many more and a special merit in the development of these researches goes to the Norwegian Michael Sars (1805-1869) and the Dane Johannes Japetus Steenstrup (1813-1897). Michael Sars was the first who made a systematic study of the fauna of the Norvegian coastal waters and, between 1838 and 1846, was able to put the problem of alternating generations in a clear perspective, while Steenstrup, who was active in many fields (zoology, palaeontology, ethnology), published his basic work in 1842.

Important contributions were also made by the Swede Sven Lovén (1809-1895), who, in the second half of the century, created the biological laboratory of Kristineberg, and who published a number of papers on the development cycles and on larval stages of many marine animals.

Thus it was found that in many animals from different taxonomic groups there is an alternation of generations: one or more reproduce either by parthenogenesis (reproduction by unfertilised eggs) or agamically (form somatic cells or buds that do not correspond with eggs) followed by usually one generation which reproduced by normal fertilisation. But for the understanding of the evolutionary significance of these, apparently odd, mechanisms we had to wait for the development of genetics.

Alternating generations was also found to be the basic mechanism of reproduction of terrestrial or secondarily aquatic plants.

Here most of the basic work was done by an amateur (who was later appointed as professor of botany in Tübingen): Wilhelm Hofmeister (1824-1877). Born in Leipzig, he did not make regular studies and, having inherited his father's shop was, for several years, an editor and a bookseller. In spite of being extremely myopic and of his obstinate refusal to wear glasses, and, perhaps because of it, he, being a passionate botanist like his father, concentrated on microscopic investigations. In 1851 he published a basic contribution: *Vergleichende Untersuchungen der Keimung, Enfaltung und Fruchtbildung hoherer Kryptogamen (Moose, Ferrn, Equisetaceen, Rhizocarpeen und Lycopodiaceen) und Semenbildung der Coniferen)* (*Comparative researches on the generation, development and fructification of higher Crypogamous plants (Mosses, Ferns, Horsetails, Rhizocarps, and Lycopodes) and the formation of the seed of Conifers*). He identified the fern's prothallus, a tiny, ephemeral, plant millimetres across, which develops from the fern's spores, and he identified it as the sexuate plant by identifying on it the archegons and spermatogones, where the eggs and sperms are formed. The eggs, once fertilised, produce the 'true fern' which has no sex, but produces spores from which develop the new prothalli.

Later Hofmeister undertook the study of the egg of the Fanerogamous plants and was able to identify the stages corresponding with the development of ferns: the sexual generation develops into a minute organism made of but a few cells, that briefly lives as a parasite.

Hofmeister worked almost to the end of his life, but his later works, though significant, are not as good and he became gradually impervious to even well founded criticisms.

Descriptive botany

After Antoine Laurent de Jussieu (1748-1836) and his *Genera Plantarum*, published in 1789, the greatest figure of systematic botany during the first half of the '800th was Augustin Pyrame De Candolle, born in Geneva in 1778. He belonged to the protestant branch of a French noble family, who had emigrated to Geneva by the end of the 16th century to avoid persecution. His father was a banker and a local politician, so Augustine had the opportunity to be introduced to Bonnet and De Saussure. He was for some time in Paris, where he worked with Lamarck and Latreille and also felt the influence of Geoffroy and Cuvier. First appointed as professor of botany in Montpellier (1808), moved in 1816 to Geneve, where he was also an active politician and a phylantropist. He died in 1841. De Candolle was at a time a systematist, a morphologist and a physiologist, but his main work was in systematics. In his *Théorie élémentaire de la botanique* (1813) he set forth what he held as essential criteria for his concepts on living organisms, especially plants. Basically De Candolle, who was a fixist, was entirely bound to the Linnean tradition. His definition of species assumes that all its individuals derive from a single one identical with the present ones. However, De Candolle took full advantage for his 'natural classification' of the advances allowed by the improvement of microscopes and made a complete distinction between vascular and 'cellular plant'. Moreover in order to improve on formal classification he suggested some precise definitions for supraspecific taxa, such as genus, family, etc.

That, as a botanist, he was not a tranformist, as proto-evolutionists were then called, is understandable. To him all observable variations were due either to the influence of local conditions or to hybridisation. Now it is just in plants that it is easy to notice considerable variation in morphology under different local conditions even in a single individual, such as they happen in *Sagittaria* and in some *Ranunculus*, which grow in water and whose submerged leaves are completely different from those on the aerial part of the plant, while hybrids occur much more commonly in plants than in animals.

In times when people, as an average, died much earlier than now, De Candolle was so sanguine as to begin a gigantic work: *Prodromus systematis naturalis regni vegetabilis*, which should have included the description of all plants known. Obviously he died before finishing it and his program was completed by a group of good scholars that included his son, so that, in the end botanists had a monumental treatise in 20 volumes, published between 1825 and 1873, and which was hailed as a most useful one.

Among the several scholars that worked on the improvement of the systematic arrangement of known plants, a prominent one is usually considered to be Stephen Ladislaus Endlicher (1805-1849), professor in Vienna and the author of a *Genera plantarum*.

Again to be mentioned, after the pioneering work on cryptogamous by the Hungarian (actually Transylvanian) Johan Hedwig (1739-1799), is the work of the Swede Carl Adolph Agardh (1785-1859) on Algae. Agardh was professor in Lund and later a Lutheran Bishop. He was a friend of Schelling and may be considered as a naturphilosopher and was also and active politician.

Elias Fries (1794-1878), also a Swede, was interested in natural history since childhood, being thus encouraged by his father, a parson. He was later a pupil of Agardh senior and in youth a keen adept of Naturphilosophie. His academic career was rather slow and he was appointed a professor only in 1835. Though he never abandoned completely the principles of Naturphilosophie, he gradually rejected all its more fanciful tendencies. He was an early evolutionist, although he maintained that each species of plant had been originally created with a very primitive structure, which gradually developed into the present one. In his late years he appreciated Darwin's work, though not subscribing to the function of selection. Fries established the foundations of the systematic of mushrooms.

Exploration, Biogeography and the study of the sea

We have seen how, during the 18th century the rivalry between England and France stimulated the exploration of the high seas and of new lands and how the minor powers tried to cut themselves some shares, while the Russian empire had launched into the gigantic task of expanding through Asia, an expansion obviously paved by appropriate explorations.

During the 19th century explorations into the interior of continents were mainly promoted either by religious organisations seeking natives to convert and, incidentally, new species, or by geographical societies, sometimes sponsored by governments, whose interest was equally divided between the progress of science and the search for new commercial outlets or sources of raw materials. Maritime exploration were usually the official enterprises of governments.

On the British side, Cook's expeditions, which were the most spectacular, also on account of their results, were followed, for the period we are discussing by that of Captain Matthew Flinders (1774-1814), who between 1801 and 1805 explored the coasts of Australia and Tasmania. The naturalist in charge with Flinders was Robert Brown (1773-1858), a botanist that we have already mentioned as the first accurate describer of 'Brownian motion' (the continuous motion of microscopic particles floating in a liquid), that had been incompletely described by Spallanzani. He made impressive collections that he studied himself very accurately. He was also the first to precisely identify the nucleus as a constant character in plant cells. He also studied fertilisation and, as a morphologists, he was close to Goethe's ideas.

It belongs to this period, but we shall deal better with it in the next chapter, the five-years circumnavigation of the *Beagle*, which sailed in 1832 under captain Fitz-Roy, himself a good amateur naturalist, and who took aboard young Charles Darwin!

Again quite important was the expedition in the Antarctic of the ships *Erebus* and *Terror* led by Sir James Clark Ross and with whom sailed Joseph Dalton Hooker *(*1817-1911*)*. He was the son of Sir William Jackson Hooker, a rich gentleman and a distinguished botanist, who became director of Kew Gardens, an appointment later given to Joseph, so that the basic features of these famous botanical gardens are largely due to their work. Joseph Dalton Hooker, after his long cruise in the Southern Seas, explored the Himalayas and other regions, so that he may well rank among the most distinguished explorers of his age as well as an eminent systematist and plant geographer. He was also a close friend of Darwin, who constantly informed him of the progress of his studies, so that Hooker could well say 'I have known all of Darwin's ideas fourteen years before they were published'.

In 1800, on Napoleon's orders, the Institut de Fance organised an expedition to the South seas by three ships, *Geographie, Naturaliste* and *Casuarina*, which naturalists were François Peron (1775-1810) and Charles-Alexandre Lésueur (1778-1846), who, apart from the collections made in the islands around Australia, mainly collected animals from the high seas, mostly new to science. Other expeditions followed: in 1817-20 by the ships *Uranie* and *Physicienne* who embarked as naturalists Jean René Constant Quoy (1790-1879) and Joseph-Paul Gaimard (1796-1858), that of the *Coquille* with René Primevère Lesson, and the famous two of the *Astrolabe* (1826-29) led by Dumont d'Urville, the first to Polynesia (1826-29) and the second in the Antarctic seas (1837-40), again with Quoy and Gaimard.

Nor were the Russians entirely absent, as we have seen mentioning the voyage of the *Rjurik*.

Quite apart from the many geographical discoveries, all these expeditions discovered a grat number of new species: while the oceans gave a steady supply of surprises, also the terrestrial and freshwater faunas collected by the landing parties and by the first settlers which followed, supplied quite sensational and puzzling discoveries, such as Dipnoans, the Platypus, etc.

Moreover the wealth of additions being assembled prompted the development of a new branch of biology: Biogeography, the study of the distribution of animals and plants on earth and in the seas.

Although the foundations of modern biogeography were laid by A.R. Wallace and are strictly linked with evolutionary theories, and, as such, they will be dealt with in the next chapter, some very important ground-breaking work had already been done, especially by Baron von Humbolt, and must be mentioned here.

Obviously regional faunas and floras had been described since the beginning of the great explorations and we have quoted several such examples. These, however, usually were mere descriptions and did not pose the problem of the reasons underlying such differences as everyone noticed between the faunas and floras of the different countries. Perhaps the first to attempt a general explanation was, as we saw, the Rev. Father Kircher S.J. Some general hypotheses, and, again we have mentioned them, may be found both in Linnaeus and Buffon; people like Galeazzi had commented on the affinities of the Pliocene Molluscs of the Appennines with those from the Indian Ocean rather than with the Mediterranean species, but these were occasional hints. The first that seriously tackled the problem was Baron Alexander von Humbolt, and his story is a good example of how a man of merit can make much of unforeseen circumstances.

Friedrich Heinrich Alexander von Humboldt (1769-1859) was born near Berlin from a rich and important family (his father was a high officer in the Prussian court, his mother was from one of the Huguenot noble families that had emigrated to escape religious persecution and whose progenies we met so often in these chapters) and studied in Göttingen and Freiburg. He soon made a number of diverse scientific investigations and at 23 he was already well known, so that he was appointed director of the Fichtelgebirge's mines. A big inheritance made him economically independent and paid for his long voyages, but it was sheer chance that brought him to America. Young Humboldt both because of the influence of his tutor Forster (who had been with Cook) and of his senior brother, a famous philologist, had become greatly interested in the natural history of India. His first idea was to join in an expedition proposed by de Bougainville to the Southerns Seas and India and that should have been led by Baudin. When the expedition was postponed because of political and budgetary problems, von Humboldt, who had had, thanks to his political connections, the opportunity to meet and become a friend with the French general Desaix, learned of Napoleon' secret plans for the Egyptian expedition (which in Napoleon's plans was preliminary to an attack on India) (1798). Thus von Humboldt decided to join the expedition. However he was deceived as to the date of sailing of the expedition by the same rumours that Napoleon had circulated to lure away the British squadron blockading Marseille, and so he arrived there just one day too late. Having been left stranded, von Humboldt decided that he might as well go to Spain, and thence reach the expedition travelling overland through North Africa. In Spain, he made some notable observations, but chiefly got for himself in the sympathies of the Royal Court (after all he was born a courtier, and his brother's political influence in Berlin got him the best diplomatic assistance). Thus he conceived the plan to go to

Mexico, from where he could sail to the Philippines, which were both Spanish possessions, and finally from the Philippines, sail to India. Thus, having got all the necessary credentials, in 1799 he sailed for central America, where he was to spend five years. He visited several South and central American regions, but made a special study of Mexico and assembled an immense quantity of evidence and collections of all sorts: geographic, geologic, climatic, meteorological, ethnographic, zoological and, above all, botanical.

In the end he had to give up all hope to get to India and, considering the amount of work necessary to sort out all the evidence that he had assembled, returned to Europe with his collections, thanks to a passage on a neutral United States vessel, and settled in Paris, where, at the Muséum, he found the ideal conditions for his work. Already famous, after Napoleon's downfall, he returned to Berlin, where he was a very successful lecturer and politician. In 1829 he was again able to make a long expedition through Russia and Siberia.

Von Humboldt was a prolific writer and was greatly interested in the current debates on the diffusion of animals, plants and cultures. He put many of his collections to the disposal of other scholars: thus his insects were studied by Latreille, the Amphibians by Georges Cuvier, the fishes by Valenciennes. He himself concentrated on the botanical collections, which he studied in co-operation with Kunth. After his return to Berlin von Humboldt conceived the idea to assemble all that was known of natural history, history and arts in a single global description. He called his work *Cosmos*, thus implying, by this Greek term (in Greek *Cosmos* is the order of the world and opposed to *Chaos*), the intrinsic rationality of the world it aimed to describe. Obviously this gigantic work was never completed, but the first volumes had a great success and were immediately translated into most European languages. Their scientific value is unquestionable and they still make a fascinating reading.

We must bypass the many contributions of von Humboldt to geography and geology (in *Cosmos* he adopted a very advanced classification and sequence for geological strata), our main interest being for his contributions to biology.

He never directly studied evolutionary problems, but he plainly stated his belief in a limited transformism and in Goethe's ideas. His descriptions of the vegetation of the different environments is the first attempt to a rational classification of the different botanical assemblages that we now call 'fitocoenoses' and to establish the relationships between the different fitocoenoses and the edaphic conditions (types of soils), the significance of local conditions in determining the development and biology of plants etc. By his character von Humboldt was not a systematist, though he was a competent one, and, having studied the plants in their own environment, he looked at them by a very different attitude from those museum scholars to whom exotic plants were merely some desiccate specimens and some drawings.

His sensitivity was of an ecological type and, while he fully appreciated the significance of differences among apparently similar plants, yet he underlined the significance of the general aspect of each plant, thus distinguishing a 'type palm', a 'type cactus', a 'type orchid' and so on and discussed their ecological significance.

Von Humboldt, just as many other contemporary scholars paid some consideration to the geographical distribution of animals and plants, but, as the first real systematisation of the subject was the work of Alfred Russel Wallace, this problem will be considered in the next chapter.

Palaeontology and geology

Biogeography, as it aims to determine the reason for the geographical distribution of plants and animals both living and past, is thus closely linked with geological and palaeogeographical evidence and its interpretation. The study of fossils soon became relevant not only for evolutionary studies, and until recently it was the only evidence directly relevant for the reconstruction of phylogenies, but, again until recently, it was almost the only evidence for the chronological correlation of the different strata at different localities.

The attitude of scholars to fossils was, in the early years of the 19th century partly different from now, as they were not yet considered as members of phylogenetic lineages (we saw how it was É. Geoffroy who first discussed this possibility).

We saw how fixist palaeontologists, like G. Cuvier and his collaborators, while admitting the succession of faunas, were led, both by their pre-conceived ideas and by the peculiarities of the geological situations that they were investigating, to assume that the succession was due to catastrophic events that wiped out each fauna, either at a local or at a global scale.

'Catastrophism' had a temporary scientific advantage: it clearly implied that if two layers had some common faunal elements, notwithstanding their possible differences in lithology or their geographical distance, they had to be approximately contemporary. In so far it was thus a bonus, as it allowed for the first great stratigraphical correlations. Not all that is wrong is always useless.

Alexandre Brongniart (1770-1846) had great merits in establishing the main outlines of French stratigraphy, which, until he began to co-operate with Cuvier, was much backward especially with respect to the results of the Italian and English scholars that we mentioned in the previous chapter. However it is fair to say that it had had an excellent start when, in the general framework of all governments attempts to make the best of all available resources, a project hat been launched in 1766 for a general geological map of France, a project entrusted to Jean-Étiènne Guettards, and that greatly benefited of the co-operation of none the less than Lavoisier. Unfortunately the project was wrecked by the outbreak of the French Revolution.

Equally influential was the work of one of Cuvier's pupils, Alcyde D'Orbigny (1802-1850), who was the first to held a chair of palaeontology at the Muséum. He wrote a *Cours de paléontologie et de géologie stratigraphique* which was printed after his death (1851), and where he proposed both a sequence and a nomenclature of the strata which is still partly used. D'Orbigny was an extreme supporter of catastrophism and admitted of 28 geological periods and, while Cuvier had considered the possibility that there had been a unique creation and that extinctions had been local affairs followed by re-population from elsewhere, D'Orbigny assumed that each epoch was brought to end by a universal catastrophe, followed by an entirely new creation. "This — he writes — is a certain, albeit being a fact impossible to understand. We must take it as such and abstain from trying to understand the superhuman mystery that it hides."

As a matter of fact the whole is a curious attitude by convinced believers, such as, for instance, was later for Louis Agassiz, as it implied that God had been from time to time dissatisfied with his work and had destroyed it in order to try to make a better one.

The groundwork for paleobotany was established by another Cuvierian of the Muséum: Adolphe Brongniart (1801-1876), a son of Alexandre, who made a special study of Carboniferous plants. Brongniart was followed by many others. Von Humboldt took all these studies in good account.

Human paleontology was born much later and its first developments were quite difficult.

Johannes Jacob Scheuchzer of Zürich (1672-1733), in 1725, had described a partial fossil skeleton as *Homo diluvii testis* assuming it to be that of a man drowned by the Noachian Flood. It was easy for Cuvier to show that in fact it was the skeleton of a giant salamander, closely similar to the living Japanese giant salamander (and in fact was later renamed *Andrias scheuchzeri*).

Cuvier and such fundamentalist geologists as Elie de Baumont, were so sure of the recent creation of man (after all one could count the generations listed in the Bible, separating Adam from Noah), that they flatly refused the idea that mankind could have been contemporary with faunas that clearly had been extinct for a long time (but not that much, as we now know that the last Mammuths, for instance, died only some 5,000 years ago!).

Moreover human fossils are quite rare, so that the evidence for human antiquity was provided by its stone implements which are extremely common and widespread.

However prehistoric archaeology was just making his early tentative steps and was plagued by numbers of misunderstandings and errors and, anyway, palaeontologists were totally unprepared to use this evidence.

Thus when Boucher de Perthes (1788-1866), thence director of the customs at Abbeville, and an amateur archaeologist, in 1838 found some human bones associated with stone artefacts and with the bones of large fossil mammals and correctly argued that those men must have been contemporary with Mammuths, Cuvier's pupils and especially Elie De Beaumont flatly refused to consider the evidence. Actually only in 1859 Lyell and other British geologists validated the discovery of Boucher de Perthes.

Clearly that of Boucher de Perthes was a discovery only in that he had actually found the artefacts associated with both human and mammalian bones, as stone artefacts had been well known since antiquity, albeit palaeolitic artefacts may be difficult to recognise as such. In fact both Palaeolithic and Neolithic artefacts had been described since antiquity, sometimes under the name 'Cerauns', as it was a popular belief that they were traces left by thunderbolts, and we may remember that the physician and naturalist Michele Mercati (S. Miniato 1541-Rome 1593) had recognised their true nature in an appendix to the *Methalloteca Vaticana* (1574) which was published by Lancisi in 1714-1715. However Mercati's opinion, though known, had been only sporadically been quoted.

We have seen that by the end of the 18th century geologists were divided between 'Neptunists' and 'Plutonists' and out of France Cuvierian catastrophism had a comparatively short lease of life. Indeed, out of France and Italy, volcanic sediments are comparatively rare in Europe, while clearly marine or fresh-water sediments clearly predominate.

So geologists became soon sceptic of catastrophism; they were ready to concede catastrophes, but they considered them as local and sporadic events as they happen today, while it appeared as being more reasonable to assume that, as today, the major changes of the surface of the earth were brought about by the slow actions of erosion and sedimentation.

As it claimed that past changes of Earth surface were due to the same factors active today, the new school was called 'actualist' and it developed especially in England. Here, as in France, both mining and building activities, linked with the economic development of the 'industrial revolution', were rapidly supplying geologists with a host of data, so that well before the middle of the century the main lines of geological successions were well established and the corresponding nomenclature was partly linked with local names, like Devonian, from Devon, or to classical reminiscences, such as Ordovician or Silurian, taken from the names of ancient Briton tribes (the Ordovici and the Siluri) mentioned by the Romans.

The main contributor to the development of the new geology was the Scot Charles Lyell (later Sir Charles) (1797-1875), who, in 1830-33 published his fundamental book *Principles of Geology* which was a turning point for the development of Geology. Lyell was a lawyer, who, because of his sight troubles had to leave the bar and, with the help of his wife, turned to geology.

Lyell's work and, later, the personal friendship between the two men, were quite relevant for the development of Darwin's theories, even if Lyell was, at the beginning, rather dubious about his friend's ideas.

Lyell, indeed, in a pubblication of 1830 had advanced an idea that went back to the cyclic developments of the Greek Empedocles: he though that there could be a turn-over in the succession of faunas and that, in due time the present fauna would become extinct and possibly the great fossil reptiles would make a come-back. The 'Punch' promptly got hold of it and published a famous cartoon, where one sees Professor Ichthyosaurus lecturing on the skull of Man!

CHAPTER XI

From the publication of the *Origin of species* to world war i

HISTORICAL EVENTS OF THE PERIOD AND MAIN SCIENTIFIC PERSONALITIES

Stanislao Cannizzaro 1826-1900, J.C. Maxwell 1831-1879, W. Huggins 1824-1910, H.L.F. von Helmholz 1821-1894, F.A. Kekulé 1829-1896, W.T. Thomson (Lord Kelvin) 1824-1907, D.J. Mendeléef 1834-1907, J.W.Hittorf 1824-1914, W. Crookes 1832-1919
1859 second war for Italian Independence.
1860 almost complete unification of Italy and establishment of the Italian kingdom.
1863-1865 United States' civil war (war of secession).
1864 Danish war.
1866 war between Prussia and Italy against the Austrian empire.
1870-1871 war between Prussia and France, fall of the French II Empire, proclamation of the German Empire.
1871 the 'Commune' of Paris is crushed.
A.A. Michelson 1852-1931, W.K. Roentgen 1845-1923, H.Becquerel 1852-1908, S.Freud 1856-1939, A.Einstein 1879-1955, H.Poincaré 1854-1912, H.G.J.Moseley 1887-1915, G. Peano 1858-1934, N.Whitehead 1861-1947, B.Russel 1872-1970, D. Hilbert 1862-1943
1884 Berlin Congress, global settlement of colonial claims and partition of Africa into 'zones of influence'.
1895-1896 war between Italy and Ethiopia.
1899-1901 second Boer war.
1905 Russian-Japanese war.
1911 Italian-Turkish war.
1912 first and second Balkan war.
August 14, 1914 beginning of World War I.

General features of the age

Since the second half of the 19th century science has become essentially modern both in its principles and methods. Thus this is the last chapter of this book, as, in order to cover both the ever increasing tempo, the extent of the scientific developments and their increasing specialisation during the last 90 years or so of the history of biology one would need a volume twice as big as this one.

The times considered in this chapter, unfortunately, were times of scanty philosophic culture by the average biologist. Most of them, indeed, limited themselves to the consideration of just the common trends of their times.

Most scientists of the late 19th century adopted more or less formally the theses of the positivist thinkers. Positivism itself was a complex phenomenon and, in

Europe, it was linked both with the political conflict between liberals and conservatives and with the difficulties of matching the traditional religious doctrines with the development of scientific, historical, philological and archaeological sciences. On the other side the positivist school, since its inception, was keenly interested in problems of economic progress and social justice (we should never forget that the 19th century was a period of great industrial developments, but it was also a time of demographic explosion and of very painful conditions for the poorer working classes). Thus not a few scientists, following, more or less consciously, in the steps of their forerunners of the 18th century 'Enlightenment', believed that progress in sciences and techniques would naturally develop together with 'civil' progress taken in the most comprehensive meaning of the word. This was an old preoccupation, which had been clearly present, for instance, in Spinoza. As this does not directly affect the scientific activities of the various scholars, we shall usually barely mention, if at all, these problems, but any reader interested in a better understanding of the personalities of several among the scholars that we shall mention, must remember that, often, the individual scientists were influenced in their choice of research strategies, by their ethical or political stance.

It should equally be remembered that throughout this period progress in chemistry and physics had a growing impact on the developments of biology. Chemistry and physics were increasingly making available methods and theories useful in the study of a number of problems, especially in physiology, which had an obvious significance in the medical practice. Thus while the university's curricula in Natural Sciences and in Medicine were increasingly diverging, yet research in histology, physiology and microbiology, was mainly practised within the medical faculties.

As we said, the enormous developments of biology in the sixty years following the publication of the *Origin of Species* make it impossible to deal with it in a reasonably complete way within the limits of a single chapter in a book like this one. Thus one forced to be even more selective in the choice both of scientists, of their theories and of the discoveries quoted. To this I must add some personal bias, as several significant scientists of this age, both biologists and not, like Haeckel, Rosa, Kölliker, Golgi, Grassi and several others, mathematicians like Peano, Volterra, etc. were friends or colleagues of close relatives of mine, so that to me their names are not merely those of the scientists, but very much those of living people with peculiar habits, tastes and so on.

During these years the lead in scientific research was taken by England, France and Germany, while Russia, Italy, Austria-Hungary, the Scandinavian and the other Western European states, just as the United States, played a somewhat lesser role in shaping the pattern of research, in spite of having a number of outstanding scientists.

A typical debate throughout this period was that between 'vitalists' and 'mechanists', a debate that, as we have seen, had begun much earlier, but that raged through this time, quite often tinged with religious and political overtones. The debate died out only after the second World War, when the progress in palaeoecology, cosmology, biomolecular chemistry etc. allowed for new perspectives that our grandfathers could not possibly foresee, and it was thus gradually substituted by the present debate between holists and reductivists.

Evolutionary ideas before Darwin

We have mentioned that the first definite transformist was Father Athanasius Kircher, and that he advanced his hypothesis in order to uphold the literal truth of the Bible; we have also seen how limited transformist hypotheses were repeatedly proposed during the 18th century and how they were finally followed by the first general theory of evolution by Lamarck. Finally we have seen how both the 'Naturphilosophie' and similar general attitudes produced transformist ideas and how they were related with the 'romantic' movement.

A typical romantic, albeit rather isolated, thinker was Arthur Schopenauer (Danzig, 1788-1860). Though on rather questionable arguments, he is sometimes quoted as a forerunner of Darwinian evolutionary theory. As a naturalist, Shopenauer is, like many romantics, a most confused thinker. He was, however, genuinely interested in natural history and for a little while worked with Goethe on the perception of colours, and made also some studies on the perception of sounds. His philosophy envisages an extreme view of the struggle for life among populations, between predators and preys and even within a single population. He considers plausible the hypothesis of evolution, but he is an extreme supporter of the then common opinion that all organisms have inborn tendency towards absolute perfection. Thus he falls back to a sort of neoplatonism: the evolution of living beings is not dependent on material descent, but is rather the materialisation of archetypes, which independently strive for perfection. Just as the whole of Schopenauer's philosophy, it is an entirely astoric view, that is a complete denial of organic evolution.

Of great significance in framing the historical premises upon which an empirical theory of evolution could be developed, was the work of Sir Charles Lyell (1797-1875), whom we mentioned at the end of the previous chapter. He had to leave the bar because of visual troubles and, helped by his wife, he begun to study geology. At the time there were two basic geological schools: Plutonists (originated by James Hutton, 1726-1797) and Neptunists (advocated chiefly by Abraham Gottlob Werner, 1749-1817). Both schools assumed rather short geological times. Plutonists assumed that the earth surface had been essentially moulded by volcanic activity (hence the name), while Neptunists maintained that almost all rocks had been formed by sediments laid in the sea (and, again, hence the name). Lyell first clearly distinguished sedimentary from plutonic rocks; moreover he maintained that such factors and mechanisms that presently mould the surface of the Earth are just the same that moulded it even in the most distant past. The first volume of his main work, *Principles of Geology*, was published in 1830, and Darwin found it in the ship's library, when leaving with the 'Beagle' and got the following two during his voyage.

In the *Principles* Lyell did not only deal with geology; he, indeed, did deal also with the problem of faunal successions. At the time Lyell did not envisage the problem of origin of species: he was a fixist and anti-Lamarckian, however he maintained that through the immense geological times new species had continually appeared, while other became extinct. He maintained that each new species had a precise point of origin and it was from there that it could become more or less widespread. As we said previously, at the time he envisaged even the possibility that there was a cyclic succession of faunas, old ones eventually coming back, and the 'Punch' made a cartoon of it.

When Darwin came back from his voyage, the two became close friends and, after the publication of the *Origin of species*, Lyell became a supporter of Darwinian evolution. Darwin himself writes that Lyell book played a decisive part in the development of his own ideas.

Charles Darwin

Charles Darwin was born in 1809 and belonged to a distinguished family of physicians: among his ancestors, his grandfather Erasmus is the one always quoted. Erasmus Darwin (1731-1802) was a reputed physician, but was also keenly interested in the contemporary debates on nature and philosophy. Thus he wrote some prose and some long and boring didactic poems, and in two of them (*Zoonomia or the laws of organic life*, 1794, in prose, and the poem *The temple of Nature*) he had advanced a transformist hypothesis, which, however, had no influence on young Darwin.

Charles, as a boy, was a less than average student, but at an early age became passionate hunter and amateur naturalist. At sixteen he matriculated in the medical faculty of the University of Edinburgh, but he left, thoroughly disgusted after a couple of years and entered the only other faculty suitable for a family like his: Theology, in Cambridge.

As apparent from Darwin's writings he took no interest in theology, even if, by desperate work during his third year, he graduated as a 'bachelor' with quite good marks. Meantime we know that he continued to make a serious study of natural history. He spent three years in Cambridge and there he struck a lasting friendship with the botanist Revd. John Stevens Henslow (1796-1861).

It was Henslow who, in 1831, secured for Darwin the opportunity to sail with the Beagle[1]. Captain FitzRoy, who was in command, was not only an excellent sailor (he

[1] When Charles sailed with the Beagle it is commonly said that Henslow gave him as a present the first volume of Lyell's *Principles*. Actually the present was a volume of von Humbolt's, but Henslow actually advised Charles to read the book by Lyell and not to believe its theses!

ended up a rear-admiral), but was a true gentleman, a cadet from the high nobility (actually a descendent of an illegitimate son of king Charles II) and a good amateur naturalist. Barely 26 he had already been in command in two expeditions and had been charged by the Admiralty of a scientific exploration of South America. FitzRoy and the ship' surgeon were sufficiently good naturalists not to need any further scientist aboard, but the captain wished for the company of a young naturalist, who had to be a gentleman of an adequate social standing. When Darwin's father flatly denied his permission, their cousin Josiah Wedgwood, later Charles' father in law, stepped in and secured Darwin's father's permit and money for Charles to join the expedition (all counted Robert Darwin paid something like over 600 pounds, an enormous amount at the time).

Thus, aged 22, Darwin sailed in December 1831. The trip lasted for four years and nine months and Darwin not only assembled splendid collections, but matured into a thorough naturalist. Moreover, both the collections and observations that he had meanwhile sent to England had earned him a good repute at home. When Darwin was back in England, his repute as a promising naturalist and the sheer bulk of his collections and notes (as he had sailed as a mere passenger, paying for his trip, all his collections were his private propriety) made it easy for Darwin to forfeit his family's plans to make him a parson and he spent the rest of his life as a private gentleman of adequate means studying natural history. Shortly after his return he married his cousin Emma Wedgwood and settled down for a quiet and affectionate family life, marred only by the loss of three of his many children and by his persistent bad health, which repeatedly brought his research work almost to a standstill for months.

Darwin was a shrewd administrator of his wealth, but, having acquired a considerable estate, he and his wife proved themselves to be also considerate patrons of their tenants and workers during the terrible years of the potato blight.

During Darwin's first years in England, his growing repute was mainly that of a geologist, and, as such, he was able to provide conclusive proofs for Lyell's actualism. Meanwhile he was working to his evolutionary ideas, but, as he expected that, if ever published, they would meet with strong criticism, he undertook a number of zoological and botanical researches, including a monumental monograph on Cirripedes, that established his repute also as an outstanding biologist.

Darwin had begun to think of the possibility of evolution during his trip on the Beagle and he was particularly struck by the fossils that he was collecting in South America and by the distribution there of the different species of living animals. His curiosity was also aroused by the causal remark of the vice-governor of the Galapagos (then a British dependency) that each island had a different species of turtles. Later, and on the advice of the ornithologist John Gould (1804-1881) to whom he had entrusted his collection of birds and while re-ordering his collections, Darwin begun to wonder about the birds, and especially on those that are now known as 'Darwin's finches' from the same islands. Darwin had begun by labelling all his specimens from the first two islands that he had visited simply as 'Galapagos', but he was later able to sort them out checking them against a parallel collection made by Captain FitzRoy, that this officer, trained in the discipline of the Navy, had more correctly labelled.

The plants and the insects were telling the same story: each island of this remote archipelago had its own peculiar species. This made sense if they had evolved by local differentiation from some common ancestor, but if one was to think of separate creations, one had to believe that the Creator had purposely endeavoured to make creation to look as the result of evolution.

In 1837 Darwin begun to record his thoughts in the famous 'notebooks' and just next year, when he was struggling with the problem of the possible mechanisms of evolution, he happened to read the Revd. Malthus' essay on how the increase of human populations naturally tends to outstrip that of resources, unless wars or other catastrophes check the natural growth of humans.

Darwin was fully conscious both of the significance of his ideas and of the fierce opposition they would arouse, so that it was absolutely essential for him to establish for them both a sound theory and the weight of plentiful and convincing evidence. In fact, though it was Malthus essay that prompted Darwin to think of natural selection, the significance of prey-predator relationships or the significance of herbivores as checks for the growth of vegetation, had been considered sporadically since Linnaeus, but they had not been considered as possible selective factors. By 1842 Darwin was able to summarise the essentials of his theory in a short essay of 35 pages, which he kept for himself. He developed it into a new expanded text reaching 230 pages and which was ready in 1844. This he gave to read an comment to his closest friends. Both Hooker and Lyell, whose judgement was particularly significant for Darwin, praised the work and prompted him to continue.

By 1856 Darwin thought to have assembled all the evidence he needed and begun to think of a gigantic treatise. In the meantime he had kept several friendly colleagues fully informed of his own progress. Again at about the same date he had got in touch with Alfred Russel Wallace (1823-1913). Of Wallace we shall say later on. Darwin had known of this young naturalist even before and knew how he was scraping a living as a collector of animals, while doing some good research work in the East. Darwin kept a rather sporadic correspondence with Wallace and occasionally bought from him some specimens. When he was informed that his junior colleague was thinking of evolution and had developed some ideas rather akin to his own, he, while praising the effort of Wallace and encouraging him, somewhat cryptically informed him that his own studies were far more advanced than his and that he was writing a book on the subject. Instead of being frustrated by this information, Wallace was stimulated by Darwin's praise and taking the opportunity of a bout of tropical fevers, which prevented his field activities, in a few days wrote a short essay which he sent to Darwin asking him, if he judged it good, to forward it to Lyell. Darwin was aghast: as he wrote to his friends Lyell and Hooker: Wallace's paper was an almost perfect summary of Darwin's ideas.

On the spot Darwin considered the opportunity to send to press Wallace's paper and renounce his own work. His friends dissuaded him, pointing how Wallace's paper, devoid, as it was, of practically any evidence to support the theory, could not possibly carry persuasion. Actually Wallace himself, when already a scientists of considerable repute, always said that he was glad to have merely had the chance of hitting on ideas similar to Darwin's, but that he thought that both the priority of Darwin and his outstanding merit in establishing the idea of evolution, were indisputable.

On the advice of Hooker and Lyell it was arranged to present to a meeting of the Linnean Society held on 1 July 1858 both Wallace's paper, a long letter by Darwin to the American botanist Asa Grey by which Darwin had outlined his ideas some years before and his draft of 1844. Given the tremendous impact of Darwinian evolutionary theory on the following developments of biology, it should have been a momentous meeting. Yet it went practically unnoticed: Darwin was absent, all taken by his domestic troubles (he had just lost a child to diphteria), Wallace was in the far East. The turning point of modern biological research escaped the attention of even most bystanders, so that the President of the Linnean Society himself, summarising the events of the last twelve months almost one year later (24 May 1859) said "The year which has passed … has not, indeed, been marked by any of those striking discoveries which at once revolutionise, so to speak, the department of science on which they bear." and, when the papers themselves were published, a notable scientist like Samuel Houghton addressing the Geological Society of Dublin in February 1859, suggested that the only reason anybody had taken any notice of the joint paper was because of "the weight of authority of the names (Lyell and Hooker) under whose auspices it has been brought forward", and commented: "If it means what it says, it is a truism; if it means anything more, it is contrary to fact".

Anyway Darwin, scared that Wallace might develop his ideas into a book (an idea that Wallace actually nursed until the *Origin of Species* was published), set to work really in a hurry to synthesise all his huge store of notes. The *On the origin of species by means of natural selection or the preservation of favoured races in the struggle for life*»was printed in November 1859 and met with immediate editorial success and, as Darwin expected, an equally immediate storm of protest. Darwin endeavoured, in the several successive edition to meet each serious criticism and, to say the truth, he somehow weakened his position.

The *Origin* was followed by a number of other publications, some of great general significance: in '62 a monograph on the mechanisms of insect pollination in Orchids, in 68 a large monograph on variation in domesticated animals, and, after Huxley had already dealt with the problem in 1863, in 1871 Darwin published *The descent of Man*, which is particularly significant , being the first work to deal with the problem of sexual selection. In 1872 Darwin published another pioneering work: *The expression of the Emotions in Man and Animals*, which ranks Darwin among the founders of Ethology; in '76 he published a study on the effects of 'proper' and 'crossed' fertilisation, that is on fertilisation between members of different populations of the same and of different species. Finally in 1881, the year before his death, came the classic study on the impact of earthworms in the formation of soils.

Darwin was for many years acclaimed both in England and abroad as the leading figure among British biologists and was awarded a number of academic and foreign honours. His merits were not acknowledged by the Crown out of consideration that, so far as the polemics on the religious impact of his doctrines raged, the Queen, being also the head of the Church of England, had to stay neutral; nevertheless, when he died, he was buried in Westminster Cathedral among the Great of England.

Basic principles of Darwin's theory

Darwin's problems were two: first to show that evolution had occurred, second to offer a plausible hypothesis as to its mechanisms. On the first point he rapidly won the day, both owing to his authority, being already an acknowledged scientists of the first class, and because of the overwhelming amount of evidence that he had marshalled in his book with punctilious precision, the very contrary to what Lamarck and other 'transformists' had done. On the second issue his ideas met with very mixed fortunes.

If we take just the essential of the complex mechanisms advocated, Darwin assumed that the high rate of mortality that always curtails wild populations, especially during larval and juvenile stages, hits in a statistically significant proportion mainly such individuals that are less fit to survive and reproduce in each different environment (natural selection), thus the infinite variety and continuous variations of environmental factors, duplicate in Nature the work of animal breeders in the improvement of given qualities in domesticated animals and in the selection of different breeds. On the other hand he assumes that selection works on a certain amount of random variability that always occurs in wild populations. Thus reduced to its skeleton, Darwin's explanation was as good then as it is today (with the caveat that we now know that there are a number of genetic variations that are selectively neutral and that may eventually be incorporated in the average genome of a population by merely stochastic chance).

In its more complete set up, Darwin's model was weak on a number of details, which were promptly recognised by his critics.

While Mendel was already paving the way to modern genetics (but Darwin did not read the reprint that Mendel had sent him, as it is proved by the fact that its pages are still uncut in Darwin's library), Darwin tentatively explained heredity by 'pangenesis', an ancient and traditional hypothesis as to the formation of reproductive materials. In fact 'pangenesis' had been precisely advocated since 1651 by Nathaniel Highmore (1613-1685) and was part of the theories of Erasmus Darwin (Charles, who always maintained that he had not been influenced by his grandfather's ideas, when his attention was called on it, simply replied that he had overlooked the relevant sentences). 'Pangenesis' as proposed by Darwin in 1868 assumes that hereditary traits are transmitted by extremely minute particles, which he calls 'gemmulae' which form everywhere in the body and then migrate to concentrate in the germinal cells. Originally Darwin had stressed that his hypothesis was merely tentative, but later, as it appears from his correspondence with Delpino and from his notes on Delpino's reprints, he became fairly convinced of this hypothesis.

In order to meet some of the criticisms raised by his previous editions, in the later editions of the 'Origin' Darwin allowed that the fenotypic adaptation to local conditions might influence the formation of the 'gemmulae', thus allowing for an almost Lamarckian transmission of acquired characters. On the other side Darwin always dismissed as insignificant the 'sports', in fact mutations, that, however had been employed by breeders. Obviously, as we now know, he was wrong in the general set up of his genetics, but it was his usual common sense that had led him to discard a 'mutationist' theory: by far the majority of the empirically verified mutations severely affect the ability of the mutant subject to survive in the natural environment, and a wholly gradualist hypothesis appeared much more 'ecologically' sound.

Even nowadays, in spite of almost 150 years of progress in genetics, we are still not having a wholly integrated picture of what actually occurs to populations under the different environmental conditions: we know a number of instances of classic neo-Darwinian evolution: random appearance of favourable traits on which selection is immediately active, but there are as many instances where a neutralist model may apply (appearance of mutants which are functionally irrelevant and on which selection can not operate, so that they are incorporated into the genome by merely stochastic mechanisms and may, eventually, become pre-adaptive. We know of horizontal genetic transmission of genes between quite different species, a transfer usually mediated by retroviruses, and even, in but a couple of instances, of induction of definite mutations by special environmental stresses during development. Finally we know of different mechanisms for the rapid radiation of populations. The complicated picture being further compound by, possibly emotional, problems for many scholars to work on semi-stochastic evolutionary models, which are necessarily irregular in their functioning and hardly predictive.

Philosophic and religious reactions to Darwin's theory

There is no doubt that Darwin's work marked a turning point in the development of biology, even allowing for the fact that evolution was 'in the air'. The fact is that the acceptance of evolution had enormous implications. Just in the field of biology is was immediately obvious that the traditional view of a *scala naturae* had become finally obsolete, and that the whole of morphology had to be re-framed in an evolutionary, that is historical, perspective, just as this was mandatory for taxonomy. All other sections of biology, such as biogeography, etc., had equally to be rethought.

Such rethinking, as well as the search for new evidences to fill the gaps of both direct (fossils) and indirect evidence (all the evidence that might accrue from the study of living beings), was the main topic for the next half century of biology and both are quite relevant even today. Only physiology escaped to some extent the pervasive influence of evolutionary thinking, this being the result of both the close relationship between the development of animal physiology and medical practice and of a growing interest of physiologists for biochemistry. Comparative physiology, which, indeed, is based on evolutionary concepts is still, in many curricula, a sort of rather poor relative of a basically mammalian 'General physiology'.

However it was soon apparent that, often for reasons of which they themselves were not fully aware, many biologists found themselves ill at ease with Darwinism. Quite apart from the serious theological problems that it did pose to many, it was obvious that the strong element of randomness that was inherent in the system made very difficult to frame the new biology within the strictly deterministic framework which was increasingly paramount in the other sciences. As for the religious aspects, had biologists been more conversant with the religious attitude of the classics, and especially of the Greeks between the 6th and 3th century BC, they would have had the surprise of finding Darwinism comfortably fitting in the then prevailing views of life and cosmos.

Anyway any analysis of the spread of Darwinian influence must take first into consideration the personalities and work of two non biologists.

The first is Herbert Spencer (1820-1903). His parents had wanted him to be a teacher; instead he studied natural sciences, engineering and economy. He made the purpose of his life the elaboration of a comprehensive philosophy which could fully justify the liberal system that had evolved in Great Britain. Thus, independently of Darwin and actually some months before Darwin's publication of the *Origin*, he had outlined some aspects of an evolutionary theory based on variability and selection. When the *Origin of Species* was published, he took upon himself to provide Darwinism with sufficient philosophic support and frame it into a general philosophic theory of the world.

Spencer had become convinced of evolution independently of Darwin and he had developed the concept of 'struggle for life' (actually the expression 'survival of the fittest' is his). He had published some aspects of his ideas in *Social statics* (1850), *Principles of psychology* (1855) and in *Progress, its laws and causes* (1857), and he finalised his biological ideas in *Principles of biology* (1864).

Later Spencer fiercely opposed Weismann's theories, as some sort of inheritance of acquired characters, some degree of 'Lamarckism' was essential for Spencer's theories of social progress.

Another philosopher who thence had a great following and whose influence extended well into the middle of the 20th century, was August Comte (1798-1857). Comte is considered as the father of 'positivism'. Again this corresponded with a diffuse trend and his is the attempt to the systematisation and the christening of the movement.

Comte was completely contrary to Lamarckian evolutionism and, given his general views during his later years, the so called 'mystic' ones, had he lived to read the *Origin*, he would certainly have rejected it. He would be entirely irrelevant for the purposes of this book, but for the fact that both his general views and his optimism as to the powers of the 'positive' sciences, had generally a strong influence on the French and on the other European scholars who came under the influence of French science. Thus French 'positivism' came to have a strong influence of the development of European biology and evolutionary thinking.

Comte upbringing had been of the most orthodox Catholicism; so, as it often happens, when he begun to doubt it, he went for an equally dogmatic opposite. He strove for elaborating a 'complete' system of philosophy, which materialised basically in the six massive volumes of his *Cours de philosophie positive* (1830-1842), which probably barred him from any permanent position. As a result his finances were so shaky that at times his wife had to prostitute herself to get the bare minimum to survive.

Comte's biology, in spite of rather extensive treatment, is completely amateurish and largely derived from De Blainville's. He strongly decried Lamarck and evolutionism in principle and always refused the emerging cellular theory. However his faith in the eventual development of 'positive' sciences was as strong as that of the French 18th century's thinkers and so was his faith that Science would eventually solve all moral and social problems. He could be plainly bypassed, but for the fact that his real interest was in social problems, and, in an age when the awareness for current social problems was increasingly acute and traditional religious values appeared to conflict with the development of sciences, his basic ideas appeared quite attractive to large numbers of scientists. In his late years, after an infatuation for a woman who died young, Comte attempted to build a new religion with proper rituals and prayers and with a set of Saints including both Moses and Gall and, in a prominent place, his deceased beloved one. All this is obviously irrelevant for us.

It was expected that the publication of the origin would occasion a hot debate and, naturally, many Churchmen were among the first in the outcry. The repeated stands, both in writing and in debates, of the Anglican bishop of Oxford Wilberforce, and especially his verbal match with Thomas Huxley in 1860 are famous (however, the current account has been edited and considerably embellished by Huxley himself).

In just the same year the Synod of the German Catholic Bishops strongly condemned evolutionary ideas. Pope Pius IX himself was strongly tempted to take an official stand and was persuaded with some difficulty to desist. Not only most of the Christian organisations were set against Darwinian theories, the Jewish organisations reacted as strongly. As a matter of fact evolutionary theory came just at a moment when philology, archaeology, history, geology were casting doubts on the literal truth of the Bible and evolutionary theory did the same in a much more obvious way for the general public. To doubt the literal truth of the whole of the Bible would open the door to doubts on basic issues like the original sin, etc.

Indeed several non Christian thinkers, whether biologists, like Th. Huxley or E. Haeckel, or not, including political theorists like Karl Marx, immediately used evolutionary theory as a weapon to attack the religious, academic and political establishment.

On the other side not a few clergymen took a stand in favour of evolutionary theory, albeit not precisely of the Darwinian type.

In due time almost all the Christian Churches have been able to come to terms with evolutionary theories. The Church of England was probably the first: in 1882 it did not object to Darwin's burial in Westminster Abbey (a fine piece of show of British fair play: it was quickly arranged by Darwin's friends against Darwin's will to be buried at Downs, and even the Duke of Argyll, one of the foremost critics of Darwin, was among the six who accompanied Darwin's bier) and in 1886 when the Archbishop of Canterbury attended the unveiling of the monument of Darwin in the British Museum. The Roman Catholic Church formally agreed only around 1970. We are not concerned with the problems that evolutionary theories posed to theologians, but we must underlie how evolutionist theologians usually look first to St. Augustine and to the neoplatonizing Greek fathers of the Church for the foundations of a synthesis of evolutionary theory and Scriptures. They also take their lead from some problems that we considered in chapters IV and VI. They assume the argument that God works through 'second causes', a largely Scotist-Lullian argument, and some considerations of St. Thomas Aquinas, which are a development of a naturalistic framework elaborated under the influence of St. Albert the Great. Such are, in the second half of the 19th century the theses of the Dominican Fathers Leroy and Sertillanges, of Canon H. De Dordolot, while in Italy the lectures for Darwinism by the popular writer Fogazzaro, were printed and had a considerable influence. Probably the most competent and influential Catholic biologist who supported evolutionary ideas was the Gesuit Father Erich Wasmann (1859-1931), born in Merano and dead in Valkenburg. He was a reputed entomologist, who advocated a God-planned evolution and had memorable debates with Haeckel.

Meantime quite different intellectual groups of individual thinkers stood for evolution, but not for Darwinian evolution. Just to take a few examples: so did a the philosopher Bergson, so did the Catholic 'Modernist' groups. All of them, more or less implicitly and, perhaps even without being aware of it, stood by the old neoplatonic assumption that every being has an inborn tendency for improvement. This is just another clear proof of the deep rooted survival of extremely ancient beliefs both among the believers in Revelation and among their foe.

Anyway, just before going through an orderly account of how the Darwinian theory and the development of chemistry affected the various branches of biology, we must consider a small group of biologists who were particularly close to Darwin: Thomas Henry Huxley, Alfred Russel Wallace, Henry Walter Bates and Joseph Dalton Hooker.

Joseph Dalton Hooker

Hooker we may briefly mention, as we have already talked of him in the previous chapter. He was a very competent botanist and he was closely associated with Darwin for many years before the publication of the Origin. Darwin himself kept Hooker constantly informed about the developments of his work, and Hooker not only was instrumental with Lyell in the first presentation of Darwin's theory, but through his extremely long life, he died at 94, he constantly promoted Darwin's ideas both in brief articles and in his big systematic treatises.

Thomas Henry Huxley (1825-1895)

Huxley was the son of a rather poor schoolteacher and could not follow a regular schooling, yet his qualities gained him admittance to the university at seventeen and thence he graduated in medicine. He then entered the medical services of the Royal Navy and, during four years of service in the Eastern Seas he was able to begin his research work in zoology, which he began by the study of different polyps and jellyfishes. This pioneer work, which enabled him to considerably improve the systematics of Coelenterates, won him immediate recognition and in 1854 he was appointed to a chair at the School of Mines, later Royal College of Sciences, and married (the Huxleys were to become a most distinguished dynasty of scientists and writers: two of his grandsons were eminent biologists (one being a Nobel laureate) and Julian is considered as one of the co-authors of the, so called, 'modern synthesis', the development of classic Darwinism that has been followed by most scholars in recent years).

The appointment to the School of Mines prompted Huxley to further widen his field of interests to the study of fossils. A convinced fixist in his first papers, he soon converted to Darwinism and, not only became a close friend of Darwin, but his partiality for all sorts of hot debates (which Darwin systematically shunned) earned him the surname 'Darwin's bulldog' even in cartoons. Both his scathing argument with Bishop Wilberforce at Oxford in 1860 and his controversy with Richard Owen, are still remembered as classics (Owen claimed that some supposed unique features of the human brain deserved a special ranking for mankind in systematics, and Huxley proved the Owen to be entirely wrong).

As a matter of fact Huxley was an eminent comparative anatomist and his important contributions are many and impeccable, but, both as a teacher and as a polemist, he was not interested either in the possible mechanisms of selection, which he barely mentions, or in the nature of variability, both crucial to Darwin's theory. To Huxley the evolutionary theory is a basic instrument for the interpretation of anatomical evidence and a club to be used on the heads of his academic, political and religious enemies. Huxley was possibly the active leader of the, so called, X-club, the group of close friends of Darwin, who used their academic and political influence to further Darwinian ideas, and thus was a force to reckon with. As such he had a great influence not only in the development of evolutionary morphology, but also in the educational and social thinking of his age, as he constantly strove for reform, and especially for the enhancement of sciences in the student's curricula.

Alfred Russel Wallace (1833-1913)

Wallace, as we have seen, was, in a sense, a sort of co-author of the Darwinian theory. The son of an unsuccessful solicitor, he left school at thirteen and eventually became a land surveyor and, meantime, became a keen naturalist, and for a brief time, a schoolteacher. Being practically jobless, he and his friend William Bates decided to try their luck as freelance naturalists and collectors in South America. With some support from Sir William Hooker, they were able to sail in 1848 and spent four years exploring and collecting. On his return voyage Wallace lost all his collections when the ship burned down and was lucky to be rescued after ten days in an open boat. He remained in England but fourteen months, penny-less, but able to publish some valuable scientific papers and a reasonably successful book on his explorations. Thence his good repute as a naturalist earned him a free passage on a Navy's ship to the East Indies in 1854. Darwin had noticed Wallace's contributions and the two begun to exchange letters; moreover Darwin began purchasing specimens from Wallace, who was earning his living collecting around in Indonesia..

We have already seen the story of the joint presentation of Darwin's and Wallace's theories at the Linnean Society and when Wallace returned to England, he became a frequent guest at Darwin's home. Wallace was by then a reputed naturalist and eventually became a fellow of the Royal Society, yet he never received any permanent appointment and was a freelance writer and naturalist-explorer until, when already an old man, he was granted a government pension. It is notable that Wallace was never a member of the X-club.

Wallace merits are twofold: on one side he is often hailed as one of the fathers of biogeography (his classic book *Geographical distribution of Animals* was published in 1886) and every student is still requested to know 'Wallace line' separating the Asiatic and Australian faunas (in fact a bifurcating line as while at the strait separating

Lombock form Bali the difference is clear cut, Celebes, to the North of it, has a mixed fauna.), but he is also the chief developer of the theory, more or less loosely termed 'Batesian mimetism' after an important preliminary paper by Bates, that conspicuous patterns and colours often either advertise potential predators that the animal is actually dangerous or unpalatable, or mimic such an animal, and thus afford some protection to the innocuous species.

Wallace was always very keen on social issues and an active social activist; on the other side, much to the disgust of Huxley and the annoyance of Darwin, he developed an acute interest in 'spiritualism' (what we now call metapsychics), which was the rage in the late 19th century, possibly as a reaction to positivism and the serious difficulties that the different churches were having in adapting to the evolving society and culture). So, somewhat like Lyell, he became ill at ease with his own theories and tried to merge them with some sort of evolution guided by a 'superior' being.

Henry Walter Bates

Henry Walter Bates (1825-1892) was foremost an entomologist. As we said, he went to South America with Wallace, but he stayed there for eleven years and his enormous collection got safely home. Besides several lesser papers, he thence published his *Contribution to the insect fauna of the Amazon Valley*, a vast monograph where he considers all the aspects of insect biology in these regions. His factual support for Darwin's theory never failed.

Darwin's critics

On the opposite side, quite apart the occasionally vociferous, but scientifically irrelevant criticisms on Darwin by incompetents, three first class scholars stood against the Darwinian theory. All three agreed that evolution occurred, it was its mechanism that their consciences did not accept: Foremost was Richard Owen, whom we have mentioned in the previous chapter. He was an extremely competent morphologist, so much that, on his return with the Beagle, Darwin had given him all his vertebrate fossils for study, and the two had been on excellent terms until the *Origin* was published. Owen simply could not accept the idea that mankind was just a big brained ape and the result of some lucky chances.

Much the same was the position of George Douglas Campbell, 8th duke of Argyll, a politician, but an extremely brilliant and learned man, who, again, in his book *The reign of Law*, tried to uphold an evolution planned and ordered by 'the Great Watchmaker': the reverend Paley updated and made an evolutionist, and this being done in a most competent and erudite manner. The book, enthusiastically praised by Owen, had a great editorial success. As usual Darwin set to work to answer the criticism and so, Argyll's attacks were largely responsible for Darwin's tackling the problem of sexual selection as a necessary complement to environmental selection. As we said, Lord Argyll dissented strongly from Darwin, but fully appreciated his scientific merits, so that he attended the bier of Darwin at his funerals.

Much on the same lines, and even with more technical insight were the systematic attacks by St. George Mivart (1827-1900). Mivart was a very competent biologist and had been appointed to a chair at St Mary's hospital Medical School on the joint recommendation of Owen and Huxley! He actually co-operated with Darwin supplying experimental material both for the book on *Variation under domestication* and for the *Descent of Man*. However he was an orthodox Catholic and the same year 1871 when Darwin published the *Descent of Man*, Mivart published *On the Genesis of Species*, turning against Darwin's theory of selection and, again, advocating a sort of planned evolution.

Moreover Darwin's theory run into serious difficulties with the physicists: William Thomson, later Baron Kelvin, basing himself on his calculation of the age of the solar system, assumed that the heating power of the Sun depended solely on his contraction (Radioactivity was discovered some 50 years later and it took a longer time to conceive the Sun as a nuclear pile and to assess the inherent temperature of the Earth) challenged the long times assumed by Darwin for geological periods. As Thomson's estimates tallied with the almost contemporary conclusions of Von Helmholtz, though Wallace advised Darwin just to ignore them, Darwin had to come to terms with such highly qualified advises and thus he gradually weakened his positions as far as the power of selection was concerned. Thus the last edition of the *Origin …* is also the worst of them all. Yet this is to the credit of Darwin, who, as an honest scientist, was always ready to reconsider his ideas in the light of what appeared as well founded criticism.

In France the Darwinian theory was badly received. There were obviously the books and pamphlets that amateurs wrote against it, but, moreover, there the naturalist's establishment was dominated by direct or indirect pupils of Cuvier, and with them belief in catastrophism was deeply rooted. On the other side Darwin himself was gradually allowing for some kind of inheritance of acquired characters and many French scholars concluded that, if there was evolution, the best was to go back to Lamarck's theory. Thus throughout the fifty odd years preceding World War I, there were very few scholars in France following the Darwinian theory, and the vast majority split between anti-evolutionists and neo-Lamarckists. The most famous among the anti-evolutionists, a stand apparently dictated by his strong religious creed, was Pasteur, to whom we shall give due space because of his basic work in microbiology. Anyway some anti-evolutionists were still active in France well in the 20th century. Such was Louis Vialleton (1859-1929), a comparative anatomist of good repute, who in 1929 published a book which title is a program by itself *L'origine des êtres vivants: l'illusion trans-*

formiste (= *The origin of living beings: the transformist delusion*). Neo-Lamarckian evolutionists were, for instance, Edmond Perrier (1844-1921) and Alfred Giard (1846-1908). In addition there was, and was hardly distinguishable from strict neo-Lamarckism, a trend towards a teleologic evolutionism which appropriated Lamarck's principle of the natural tendency of organisms to perfection and joined it with religious requirements. Such were, for instance Lecomte de Noüy (1883-1947) and Father Teilhard du Chardin (1881-1955), both, however, active mainly after World War I.

Among the chief zoologists who, in France, continued the tradition of Cuvier, the most notable are probably Milne Edwards, De Quatrefages and Lacaze-Duthiers.

Henry Milne-Edwards (1801-1885)was born in Bruges from English parents; he eventually, became a professor both at the Muséum and at the Faculty of Sciences of the Sorbonne. He was, like Victor Audouin, one of the main French students of invertebrates. Being an upholder of Cuvier's ideas, he was basically anti-evolutionist. However, he had the great merit to study animals as living beings, that is in their relationship with environment and not only as a mere anatomist. His main work, the *Leçons d'anatomie et de physiologie comparée* (14 volumes, published between 1847 and 1881) was, at the time, a precious synthesis, rich with notable personal contributions.

Milne-Edwards attempted to a synthesis of Cuvier's main ideas with some of those of Geoffroy. According Milne-Edwarsds, Nature employs but a few basic types of morphology and varies them continually, just in order to meet the requirements of different functions; that is for reasons of economy of effort. Milne-Edwards also believes that progressively, with the increasing perfection of organisms, each function has become the work of specialised organs. As a whole he appears to believe in a sort of evolution of archetypes, like that envisaged by romantic protoevolutionists. Selection and branching phyla he refused even after the publication of the *Origin*. Moreover, and for a while with some justification, given the still poor microscopic data, he held that, while plants had a cellular organisation, this obtained in animals only as a temporary condition during embryonic development.

Jean-Louis-Armand de Quatrefages de Bréau (1810-1892) was a noble Belgian, who was for a while a medical practitioner, thence became a teacher and finally was professor of Anthropology in Paris. He had a great influx on the development of this branch of biology. As an anthropologist he occasionally propounded some crazy theories; for instance, in 1879, shortly after the Prussian-French war, he maintained that Prussians were natural destroyers as they were of Mongolian origin! The funny suite being that the great German pathologist Virchow, who was totally devoid of any sense of humour, organised an immense anthropologic census which involved some 6,000,000 German children to show that de Quatrefages was wrong!

De Quatrefages was rather sceptic on evolution and was very critic of Darwin. Anyway, he maintained, Lamarck's theory was better than Darwin's.

Finally we must mention Henry de Lacaze-Duthiers (1821-1901); born to an old and wealthy family, he was a professor, first in Lille and thence in Paris, and made important work on marine animals, especially Molluscs. He established the two great biological stations at Roscoff, near Cape Finistère, and of Banjuls sur Mer, on the Mediterranean. Also he was an opponent of Darwin, whose ideas could not be made to fit with his strictly Cuvierian outlook, but he, nevertheless supported Darwin's election as 'Membre corresponent' of the French Academy.

As a whole, while the French school was little permeable to the new evolutionist outlook, nevertheless they had the great merit to pay much attention to the interrelationships between organisms, which they considered as morfo-funtional units, and to environmental requirements; thus they were the main founders of functional morphology in the very best Cuvierian tradition, as opposed to pure descriptive morphology, which mainly characterised the German school. The French thus paved the way to comparative physiology and to ecology. Moreover the French systematists were the first to methodically consider, for purposes of classification, all the developmental stages of organisms, thus foreshadowing by some years the great age of comparative embryology.

In Germany Darwin's theory met with a favourable environment, as the ground had been prepared by the Naturphilorophers and it spred quickly. We shall briefly mention some of the leading German evolutionists, though remarking that also here, not a few scholars found the idea of selection operating on merely random variability, rather indigestible.

Fritz (Johann Friedrich Theodor) Müller (1821-1897) published in 1864 a book, 'Für Darwin' where, just as in his other works, he tried to incorporate von Baer's theories into Darwinism (actually the old von Baer was a convinced anti-Darwinian: he, obviously, believed in evolution, but had difficulties to accept the branching phylogenies envisaged by Darwin's theory). Friz Müller may also be listed among the many fathers of the 'biogenetic law' and did much important work on the larval development of several Crustaceans.

The most famous, albeit certainly not the best, of the German Darwinians, was Ernst Haeckel (1834-1919). He begun his medical studies in Würzburg, but he later passed to Berlin, where he studied with Johannes Müller. When he begun his zoological studies he first studied the Radiolaria of the Mediterranean and his researches earned him a chair in Jena. His studies on the 'Protozoans' led him to maintain that all these unicellular organisms, whether photosynthetic or not, should be grouped together in a single 'realm': the 'Protista'. His proposal was then disregarded, while it was re-advanced some years ago and it is now followed, at least in principle, by most scholars, even if it is admittedly a 'horizontal' classification, grouping together entirely independent organisms.

He later went on to study both calcareous sponges and jelly-fishes, and, being a good amateur painter, he wrote a sort of popular book on their aesthetics.

As an enthusiast for the new evolutionary ideas, he spent much of his activity to advertise them, beginning in 1866 by his *Generelle Morphologie der Organismen*. However he generalised and simplified Darwin's ideas so as to make them fit his philosophical and political ideas (and Darwin was somewhat distressed by it). Indeed Haeckel was a pure mechanist and materialist, quite active also on the social battlefront, so that, for instance, when the Italian Anarchist League unveiled at Campo dei Fiori a monument to Giordano Bruno, who had been burned at the stake there, Haeckel was invited as the guest of honour.

Haeckel's vocation as the advocate for a positivist evolutionism is increasingly clear in the works following the *Generelle Morphologie* and he had a great success as such. Finally in 1899 he summarised all his ideas in *Die Welträsel*. Haeckel was an enthusiast for drawing phylogenetic trees (the term was actually proposed by him), and, whenever he felt it suitable, for imagining transitional beings, which was correct in principle, but that, as he also bestowed them Latin names, sometimes made for considerable confusion in nomenclature (for instance the generic name *Pithecanthropus* was proposed by him for an imagined link between higher Primates and Man, but was later used by Dubois for his Java fossil).

Haeckel should be remembered because of some theses of great general significance. First he revived some old ideas which he reshaped into a phylogenetic framework: Leibniz monads, the 'living monads' of Maupertuis, the particles of Buffon, and with them he advanced a general theory holding that elementary living organisms, called Monerae, aggregated to form the first unicellular organisms, these originated colonial organisms (which corresponded with the embryonic stage that he called 'Morula', a still used term that means 'little blackberry' or 'little mulberry'). Such organisms eventually became Metazoans passing by the stage of 'Gastrula'[2].

Quite apart from a useful set of new terms, the theory itself is just a variety of Oken's ideas. In fact the objects that Haeckel believed to be living Monerae fished at sea from considerable depths, were just inorganic aggregates, badly preserved protozoans, and artefacts. To the same range of objects belongs *Bathybius haeckeli* which Huxley in 1868 thought to have discovered and had dedicated to Haeckel (Huxley honestly recanted as soon as his mistake was verified, while Haeckel in his later papers either mentions it but vaguely or just forgets to mention it!).

Second, as we already mentioned, Haeckel advanced his 'gastrea theory': there he took as a starting point older evidences of some similarities between the structure of some embryos and that of simple Coelenterates, to maintain that all metazoans had passed, during their evolution through a coelenterate-like stage.

Finally Haeckel advocated, under the name of 'fundamental biogenetic law' the principle of recapitulation: In fact he re-elaborated older ideas, mainly by von Baer,

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[2] Just for the sake of better understanding Haeckel 'gastrula' derives from the Greek gaster = belly, a term used in the sense of stomach only by Rufus; 'blastula' derives from blastos= bud). Haeckel suggested that all metazoans must have ultimately derived form basically diblastic organisms like the simplest Hydrozoans.

and assumed that all animals must pass, during their embryonic stages, through morphologies that, albeit simplified, correspond with those of the adults of their ancestors. As we have already repeatedly mentioned, this is an idea that had a number of 'parents', even if it is Haeckel who definitely made it popular, and has no real general validity.

His unrestricted passion for generalisations, curiously prompted Haeckel, a pure materialist, to associate with the idealist physicist Wilhelm Ostwald, to create the 'Monist society' which aimed to propose the maximum of generalisation of scientific theories. Haeckel was sometimes a guest at Darwin's home, but it seems that Darwin himself was a bit unhappy with the daring flights of fantasy of his German guest.

Just before leaving Haeckel we shall remember how he was fascinated by the elegant symmetries of Coelenterates and Protozoans, that he illustrated (and embellished) in his book *Kunstformen der Natur* (= *Artistic forms of Nature*) that considers such animals from the purely aesthetic side.

By the way, the problem of the regularities in the structures of organisms was tackled with an entirely different outlook by Giovanni Schiaparelli (1835-1910) and by D'Arcy Wentworth Thompson (1860-1948).

Schiaparelli graduated as an engineer and was a well known astronomer. He, in 1898, published his only contribution to biology, as an appendix to a series of essays by T. Vignoli. His *Studio comparativo tra le forme geometriche pure e le forme organiche naturali* (= *A comparative study between the pure geometrical forms and those of natural organisms*) was translated in Polish and was much appreciated by mathematicians such as Lubitscheck and Volterra, but it went otherwise unnoticed.

Results which were at one time both parallel with those of Schiaparelli, and that considered a wider range of problems, were published in 1917 by Thompson in his famous book *On growth and Form*. Thompson was a rare example of a notable biologist who was also an excellent mathematician and classicist. By profession a zoologist, he was also an excellent master, as testified by the many outstanding pupils of his.

Both these authors were fascinated by the geometric and mathematical relationships that are commonly found in the proportions of the various parts of living beings, something that had been sporadically noticed before them (for instance we mentioned in chapter IX the contribution of Giuseppe Olivi). Both, independently, proved that several structures and entire organisms may be described by mathematical expressions and that organisms that are phylogenetically related may be considered as regular topological transforms of the same basic form, a fact that, properly used, may produce crucial evidence when assessing true homologies. The mathematical complexities of the problems posed by Schiaparelli and Thopson were well beyond the abilities of almost all biologists, and so only recently, when computers have made much easier to deal with them, there is a renewed interest in these approaches. Anyway we are just beginning to glimpse at how they may be related with genetic mechanisms.

While Schiaparelli practically ignored the problem of the relationships between the topics that he was raising and evolutionary theory, Thompson basically rejected the Darwinian evolutionary model, took no account whatsoever of the emerging genetics and, to some extent subscribed to a near Lamarckism.

Coming back to the German reactions to the Darwinian theory, the scientific environment there was considerably divided.

Albert Wigand (1821-1866) in his *Der Darwinismus*, published posthumous in 1874-1875, made a punctilious, and for the times, accurate criticism of the theory.

Oscar Hertwig (1849-1922), who was professor in Berlin, was an embryologist. He, together with his brother Richard, proposed the, so called, 'Coelomic theory', one of the general theories proposed in the 19th century for the interpretation of the organisational plans of animals and that still has some room in textbooks. As far as evolution was concerned, he maintained a stand intermediate between Lamarckism and evolution by internal causes. He maintained that, although the environmental physico-chemical conditions may frame to some extent evolution, in reality the 'Urtier', the 'primitive animal' has only a limited range of evolutionary pathways open in order to attain the more complex structures and these pathways are pre-determined. Oscar Hertwig was reluctant to accept the Darwinian theory also for moral reasons: he was afraid that the assumption of a world without a scope, would cause a crisis in the ethical set-up of society.

The pivotal point for many other scholars in their attitude to the evolutionary theory was its possible social implications, an example in this sense being August Antinous Rauber (1814-1817) a Bavarian human anatomist, who was concerned with human and social evolution.

The great pathologist Virchow was worried that Darwinism might pave the way to socialism (a notion entertained for a while even by Marx and Engels), while Eduard Oscar Schmidt (1823-1886) another pupil of J. Müller, who, after some exploration cruises, had become a professor in Krakow, thence in Graz and Strasbourg, and himself a student of Sponges and Plathyhelmithes, acutely defended Darwinism both on the general plan and on this specific issue.

Theodor Eimer (1843-1908), professor at Tübingen, was quite willing to grant the importance of selection, but he thought that this worked on variations pre-directed and imposed by the environment.

Carl Wilhem Nägeli (1817-1891) was a Swiss and first a professor in Zürich, thence in München. He is mainly remembered both as a general biologist and as a botanist. He deservedly ranked among the best botanist of his age, but he also made the inexcusable mistake of misunderstanding the significance of Mendel's observations, so that he advised Mendel to continue his researches on quite unsuitable materials. As these failed, Mendel, discouraged, abandoned his researches, probably delaying the development of genetics by some thirty years. We shall see later on which were Nägeli's ideas on heredity and how he was misguided. In addition to Mendel, Darwin himself was to some extent side-tracked by Nägeli's ideas and authority, as he was influenced by him in under-estimating the significance of 'sports' (actually mutations). On the positive side for Nägeli, we must remember that he was the first, in 1842-43, to counter Schleiden's theory that new cells developed from the nucleus. He also proved that cells always hold nitrogen compounds. Between 1844 and '46 he described the sperms on the prothalli of Ferns, though he could not understand their significance. Working on topics that were later considered as pertaining to genetics, he advanced the hypothesis of a separation between what he termed 'Specific idioplasm', responsible for the transmission of hereditary characters, and 'trophoplasm', responsible for all the other normal functions of the cells. Thus he may be considered to be a forerunner of Weismann's theory of the separation between the 'somatic' and 'germinal' lineages of cells in multicellular organisms. Again because of his interest in cell physiology, he adopted the distinction proposed by Francesco Selmi (Vignola, 1817-1881, professor in Reggio Emilia, Turin, Modena and finally Bologna) between true solutions and colloid solution, and therefore proposed the term 'Micelle'.

Richard Semon (1859-1919), a pupil of Haeckel, maintained that environmental factors operated on organisms and that there was some sort of 'somatic inductions'; these were recorded as 'engrams' which were preserved by a mysterious faculty called 'Mneme' (= memory). His theory was further developed by August Pauly (1850-1914) of München, who also maintained the existence of a natural tendency of organisms to attain definite evolutionary goals.

Again: Ludwig Plate (1862-1937), who succeeded Haeckel in Jena, advocated both natural selection and a direct influence of the environment, thus suggesting a compromise between Lamarck's and Darwin's ideas.

It is thus clear that also in Germany there was a widespread dissatisfaction with Darwin's basic ideas, and this explains the rapid diffusion of Weismann's theory.

August Weismann (1834-1914) was a pupil of Leukart and a professor in Freiburg. He may be listed, albeit indirectly, among the founders of the 'Neo-Darwinian' theory. Not to mention a group of important papers on the development of Cladoceran Crustaceans and the considerable time that he lost trying to counter Lamarckism by cutting the tails to generations of unlucky laboratory mice, thus showing that they persist in being born provided with the tail and meantime also that he had completely misunderstood Lamarck's theory, as one wonders how could he think that to cut the tails of caged laboratory mice could in any way correspond with an environmental adaptive pressure capable to influence hereditary characters!. Anyway, Weismann had the unquestionable merit to stress the early separation in many animals (but he claimed it for all and just forgot to fit plants in the picture) of the somatic and germinal cell lineages. This hypothesis had been originally advanced by A. Naussbaum, but Weismann generalised it. It was, indeed, found that in many animals the cells which will ultimately produce the gametes, become distinct at a more or less early stage from those which will evolve into the different organs.

As any biology student will remember, the most extreme instance of this separation obtains in Nematodes, where at the four-cells stage, the chromosomes of three of them become fragmented, and finally produce the entire body of the animal, while one preserves his chromosomal organisation and its destiny is to produce only gametes. In other instances, for instance in several Vertebrates, the cells that will evolve into the germinal lineage, are formed outside the embryo and later migrate into the 'Anlagen' of the gonads. They become somewhat different form the other cells of the organism and, in a sense, live on as guests, or rather 'symbionts' of the individual.

Weismann thought of the individuals as sort of boxes made to host and protect from one generation to the next the, so called, 'species idioplasm'. This he conceived as formed by units called 'determinants'. These were grouped into 'ides', which were, in turn, grouped into 'idants'. Roughly Weismann's 'determinants' correspond with what we would now call 'genes', while the 'idants' correspond with chromosomes, and, indeed Weismann was among the first to suppose that chromosomes were responsible for heredity. Weismann suggested that the two sets of 'determinants' mixed during the 'Amphimixis', a term proposed by him. As it often happens, Weismann had assumed for his theory a general validity that it does not have, though, indeed, the early separation of the somatic and germinal cell lineages is very widespread among metazoans. Weismann later completed his theory by the concept of induced germinal selection. This he largely derived from an early paper by Roux and it assumed that within the organism there is some sort of competition between the cells themselves and that they develop according the functional stresses that they receive. Thus cells and even molecules within the cells may grow and multiply according their idioplasm's capabilities, but also according the intensity by which this idioplasm is made to work, so that, eventually, weakly stimulated sections of the idioplasm may be crowded out of existence by the strong one. Indeed by this circuitous path Weismann, who struggled all his life against any variety of Lamarckism, came to reintroduce the inheritance of acquired characters, very much in a way close to that originally posed by Lamarck!

Weismann theories may well be recruited in the service of theories for evolution by internal causes as, in its original formulation, it assumed that the hereditary pool (specific idioplasm) was sheltered from environmental influences, thus it was possible to assume either an entirely planned evolution or for the Darwinians model, granted that only random variability and selection did the job.

Among the followers of Weismann, some credit had, for a while, Jan Paulus Lotsy (1867-1931), a Dutch botanist, who was for a while a professor in the United States, but finally settled in Leiden. He, while basically Darwinian and agreeing with Weismann theory, assumed that 'determinants' were unchanged since creation and that evolution was possible because of continuous rearrangements due to hybridation, This was a revival of a basically Linnean theory and, just for plants it has some justification, as stable populations of hybrid origin are not that rare among plants, and they occur also among animals, albeit much more rarely. Anyway as such a recombination occurs among already existing traits, this idea could not possibly explain the appearance of entirely new characters.

In Italy the debate on the Darwinian theory was formally opened in 1864 by Filippo de Filippi (1814-1867), who was professor of Zoology in Turin, when he gave a public lecture on the subject *Man and the Ape*. The lecture, in spite of its challenging title, was in fact an effort to clarify the Darwinian principles and their possible interpretation in connection with the origin of Man such a way as to conciliate them with the common religious feelings. Naturally it failed this last purpose and it became a battling ground at a national level, a quarrel tinged with political overtones, as, particularly in the general press, De Filippi was mainly supported by naturalists and writers from the republican and liberal left, while it was attacked by the more conservative opponents of national unity and especially by the clergy, who was complaining of the new State's encroachments on the papal states and on old privileges. De Filippi, anyway, did not contribute anything to the scientific debate: he joined on a scientific cruise by the frigate 'Magenta' and soon died in Hong Kong.

Among the active supporters of Darwinism, and of de Filippi, apart from the already mentioned novelist Fogazzaro, were, in the academic media, Michele Lessona and Giovanni Canestrini. Lessona (1823-1894) was professor in Turin and was quite a competent zoologist, working mainly on the 'lower' Vertebrates. He was also most active as a writer for popular science, so that he wrote a number of articles on animals in the children's magazine 'Giornale dei Bambini' just at the time when Collodi was publishing in the same issues his world-famous *Pinocchio*!

Giovanni Canestrini (1835-1900), professor in Padua, though providing little in the way of original contributions, did much to establish the theory of evolution in the Italian scientific media by two excellent volumes: *La teoria dell'evoluzione, come introduzione alla lettura delle opere del Darwin e dei suoi seguaci* (= *The evolutionary theory, an introduction to the reading of Darwin's and his follower's works*) (1877) and *La teoria del Darwin criticamente esposta* (= *A critical appraisal of Darwin's theory*) (1880). Both books basically support Darwin, but include some pertinent criticisms and are still useful to the historian of science as they provide a very good picture of the scientific activities in Italy just after unification.

In the years when Darwin's books were being translated in Italian, other relevant Italian Darwinians were Paolo Mantegazza (1831-1910) an anthropologist, hygienist and educator, who had a considerable impact on public opinion, but that is almost a scientific non-entity, whose only real merit was the development of the National Museum of Anthropology in Florence. The scientifically most relevant Italian Darwinian is probably Giacomo Cattaneo (1857-1926) a comparative anatomist, who spent most of his life in Genoa and who gave some good contributions.

Unquestionably the most originals of the Italian evolutionists were Daniele Rosa (1857-1944) and Ermanno Giglio Tos (1865-1926). Giglio Tos was a Piedmontese and studied in Turin, first with Lessona. He became a professor first in Cagliari, thence in Florence, Pavia, Turin, Cagliari again and finally again in Turin. Apart from a number of good contributions in the fields of entomology, on the histology of blood cells, etc. he attempted a monumental synthesis of all biology trying to build a strictly logical and mechanistic model based on the, theoretically simplest mechanisms of physics and chemistry (1900, 1902, 1904, 1910). In fact, if we discount an imagined structure of the living matter that was plausible at the time and, consequently, an imagined cell physiology, which, to our eyes is just an interesting example of complex rational speculations, he stressed the importance of symbiosis in the general framework of life, as he generalised the concept of symbiosis to the extreme, assuming that the cells originated and lived by the interaction of symbiotic molecules and so on upwards the various degrees of organismic complexity. Giglio Tos also assumed that the fundamental fact of the duplication of the basic units of living beings was the the consequence of growth by assimilation and could not possibly result in really identical new elements. Therefore this insured variability at all levels of organisation. Such variability could possibly work only if the new system of symbioses was an efficient one by itself, otherwise it would be clipped by its internal incompatibilities. Thus he attributed all progressive innovation to the development of internal variability and denied any constructive action to selection, which function he rated as a secondary, destructive one. Giglio Tos theories did not go as unnoticed in the international media as those of Rosa, but did not gain him any following, although, in spite of their excessive generalisation of the concept of symbiosis, they included an, hitherto overlooked, appreciation of the evolutionary significance of symbioses, a field of investigation that at the time was independently attracting a small number of scientists and theoricians. Giglio Tos was thence practically forgot for some fifty years, but some of his ideas have recently made a come-back, obviously in a very different perspective from that possible a century ago.

Rosa, who taught in Sassari, Florence, Turin and Modena, was basically a systematist and made important contributions to the systematics of the Oligochaeta. He was deeply conscious of that which was called ‘the crisis of Darwinism’ or ‘of evolutionism’ in the years between the end of the ‘800 and the beginning of the ‘900, when, as we have seen, a number of biologists and for very different reasons, were dissatisfied with the classic Darwinian theory. Thus Rosa proposed, first in 1909 and in its final elaboration in 1918, his theory of Hologenesis. Hologenesis was one of the evolutionary theories that were advanced as alternatives to the Darwinian theory before the advances in genetics allowed for that which was called ‘the Darwinian new synthesis’.

Rosa assumed that phylogenetic development is entirely pre-determined, apart from possible environmental influences like those assumed by Weismann, which, anyway, are of little significance. Just as the whole development of the zygote and the embryo is directed by the 'specific idioplasm', so is the evolution of taxa. A consequence of this program is that each phyletic lineage regularly branches dichotomously into a 'precocious branch' which acquires new characters, and a 'slow' branch. The function of selection being merely that to regularly clip the branches which are not fit for the environment. Two necessary consequences of this assumption are the assumptions of 'batisynphily' and of 'progressive reduction of variability': Rosa held that, just as the embryonic cells gradually specialise into the different tissues, thus there must be a gradual and inescapable reduction in the ability of each phyletic lineage to vary its structures, going parallel with the progressive specialisation of its members. Moreover the separation of the different species lineages must be extremely ancient. The reason for the lack of representatives of the modern lineages in the ancient strata being faulty evidence or, as some later hologenists, like Colosi, maintained, because the ancestors of the present species had not acquired any structure which could be preserved as fossil.

The theory was devoid of factual evidence to support it and was based on a circular argument: (a) the presumed inadequacy of the Darwinian model, which, indeed appeared liable to be faulted on the evidence then available, (b) on the assumption that dichotomic classifications were really natural ones and were sufficient evidence that evolution went by dichotomous branching.

Hologenesis had a recent revival in somewhat modified form and with other names by Croizat, who acknowledges his links with Rosa, and by Willi Hennig, who, while quoting several papers by Rosa, never quotes the Hologenetic theory, although it clearly has significant connections with his 'cladistic' theory, and in spite of having paid a visit on purpose to Colosi, the last pupil of Rosa, in Florence, shortly after World War II.

During the second half of the 19th century the United States became a prominent centre of scientific research in the field of biology. Indeed there were significant scientists even before[3], especially as far as faunal research was concerned (we may remember the contributions by Audubon, by trade a grocer, to North American Ornithology), but in fact it was immediately after the Civil War and in coincidence with the rapid colonisation of the Central and Eastern States, that a number of American biologists acquired an international renown.

Also in the United States the debate on evolutionary theories became immediately a crucial topic. We have seen how Darwin himself was for years in close mail touch with Asa Gray (1810-1888) a botanist in Harvard; Asa Gray agreeded in principle with the idea of evolution and actually championed it, but, being a deeply religious man, spent both time and energy searching for a possible agreement between evolution and deist teleology.

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[3] An important and picturesque personality in the early development of U.S. zoology, was Constantine Rafinesque (of French-German ascent), a thorough amateur, always busy describing new species both of animals and plants, mostly valid, and who, by two passing remarks in a book of 1836, qualifies as an evolutionist!

Jean-Louis-Rodolphe Agassiz (1807-1873) was a Swiss of French origin. He had among his teachers Schelling, Oken and Döllinger, the teacher of von Baer. After graduating in medicine he spent some time in Paris. He was first professor in Neuchatel, but in 1847 moved to the United States, where he made most of his really important work. His main contributions are in the fields of comparative morphology of both fossil and recent fishes (several of his findings being still commonly quoted in textbooks), and in glagiology. Actually he may be considered the first who proved the occurrence of past extensive glaciations both in Europe and in North America. He has also the merit of having created the Woods Hole institute for the study of marine biology, which shortly became one of the leading points for marine research. In the theoretic field his basic work: *Essay on Classification* still deserves serious consideration. Agassiz's logics are unbreakable where he distinguishes between natural populations, that are what actually occur in nature, and formal classification. There his stand is interesting: on one side he is a strict nominalist: systematic categories, including species are mere concepts, which purpose is to group empirical evidence, this for the practical side; however, his deep religious faith, which made Darwinism unacceptable for him, implied that the world of Nature embodied a superior design. In this sense the 'true' systematics had to be God's ideas that He had materialised in Creation, true Platonic-Augustinian ideas, which the systematist aimed to discover beyond individual and population variability.

Agassiz flatly refused the stochastic element that is pivotal in Darwin's ideas and, if evolution had to be granted, that must be the development of the plans of the Great Watchmaker. Near the end of his life Agassiz had planned an expedition to the Galapagos, hoping to discover there the evidence necessary to refute one of the best pieces of evidence that Darwin had marshalled in his batteries.

Thus, as we have seen, at the turn of the century there was a widespread dissatisfaction with Darwinism as the theory of evolution had been originally framed. The more Christian or Jewish religiously minded scientists, could agree on evolution as the development of the plans of the Supreme Being. Nothing new, as we had seen St Augustine advocating the difference between creation *in potentia* and *in actu* in the last years of the Roman Empire. It is a solution that does not hamper the biologist's or the palaeontologist's efforts to discover what happened, while to ask why God took such circuitous and dissipative ways to get us here is just as nonsensical as to ask why He allows Evil and so on. At the turn of the century, the materialist scholar was in a somewhat worst predicament: several of the criticisms addressed to Darwin appeared to be well founded on the evidence available, and the stochastic core of the theory made it appear weak in comparison with the deterministic and reductionist scientific attitudes prevailing at the time (and still very much at home in our faculties).

After all none the less than Einstein never fully accepted the 'principle of indetermination', as he himself wrote that he was disturbed by the idea of God playing dice with the world. The Greek Moirae would have been a satisfactory answer, but they and the other squabbling Gods had been long relegated among the fables by an efficient mix of wishful thinking, persecution and good propaganda.

In Russia the debate on evolution was equally prompt and fruitful, although, under Stalin's regime evolutionary theories became fully a matter of politics.

Among the Russian scholars who actively contributed to the evolutionary debate, I wish to remember Kliment Arkad'evic Timirâzef (1843-1920). He was born in St. Petersburg from a noble family; and is chiefly remembered as a plant physiologist for having established the relationship of chlorophyll with photosynthesis. A convinced positivist, he advocated for years a simplified Darwinism and social and political reform. When the revolution came, he tried to avoid the Bolshevicks, but was later induced to come to terms with them and to settle in Moscow. After his death the Communist party 'appropriated' him for propaganda purposes.

Another important Russian is Lew Semënovic Berg (1876-1950). He was a man of universal interests: geographer, botanist, gemmologist, pedologist, climatologist, biogeographer and, as a zoologist, a notable student of fishes; he was also a philosopher and a historian of science. Born in Bessarabia from a Jewish notary, in 1894 he baptised in order to gain admission to the university of Moscow. His career was as a geographer and an applied ichthyologists. He was constantly in trouble with the authorities, be they the Czar or the Bolshevicks. He was a prominent anti-Darwinian and advocated instead 'Nomogenesis', basically one of the theories of evolution by internal causes which were developed by a number of authors at the end of the XIX century and in the following years, at the time when many scholars refused the stochastic factor in evolution.

Morphology

Let us now turn to the development of the other branches of biology.

Obviously there was a constant feed-back between all such branches, but, nevertheless specialisation was unavoidably increasing.

During the second half of the century there was a steady improvement in the understanding of cell and tissue structures and a parallel development of embryology. This was much fostered by the development of fixing, cutting and staining techniques, the first step being the introduction of carmine staining by Joseph Gerlach in 1852. As far as microtomes are concerned a special mention deserves the Prussian Bogislaus Reichert (1811-1883): Reichert was a pupil of both von Baer and Johan Müller. He gave several good contributions to descriptive histology, but he is chiefly remembered as the inventor of the rotative microtome, which, with obvious improvements, is basically still with us and which was of great help in the development of serial sectioning techniques, a major advance for embryological studies.

In order to follow the main lines of the development of histology, let us consider separately what concerns the nucleus and the cytoplasm.

The first who clearly understood the function of the nucleus was Franz Leydig (1821-1905), a professor in Bonn, while Robert Remak (1815-1865) of Posen, one of the many excellent pupils of Johan Müller, in 1852 confirmed the division of the nuclei. Remak, moreover deserves to be mentioned as the discoverer of the synaptic nervous fibers which were named after him, and being one of the first investigators of the differentiation of nervous cells.

Eduard Strasburger, though born in Varsaw, was professor of botany in Bonn. In 1875 he described nuclear chromatin and provided a first description of mithosis. These were both independently described by Walter Flemming (1843-1905), professor of anatomy in Prag, in 1879 and 1882. Flemming will be mentioned further on as an advocate of the filamentous theory of cytoplasms.

Further basic advances were done by Eduard van Beneden (1845-1910), professor in Louvain, Belgium, who described meiosis (which had already been postulated by Weismann on theoretical considerations) and who in 1883 well understood its significance.

Theodor Boveri (1862-1915), professor in Würzburg, studied the centrosome and in 1888 described the functioning of the achromatic sphere and fuse in the reproduction of the cell. Moreover he, during researches done at the biological station in Naples, was able to show that chromosomes (a name proposed by von Waldeyer-Harz in 1888) in a cell had different shapes and lengths. Finally heterochromosomes were first described by H. Henking (1858-1942) and their function was correctly understood by Mc Clugh (1870-1946), studying grassoppers, in 1902, and by E.B. Wilson (1856-1939), of Columbia University of New York, and by Miss N. M. Stevens[4]. They, in 1905, studying the males of *Protenor,* and the females of *Lygaeus* gave a full description of meiotic reduction and of its significance.

Coming to consider cytoplasmatic structures, we must remember Hugo Mohl (1805-1872) from Stockhard and professor at Bern and later at Tübingen, who revived the term ‘protoplasm’, which he conceived very much like Purkinjie.

Max Schultze (1825-1874) from Freiburg , studied in Berlin with Johan Müller and was later professor in Bonn. As a zoologist he studied mainly ‘worms’, Molluscs and Protozoans, but his main contributions are as a histologist: in a paper of 1861 (*Ueber der Muskelkörpchen und was man eine Zelle zu nennen habe*) he improved the concept of animal cell and argued that in animal cells their surface was not necessarily limited by a membrane like that of plant cells. Finally he defined the concept of syncytial and plas-

[4] Probably the first women to publish contributions of microscopic anatomy in the 19th century were Rina Monti (1871-1937), professor of comparative anatomy in Pavia, Siena, Sassari and Milan, wife of the fellow of the Academia dei Lincei Augusto Stella, and Maria Sacchi (1863-1957), the wife and assistant of Giacomo Cattaneo, who had graduated in Pavia in 1885.

modial cells, where, like in striated muscles, such delimited structures as we can see have many nuclei. This contribution was essential in order to dispose of the idea of several scholars, like, as we mentioned, Milne-Edwards, that, as in several instances, no cell limits are visible, thence the cellular structure was typical only of plants, where cellular walls had been clearly seen since about two centuries, while in animals cells, where cell limits were often either obscure or invisible with the common microscope, cellular structure could well be transitory, and occurred only in the early stages of development.

As different histological techniques were being developed, there arose a critical problem: which was the basic structure of cytotoplasms? At the time there were three main theses: the 'granular theory', advocated mainly by Richard Altmann (1852-1901), professor in Leipzig, and later developed by Leopoldo Maggi (1840-1905), professor in Pavia; the 'alveolar theory' (proposed by the protozoologist Otto Bütschli (1848-1920); and a 'filamentous theory', advocated, as we said, by Flemming.

Finally Michael Heidenhain (1862-1949) son of Rudolf Heidenhain (1834-1897), maintained that all three types of structures could well occur in the cells. However, only the advent of the electronic microscope was to provide the final answers as to the true nature of these and of other cell structures, like Golgi's apparatus, mitochondria (which had been discovered in 1897 by Benda), the centrosomes etc, as well as about their real permanence or presence in all cells and during the whole cell life or only during some stages of it.

Likewise mitochondria had been taken by some authors as evidence for the granular structure of cytoplasms, while other scholars, because of the fact that they had the same affinities for hystologic stains as bacteria, supposed that they were symbionts; again only electron microscope and refined biochemical investigations were to prove their true nature and functions.

Because of the obvious significance of human histology for medicine, the number of investigators dealing with the structure of different organisms was in direct proportion with the presumed similarity of the animals with man or of man's parasites. Thus, and until now, invertebrate histology was comparatively overlooked. This was also the consequence of special difficulties in the preparation of invertebrate tissues, the exceedingly small cells of several taxa etc. It is therefore worth of special mention Franz Leydig, from Württenberg (1821-1905), who made the study of invertebrate histology his special subject.

In the field of Vertebrate histology we may recall Rudolf Albert Kölliker (Zürich, 1817-1905), another pupil of Johan Müller and of Henle. He was professor in Würzburg, and we have already mentioned him. He wrote some excellent treatises on histology and embryology, who were the blueprint for all later treatises. Kölliker has the merit of having established the cellular nature of sperms and eggs, which finally clarified their function in fertilisation. Moreover Kölliker, since around 1850, had strongly advocated the function of the nucleus as the repository of hereditary characters.

At this time a special interest was devoted to the study of the nervous tissues.

Interest for the development and function of the nervous tissues goes back to antiquity, and that for obvious physiologic and medical reasons. We have indeed mentioned Galen's experiments with nerves. Clearly there was much interest for the mechanisms of repair of damaged nerves, and the first who made some substantial contributions to the understanding of the problems of the regeneration of axons, was Augustus Volney Waller (1816-1870) in 1851.

Camillo Golgi (1844-1926) was professor of pathology in Pavia. Nowadays he is mainly remembered for his discovery in the cells of the 'Golgi's apparatus' (which precise structure and function was much later established by ultrastructural investigations), and by his important contributions to the study of the cycle of the Malaria parasite. Golgi developed a new technique for the staining of individual nervous cells which, as it made possible to investigate the precise distribution of even the finest branches of individual nervous cells, allowed Golgi himself and a number of other scholars, to better understand the organisation of the different regions of the nervous system.

Shortly afterwards the Spaniard Stantjago Ramon y Cajal (Petilla de Aràgon, 1852-1934) developed other techniques which allowed for a better understanding of the internal structure of the nervous cells. Thereafter Ramon y Cajal advanced the, so called, 'neuron's theory' (the term neuron had been proposed by A.W. von Waldeyer-Harz in 1891, the same who, in 1888, had proposed the term 'Chromosome'). According the theory, all nervous messages can travel through the nervous cell only from the terminations of the dendrites, which usually are short and numerous ramifications of the cell, towards the ends of the neurite, which is a single, differentiated and ramified branch of the same cell. Golgi was never fully persuaded by the theory, though it allows to provide a clear explanation of a number of typical features of the working of the nervous tissue.

A true pioneer in the study of peripheric nervous terminations worth remembering here, though his important contributions were done early in the century, was the Tuscan Filippo Pacini (1812-1883). Pacini was first 'assistant' to the zoologist and anatomist Paolo Savi in Pisa, thence he became in 1847, professor of human anatomy, and later (1849) of topographic anatomy and histology in Florence. Pacini was the first to describe the peripheric nervous terminations in sensory organs ('Pacini' tactile bodies). He discovered them when still a student in Florence (1835), but published his observations only in 1840. His last anatomical contribution was on the electric organ of the fish *Gymnotus* published in 1852. His later work was entirely on the pathology of Asiatic cholera, then a permanent threat and in 1854 he saw and figured the vibrio which was independently discovered in 1884 by Robert Koch, who established it as the causing agent of the disease: Pacini's observations had gone completely unnoticed.

The advances in the study of nerve cells by Golgi and by Ramon y Cajal tallied well with the classification of cells proposed by Giulio Bizzozzero (1846-1901), pathologist and histologist in Turin and, himself, a student of the blood-cells and platelets, who had sub-divided the cells in 'labile', which were continually substituted by the proliferation and differentiation of other cells, 'stable' which, although usually not multiplying in the adult, yet they could reproduce under special conditions, and 'perennial', which reached their final number rather early in the life of the organism and where, thence, totally incapable to multiply. Such appeared to be the nervous cells, who could, at most, regenerate their branches.

Among the many histologists of value of this age, we may quote the Hungarian Stefan Apathy (1863-1923), the Swede Emil Holmgren (1866-1922), the German Franz Nissl (1860-1919), the British Charles Ernest Overton (1865-1933) a pioneer of cell biololecular physiology, the French Louis Antoine Ranvier (1835-1922).

A special position is that of the Russian Il'ja Mecnikov (1845-1916). Though born in the landed aristocracy, since his youth he was somewhat of a radical and thus he had repeated problems with the authorities. Greatly interested in evolutionary theories, all the first part of his scientific career was devoted to studies on the invertebrate morphology, embryology, physiology and life cycles. Apart a number of notable observations, his main discoveries during this period were the alternating generations, parthenogenetic and myctic, in some Nematodes and the intracellular digestion of Plathyhelminthes, this last discovery forecasting his main interests and discoveries of the later part of his life. He worked repeatedly in Germany and in Naples and held chairs in Russia, until he was forced into exile with his family. He then went to Messina (where he had the chair of zoology) and there discovered the white cells' of the blood power of phagocytosis: the power to engulf and digest foreign corpuscles entering the blood, and especially bacteria. This discovery was the first step towards the understanding of immunity. He thence, after a new brief spell at the university of Odessa, went to the Pasteur institute of Paris, where he spent the rest of his life, entirely engaged in the study of immunology and on the intestinal flora and its significance for digestion.

Embryology

As usual, to deal on this subject as of a special section is by and large, artificial, as several of the scholars mentioned so far have made also significant contributions to embryology. Anyway the mystery of the progressive differentiation of organisms starting from what appeared a single, undifferentiated beginning had always fascinated biologists.

In the early years of the period covered by this chapter, the only approach to the problem was mere description and scholars were surprised by the amount of differences in development among the different organisms. Clearly these had to be explained in the light of evolution and by the additional support of von Baer's and

Haeckel's theories. Later scholars began to ask themselves the problem of the mechanisms which regulate differentiation and to try experimental approaches.

As we saw in the previous chapter the theory of the embryonic leaflets had been extended to invertebrates and, as we said, in 1849 Huxley had suggested that the two cellular layers which may be distinguished in the simplest of the Coelenterata (jellyfishes, sea-anemones and polyps) corresponded with the ecto- and entoderm of embryos of higher phyla. This theory, like others that we shall meet with in this chapter, remained standard until not many years ago and if any of my readers has had to pass an examination in zoology, he must have studied it. Huxley assumed that Coelenterates had no true mesoderm, an assumption that can be maintained for most of these animals, but that does not hold for all; Huxley thought this character to be highly significant and thus introduced embryological evidence into systematics.

Shortly afterwards the Austrians Berthold Hatscheck (1854-1941), from Kirwein (Skrben, Moravia) and Karl Grobben (1854-1945) from Brünn, again on embryological evidence, introduced the distinction of 'triblastic' metazoans into protostomes and deuterostomes, a division which absolute value is still disputed, as, anyway, there is a certain number of phyla that are continuously shifted to and from among the two.

Arnold Lang (1855-1914), Swiss, after his studies in Switzerland, was for years at the Stazione Zoologica of Naples and afterwards a faithful pupil of Haeckel in Jena, where he got a chair of animal phylogeny. Later he went back to the University of Zürich, where he died. Though less known, Lang may be considered for the comparative morphology of the invertebrates as much the equivalent of Gegenbaur in the field of vertebrate morphology.

Carl Gegenbaur (from an old and rich family in Würzburg, 1826-1903), was a pupil of Kölliker, then professor first in Jena, where he collaborated with Haeckel, and later in Heidelberg: After some early and important studies on the anatomy of different marine invertebrates, that he begun during an trip with Kölliker to collect animals in the Mediterranean, he turned completely to vertebrate morphology considering all developmental stages. In 1861 he was able to prove that even in yolky eggs, such as those of birds or selachians, the true egg is a single cell, the early germinative disk having been previously misunderstood for the egg itself. He thoroughly described the early stages of segmentation and gastrulation in different animals, thus confirming and completing what had been described by Rusconi thirty years before (and which Gegenbaur did not know). Among his main contributions one must mention his work on the old problem of segmentarity of the head. Here he finally disproved the 'vertebral theory of the skull', as he was able to prove that only a small part of it (parachordals, part of the otic capsules and tectum synoticum are, indeed, homologous with vertebral structures, while dermal bones are actually highly modified homologues of scales. He also proved that some nerves and the muscles of the eye are segmental structures However he originally went wrong as he tried to equate the other cartilage structures, and the bones deriving from them, as ribs, while they actually are linked with gill structures and ultimately derive from the neuroectoderm of the neural crests, which cells are also responsible of the development of major parts of the nervous system, and, at least in some fishes partly from placodes of the lateral line. Another famous, and wrong, theory by Gegenbaur concerns the evolution of the terapod linbs, which he supposed to have evolved from a basic structure comparable with the fins of the Dipnoan *Neoceratodus* and of some fossil sharks.

Gegenbaur had a powerful personality, albeit a rather dogmatic one, a friend to his friends, and especially to his pupils, who were many and excellent, and a dangerous enemy for his enemies. With Haeckel he was never on true friendly relations, but their relationships definitely cooled off when Haeckel became more and more involved with the politics of science.

Among Gegenbaur pupils four deserve special mention:

W. Hubrecht (1853-1915), explorer and collector; he studied with Gegenbaur and was later professor in Utrecht. Originally interested in invertebrates, later he concentrated on the evolution of Mammals and suggested considerable improvements in phylogeny.

Max Fürbringer, (1846-1920), was the immediate successor of Gegenbaur and gave important contributions in many fields of comparative anatomy. Fürbringer was the real founder of functional morphology, especially by his studies on the mechanics of the skull.

Jan Versluyis (1873-1939), whose most notable achievements were in skull morphology, where he emphasised the functional aspects as prerequisite for the understanding of evolutionary changes.

Finally Hans Friedrich Gadow (1855-1928), born in Germany, but who moved to Great Britain in 1880, first working at the British Museum and later in Cambridge. He, eventually, became a British citizen. Gadow was a most eminent Vertebrate morphologist, especially of Birds, and one of his merits was to be not only a morphologist, but an all round naturalist.

To the list of the notable pupils of Gegenbaur may be added the name of the English Edwin Ray Lankester (1847-1929). He was the son of a well known botanist and microscopist. He enjoyed all facilities in education and schooling: He studied both at Oxford and Cambridge, later in Vienna, thence in Leipzig with Ludwig, in Jena with Gegenbaur and Haeckel, at Naples with Dohrn and, back in England, with Huxley, and well deserved of all of them, as he was able to publish his first paper when barely 15. Ray Lankaster (later Sir) was appointed as a professor in London (to begin with without salary) in 1872, when 25. Appointed to Edinburgh, he renounced the chair and returned to London. Later he became professor of Anatomy in Oxford and from 1898 to 1907 was director of the British Museum Natural History. Though basically a conservative, he was a personal friend to Karl Marx and one of the nine people that attended his funerals.

Lankester was a distinguished embryologist and evolutionist who worked mainly on the Articulata and the fishes, he especially elaborated the 'coelom' theory.

The true legacy of Gegenbaur remains his further development of the concept of 'Bauplan' to its modern shape. In his view of Bauplan there is a practical most fruitful synthesis of comparison with archetypal structures, be it as whole organisms or as single organs, as they may have occurred in past and more primitive organisms, consideration for natural variability, within the limits set by the pre-existing structural plan, and of the function of natural selection.

Other outstanding morphologists of this age were Robert Eugen Gaupp (1865-1936) and Erwin Goodrich (1868-1946), whose activities, however, ranged well beyond the time limits of this chapter.

Among the many descriptive embryologists more properly pertaining to this period, perhaps the most notables were Francis Maitland Balfour (Edinburgh, 1851-1882), H.A.W. Hubrecht (1853-1915), W. Lemche (1850-), a Swede who concentrated on 'lower' vertebrates, the Dane J.E.V. Boas, Anton Dohrn (Stettin, 1840-1909), who is mainly remembered for having established the 'Zoological Station of Naples', where were done so many capital studies, but who made important contributions himself on selachian embryology.

A particular significance had the studies by the Russian Alexandr Onufrievic Kovalevskij (1840-1901). He was born from the landed aristocracy and was a very precocious scholar, many of his main contributions being published when he was between 24 and 34 years old. His studies ranged on the morphology and embryology of an extraordinary variety of invertebrates and, for this purpose, even while holding chairs in Russia, he was often on the move through many of the zoological institutions of Europe. Actually his most critical papers on the development of the Lancelet and of Tunicates were done at the Stazione Zoologica of Naples. Although his contributions were notable on all the range of animals studies (for instance those on the metamorphosis of Insects had a great and lasting effect for the understanding of this complex phenomenon), he is mainly remembered for his studies on the development of chordates and especially on the development of the notochord itself. Kovalevskij was a supporter both of recapitulation and of the germ layers theory, which, however he considerably modified, to the annoyance of Von Baer, and made detailed studies on the ontogeny both of Tunicates and the Lancets (Acrania). His studies established beyond doubt the relationship of both groups to typical Chordates and, until recently some dissent was voiced on the evidence of recently discovered Cambrian fossils, on the rather close affinity of Lancelets to Vertebrates.

Strictly linked with the development of embryology was the study of fertilisation.

We have mentioned how J.-L. Prevost and J.B.-A. Dumas in 1824, by using filtrated semen proved that fertilisation was due to the sperms. Thus, and having to consider that both the egg and the sperm had been proved to be cells, the problem was to discover the mechanisms of fertilisation.

In 1850 George Newport, an amateur biologist, discovered the micropyle of the Amphibian egg (the micropyle being a tiny, specialised part of the egg' surface, that in several animals is the only spot through which the sperms can enter the egg). In 1875 Oskar Hertwig (Friedberg, 1849-1922), whom we have already mentioned, described the cariogamy, that is the fusion of the paternal and maternal nuclei in the eggs of Sea-urchins. The actual penetration of the sperm into the egg, was described by Herman Fol (1845-1892), born in Paris, but a Swiss citizen.

Meantime the fertilisation in plants had been described in 1855 by the botanist Nathanael Pringsheim (1823-1894.) in the unicellular genus *Vaucheria* and between 1856 and 58 in the freshwater genus *Oegonium*, while the complex mechanisms of fertilisation in plants were more completely described by the French L. Guignard (1852-1928) and by the Russian S.G. Navaschin (1857-1930).

Theodor Boveri, a Swiss, in 1892, completely described some typical aspects of both spermagenesis and ovogenesis, while in 1894 Eduard Adolf Strasburger was able to explain the mechanisms of gametogenesis in plants.

However the achievements of descriptive embryology in the light of evolutionary theory, still left entirely unanswered all questions as how was development determined on some lines rather than on any other and how began the segmentation of the egg.

This was the main field pursued by early experimental embryology and developmental mechanics.

We have mentioned how É. Geoffroy St. Hilaire and his collaborators had made the first rudimentary, yet to some extent successful attempts to interfere with the natural development of embryos.

In a short time experimental embryology largely displaced descriptive embryology. The reasons for this deserve some consideration. Experimental embryology had several reasons of appeal: there was first a very serious scientific reason, as it is never sufficient just to describe phenomena, it is important to understand how and why they happen; a second reason was the very human tendency of the brilliant youngsters to do something different from what had been done by their old masters. A third reason was a basic cultural mistake, for which the main responsibility rested on descriptive embryologists: they had studied a very limited range of animals, this being due to very practical reasons: first the availability of materials allowing research without incurring the risk of delays due either to technical difficulties in their preparation or in order to gather the materials themselves. However most descriptive embryologists had boldly generalised such results as they had gathered, without bothering that they were assuming that but a few animals could supply a sufficiently exaustive evidence. Thus many people thought that such problems as could be studied by merely descriptive methods had been solved and that there was little scope left for this kind of research. Finally one reason for the appeal of explerimental embryology was the wish for an approach to problems that would allow morphology to approach to the research patterns more typical of orthodox scientific research and less similar to that of the historians, who merely look through the archives, searching for some unknown document supporting some more or less subjective interpretation of the past. It is notable that this last problem is still a bone of contention of philosophers of science: does research on evolution belong with the experimental or with the historical studies?

Such was the background for the development of experimental embryology.

In 1874 Wilhelm His (Basel 1831-1904), an excellent anatomist who discovered the 'His bundle' or auriculo-ventricular conductive bundle, published a most important contribution: *Unsere Körperform und das physiologische Problem ihrer Entstehung* (*Our corporeal structure and the physiological problem of its formation*). The text has no pity, in its sarcastic tone, for the pseudo-explanations that had been offered, which were extremely weak especially when considered from a mechanistic standpoint. His suggested to complete the theory of embryonic layers by the concept of 'organ-forming areas' both in the egg and in the embryo and emphasised the significance that in morphogenesis have the bending of cellular layers. His especially denounced the 'fundamental biogenetic law', which to most embryiologists had become an article of faith, while it said nothing as to why happened what one sees to happen.

Alexander Wilhelm Goette (St. Petersburg, 1840-1922) belonged to that Balt aristocracy that gave to the Czar's Russia so many scientists. He studied first in Dorpat and thence in Göttingen and was first professor in Strassburg (then in German territory). His special field was the embryology of Amphibians and he strongly supported His.

Another Balt was Nikolaus Kleinenberg (Jeglava, Latvia, 1842-1897), who was also an embryologist, but not a merely descriptive one, indeed he was not exclusively interested in phylogenetic reconstructions. He studied with Dohrn in Naples and was later appointed as professor of zoology in Messina and Palermo. Kleinenberg was a student of invertebrate embryology and made a punctilious and scathing criticism of Haeckel's 'gastrea theory', he also denied a general validity to the theory of coeloms and even challenged the very existence of mesoderms! His work, at the time, was scarcely considered, being, as it was, too heterodox, so that Kleinenberg was soon almost forgot. Yet seen in the light of present knowledge, he was on the right track on several issues.

The first true experimental embryologist was Wilhelm Roux (Jena, 1850-1924), from a lower middle class family, his father being a fencing master; he studied in Jena with Haeckel and in Strassburg with Goette; in Jena he was also strongly influenced by the teaching of Wilhelm Preyer (1842-1897), an embryologist interested in the physiology of embryos. He became a professor first in Innsbruck and later at Halle. A tireless worker and as tireless a polemist (not only in biology), his achievements and verve attracted a number of young scholars from all over the world. Roux was, to begin with, strongly influenced by the mechanist credo of Haeckel and his first researches were in the field of the functional morphology of vertebrates (relative proportions of different muscles, mechanical aspects of the tail of dolphins), but he soon felt that Haeckel approach was experimentally poor and more often than not his arguments marred by circularity with his one-sided emphasis on phylogenetic causation. Actually Roux first experiments on frog's embryos were a repetition of one attempted and failed by Haeckel himself: to kill one of the first two blastomeres formed by the first division of the fertilised egg. Roux did improve the technique and, by puncturing one of the blastomeres with a sterilised needle, he was able to show that, while the surviving blastomere evolved regularly for some time, even into a regular half gastrula, the half embryo corresponding to the killed blastomere became a sort of scar of jumbled cells. Roux results matched with those of the French I. Chabry (1855-1893) who, by separating the blastomeres of the Ascidians had found them to develop into half embryos. Roux, who had early adopted a strict Weismannian view of the separation of the somatic and germinal lineages, since 1881 assumed that the mutual influence of the various parts of the developing embryo was in some ways analogous with a selection process. However he soon began advocating the idea of the 'mosaic egg', that is: the different future organ forming substances were parcelled in the egg, so that they came to be differently allocated to each cell by the successive divisions. In a way this revived the ancient idea of 'pre-formation', that we met in the previous chapters. His passionate belief in a strictly materialistic-deterministic developmental biology led him to choose for this approach to research the term 'Entwicklungsmekanik', roughly 'developmental mechanics' and to create a journal (*Archiv für Entwicklungsmekanik*) to propagate his credo. The subsequent work by Roux was devoted to the refinement and multiplication of his experiments and to the effort to bring the experimental results of his critics in line with his theories.

Indeed Roux was immediately attacked by Oskar Hertwig, who being a convinced vitalist and an equally convinced critic of Weismann, could in no way accept Roux results and their interpretation. He repeated the experiments of Roux, but, instead of killing one blastomere, he succeeded in separating the first two blastomeres and saw that they usually developed into two complete, albeit smaller embryos.

Anyway, the foremost opponent to Roux was Hans Driesch (Keruznach, 1867-1941). He also had studied with Haeckel and, for a while, with Roux himself. He became a professor in Heidelberg, Köln and finally completely abandoned biology to become professor of philosophy in Leipzig. In 1891, while at the 'Stazione' in Naples, he was able to separate not only the first two, but the first four blastomeres in the embryos of some sea-urchins and prove that they developed into complete, if small embryos (but it was later found that at later stages each group of cells became strictly determinate into giving only certain tissues, just as Roux's theory demanded); he also succeeded in uniting two fertilised eggs and thus obtained regular, but bigger larvae. Driesch also worked on the Ascidian *Clavelina*, and, again, even by using the cells which normally produce only the branchial basket, he obtained smaller, but complete embryos. He was able to show that Chabry's results could be obtained in some species only, even within a single class).

To account for his results, Driesch opposed to Roux's theory of the 'mosaic egg', the theory of 'regulative egg', assuming that the developing embryo, being initially essentially a homogeneous equipotential system, rearranged itself reacting to external factors. Thus Driesch developed the complementary concepts of 'prospective value' and of 'potential value', the prospective value being the normal destiny of each part of the embryo or even of the egg, while the 'potential value' was its ability to cope with 'emergencies' by rearranging itself. A typical example of regulation being uniovular twins that result when the earliest blastomeres become separated to produce two complete babies.

The two opposed theories having been essentially stated between 1890 and 1900, they produced a growing flood of new research in the following decades, while their proponents did not produce anything new themselves.

So, for instance, Yves Delage (1854-1920) devised new methods for activating the egg.

The German Hans Spemann (1869-1941) was able to procure a critical advance in this field. An experimental embryologist, he was able to transplant parts of developing embryos from one place to another in the embryo itself or to graft on an embryo parts of other embryos. The results showed that certain groups of cells of the embryo, for instance the dorsal lip of the blastopore, were able to induce the neighbouring cells to develop into given structures, Thus was born the concept of 'organiser': embryonic structures which could direct the development of other embryonic structures. Being considerably interested in general theoretical problems (he was a friend of Heidegger), Spemann developed ideas that merge developmental mechanics with Boveri's psycho-Lamarckism and Driesch's vitalism.

Another important issue that was tackled in the same years was that of the mechanism which initiated the segmentation of the egg.

By 1866 the Russian Tichomiroff had succeeded to provoke the segmentation of the unfertilised egg of the silkworm.

In 1869 Eugène Bataillon, professor in Montpellier, succeeded into getting the segmentation of frog's eggs by simply picking them with a fine needle, the segmentation in rare instances did even achieve the development of complete tadpoles and in 1 every 10,000 instances the tadpoles metamorphosed into a froglet.

Jacques Loeb (Mayen, 1859-1924), who emigrated to the United States and eventually was a professor at Chicago, Berkley and, finally worked at the Rockfeller Institute, in 1899 succeeded in obtaining the segmentation of unfertilised eggs of sea-urchins by treating them with fat acids and hypertonic sea-water. He even got some living larvae.

Closely linked with the problems of reproduction and differentiation are the problems of the growth of cells in artificial media. These will be merely mentioned here as practically all the work on them was developed after World War I.

The first successful vitro cultures of cells were obtained by the American Ross G. Harrison (1870-1959) in 1907 and shortly later by Alexis Carrel (Sainte-Foy-les Lyon, 1873-1944).

As it is obvious and as we have already stressed, practically all hystologists and embryologists shared with zoologists and botanists an interest for the development of phylogenetically sound classifications and, thus, the division between embryologists, systematists, etc. is largely artificial and one either has to consider the same persons under different headings or has to list each scholar according their most significant results.

Animal systematic

Both the accumulation of new evidence and the new evolutionary perspectives had to impinge on both zoological and botanical systematics and thus important new proposals multiplied.

An important contribution was first made by Rudolf Leuckart (Helmstädt, 1822-1898) when still quite young, and is therefore pre-Darwinian in outlook. We have already repatedly, incidentally, mentioned Leuckart; he was the son of a businessman and was first a student and then a lecturer in Göttingen, before being appointed as a professor of zoology first in Giessen and finally in Leipzig. His outlook was 'Cuvierian' in that he considered the animal kingdom to be split in several basically different groups, but he was immediately aware of the many faults of Cuvier's embranchements. He thus reconsidered the more split classification proposed by Lamarck and advocated the division of the supposed 'Radiata' into the Coelenterates (into which he included also the sponges) and Echinoderms. Moreover he gave essential contributions to the understanding of the morphology of the colonial Coelenterates. He also clarified the biological cycle of several parasitic 'worms', both tape- and roundworms, and having discovered the micropyle in the insect egg (it had been already discovered by J. Müller in the eggs of sea-urchins) was able to prove that the sperms enter through it, thus contributing an essential advance to the understanding of fertilisation. His human virtues of sympathy and consideration attracted a number of pupils to his laboratory, some truly outstanding. Anyway, Leuckardt classification, albeit leaving several phyla grouped with entirely unrelated ones, as with the Sponges united to the Coelenterata, was the first major advance on Cuvier's and, moreover it implied a methodological approach which proved to be most productive.

As most advances in systematic were proposed in the framework of studies in comparative anatomy, we have already mentioned a number of scholars who contributed to the amendment and improvement of both animal and botanical systematics.

A scholar who delved successfully in a number of disparate fields was Giovanni Battista Grassi; (Como, 1854-1925), he contributed important advances in disparate fields of biology, while he never dealt with general problems. Grassi came from a very poor family of farmers and was through his life an uncouth, rude and assertive man and this almost cut his career as, being a pupil at the Collegio Ghislieri of the University of Pavia, where his merit and his rather destitute conditions had earned him a scholarship, he was expelled for indiscipline! He managed, nevertheless to complete his medical studies and eventually became first professor of Zoology in Catania and later of Comparative anatomy in Rome. His scientific achievements got him also the appointment as Senator. Also as a legislator he made himself useful, while his rudeness and polemical character probably prevented his sharing in the Nobel prize. We shall deal again with Grassi's contributions in the fields of the study of life cycles and of parasitology. As a zoologist his first papers concerned the Chaetognatha, the morphology of different Arthropods: Myriapods, Insects, his main subject being the Thysanura, and include the discovery of the arachnid order Palpigrada and fundamental studies on the biology of Termites. In the framework of brilliant studies on Fishes, he finally solved the age-long problem of the metamorphosis of Eels. In fact he was able first to clarify the ontogeny of Murenas, which are sedentary in the Mediterranean, and thence, partly with the material help of his assistant Calandruccio, of Eels, though he could not identify their reproductive area in the Sargasso sea. Typically during these researches he quarrelled with and fired Calandruccio and thence the two engaged into an unpleasant argument on priorities.

The study of life cycles, of synbioses and of parasitology

During the years covered by this chapter there was a dramatic increase in the knowledge of the developmental cycles of both parasitic and non parasitic animals.

Among those who acquired the greatest merits we must remember, first of all, Rudolf Leuckart, whom we already mentioned. He discovered the life cycles of several tapeworms, some being parasites of man, other of domesticated animals or both, of *Trichinella* among roundworms, etc. His treatise of human parasitology (1863) was an epoch-making publication and from it stemmed a number of new researches which are continuing today and which eventually clarified the cycles of a number of parasites of man, domesticated and wild animals and plants. Quite apart from their practical value, we owe to them the present possibility to asses, in many cases, the evolution and affinities of the various taxa.

The leading figures in this field were Sir Patrik Manson (1844-1922) and Joseph Bancroft (1836-1894) who both studies the cycles of various tropical filarial worms, and Raphael Blanchard (1857-1919). Their studies were prompted by the emergencies arising during colonial expansion in tropical areas where the various metazoan parasites are particularly common and varied. Manson was a Scot and graduated in Aberdeen. As soon as he graduated he went to China; he became interested in parasitology after reading a paper on roundworms during a visit to England in 1875. Both his most notable discoveries, the life cycle of *Filaria (=Wuchereria) bancrofti* and the lung Trematode *Paragonimus* were done in China. As a practitioner he was extremely successful and in 1889 he retired a rich man to Scotland. However, devaluation of Chinese currency soon cut his fortune, so that he had to resume his medical activities and he begun additional lectures and resumed research. Having become interested in Malaria, he was soon convinced that it was transmitted by some sort of mosquito and was thus able to suggest to Ross the proper approach to the problem.

Among the Italian parasitologists special mention deserve Angelo Dubini (Milan, 1813-1902) who discovered the microscopic fungi who produce some types of scab and, in 1843, the very dangerous worm *Ancylostoma duodenale* (later Perroncito proved that it was the causative agent of the so called 'miner's anemy'). Battista Grassi was also a parasitologist of value and studied the cycles of various round and tapeworms.

A particularly interesting discovery was made by Geoffrey W. Smith while working at the 'Stazione' of Naples: the parasitic castration of crabs by the parasitic crustacean *Sacculina*, with subsequent loss of male sexual characters and acquisition of female-like features by the parasitised animal. Again a discovery which prompted a whole series of researches and the additional identification of a number of more or less similar conditions.

Strictly linked with the problems of parasitism are those concerning symbiosis, that is of these necessary relationships between organisms which are either mutually advantageous of, at least, that do not damage either organisms involved.

The term 'symbiosis' was proposed by the botanist Anton Heinrich de Bary (Frankfurt, 1831-1888), who first proved Lichens to be 'plants' which are, in fact, made by a meshwork of fungal iphae, within which live special kinds of Algae. Each Lichen 'species' being characterised by its own special association of different fungi and algae. Soon followed the discovery of a number of such symbioses of all sorts: between different 'plants', between plants and animals, between plants and bacteria, between animals, between animals and fungi etc. For instance in 1909 Umberto Pierantoni (1876-1959) discovered that several insects, mainly wood-eating beetles, have special structures which house some fungi, the 'mycetomes' and it was later proved that thus the beetle may transfer the fungi from one tree to another, thence the fungi grow in the galleries dug by the beetle and play an essential part in the feeding of the beetle itself. In these, as in other instances special adaptations were discovered allowing for the transmission of the symbionts from one generation to the next. Darwin himself had paved the way by his studies on Orchids, which showed the special adaptations of these flowers to help the collect and transport of pollen by insects from one to another flower.

Equally important were the studies of Federico Delpino (1833-1905), who begun his botanical researches as an amateur, but later became a professor of botany. While as a botanist he may be remembered for his studies on the causes and mathematical regularities in fillotaxis, his studies on the symbioses between plants and ants (1883-1896) are of crucial significance. Delpino was basically anti-Darwinian, though between Darwin and himself there was mutual appreciation and good, albeit not close, personal relations. Delpino was decidedly an evolutionist and a student of adaptation; and his attitude may be explained by the difficulty for the scholar of such co-evolutions of special organs of completely different organisms such as Acacias and the ants inhabiting them, to imagine how this could be brought about by the interplay of random variability and selection. After all even the different evolutionary models of evolution presently available are not entirely satisfactory for dealing with these particular instances.

In the same years, and just to pick up a few examples, A.B. Frank (1839-1900) and others studied the mychorrhizae, associations of fungi with the roots of higher plants, while an immense practical value had the complex cycle of researches, which involved a number of scholars (Schulz-Lupitz, Hellriegel, Berthelot, Beijerninck and others) and which brought about the discovery of the nitrogen fixing bacteria and their essential relationships with the roots of leguminous plants.

Speaking of symbioses it is usual to quote the name of Prince Petr Alekseevic Kropotkin (1842-1921). Kropotkin was not a biologist and his only scientific work was as a geographer in his youth. Having renounced his aristocratic privileges in 1871 and as an anarchist, he was soon jailed, evaded and spent the rest of his life as an exile, going back to Russia after the revolution, as a preacher of anarchy. He, probably, was saved from the attentions of the communist regime by his death in 1921. Anyway, during his exile, he wrote on many scientific problems, though not in a technical framework and his book *Mutual Aid*, where he collected several previous essays and where he maintained the great importance of all sorts of symbioses in evolution and criticised the stress on the 'struggle for survival' by orthodox Darwinians, though clearly dependent on his political views, attracted much attention and was translated in several languages. His views both in politics and on symbioses did also fit with his neo-Lamarckian faith

Botany

For a better understanding of the developments of botany, we must first move a step backwards and remember Stephanus Ladislaus Endlicher (1805-1849), born in a rich family of Pressburg, he was at the same time both an excellent botanist and notable scholar of Chinese language! As a professor of Botany and head of the botanical gardens he paid from his own pocket both the publication of a botanical journal and other scientific activities. In spite of his popularity with the students, being a conservative, he was finally chased from Vienna during the troubles of 1848 and died shortly afterwards, possibly a suicide.

His main works are a great *Genera plantarum*, a general survey and systematic arrangement of all the known genera of plants and, more important his *Enchiridium botanicum*, a summary, where he for the first time separated the Tallophyta, which have no differentiated tissues, from the Cormophyta, which have them. He grouped in the Cormophyta the Fanerogamous, Ferns and mosses.

Meanwhile Adolphe-Théodore Brongnart (1801-1876) in some ways proposed an arrangement that was a step backwards: he suggested to group all the flowerless plants as Cryptogamous as against the plants with flowers or Fanerogamous, these including monocotiledonous and dicotiledonous, finally he divided the dicotiledonous into Angiosperms and Gymnosperms, a rigorous dichotomic classification that would have delighted Cesalpino. Much better was the suggestion by Alexander Carl Heinrich Braun (1805-1877) from Regensburg, who, vice versa, advised to divide the Fanerogamous into Angio- and Gymnosperms and to divide the Angiosperms into Mono- and Dicotiledonous. Braun was also a microscopist and made important studies on algae and microscopic Fungi. In collaboration with his friend, Karl Friedrich Schimper (1803-1867), he developed the basis for the study of phyllotaxis: the mathematical description of the distribution and growth of leafs. Schimper himself belonged to a distinguished Alsatian family, who produced a remarkable number of naturalists. Schimper's contributions were pioneering and manifold, as he wrote on palaeontology, geology and climatology. However his unstable character prevented him from a regular career; he was a passionate anti-Darwinian and could not believe in the relationship of organisms by descent.

Little by little botanists were approaching to modern systematics, and August Wlhelm Eichler (1839-1887) finally divided the Cryptogamous into Tallophytes, Bryophites and Pteridophytes and the Fanerogamous into Gymnosperms and Angiosperms, these last divided into Mono- and Dicotiledonous.

At this moment the Japanese botanists, who had adopted Western approaches, begun their activities and gave important contributions to the understanding of some archaic plants, presently surviving with but a few relic species. Hirase in 1897 described the flagellate sperms of *Ginkgo* and Ikeno did the same in 1898 for the Cycadeae.

Finally Adolph Engler (1844-1930) and K. Prantl (1849-1893), in their monumental *Die natürliche Pflanzenfamilien* (1897-1915) proposed that which was to be for many years the standard systematic.

Meantime plant physiology was developing as well and, perhaps, the most notable, among those that we have not yet mentioned was Wilhelm Friedrich Philipp Pfeffer (1845-1920), who investigated with great rigour and exactitude a number of different aspect of plant life.

Microbiology

After 1840 the idea that most, if not all diseases were due to minute parasites, supported by the authority of Henle, soon became widespread as the improvements in the hystological techniques and of culture methods were allowing for their actual discovery.

As we cannot, for mere reasons of space, cover in any exaustive way a wide field, which ranges from basic studies on protozoans to the discovery of such minute beings as Rickettsiae and Viruses, we shall just pick up a couple of the more significant microbiologists as being typical.

The first, who is certainly the most important of all, is Louis Pasteur (Dôle, 1822-1895), his father was a tanner and poor circumstances made his years of schooling hard for the future scientist. Having finally graduated, he was first appointed as assistant to the chair in chemistry at the École Normale Supérieure, where he made the discovery of stereoisomeres; he soon got a chair of physics in Dijon and, less than one year later, of chemistry in the high school of Strasbourg, where he married the daughter of the director. In Strasbourg he made his first studies in biology showing that the mould *Penicillum glaucum* can metabolise the destrogyre paratartaric acid, but not its levogyre isomere. Having been transferred to the new faculty of Lille, Pasteur began there his studies of fermentations which made him world famous. It is notable that in this first instance, just as throughout all of his following scientific activities, Pasteur was prompted to investigate new problems by the practical needs of some operators: alcohol factories, stock-breeders, the silk industry and so on, and the research plans that he developed in each case are impeccable. Indeed, as we already said, Theodor Schwann and Frantz F. Schultz (1815-1870) had already claimed that fermentations were the result of the activity of micro-organisms, but their experiments, though in some ways anticipating those by Pasteur, being less rigorous, were inconclusive and had been decried by the most influential chemists. Indeed also Pasteur met with persistent criticism by chemists, foremost Pouchet, Joly Musset, Fremy, Trécul and the British Henry Charlton Bastian (1837-1915), but Pasteur's experiments were every time unbeatable. In 1837 Schwann had maintained that the alcoholic fermentation was due to a yeast, but it was Pasteur who proved that several 'diseases' of wines are due each one to a different microorganism. He than made a systematic study of alcoholic, lactic and acetic and other fermentations (and incidentally, in 1861, while studying the butirric fermentation, he discovered anaerobiotic bacteria). The problem of fermentations at the time was a sensitive one: the prevailing opinions were those of Berzelius and of von Liebig, who considered fermentations to be purely chemical reactions independent of life or, better, they considered the 'diastases' capable to produce albumine-like compounds, such as yeast, and that microbes arose within them by spontaneous generation. However, since 1836, and independently of Schwann, Charles Cagniard de Latour (1777-1859), in many ways a gentleman of the 'Ancien régime', had maintained that all fermentations were due to bacteria.

If we remember how the alchemists had always maintained a strong link between fermentations and a number of biological phenomena and that similar ideas had been advanced as to the possible origin of cells, the idea that there were omnipresent germs capable of causing a diversity of fermentations suggested the revival of ancient hypotheses of panspermy and the like, that 'modern' scientists hoped to be defunct. Anyway, considering the chain: microorganism-production of the enzyme-fermentation, it appears that the difference with chemists like von Liebig, was that Pasteur put the microorganism at the beginning of the chain, while von Liebig put it at the end.

Some people wanted spontaneous generation 'as a phylosophic necessity', a ridiculous argument as , indeed, when discussing the origin of life, you have no other alternative than to suppose either an act of a God or spontaneous generation, but in the present world either you assumed evolution, and Darwin's book was being published just at the time of the achme of the debate, and then all you needed was spontaneous generation 'once upon a time', or you refused it and then spontaneous generation was useless to explain the present world. Curiously, while the 'philosophical' argument was really irrelevant, Darwin himself, who greatly admired Pasteur, when writing to Bentham in 1863, said that, although he did not see any possibility to prove spontaneous generation, yet he considered that, should its possibility be proved he deemed it very important for his theories.

Pasteur proved that heath could kill the bacteria and yeasts and block fermentations: the practical advantages of Pasteur's discoveries for medicine, veterinary and agriculture were obviously manifold and great. Conceptually his experiments are the precise adaptation to the minute size of the organisms of Spallanzani's experiments on infusorians. Obviously the possibility of contamination by air carried dusts had always been considered and already in 1854 and 1859 Heindrich, Heinrich Schroeder and Theodor von Dusch, in order to allow for the circulation of air and yet stop dust, had used, to stop their experimental bottles, cotton wool bundles, yet the results were inconstant. Pasteur, in order to meet criticisms by Henry Charlton Bastian (1837-1915) perfected the 'Pasteur's tube'[5]. This is simply a long, thin glass tube with some bends up and down, thus the air inside the bottle and that outside are in continuity, jet their possible exchange is so slow that all suspended particles fall down and deposit in the bends. Culture stopped with stoppers provided with the 'Pasteur tube' always remained sterile (somewhat later, in 1876-77 John Tyndall (1820-1893) proved that completely purified air could not contaminate cultures).

Thus in 1864 Pasteur stated as a general rule that the spontaneous generation of any organism was impossible. This was, in fact, the only real contribution of Pasteur to pure biology: all his other splendid discoveries are model researches, but have purely practical aims, be they medical or otherwise.

The impossibility of spontaneous generation was a conceptual prerequisite for the

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[5] Actually Hoffmann and Chevreul had already used a similar device.

rapid development of bacteriology, but it created also some conceptual difficulties: Indeed it became a cardinal issue in the debate between mechanists and vitalists and between creationists and anti-creationists. Indeed, assuming that every living being can only be generated by another living being, this seems to imply a complete separation between biotic and a-biotic phenomena. To assume the existence of a *vis vitalis*, whatever this life force was, appeared almost a necessity, while the first appearance of life on Earth appeared well nigh a miracle! Nowadays progresses in our understanding of environmental conditions on Earth at the appearance of the first living beings let us see the problem in a very different way from what was possible for Pasteur, Darwin and their contemporaries.

In 1865 Pasteur, again for occasional practical reasons began the study of pebryne, an epidemic disease of silkworms, and obtained results similar to those reached by Bassi on muscardine, which he ignored, but was further able to show that the disaese was due to the microsporidian *Nosema bombycis*. In 1871 the brief civil war of the Commune forced Pasteur to leave Paris and then, on demand by its producers, to begin the study of ale fermentation. After this momentary pause, he went back to the studies of animal pathology and his successes on anthrax are dated 1877, those on the pyogenic streptococcus and on the 'red disease' of swine are from 1878, and from then date the first successful attempts to produce artificial vaccines. Finally in 1880 Pasteur obtained the first success against a viral disease, as he was able to produce an effective vaccine against rabies.

Pasteur was a superb experimenter, but just an average, if not a poor theorist, as it is proved by his complete misunderstanding of the Darwinian theory, which, as it was later amply proved, could be most useful just in theoretic bacteriology. Anyway his discoveries in the field of microbiology had an immense influence on the whole later development of pathology and on several fields of biology: sufficit to think of the significance of the many symbioses between bacteria and multicellular organisms.

The whole of the activities of Pasteur after 1868 are even more remarkable when considering that that year he suffered from a stroke that left his right arm partly paralysed.

We said that Battista Grassi has been chosen as typical of a different kind of microbiologist. We have already mentioned Grassi as a zoologist, his activities as a microbiologist mainly concern the study of Malaria, albeit this was not his only contribution in this field.

That Malaria could be transmitted by mosquitoes had been already supposed by Lancisi in 1717 and, as a hypothesis had crept up now and then, but, as there was no evidence for it, such early suggestions are mere curiosities.

When, with the second half of the '800 the idea that most, if not all diseases, and especially the epidemic and the locally endemic ones were due to parasites became prevalent, it was obvious that the search for the causative agent of such a common disease as malaria was imperative.

The first real contribution was due to the French physician Charles-Louis-Alphonse Laveran (1845-1922), who, in Algeria, succeeded in seeing inside the red cells of the blood of infected people, besides the pigment granules, which were already known, some small mobile organisms, which he described and named *Oscillatoria malariae* which is an invalid name by definition as *Oscillatoria* is the name of some Blue algae: Laveran in 1881, showed his slides to several Italian biologists, including Ettore Marchiafava (1847-1916) and Camillo Golgi. They were immediately interested and first Marchiafava and Angelo Celli (1857-1914) confirmed the results of Laveran and corrected the name of the parasite into *Plasmodium malariae*, thence Golgi was able to describe the whole cycle of the parasite in the red corpuscles.

Meantime, in 1883, Albert Freeman Africanus King (1841-1914) had published a paper listing 19 reasons to consider malaria as being transmitted by mosquitoes and Patrik Manson, prompted by his discoveries on Filariae, which he had proved to be transmitted by blood-sucking dipterans, had maintained, but could not prove, that also malaria should be transmitted by mosquitoes. Ronald Ross (1857-1932), a physician working in India, had been working inconclusively on the problem for some time. He met Manson during a visit in London and Manson outlined for him the research plan which was finally successful. Thus Ross resumed his studies, he tried on *Culex* and *Aedes* with no results, but in 1898 was able to prove the transmission of the Bird's malaria (due to *Plasmodium cathemerium*) by *Anopheles* and to show that carrying mosquitoes had their gut's lining punctuated by nodules packed with protozoans, which he, correctly, thought to represent the insect phase of the parasite. As the Russian Danilewsky had shown the close affinities between the bird and human malaria, Ross had no great difficulty into reconstructing almost the whole cycle of the parasite in birds.

Meantime Grassi had being studying on human malaria, and with his collaborators in 1897 was able to transmit malaria on volunteers by means of *Anopheles.* But his experiments, made in a strongly malarial environment were not fully conclusive. So it was Manson, who, having received from the Italian Sambon some living *Anopheles* previously infected by picking a malarial patient, was able to transmit it to his son having him punctured by the mosquitoes. As there is no malaria in London, it was undoubted that it had been carried by the Italian mosquitoes. Grassi continued his studies producing further important evidence and, given his character, engaging in a fruitless polemic on priorities with Ross. In truth the first ideas for a correct research plan were by Manson and both Ross and Grassi had worked independently and had both provided crucial evidence, though neither of the two was able to find out what was happening during incubation.

Meantime in the U.S., since 1895, MacCallum had shown that the then so-called 'flagelli' were actually the male gametes of the parasite.

All countries in Europe produced, in the last decades of the '800 a number of outstanding bacteriologists, the most famous being Robert Koch (1843-1910), but two only are really relevant from the standpoint of the non-medical biologist: Paul Ehrlich (1852-1915), who found that the pathogenic activity of bacteria was due to toxins, thus paving the way for the whole development of immunology, and, Fritz Schaudinn (1871-1906) who, during his career as a medical officer was often in trouble with the senior establishment and especially with old Koch and the bourocrats of the Health service. He made most valuable experimental work on parasitic amoebae (he finally died because of infection by one of them), on malaria, but he also studied Foraminiferans and Heliozoans. Nowadays he is usually remembered by one of his last discoveries: *Spirochete pallida,* the causative agent of syphilis.

Genetics

We have seen how Darwin, in order to explain the inheritance of characters and the origin of variability on which selection could work, had adopted the theory of 'pangenesis' of ancient Greek origin. However, soon after the publication of the *Origin of species* several scholars, including Darwin's friends like Huxley, had stated their doubts as to the truth of 'pangenesis'. They had considered the possibility of sudden changes, which they called 'saltations' and that selection could take advantage of these (Huxley, 1870; Bateson, 1894). Particularly William Bateson (1861-1926) assembled a significant amount of evidence to support this thesis. As soon as Mendel's laws were rediscovered, Bateson became their enthusiast supporter. Unfortunately for him he was equally adamant that it was impossible that genes were located in the chromosomes, so that he soon became a sort of 'left over' among the growing number of geneticists.

The hypothesis that occasional strong changes were responsible for evolution, was later developed into the theory of the 'hopeful monster', in order to fit with de Vries theory that macromutations were the sole responsible of major evolutionary changes. This theory assumes that, though almost all fenotypically observable mutations lessen the fitness of their carrier, it must be assumed that, from time to time, there will be some that give to their carrier an immediate selective advantage, which possibly make it fit to colonise a previously inaccessible environment, and thus be at the origin of all significant evolutionary improvements. Though this is not absolutely impossible, and may, indeed, have occasionally occurred, modern genetics clearly proves that such an event must have been so exceptional as to be irrelevant in the whole development of evolution.

It must be added that the whole debate on evolutionary genetics has been largely side-tracked by a logical confusion between the 'study of the evolution of populations' and the fictious problem of 'the origin of species, or speciation'. Though Darwin himself clearly stated his nominalist attitude to the species concept and used for his book the title *Origin of species* out of expediency; yet a lot of paper is still wasted to debate the 'species problem', a problem that, as we saw in chapter IV had been already basically solved by Medieval terminists.

If we forget about this sort of superstructure of the debate, there is no doubt that genetics investigates the origin and inheritance of characters within populations, the flux of genetic information among populations and the origin of variability. Though we might safely forget here about the 'species problem', we must say that through the period covered by this chapter and also afterwards, this was a crucial, albeit wrongly posed problem, for a number of eminent scholars.

The first biologist of the Darwinian era who tackled the problems of heredity, was Sir Francis Galton (1822-1911) a cousin of Darwin, an African explorer, meteorologist, student of psychotechniques and, above all an anthropologist.

Apparently under the influence of the classic theory, of Aristotelean-stoic parentage, which assumed that the sperm was the result of a 'purification' or 'refining' of blood, he experimented with the injection of blood of one breed of domestic stock into females of another breed to see whether it produced any change. It was, obviously a complete failure. Better results he got by his generalised statistical approach to biological problems, which prompted a widespread development of biometrics in the study of heredity. Unfortunately, but quite naturally because of his interests as an anthropologist, most of Galton's work was on man, which is a very poor material for this sort of investigations as his hereditary pool is extremely rich and complex (intelligence included) and practically very plastic under environmental circumstances. Man is practically a 'specialised opportunist' and, as later research has amply proved, it is extremely difficult to experiment on mankind under well controlled conditions, so that all results leave ample margins for debate.

Anyway Galton thought to be able to give a rule-of-thumb, but statistically based, rule by which the characters of each individual were dependent in definite proportions from the character of his parent, of his grandparents and so on.

It is interesting to note that the investigations by the botanist Wilhelm Ludwig Johanssen (1857-1927), which we shall mention further on, were originally aimed to verify Galton's hypotheses.

A very important result of research before the advent of Mendelian genetic was achieved by the French Alexis Jordan (1814-1897), who tried to modify species by selection. He discovered that this was impossible, at least on short term, but that it was possible to show that many 'Linnean species' were in fact a pool of 'elementary species', which were later termed 'Jordanions' as contrasted with the comprehensive species, called 'Linneon'. Jordan's work, moreover, fits well with that of Johanssen, as we shall soon see.

It was approximately during the same years that Gregorius (Johann) Mendel (1822-1884), born from a peasant family at Heizendorf (Silesia) and an Augustinian monk since 1843 at the monastery of Brno in Moravia, was sent by his superiors to Wien for three years to study sciences and mathematics, so that he could be used as a teacher by the convent (1851-1853); later Mendel became a professor at the royal school of Brünn (1856-1871); in '68 he was appointed as Prior of his monastery and his new duties began to divert him from research, which he completely abandoned in '74. Since then he was completely absorbed in the battle to save the patrimony of the monastery from the politicians, who aimed to the confiscation of all ecclesiastic patrimonies.

Mendel published his famous paper in 1866 (*Versuche über Pflanzenhybriden*), where he stated the three 'laws of Mendel'. The paper is so good in the choice of the experimental plant (the pea), in the planning of the experiment, in the quality of the evidence obtained and in their analysis, that some critics have argued that it is too perfect to be true, and that Mendel must have somewhat tampered with his evidence. There is absolutely no evidence for that and it is much more credible that, pursuing a perfect scientific plan, Mendel was also lucky: just as one may hit the jackpot in a big lottery, so he got precisely the kind of evidence which as necessary to prove his thesis. Mendel's paper was published in a rather obscure journal with a restricted circulation, but Mendel was regularly in touch with Nägeli, one of the most famous botanists of the time, who, had he understood the significance of the discovery, would certainly have publicised it. Mendel, moreover, sent a reprint to Darwin, who, being a punctiliously well bred gentleman, probably answered with thanks (the personal papers of Mendel were all destroyed), but did not even cut the pages of the reprint, which has remained since in Darwin's library.

Later, as his peas were heavily attacked by parasites, which almost wiped out his plots, Mendel, on the advice of Nägeli, turned to experiment on *Hieracium*, which could not allow for clear results, and on Bees, where the problem was complicated by the fact that males are parthenogenetic haploids. The following paper by Mendel, therefore, apparently did not support his first results. Little by little Mendel abandoned botany and, as everybody knows, no one paid any attention to Mendel's discovery.

In 1900 the three Mendel's laws were independently rediscovered by the German Carl Correns (1864-1933), by the Czeck E. Tschermak von Seysenegg (1871-1962) and by the Dutch Hugo de Vries (1848-1935).

There is no question that de Vries is the most important of the three; he was born in Haarlem and begun his research activities as a plant physiologist with good results. Quite apart from his being one of the re-discoverers of Mendel's laws, he had both luck and merit in choosing to study *Oenothera lamarckinana* (an American plant that has gone wild in Europe) and was able to find in natural habitats different mutants in the incredibly high percentage of 1.5%. The study of such mutants induced him to propose in 1901-1903 his 'Mutationlehre'. Practically he maintained that only mutations were responsible of determining the conditions on which selection could operate (macromutations to use a more modern term). He added, in order to explain the mutations, the hypothesis of an 'intracellular pangenesis'. De Vries theory had an immediate, albeit ephemeral, success and it was practically abandoned within some

15 years. It was indeed possible to show that just *Oenothera* is an abnormal plant just in his chromosomal characters.

The time limits that we gave ourselves are such that only a few, but very important students of genetics worked within these boundaries.

Wilhelm Ludwig Johannsen (Copenhagen, 1857-1927) after several valuable researches in plant physiology, started work on beans, where he could systematically produce self fertilisation. Thus systematically selecting plus and minus variants, in due time he obtained pure lineages for different characters, and he found that these, in successive generations and as soon as environmental conditions allowed, tended to approach the original means. Johanssen's finds have been considered a critical evidence against Lamarckism.

The validity of Mendel's laws in animals was proved as soon as 1902 by Lucien Cuenot (1856-1951), professor in Nancy, and by William Bateson (1861-1926), whom we have already mentioned.

Meantime the persuasion that chromosomes were permanent entities was growing (Heidenheim, Fleming, Veidowsky) and in 1901 Thomas Harrison Montgomery (1873-1912), American, maintained for the first time that in the paired chromosomes that appear in the early phase of mitosis one member of each pair was inherited from one parent and the other from the other parent.

Shortly afterwards, in 1903, Walter Stanborough Sutton (1876-1916) logically associated Mendel's laws with the behaviour of chromosomes during meiosis. All Sutton's papers were published within 1900-1903, and he thereafter abandoned scientific research, working first as an engineer and thence as a private surgeon!

Just before the First World War, in 1911, Thomas Henry Morgan (1862-1946) published the first results of his studies on the genetics of sex-linked characters in the fly *Drosophila*, studies that finally experimentally proved the hypothesis that the 'linkages', that is groups of characters which were normally inherited as a unit and which had been evidenced also by Bateson, were actually features of each individual chromosome.

Meanwhile, in 1908 the British G.H. Hardy (1877-1947) and the German W. Weinberg (1862-1937) published the first theoretical models of population genetics including the famous equations of Hardy-Weinberg on the preservation of the genetic balance (in fact they correctly posed unlimitedly great populations, no crossings with populations with different gene frequencies, that for each gene its mutation rate in one way is the same as that in the opposite way and that neither allele is advantageous, all conditions which never obtain in natural populations).

The Swede N-H. Nilsson-Ehle who had done valuable work pointing to faults in de Vries experiments, thought that mutations only caused losses in the gene endowment of individuals and practically denied the possibility of progressive evolution!

Biogeography

We have seen how the listing of regional floras and faunas had an ancient tradition and how the problem of how local populations arose and of their migrations had been debated since the age of the great geographical discoveries had confronted scholars with an unexpected richness and variety of organisms. This had also originated the first hypotheses on transformism. Among the early authors debating these problems we have mentioned Father Kircher, Linnaeus and others.

The debate was since his origin interwowen with problems of chronology as, after a number of attempts to find ways to harmonise the chronologie of the different peoples with the Biblical account and the problems of the consequences that the different geological catastrophes that were supposed to have moulded the Earth surface, gradually, during the 18th century the scientific community begun to accept the idea that the age of the Earth was much greater than the approximately 5,000 years of the Mosaic tradition, though much ingenuity was still spent by a number of authors, including, for instance Cuvier to save the Noachian Flood and to grant mankind a recent origin.

The generalised acceptance of evolutionary theories automatically posed the problems of the origin and distribution of organisms in a new light and it is not surprising that Wallace, the 'co-author' of Darwinian theory, is generally considered as the 'founding father' of modern biogeography.

On the other side biogeographers, until plate tectonics were generally accepted, that is in the 1950-1960, were devoid of an essential instrument of interpretation and built their theories quite often on intercontinental bridges hardly better founded than those supposed in 1675 by Father Kircher!

It was anyway necessary to frame all the new materials that explorers were continuously providing during an age of steady colonial expansion.

Biogeographic and ecologic researches were thus developed by a number of zoologists and botanists, and often they suggested interpretations that were later basically confirmed.

Obviously Darwin himself was among such scholars, as he had collected a vast amount of evidence during his Beagle voyage. Anyway, just to try to remember some highlights in an approximately chronological order, we must begin by Alphonse de Candolle (1806-1893), who, in his *Géographie botanique raisonnée* (1855) emphasised the need to understand the geological history in order to understand the distribution of plants.

Oswald Heer, Swiss (1809-1893) made a special study of the Arctic and Alpine ecosystems.

Adolph Engler, whom we have already mentioned for his great work as a systematist, tried to consider also geographical evidence in his phylogenetic reconstructions (*Versuche über die Entwicklungsgeschichte der Pflanzenwlet*, 1879-1883).

August H.R. Griesebach (1814-1879), professor in Göttingen, in his *Pflanzengeographie* emphasised the significance of present ecological factors in the distribution of the different floras. Equally important was the contribution of Andreas F.W. Schimper (1856-1901) of Basel whose *Pflanzengeographie auf physiologischer Grundlage* even in the title considers the biogeographic problem in a most comprehensive approach.

As far as animals are concerned, Wallace works were improved by a number of scholars, and especially by R. Lydekker (1849-1915) with a study of 1896, that completed a pattern of zoogeographical regions which is still generally accepted.

Extremely important results were achieved by a number of oceanographic expeditions.

Possibly the most famous, because of the immense amount of materials collected and published, was the British expedition of the 'Challenger' (1872-1876), but quite notable results were obtained by the German 'Gazelle' (1874-1876) and by the Italian 'Vettor Pisani', etc. In the Mediterranean it was possibly the Italian 'Washington' who attained the best results.

As apparently the marine environment does not pose to the diffusion of animals and plants the same barriers that occur in terrestrial environments, the problems of the biogeography of the seas are apparently quite different. These were studied by E. Forbes in 1846, by S.P. Woodward (1856), A. Gunther (1881), A.E. Omann (1886), P. Schlater (1897) and, among the Italians, by Enrico Hillier Giglioli (born in London from Italian exiles in 1845, died in Florence in 1909), Carlo Emery (1848-1925) and also by the ever present Giovan Battista Grassi (1854-1925).

Ecology

The evolutionary standpoint of practically all basic biological research after Darwin did necessarily increase the interest for environmental interactions, which, after all are the forces that prompt natural selection. Such study, at the time, remained, however, largely virtual or amateurish. It got its name, ecology, from the ever fanciful Ernst Haeckel, who praised its significance, but did nothing about it.

The success, also in terms of academic promotion, of the descriptive branches of biology outbid ecology, which requires both long and painstaking field work and an adequate statistical treatment of the evidence. Thus, apart purely descriptive, empirical accounts, ecology was pursued in a sporadic way and most scholars delving into it, rather than trying to understand the dynamics of the biocenoses that they were describing, were mostly concerned with a formal description and classification of the different environments in the well established pattern laid down by botanists since von Humboldt.

Actually quantitative ecology was first developed by institutions concerned with high sea fisheries and its regulation for long term exploitation.

Anyway, a few names deserve mention, such as that of Victor Hansen (1835-1924) from Kiel, who proposed the classical, and to some extent misleading, subdivision of aquatic organisms into benthos, necton and plancton.

Karl August Möbius (1825-1908) in his *Die Fauna der Kieler buch* (1859) did in fact establish a model for ecological research.

Vladimir Ivanovic Vernadskij (1863-1945) was the son of a university professor of liberal ideas. He was a mineralogist, geologist and climatologist. An active political reformer in the Czar times, when the comunists took over in St. Patersburg and Moscow, he tried to avoid them moving to Ukraine, but later came to terms with the new regime. He does have a place in history of biology as he rightly stressed the significance both for the past evolution and for the study of the present conditions of the study of palaeoecology as it can be inferred from the study of the rocks themselves and of the evidence that they provide as to environmental conditions which presided on their formation and successive alterations. He was thus instrumental both in improving on the concept of biosphere and as a founder of geobiochemistry

A special place deserves Carl Gottfried Semper (1832-1893), from a rich and intellectually distinct family. He was an excellent zoologist in the tradition of Gegenbaur, but, having in his youth made a long study in the field of the faunas, floras and anthropology of the Philippines, was fully aware of the significance of ecological conditions in all aspects of zoological research. He deserves remembrance both as a pioneer ecologist and as a well balanced Darwinian, who seriously opposed the worst dogmatism of Haeckel and his oversimplifications.

Ethology

Through the centuries and starting with the accurate descriptions by Aristotle, a large amount of evidence had been collected concerning the behaviour of animals. Such behavioural characters, be they true of imagined, had been used, for instance by Linnaeus, even for taxinomic purposes as features typifying some species. However a systematic, experimental approach to the study of animal behaviour dates from the studies of Jean-Henri Fabre (1823-1915). Fabre was originally a schoolteacher. He began to publish the series of his *Souvenirs enthomologiques* only in 1879. These were an immediate editorial success, which allowed him to retire from teaching and to devote all his time to his studies. He was never attached to a scientific institution and has often been reproached to have often both described and interpreted his observation through the skewed outlook of an excessively anthropocentric standpoint. Moreover, as he was an extreme antievolutionist, he completely missed the possibility of interpreting behaviour as one of the results of evolution, the way followed by Darwin in his study on the expressions of animals, which stands as the other starting point for modern ethological research. Anyway

Fabre was an excellent experimenter and describer and his studies opened an entirely new field of research.

A completely different type of scholar was Sir John Lubbock Lord Avebury (1834-1913). By trade a banker, he was also an economist, a social reformer, and anthropologist and a student of prehistory. In the field of zoology he made valuable researches in invertebrate anatomy and especially on the anatomy of the sense organs of the social insects and their relationships to behaviour, which always fascinated him.

Other scholars deserving mention are, for instance, the German Alfred Edmund Brehm (1829-1884), zoologist and traveller, who, apart other works, produced the 6 volumes *Illustrierte Tierleben* where he carefully collected a number of varied evidences on animal behaviour, and the Revd. Wasmann S.J., whom we have already mentioned, and whose entomological works give space to a number of ethological observations.

Palaeontology

All studies in biogeography, as they try to reconstruct the history of the various groups of organisms, are directly linked with a basically historical discipline, Geology, and thus is, in turn, with Palaeontology, which, until recent advances in molecular genetics, was the sole direct evidence of past evolution.

The diffusion of evolutionist ideas gave a powerful impulse to palaeontological researches, while the Western penetration in so far unexplored regions made available for study ever new and rich fossiliferous layers (and for that it is sufficient to think of the spectacular layers in the United States which were discovered just during the colonisation of the West in the wake of the Civil War).

Quite obviously the debates on evolution were paramount also for the palaeontologists.

We have seen which was the status of the art at the demise of Cuvier. With the triumph of 'actualism' in Geology and of evolutionary ideas all available evidence needed reinterpretation.

Plant palaeontology and especially the study of Carboniferous layers was first developed, as we saw in the foregoing chapter, by Adolphe Brongniart (1801-1876), the son of Alexandre, and later by many other scholars.

In Germany the transformist tradition linked with Naturphilosophie was utterly discredited when the *Origin of species* was published, yet the German academic environment was familiar with the idea of evolution and so both studies and debates soon flourished. Moreover the Darwinian theory provided an excellent interpretative instrument to morphologists and in Germany they enjoyed a strong tradition.

Anyway the availability of fossiliferous deposits and their nature, largely directed the emphasis of research. Where, like in Italy, Vertebrate-rich deposits are rare and mostly not very ancient, palaeontology developed mainly as a support to stratigraphy, where, instead, there were rich deposits, especially of Vertebrates, and there was a widespread interest in evolution and a strong tradition in morphology, the majority of palaeontologists produced mainly descriptive work in an effort to illustrate as many fossil species as possible. This easily explains the great preponderance of palaeontologists of either British or German schools in any list of the representative personalities of this period: such were Karl Alfred von Zittel (1839-1904), professor at München; Ludwig Rütimeyer (Basel, 1825-1895), who mainly studied Mammals. A special importance had Louis Dollo (Lille, 1857-1911) who, however, worked in Belgium. He became famous on one hand because he had the good luck to recover a whole troop of dinosaurs (*Iguanodon benissartensis*) which had died together, a material which allowed for considerable advances in the understanding of the morphology of these animals, but Dollo's claim to fame rests even more on the famous 'Dollo's law', that claims that evolution is a purely unidirectional process. He maintained that evolution advances by successive specialisations and that there could not be any evolutionary reversal, that is any given phylum will never be able to recover ancestral characters lost and simultaneously loose the more recently acquired characters. Reading the evidence in the light of modern advances in genetics, it is easy to understand the reasons that make it so improbable or rather practically impossible for a group of organisms to exactly reverse an evolutionary sequence, while, at the level of single mutations, inverse mutations are not rare and single characters may well violate 'Dollo's law'. Dollo's study of the Benissart fossil locality as well as some others of his excavations make Dollo a pioneer in the field of palaeoecology.

Perhaps the most brilliant Russian palaeontologist of this age was Vladimir Kovalevskij (1843-1895), the very romantic brother of the embryologist, who spent a considerable part of his life as a wondering political exile and died a suicide. All his original work in palaeontology was concentrated within four years, yet it established a new model for palaeontological research using the evolutionary ideas as a key for interpretation.

In the United States a special place have Othaniel Charles Marsh (1831-1899) professor at Yale, and who became famous especially as the first to publish a phylogeny of horses, as well as for having discovered the first Cretaceous Birds, still provided with thecodont teeth. (*Hesperornis*, with extremely reduced wings, and *Ichthyornis*, a small normal flying bird), and Edward Drinker Cope (1840-1914), professor in Philadelphia, who was a famous 'dinosaur hunter'. These two were not only two great palaeontologists, but practically engaged in a sort of sporting competition as who was the one who discovered the most spectacular fossils, especially of Eocene Mammals from Wyoming.

Human palaeontology began by the discovery, in 1856, at Neanterthal, near Düsseldorf of the first fossil man. Naturally there was immediately a furious polemic between those, such as Th. Huxley, who maintained that it was really a fossil man and those, led by the famous pathologist Rudolf Virchow, who maintained that this was simply the skeleton of a diseased man. Virchow was a man whose fame for integrity earned him to be invited by Schliemann to visit his excavations at Troy, and Mycenae to testify the truth of Schlieman's discoveries. Clearly the following discoveries of 1895, 1897, etc. proved Huxley right.

Another significant episode concerned Eugène Dubois (1858-1941) whose name is unfairly associated with a single discovery, that of *Pithecanthropus erectus*. Dubois, who in spite of his French name was a Dutch, decided to prove that the suggestion by Darwin that the cradle of humanity should be searched in the tropics. So he succeeded to get an appointment in the Dutch colonial administration and was first sent to Sumatra and later to Java. He assembled very important collections in both islands, which were mainly studied by other scholars. When he discovered and published in 1894 the first calvarium associated with a femur and named the new species by the name that Haeckel had granted to his hypothetic ancestor of mankind, he was immediately attacked by furious critics. The crux of the debate was that while the skull was clearly extremely primitive, the femur was hardly different from that of a modern man; were they really associated? Dubois, as it was later proved, had been right in associating them; he, nevertheless, had a most peculiar reaction: he shut the specimen in a safe and it became impossible to study it until Dubois' death! Another curiosity: the lower jaw of a *Pithecanthropus w*as discovered at Heidelberg in 1908, but was not recognised as such until after World War II.

We have said that there was an obstinate opposition to recognise the validity of the early evidences for the existence of men contemporary with the typical Pleistocene faunas and how the early discoveries by Boucher de Perthes were validated by Lyell and other British geologists only in 1859. Also the successive studies by Edouard Lartet (1801-1871) were badly received by the establishment. Lartet was a by profession lawyer, and begun his amateur excavations in a cave at Aurignac in the Garonne, and the evidence that he discovered gave the name to one of the Palaeolitic cultures, 'Aurignacian', and at the cave of Madelaine (Dordogne), which gave its name to the 'Magdalenian culture'. At the Madelaine Lartet discovered in 1864 a piece of Mammoth ivory on which was sketched the figure of a Woolly Mammoth. Also this discovery was claimed to be a forgery by several pundits. However soon the scientific community was forced to accept the idea that a palaeolithic art was real indeed. Anyway problems were complicated both by the rarity of human fossils themselves and by the fact that such early discoveries were not associated with any sort of implement or other material remains.

Stone implements both paleo- and neolithic had always been known, and they had been considered as magic objects by a number of cultures (the Greeks considered them to be produced by thunderbolts hitting the earth and called them 'Ceraunies'), and such beliefs lasted well into the modern age, However since '500 the physician and naturalist Michele Mercati (San Miniato, 1541-Rome 1593) following Lucretius had correctly understood their nature (*Methallotheca Vaticana*, 1574, appendix published by A. Lancisi in 1714-1715).

Physiology

It is almost impossible to summarise in but a few pages all the advances of physiology in the fifty years following the publication of the *Origin of species*. These were regularly correlated with the growing evidence of cell and tissue morphology and, on the other side, they were necessarily linked as well with the developments of biochemistry. Indeed since 1847, von Helmholz, possibly the most brilliant pupil of Johan Müller, and Ernst Brücke, had published a true 'manifesto', where they claimed that the whole development of physiology and, more generally the whole science of biology not only must be strictly linked with the advances in chemistry and physics, but should aim to frame the whole biology, including the methods and principles followed in research, in physico-chemical terms. This stand had its philosophical basis in positivism and was reviving, in a modern framework, the ideals of Cartesian mechanistic model; it was immediately immensely fruitful in the field of analytic physiology (that is the study of single cells, organs or apparatuses individually considered), while it did not forbid a similar 'holistic' approach, that is the consideration of the whole organism as a unit; yet this last approach was tackled but sporadically and with limited success.

Nevertheless, from the standpoint of the history of these researches, we are faced by a technical problem: the mechanisms which were proposed to explain the various functions of cells and tissues were increasingly complex and were later often found to be at variance with the structures as revealed by electron microscopy, they are, thus, considerably different from those with which we are presently familiar. Given their intrinsic complexity and their links with the chemical and physical theories of their times, to give an adequate account of them involves such explications as are impossible in a book of this size.

This difficulty is compound by the fact that, while the great majority of the best physiologists were working strictly in the field of the medical faculties and institutes, there was an increasing separation between the cultural outlook of the medical and of the naturalist professions.

While naturalists were basically engaged in the interpretation of living floras and faunas in terms of evolution and, consequently, were embroiled into the debates between the different schools of evolutionary theory, medical biologists were allured by the self-evident possibilities that laboratory experimentation offered in order to approach in their work to models comparable with those of physics and, even more, of chemistry (albeit, as it has been remarked by several scholars, of a physics or chemistry some twenty years old, in a sort of perpetual chase, in which the advanced biologists were trying to tailor their work to a theoretic apparatus, that the more advanced physicists and chemists already considered as becoming obsolete). Moreover most physiologists, being trained as physicians, had rightly the impression that they could be more useful to humanity by leaving to someone else to amuse himself by the construction of more or less problematic phylogenies and the description of new species.

The serious problem which gradually developed and which consequences we are paying, was that specialisation, instead of corresponding to the great phyla, so that we could have a specialist, say, of insects, who studies both their evolution, and their genetics, morphology and physiology, and thus be able to build a balanced assessment of the different evidences available, we have had usually, say, the systematist and, for instance , the physiologist of secretions, who happily worked each one in his own little plot, blissfully ignoring the problems of the specialists of other branches or, anyway, with little interaction with their colleagues working on the same organisms under different standpoints.

Thus, while we are forced to renounce an organic account of the developments of physiology, we shall choose a few names who are especially representatives and especially of those representative of the then raging debate between 'vitalists' and 'mechanists'.

A scholar who may be considered as 'transitional', between those of the first and of the second half of the century, was Carlo Matteucci (Forlì, 1811-1868); he graduated in mathematics, but was an extremely versatile scholar, who made valuable researches in geology, physics and electrophysiology. On a recommendation of von Humboldt, in 1841, he was appointed professor of physics in Pisa. An active patriot, he was even for a brief time, in 1862, minister of education in the newly born kingdom of Italy. He was one of the first and constant supporters of strict links between physiology, chemistry and physics (*Discorso sul metodo razionale scientifico*, 1835 = *A discourse on the rational, scientific method*). For the history of biology he deserves mention for his studies on the electrophysiology of the muscular and nervous systems.

One of the most influential physiologists of this age was the French Claude Bernard (St. Julien sur Rhone, 1813-1878), born from a peasant family, Bernard made good high school studies, but, out of necessity, he was for a while an apprentice with an apothecary and later wrote a fairly successful 'vaudeville', a sort of musical comedy, and a long drama (which he was dissuaded from publishing and was printed after his death as a sort of curiosity). He then (1834) studied medicine, being selected, in spite of having succeeded 26th out of 29 candidates, by an arbitrary and justified decision of Magendie; he was later his assistant and eventually his successor both at the Sorbonne and at the Collège de France.

Bernard was a superb experimenter, but not a pure experimenter. His most famous and rather early discovery was that of glycogen. His starting point was a set of studies on the distribution of sugars in the blood in the various sections of the circulatory system. So, having found how sugar was depleted passing through the tissues, but was newly enriched after passing through the liver, he was able not only to isolate glycogen, but to prove that its synthesis occurred in the liver (how it was stored, demolished and rebuilt in the different tissues was discovered later). He called the production of glycogen an 'internal secretion', thus introducing this new concept, though in a much broader sense than that currently used. Later Bernard studied the pancreatic secretion and its function, the production of animal heath, the nervous control onto circulation, the mechanisms of action of toxic substances on the nervous system (curare, carbon oxide) and the physiology of synaptic connections, etc.

Bernard was also an excellent teacher and the master of a number of distinguished physiologists and pathologists.

In his youth Bernard was a pure materialist, but with advancing age he became more flexible, especially as he could not find a purely physico-chemical explanation of the phenomena of growth.

The German school of physiology played an essential function in the development of this branch of biology at this time.

Julius Robert Mayer (Heilbronn, 1814-1878) was the son of an apothecary. On the evidence of the observations that he made in Java, where he was for a while practising medicine, but basically on theoretic considerations on the works of Young, Carnot and Gay-Lussac, he published in 1842 a general essay on the dynamic equivalent of heat which is hailed as one of the foundations of thermodynamics. Later, in 1845, he published at his own fees a paper on the application of this principle in biology, a basic essay which no one of those who read it before publication took seriously (*Die organische Bewegung in ihrem Zusammenhange mit tem Stoffwechsel = Organic movement in his relationship with material metabolism*), a work that, dealing also with photosynthesis, showed how all the energy used by the organisms, finally was derived from that of the Sun.

Mayer's ideas met with a hostile reception. The revolutions of 1848 caused him a serious depression from which he later recovered. An important consequence of his work was to show how an essential difference between living organisms and non living structures was that photosynthesis allowed living beings taken as a whole violate the second principle of thermodynamics, and this was assumed by 'vitalists' as good evidence against the mechanist's cause.

Hermann von Helmotz (Postdam, 1821-1894) was professor of Physiology in Koenigsberg, Bonn and Heidelberg, before finally settling in Berlin as professor of Physics. We have mentioned him as a co-author of the mechanist medicine's manifesto, and was one of the major personalities of this trend. Among his most brilliant results in the field of physiology, was the measurement of the speed of transmission of nervous impulses (which just a few years before his master Müller had declared to be immeasurable). As this was found to be comparatively slow, this strongly suggested that it was not a purely electric phenomenon, but rather that it was a physico-chemical process. Von Helmoholtz gave also important contributions to the physiology of sense organ, particularly sight and hearing, but later he abandoned almost completely physiology for physics and it is just to his researches in physics that his fame is mainly due. In 1847 he reached independently the same conclusions as von Meyer in the field of thermodynamics and in 1848 was able to show that muscular contraction had, as expected by the theory, as a by product, the production of heath.

Emil Du Bois Raymond (Berlin, 1816-1896) in spite of his French name and distant Huguenot origin, was a Prussian, and was also a pupil and later the successor of J. Müller. He was basically a student of electrophysiology and a theorist of science and as such one of the classical Berlin school of positivist and mechanist biologists, even more in the line of von Liebig than of Müller. However he thought that some essential problems: the essential nature of matter, the nature of conscience and of life were beyond the powers of empirical investigation.

Karl Friedrich Wilhelm Ludwig (Hessen, 1816-1895), yet another pupil of J. Müller, was professor in Zurich, in Wien and finally in Leipzig. He was another typical product of the Berlin positivist medical school of Berlin. He is commonly remembered as having measured the speed of the blood flux in different vessels and under different physiological conditions; but his main contributions were in the study of secretions. However he interpreted them in a purely mechanistic interpretation. He was the first to adopt chimographers in order to precisely record and measure the rhythm and intensity of phenomena during given times.

Another absolutely typical produce of the German materialist medical school was Jacques Loeb (1858-1924), whom we have already mentioned. In youth he had been much interested in philosophy and such interest never completely vanished. Under the influence of Schopenhauer he was always an outright determinist, fundamentally sure that free will was an illusion. This credo and the ensuing corollary that it was of basic significance to clarify the essential mechanisms of what is commonly called 'will' led him first, in 1880, to register at the University of Strasbourg and, in order to study the working of the brain, to attend the laboratory of Leopold Golz (1834-1902) one of the many materialist pupils of Müller. Thinking Golz approach to the problems inadequate, Loeb went as an assistant to Würzburg, with the then famous physiologist Adolph Fick (1829-1901), a pupil of Carl Ludwig (1816-1895), an eminent offshoot of the Berlin school. Ludwig had a notable influence of Loeb, and was instrumental in his becoming acquainted with the botanist Julius von Sachs (1832-1897), a former pupil of Purkinje and the master of Hugo de Vries and for a time of Francis Darwin, the botanist son of Charles. Von Sachs was himself a convinced mechanist.

Von Sachs had made a series of fundamental studies on plant's tropisms, authomatic answers to given environmental conditions and which, in some instances could be directly correlated with the action of given chemical or physical factors.

Also the presence in Würzburg of Svante Arrhenius (1859-1927), the great Swedish chemist, during the years when Loeb was assistant there was not devoid of influence. Arrhenius had the same age of Loeb and was a close friend and correspondent of Jacobus Van't Hoff (1852-1911). Thus Loeb could absorb the very best of the mechanist and experimentalist approaches.

Loeb main purposes in his subsequent research were twofold: first to extend to animals, Man included, the theory of tropisms and, second, to prove the function of chemical and physical factors in all basic vital phenomena. The main original contributions by Loeb were, therefore, in the fields of cell biology (and especially on artificial parthenogenesis, fertilisation and osmoregulation in cells), as well as the study of animal tropisms.

In the fields of cell biology and embryology Loeb was able to prove that both the simple puncturing of the egg or changes in the salt concentration in the water were sufficient to start the segmentation of the unfertilised egg of sea-urchins and that, albeit very rarely, the larvae thus obtained could metamorphose. As we have already said he got very much the same results with frogs. As far as fertilisation was concerned, he proved that the sperm was charged with a substance, which he called 'Lysin' which functioned as an activator of the egg. Nowadays we know well the structure and functioning of the acrosome of sperms, which is responsible for the phenomena described by Loeb.

In the field of tropisms, Loeb concentrated on fototropisms in insects. He was able to show how the typical behaviour of the caterpillars of some butterflies depend on an automatic search for best lighted point, so that it was possible by an appropriate illumination to induce the caterpillars to leave the leaves that they are eating and to starve to death.

Loeb spent much of his time, particularly after he moved to the United States, to maintain, extend and generalise his theories. He had summarised them in a paper read at the first international monist congress, titled *The mechanist concept of life* and later in a book, which he published in 1912. In the United States he became particularly close to Th. Morgan, whose results in genetics appeared him as being well fitting into his general theories.

Clearly the extreme mechanistic and reductionist views of Loeb were to undergo drastic revision , but well until after 1920, when his influence was prevalent, as well as later, as it stimulated much critical research, the scientific activities of many biologists were related to the debate on his ideas.

Parallel with the activities of the Berlin school, other scholars were at work elsewhere and some of the more significant results were attained in the physiology of the nervous system.

This field could benefit of the wealth of evidence that morphologists had gathered and continued to accrue, especially by perfecting the old method, first used by Galenus, of the localised lesions and both the study of the functional alteration induced, as well as, by appropriate histological techniques, by examining the distribution of the lesions caused by the cutting of nerves or of fasciculi in the central nervous system.

The two scholars who made the greatest contributions to the advancement of the physiology of the nervous system were the Russian Ivan Petrovic Pavlov (1849-1936), whose studies on conditional reflexes earned him the Nobel prize, and the British Charles Scott Sherrington (1857-1952).

Pavlov was born in Ryazan, the son of a pope (and remained faithful to the Orthodox church even under the soviet regime), and studied in St. Petersburg, where he had as a teacher M. Sechenov (1829-1905), who had studied in Berlin with du Bois-Raymond, Ludwig and von Helmholtz and later in Paris with Bernard. Shortly after 1860 Sechenov had made brilliant experiments on the inhibition of reflexes by the central nervous system, but, under the influence of the reductionism of his German masters, he had tried to explain everything in terms of reflexes, inhibitions and stereotyped potential increases. Pavlov felt deeply the influence of his master and, as far as his researches were concerned, of positivist and materialist philosophers, and this, incidentally greatly helped him, when the bolshevik revolution arrived, and in spite of his religious faith, to pass through all its purges unscathed and regularly supplied with the means for his researches.

Up to 1902 Pavlov studied the dynamics of blood circulation and the secretions of the digestive system. A fortuitous observation during these last studies led him to the study of conditional reflexes, for which his schooling had just trained him. He was thus able to show, by a series of classical experiments, that signals, even if not directly connected with a specific physiologic activity, provided that they are repeated in connection with such signals that are really connected with that activity, for a sufficient length of time, may become able, by themselves, to start the required reaction. Thus, for instance a sound, regularly associated with the sight of food, may, in time, become sufficient to start all the secretions that are connected with feeding. After such a sound beginning, Pavlov tried to generalise the theory of reflexes to the whole functioning of the nervous system, so as to frame within it all the evidences that himself and his pupils were collecting. The result was that his ideas became increasingly vague and confused.

Pavlov work was both directly and indirectly most influential in shaping the development of both Russian neurophysiology and psychology and of American 'behavourism', while the European tradition favoured much more complex theories. And this was probably in connection with the cultural temper of Soviet Russia on one side and with the American traditional outlook on the other, whose opposed ideals both assume the possibility of unbound improvement of humankind by education.

The approach by Charles Scott Sherrington was completely different. Sherrington. had an opportunity to study for two years in important German institutes, where he, like Loeb, studied with Golz. Nevertheless he went back to England thinking that the methods and theories of the riductionist physiologists had a limited heuristic power. He started from the 'neuron theory' which we have mentioned and by the fact that even the simplest of the reflexes, involved a complicated pattern of reactions, for instance the contraction of a given muscle and the simultaneous relaxation of its antagonist. So, by concentrating his researches on the simplest of the systems: the spinal reflex arches, he was able to show how the nervous system must be considered as a totally integrated system, provided with facilitated and alternative pathways, both endowed with stereotypes answers and with flexible ones, and so on..

The first discoveries of hormones date from the early years of the 20th century: in 1901 Jôkichi Takamine (1854-1922) and Thomas Bell Aldrich (1861-?) were able to crystalize adrenalin; between 1902 and 1905 William Haddock Bayliss (1860-1924) and E.H. Stirling proved the existence of a substance, which they named 'secretine', which is produced by the cells of the epithelium of the gut and that prompts the secretion of both bile and pancreatic secrete, while, to complete the picture of pancreatic secretions related with digestion, A. Pagen and F. Persoz isolated pancreatic diastase.

Also biochemistry moved its early steps usually from departments of Physiology.

One of the first fields to be investigated was the chemistry of respiration. As we know this may be either anaerobious, which occurs either in oxygen free environments or by organisms that do not use oxygen for their respiration , as, for instance, in alcoholic fermentation, or, finally as intermediate phases in complex biochemical pathways, as in the transformation of sugars into lactic acid.

When began the development of the study of fermentations, the first problem was to verify whether such theories that were derived from von Liebig's studies were valid. Von Liebig's approach had been purely quantitative, which was the only one possible at the time. Thus, after carefully measuring the total amounts of the substances that one was studying and of the final produces, the possible pathway was assumed by simple stechiometry.

In 1897 the German chemist Eduard Buchner (1860-1917) identified a substance, which he named 'zymase', and which could be extracted from the cells of yeast that he labelled as a 'ferment' and which he proved to be capable of causing the fermentation of sugars even in the absence of living cells (in truth the existence of substances capable of causing fermentations had been supposed by Berzelius in 1835 and 'diastase', capable of turning starch into glucose had been isolated by 1832).

The first paper on vitamins was published by Funk in 1912. The first hormone, adrenaline was isolated in J.J. Abel in 1897, but not immediately recognised as such, while there was no question when 'secretine' was isolated in 1902 by Ernest Starling and Sir William Bayliss).

Buchner and others, and especially Franz Hofmeister (1860-1922), the son of Wilhem, whom we have mentioned in this same chapter, thought that all ferments, or 'enzymes' as they were soon named, were proteins, and independent of the cell structure and each function of the cell was due to that of a given protein, as this would have allowed for the interpretation of all phenomena of life entirely in terms of chemistry. At most they allowed that the complex internal structures of the cells, which histologists were describing, could subdivide the cell into functionally separated compartments, so that the chemical processes happening in a given section of the cell, would not interfere with those happening elsewhere.

This extremist thesis was soon criticised on the evidence that enzymatic processes occur in living cells at a much grater speed than that possible with purified extracts of enzymes. Moreover protein structure was almost unknown at the beginning of the century, so that to say that enzymes were protein explained almost nothing. Thus 'vitalists' immediately argued that in fact enzymatic reactions were only a part of the 'vital' processes.

As it is clear at such an early stage of the debate there was almost no evidence for sound reasoning.

Thence a vital role was played by Otto Warburg (1883-1970), who began his classic studies in 1908. Otto Warburg was the son of a famous German physicist, he studied medicine at Heidelberg and, long before graduating he worked with Emil Fischer on the synthesis of polypeptides. Having remarked that the respiratory rate of cancer tissues was higher than that of the surrounding healthy tissues, and being aware of the difficulties in working with mammalian tissues, he left Heidelberg (where, however, he returned to graduate) and went to the Stazione Zoologica of Naples, where he worked with Curt Herbst (1866-1946), who was a friend and collaborator both of Driesch and of Morgan. In Naples Warburg met, besides a number of other people, with Loeb, with whom he became a good friend and that, possibly, introduced him to the works of Sachs and his collaborators on plant physiology. The main steps of Warburg's work may thus be synthesised: first he proved that the thesis of his friend Loeb that respiratory processes were concentrated in the nucleus was wrong; second that the cell membrane has an important role (he initially thought a fundamental one), that there was a respiratory enzyme, but that this ought to be spatially organized in the cell to achieve its full efficiency. This last result was attained by Warburg in 1914, just the year that we have chosen as the closing on for our history. It is just with this discovery that made for a harmonious synthesis of morphology and the physico-chemical mechanisms of cell physiology we close this final chapter.

Farewell

For the reasons argued in the preface, we leave this history of biology with the beginning of World War I.

However, I wish to add a few lines before saying 'farewell' to the patient reader who has followed me so far.

Space, obviously, has not allowed for a sufficient discussion of many subjects and problems. However, it should be clear that it would be highly desirable for the biologist to have a better philosophical and logical training. These would not help in obtaining new evidences, but would certainly help much in the sober and correct assessment of the evidence itself.

I also hope that my reader may have been occasionally surprised by what he was reading and I hope to have been able to make him feel like someone who has inherited an ancient palace and its furniture, and, while inspecting it, becomes aware of the debt that links him to his ancestors and than becomes curious to know more about such ancestors. I have, indeed tried to show what kind of links obtained between the different scholars and of the different philosophies that shaped their theories. Unfortunately I could scarcely say anything of them as human beings. Each one of them was not only thinking within the framework of his age's culture: they had, indeed, inherited a number of cultural 'conditioning' patterns which they used, modified and occasionally refuted, but they were not mere computers that, having stored some programs, then fed in the evidence for its treatment. In their intellectual development, as in that of any human being, there was a steady interplay of personal, family, sometimes economic or political factors, which made each one of them unique, and all these I could not hint. This is unquestionably a great fault of this book, but it may also be said that for most of the scholars mentioned in this book, their human story is still almost entirely unknown, as the evidence necessary to paint their true portrait both as scientists and men, when it still exists at all, quite often lays unexplored in the archives.

We have seen how the debate between 'holists' and 'reductionists', which is just now taking a new impetus, has ancient origins and its increasing radicalisation was largely the consequence of the increasing specialisation of the individual scholars. Moreover biologists, at least since the 19th century, have often suffered of a sort of 'inferiority complex' towards their colleagues physicists and chemists, as, their disciplines, because of their greater formalism, mathematical content and rigour, have been considered commonly as the 'strong' ones. This attitude has been prevalent at least until the public awareness of the significance of ecology began changing this attitude. Thus many biologists felt that, unless they were going for chemistry, their colleagues would consider them as sort of craftsmen, barely able to cobble some sort of approximation to true science.

That the problem is theoretically serious is proved by the difficulty that one of the most famous modern philosophers of science, Karl Popper, had in order to find justification for ranking evolutionary theories among the scientific ones according his definition of scientific theory, and this, at last, he did by a truly screwed argument.

Clearly the effort to bring as much chemistry and physics as possible into biology has been and still is an extremely fruitful one; the problem has been that, in order to study the tree, many have lost sight of the forest. True enough: the very complexity of any biological problem is such that one should spend a good half of his study time, reading the most disparate papers and this, under the 'publish or perish' rule is hard to do.

Luckily some developments of moderns mathematics have shown that as the complexity of a system grows, so an increasing amount of indetermination gets into it and stochastic factors, so dear to Darwin, play an increasing role. It is clear that, as biological systems are, by their nature, extremely complex, any rigidly deterministic logic does apply with them only to a limited extent. The when and to which extent Bayesian logic, fuzzy logic etc are preferable when dealing with biological problems is still an open debate.

The increasing urgency of ecological problems, which require the most comprehensive, 'holistic' approach, makes really urgent a serious reassessment of the basic philosophy of biology. On the other side, the very history of sciences plainly shows how great was the impact of the general culture of each scholar on his scientific achievements. It is thus easy to argue that, while early specialisation is probably good to produce an average professional, it is certainly a very bad background for an 'all round' scientist.

To sum up: I hope to have been able to stimulate in some readers the curiosity to learn more as to the historical development of their discipline.

Some useful references book

Useful references or whole chapters are usually found in the general Histories of Sciences and in the general biographic dictionaries of History of Sciences such as:

Gillispie, C.C. ed. (1981) Dictionary of Scientific Biography. Scribners: New York (reprint in 8 volumes of the original 15).

Dragoni, G. Bergia S., Gottardi G. (1999) Dizionario biografico degli Scienziati e dei Tecnici. Zanichelli: Bologna.

Useful handbooks are:

Barsanti, Giulio (1992) La Scala, la Mappa, l'Albero. Immagini e classificazioni della natura fra Sei e Ottocento. Sansoni: Firenze.

Nordeskiöld, Erik (1928) The history of biology, a survey. Tudor Publishing Company, New York

Just as there are few general books on the history of biology, rather comprehensive monographs are few and most are out of print since long.

De Wit, Hendrik C.D. (1982-1987) Ontwikkelingsgeschiedenis van de Biologie. Pudoc, Wageningen.

Idem (1994) French translation: "Histoire du developpement de la biologie", 3 vols. Presses Polytechniques et Universitaires Romandes, Lausanne , 1993.

Duris, Pascal & Gohau Gabriel (1997) Histoire des sciences de la vie. Editions Nathan, Paris

Dusch von, Theodor.

Evolution and related subjects are exhaustively treated in:

Mayr, Ernst (1990) The Growth of Biological Thought, Diversity, Evolution and Inheritance. Harvard Univ. Press, Cambridge, Mass.

Tort, P. (ed.) Dictionnaire du Darwinisme et de l'Évolution. (3 vols.) Presses Universitaires de France, Paris

Index of names

This “Short History of Biology from the origins to the 20th century” tells the story of the development of both knowledge and theories about the living organisms since Antiquity until the beginning of the 20th century. It also endeavours to link the advances in sciences and the events in the life of scientists with the historical and social framework within which they lived. When possible some hints have been provided about the personalities of the scientists themselves.
This volume is especially meant both for biologists and naturalists, but an attempt has been made to make it interesting reading also for any layman with an interest in history and sciences.

Alberto Mario Simonetta was born in Pisa (Italy) the 26th of March 1930.
He matriculated when sixteen and graduated both in Medicine and in Natural Sciences. After a short spell as a “voluntary assistant” in the department of Surgery of Florence, moved to the Department of Zoology of the same University. In 1970 he was appointed as a Full Professor of Comparative Anatomy at the University of Camerino, where he also taught at different times Zoology and History of Sciences. From 1994 to his retirement in 1972 he was Full Professor of Zoology at the University of Florence, where he was also charged with courses in Evolution and in Wildlife management and conservation.
His main research activities were concerned with evolutionary morphology and systematics of both living Vertebrates and fossil Arthropods. He has also done extensive field work in Africa (Somalia, Mozambique, South Africa, Congo, Afghanistan, India). He has also been active in conservation, acting for years as a member of the Committee for Nature conservation of the Italian National Research Council, as a member of the trustees of different National Parks and as an technical adviser for F.A.O. and for the Italian Co-operation programs in Somalia.