Chapter 1

The parallel paths of Economics and Political Science: from the early XXth Century to the Postwar

The interwar and second postwar years witnessed dramatic changes in the history of the social sciences. The following pages aim to briefly reconstruct this history with reference to the cases of political science and economics.

In economics, the main development consisted in its transformation into a mathematical discipline. The evolution of economics toward its contemporary mathematical form has been explored by many scholars, beginning with Weintraub's pivotal studies (e.g., Weintraub 1983; Weintraub 2002; Ingrao and Israel 1987; Mirowski 1992; Mirowski 2002; Giocoli 2003b; Giocoli 2003a, among others). A central role in these analyses is played by the increasing level of abstract mathematical formalism, which progressively turned economics into a mathematical science. As a result, both the conception and the teaching of economic theory were radically transformed within a relatively short period (roughly 1930–1960). This transformation entailed a shift from what Giocoli defines as the traditional image of economics as a "system of forces" toward a new image of economics as a "system of relations"(Giocoli 2003b;Giocoli 2009a). According to the first view, economics analyzes economic processes generated by market forces, including but not limited to equilibrium outcomes. According to the second view, instead, "economics is a discipline whose main subject is the investigation of the existence and properties of economic equilibria in terms of the validation and mutual consistency of given formal conditions, but that has little if anything to say about the meaningfulness of these equilibria for the analysis of real economic systems" (Giocoli 2009a, p. 24).

As an illustration, Nobel laureate Robert Fogel reported a remark made to him by his colleague Lionel W. McKenzie, one of the most prominent postwar mathematical economists, during the Sixties, when both were members of the faculty of the University of Rochester's Department of Economics. Commenting on the existence and uniqueness of general economic equilibrium, McKenzie stated: "We know that equilibria exist because markets produce them every day. The problem is that we ran into difficulties in demonstrating their existence in our models" (Fogel et al. 2013, p. 84).[1]

[1] McKenzie's role as chair of the Rochester economics department from the late Fifties will be discussed in a separate chapter of this dissertation, since Rochester was also

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Giocoli's distinction thus highlights the emergence of radically different questions generated by the progress of the discipline.

From a historical perspective, the steady advance of mathematical economics also affected how the history of the discipline came to be understood by its practitioners. It implicitly legitimized a view of economic theory as progressing through the continuous refinement—and creation—of new mathematical tools to address novel problems or to shed new light on existing ones. Gérard Debreu (Nobel Prize winner in 1983), perhaps the most "extreme" representative of mathematical economics, defended the use of mathematics in economics (particularly the axiomatic method) by repeatedly emphasizing four advantages: generality, weakness of assumptions, clarity of expression, and freedom from ideology (Düppe 2010). Of these, the latter is particularly relevant in the case of economics. It can be associated with a "Whig" perspective that emphasizes ongoing theoretical refinement and progress. In reconstructing the history of economic theory, this perspective makes it possible to detach theoretical developments from the institutional and social contexts in which they emerged. As a consequence, Debreu's own assessment of mathematical economics would seem to hold: "[...] even though a mathematical economist may write a great deal, it usually remains impossible to make from his works, a reliable conjecture about his personality" (Düppe and Weintraub 2014b, p. xiii).

Important transformations also occurred in political science during the twentieth century. However, unlike economics, these changes were not accompanied by a comparable narrowing of methodological pluralism. The development of political science can instead be described as an "epistemic shift" from what some authors have labeled "developmental historicism" to "modernist empiricism." By the former, Robert Adcock and Mark Bevir refer to several nineteenth-century traditions in the social sciences, including philosophical idealism, positivism, Whiggism, and early evolutionary theories. "Modernist empiricism," by contrast, is commonly understood as an approach "[. . .] based on a rigorous accumulation of facts coupled with the modernist view of science and reality as parts of a probabilistic world and of various new ways of ascertaining reality" (Smith 2009, p. 118; Adcock and Bevir 2010). In the Fifties, efforts to establish a more systematic political science culminated in the Behavioralist movement, which became the dominant approach in political studies during that decade and part of the next. These efforts entailed extensive debates concerning the proper methods and scope of a scientific analysis of politics.

Thus, constructing a "real science of politics" also required addressing the appropriate relationship between political science and political philosophy, political theory, history, and constitutional law—disciplines that had traditionally been devoted to the study of political order. Defining this relationship has been one of the central challenges in the development of political science as an autonomous discipline. In this sense, the history of political science can be understood as the history of successive answers to

the institution where Riker established his formal approach to political science, and there existed a certain proximity between the two departments.

questions concerning the nature of political phenomena. As in the histories of other disciplines, political science appears as a sequence of attempts to address different problems through the elaboration of distinct theoretical and methodological approaches.

In the following chapter, it will be offered a general, though not exhaustive, reconstruction of some key features in the development of economic theory from the interwar period to the Fifties. Its purpose is to provide the background necessary to analyze the development of Game Theory and other formal approaches beyond the boundaries of economics from the Fifties onward.

A "linear narrative" of the development of political science, focusing primarily, though not exclusively, on the American case, is presented. In doing so, two widely shared interpretations are implicitly adopted: first, the use of Behavioralism as a key lens for understanding the development of political science; and second, the narrative of the "Americanization" of the discipline (Somit and Tanenhaus 1967).

Given the pivotal role of the behavioral movement in shaping political science in the second postwar period, a common framework for the discipline's history is the periodization into "Pre-Behavioralism, Behavioralism, and Post-Behavioralism."[2] Consequently, the "Behavioral Revolution" occupies a central place in how contemporary political scientists conceptualize their discipline's past. The narrative of "Americanization," for its part, holds that American scholars pioneered the institutionalization of political science as an autonomous discipline (Adcock and Bevir 2010, p. 71). While American scholarship was undoubtedly shaped by transnational exchanges of ideas and individuals, for nearly half a century the existence of an autonomous discipline of political science, as well as independent scholarly associations, remained a North American anomaly. In most other countries, political science emerged as an autonomous discipline primarily in the second postwar period (Easton, Graziano, and Gunnell 2002). By way of illustration, the American Political Science Association was founded in 1903, and the first issue of its journal, the *American Political Science Review*, appeared in 1906. At the same time, the American Economic Association had already been established in 1885, and the Royal Economic Society in England in 1890. By contrast, the British Political Studies Association was founded only in 1950, following the UNESCO Symposium on "Contemporary Political Science" (Kenny 2009).

Both interpretations, however, have been challenged by a number of scholars and historians. Adcock, for example, has convincingly argued that the belief in the revolutionary role of Behavioralism must be qualified (Adcock 2009). While Behavioralism was undoubtedly innovative in both character and impact, some of the changes it introduced were neither immediate nor radical, contrary to what is often implied. Moreover, Behavioralism lacks a precise definition, and interpreting it as a scientific revolution is highly problematic (Dahl 1961). John Dryzek reconstructed

[2] Despite the lack of a precise definition of Behavioralism, its features and meanings will be discussed in the second section.

the history of American political science in terms of five different "revolutions without enemies" (Dryzek 2006). In his account, the notion of revolution, in a Kuhnian sense, is largely metaphorical, since none of these developments qualifies as a genuine "paradigm shift." According to Dryzek, only two revolutions can be considered successful, and their success is explained by the absence of organized resistance rather than by radical intellectual rupture (Dryzek 2006, p. 487). That Behavioralism was one of these two "successful" revolutions (the other being Statism) is insufficient to justify interpreting the history of political science exclusively through this lens. Unlike mathematical formalism in economics, Behavioralism did not fundamentally transform the overall "image" of the discipline.

Finally, it would also be misleading to portray political science in the Fifties and Sixties as being entirely dominated by Behavioralism. As it will be shown, Riker's formal approach was developed during this period partly in opposition to Behavioralism, though not exclusively so (Adcock 2009). Other scholars have also demonstrated that, at least up to the Fifties, the historical development of political science followed broadly similar trajectories in the United States and Great Britain (Ross 2009). This observation could be taken to justify writing the history of political science primarily as a function of the American experience. However, alongside the functionalist interpretation of Behavioralism as the unique and narrow path leading to the contemporary discipline, the main risk remains that of producing a "Whig" narrative of the history of political science.

Despite the fairness of many of these critiques, nonetheless retaining an incremental view of the development of political science is justified by the aim of the present chapter, which is not to provide a comprehensive history of the discipline, but rather to outline a simplified, yet sufficiently detailed, framework of the institutional and disciplinary status of political science up to the Fifties.

1.1 The Development of Economics

1.1.1 The rise of Mathematical Economics

This section presents some features of the dramatic changes that occurred in economic theory in that period, culminating in the creation of the Theory of Games and in the broader mathematization of economics. However, issues such as business cycles, theories of utility, and theories of costs will not be discussed. Instead, the focus will be on selected aspects of the mathematical transformation of economics and on its role in reshaping economic theory in the axiomatic neo-classical fashion of the second postwar.[3]

[3] Usually, comprehensive histories of economic thought end with Keynesian economics, offering only a glance at the most contemporary theories or at Mathematical Economics. By contrast, the works devoted specifically to these topics are often extremely detailed historically, yet they do not always provide a broader account of the development of economic theory. It therefore often seems difficult to read them properly without mastering the earlier literature. A classical general work that remains highly detailed on the development of economics between 1870 and

In the Thirties, despite the so-called marginal revolution of the last quarter of the nineteenth century having introduced increasing levels of abstraction to deal with counter-intuitive concepts such as marginal utility or marginal productivity, significant methodological differences still separated distinct "schools." These differences concerned the use of mathematical reasoning, statistics, and competing views about the meaning and scope of economics. In the "years of high theory" (Shackle 1967), the main issues at stake included consumer theory, business cycles, production, and capital theory; and, from a methodological standpoint, whether economics was an a priori discipline or could instead adopt a neo-positivist perspective. An important issue, largely absent from Shackle's narrative but addressed by Weintraub and by Ingrao and Israel (Weintraub 1983; Ingrao and Israel 1987), was the adoption of the axiomatic method to tackle fundamental economic problems such as modeling individual rationality and, on that basis, establishing early results on general economic equilibrium. Moreover, these scientific and theoretical efforts were deeply embedded in parallel transformations in mathematics and theoretical physics.

Some authors have described the radical mathematization that followed the Second World War as a "formalist revolution" (Ward 1972; Blaug 2003). In Mark Blaug's words, its central tenet is "not just a preference, but an absolute preference for the form of an economic argument over its content." Moreover, "the Formalist Revolution was much more than applying mathematical techniques to economics. It was, rather, reveling in mathematical modeling as an end in itself and treating the equilibrium solution of the economic model as the final answer to the question that prompted the investigation in the first place." (Blaug 2003, p. 396)

Other scholars, by contrast, have preferred to avoid this label, given the double ambiguity of the concepts of "revolution" and "formalist" (Giocoli 2003b, p. 6). Yet, whatever the terminology, the change in economic theory and practice between the Thirties and the Fifties is undeniable.

Debates over the appropriate role of mathematics in economics date back to the mid-nineteenth century, to authors such as Augustine Cournot, William Whewell, Hermann Heinrich Gossen, Jules Dupuit, and Johann Heinrich von Thunen, among others. The marginal revolution after the 1870s strengthened the relationship between mathematics and economics, while also introducing a new element. The standard justification of mathematical economics rested on the intrinsically quantitative nature of economic phenomena (a position still broadly associated with authors such as William Stanley Jevons and Leon Walras). However, more mathematically trained scholars, such as the American Irving Fisher, explicitly defended the use of mathematics in economics on different grounds: as a means of assessing the

1930 (and whose knowledge helps to understand more specific results) is Blaug 1997. Another classical work, although much less general than Blaug, is Shackle 1967. Particular issues are treated in Ingrao and Israel 1987; Düppe and Weintraub 2014b (General Economic Equilibrium); Morgan 1990 (Econometrics); Weintraub 1991 (Economic Dynamics); Weintraub 2002 (Mathematical Economics); Giocoli 2003b (Game Theory and Rationality); Leonard 2010 (Creation of Game Theory); Moscati 2018 (Utility Theories)

internal consistency of a theory (Fisher 1892). Fisher's view anticipated positions that would become customary decades later.

In 1909, the Irish-born, Oxford-based Francis Ysidro Edgeworth offered a synthesis of both perspectives. For Edgeworth, economics possessed the essential condition for applying mathematics, namely the constancy of quantitative relations (his example being the law of diminishing returns) (Edgeworth 2008). At the same time, he argued that the usefulness of mathematics was not limited to expressing quantitative relations. As he put it: "[. . .] the mere statement of an economic problem in a mathematical form may correct fallacies. Attention is directed to the data which should be required for a scientific solution to the problem [...] The mathematical method is useful in clearing away the rubbish which obstructs the foundations of economic science, as well as in affording a plan for the more regular part of the structure." (Edgeworth 2008, p. 461) Following Alfred Marshall's prominent position, Edgeworth also emphasized the risks of abusing mathematical reasoning, particularly its tendency to be overrated. He therefore maintained that mathematical treatment is unlikely to be helpful unless a problem has first been studied and analyzed economically.

From the Thirties onward, mathematics acquired a new role within economic theory. Historians, philosophers of science, and practitioners have offered different interpretations of this transformation. The common challenge is to explain how changes in economics reshaped its relationship with mathematics, up to the incorporation of certain features of the latter (Weintraub 2002). In this regard, the radical transformations of late nineteenth- and early twentieth-century mathematics,especially axiomatization and David Hilbert's formalist program, played an important role. The key question is therefore how axiomatic mathematical theory entered economics, and how this differed from earlier uses of mathematics (such as calculus). More broadly, the issue is closely tied to debates in the philosophy of science about the nature of scientific knowledge, particularly within the German-speaking intellectual, scientific, and philosophical world (including neo-positivism).

Closely related is the question of how "mathematical economics became only economics" (Orozco Espinel 2020), that is, how mathematical methods became dominant within the discipline, eventually marginalizing most theoretical approaches other than the mathematical ones.

A historian may thus propose the following rational outline of the interwar debates in economics. Proper definitions and concepts were needed to provide reliable knowledge of economic phenomena. One potentially helpful concept was equilibrium, which in turn raised new issues: how equilibrium might be reached and how it should be defined. These discussions also involved rationality and the analysis of economic action, and thus broader questions about the nature of economics as a science. Psychological and hedonistic explanations, it was argued, had to be set aside insofar as they could not be adequately demonstrated. The same was said of theories relying on perfect foresight. For some authors, this undermined the general validity of equilibrium approaches altogether (a route followed by Friedrich August von Hayek: Hayek 1937). Others, while raising concerns similar to

Hayek's (see, for instance, Morgenstern 1976a), pursued instead a possible way out through the increasing mathematization of economic analysis. Philosophical discussions concerning the status of economics as a science, and, more generally, the nature of scientific knowledge, also provided a favorable assessment of the mathematical approach, beyond the standard appeal to quantitative analysis.

In addition, contingent political and social concerns played a role. As Robert Leonard has shown in the case of John von Neumann, concrete political developments such as the disruption of the European political order after the Nazi rise to power were central to his reflections on the concept (and meaning) of equilibrium and, more generally, on the concept of solution in his work (Leonard 2010). Similar considerations apply to figures such as the Austrian mathematician Karl Menger, whose role in connecting mathematics and economics, and the communities of their practitioners, was pivotal.

To appraise these developments properly, historians of economics have explored the evolution of economic theory, mathematics, and the philosophy of science, as well as the personal and institutional connections among scholars across different places. According to Weintraub: "[...] The history of economics involves a history of not only the development of economic knowledge but the development and changes in the image of economic knowledge [...] Consequently, a discussion of the interconnection of mathematics and economics requires not just attention to the interconnection of the bodies of knowledge, as is reflected in the historical discussion of mathematical economics, but a historical discussion of the interconnection of their respective images of knowledge." (Weintraub 2008, p. 462)

Giocoli, as noted above, summarizes this process as a transformation of the image of economics from a "system of forces" to a "system of relations," in which general equilibrium and its "existence theorem" occupy a central position. More specifically, Giocoli and Weintraub apply the historian of mathematics Leo Corry's dichotomy between "body of knowledge" and "image of knowledge" to twentieth-century neo-classical economics (Corry 2003; Weintraub 2002; Giocoli 2009a). In Corry's view, the "body of knowledge" comprises a discipline's theories, facts, methods, and open problems. The "image of knowledge," by contrast, concerns attitudes about what counts as an open problem, what kinds of arguments are relevant, how disagreements are adjudicated, and which methodological standards apply. It also includes the emergence of institutions devoted to evaluating intellectual contributions, new academic curricula, and related developments (Giocoli 2009a, p. 23).

According to Giocoli, a major shift in the image of economics occurred after the Second World War, with significant consequences for its body of knowledge. The two poles of this transformation were, on the one hand, the traditional view of economics as a discipline dealing with a system of forces (an image associated with conventional economics and embraced by early marginalists) and, on the other, the new view of economics as a discipline dealing with a system of relations. This shift in the image of the discipline also helps explain the modern conception of rationality in economics, that

is, Rational Choice Theory.

The movement toward the "system of relations" image was influenced by new philosophical concerns about the epistemological foundations of science and the logical foundations of mathematics. Equally important, however, was the perceived stalemate of neo-classical theory in the interwar years. This stalemate arose from a series of unsatisfactory attempts to explain how equilibrium could emerge from the adjustment of individual agents' expectations and plans. Neo-classical economists struggled, on the one hand, to explain equilibrium as a balance of forces, namely "as the rest point of the system's dynamics," and, on the other hand, to incorporate into their models mental variables—that is, "the actual forces which determine the process of adjustment toward an equilibrium." (Giocoli 2003b, pp. 368–9)

Neo-classical economic theory followed two distinct, though not strictly parallel, paths in addressing these theoretical and empirical challenges. In Giocoli's terms, these were an "escape from psychology" and an "escape from perfect foresight." The first refers to freeing economic analysis from hedonistic accounts of valuation. Closely related is the adoption of ordinal utility in place of cardinal utility (see the classic contributions of Vilfredo Pareto, John Hicks, and Roy Allen). More broadly, psychology-free analysis—particularly in the work of Paul Samuelson, also enabled a more explicitly empirical foundation for economic theory through an operationalist approach (Samuelson 1947. On Samuelson's early life in the Thirties and Forties, see Backhouse 2017). The second path refers to attempts to develop a more comprehensive and purely theoretical framework, namely "a pure logic of choice," in which economic agents face informational problems without perfect knowledge.

These developments were also deeply influenced by debates in the philosophy of science. In this respect, the spread of neo-positivism, its commitment to a scientific conception of the world (including the possibility of a unified science), and its emphasis on logical-mathematical deduction had a profound effect on how economic theory was conceived. It is not accidental that many of these debates were concentrated in Vienna, where discussions of mathematical social science intertwined with debates on the logical foundations of mathematics and with the development of new forms of pure mathematics.[4]

Neo-positivism focused on the relation between mathematical propositions and the empirical verifiability of scientific statements as a basis for knowledge of the external world. Empirical verifiability, together with an emphasis on logical consistency, underpinned the key feature of logical positivism: the distinction between scientific and metaphysical assertions.

[4] In the case of economic theory, these debates involved figures such as Karl Menger, Abraham Wald, Morgenstern, and Karl Schlesinger, and also, albeit briefly, von Neumann. They engaged primarily with Walras's General Economic Equilibrium theory, which was reshaped in an axiomatic fashion in order, according to some interpretations, to establish a 'meta-theory' of economics, analogous to attempts to establish the logical foundations of mathematics (Punzo 1991). The postwar debates about the existence and properties of General Economic Equilibrium built on results developed in Vienna.

In debates on economic methodology, these issues can be summarized by the tension between a deductive, aprioristic approach grounded in reasoning and an empirical approach. Propositions in "pure economics," derived from generalized claims about human behavior in order to establish basic principles (such as diminishing utility or maximizing behavior), belong to the first. The opposing position instead emphasized verification as the relevant standard for economic claims and conclusions.[5]

As noted above, the development of mathematics between the nineteenth and twentieth centuries profoundly affected its later employment in economics. In the last decades of the nineteenth century, mathematics underwent a deep process of foundational reconstruction, which changed how it was understood, developed, taught, and applied. By 1900 the image of mathematics had changed, as had its practice. New problems emerged, particularly those concerning the foundations of mathematics, and new concepts such as infinity and the continuum of real numbers became central. Physics, too, went through a crisis, with new problems such as quanta and relativity. Addressing these new physical concerns required new mathematics.

New mathematics was also needed to overcome logical weaknesses in foundational questions, and one of its main outcomes was the elaboration of the axiomatic method. Under this approach, rigorous arguments are built from explicitly stated foundations and axioms. This logical grounding enabled the development of new branches of mathematics and provided new conceptual tools for producing mathematical knowledge. The notion of "rigor" acquired fundamental importance, both for explorations in mathematical logic and for supporting an autonomous notion of progress within mathematics. This process reshaped the relationship between mathematics, physics, the natural sciences, and the social sciences (Weintraub 2002. See also Weintraub 2008).

The central feature of this transformation was the development of the axiomatic method, largely associated with David Hilbert and his so-called "formalist program" for mathematics. With Hilbert's commitment to proof theory or "meta-mathematics," twentieth-century mathematics profoundly changed its face.[6] Historians of mathematics have offered different interpretations of the "formalist program," especially regarding Hilbert's aims and broader ambitions. In any case, it raised fundamental

[5] The champions of these positions are usually identified as two English economists: Lionel Robbins (for the abstract-deductive view) and Terence Hutchinson (Robbins 1932; Hutchison 1938). From a history of ideas perspective, this contrast is not unproblematic. It seems clear that mathematical economics, given its reductionism about economic action, and its reliance on mathematically convenient postulates about behavior, is closer to the "pure economics" view. Indeed, some scholars have emphasized the influence of Robbins's methodological views and of his famous definition of economics on postwar axiomatic economics (Backhouse and Medema 2009). However, as those scholars also recognize, Robbins, although not opposed in principle to mathematical economics, was not an advocate.

[6] Note, however, that Hilbert's conviction that all mathematics can be reduced to axiomatic form was undermined by Kurt Godel's incompleteness theorems (Weintraub 2002).

questions about the relationship between mathematical knowledge and reality, and about how the former could successfully represent the latter. In the development of twentieth-century mathematics, these issues generated intense philosophical debates among mathematicians; they also shaped how mathematics would later be employed in economic analysis. In Debreu's own view, a pivotal role was played by von Neumann and Morgenstern's *Theory of Games and Economic Behaviour* (1944), which opened the way for axiomatic economic theory (Debreu 2008; Neumann and Morgenstern 1944).

If, in the late Thirties and early Forties, "classical" mathematical economics reached a peak through the use of differential calculus and linear algebra to address economic problems framed as optimization problems (see, for instance, the work of John Hicks, Paul Samuelson, and Maurice Allais), *Theory of Games* opened a new course by introducing logical rigor into economic reasoning and, at the same time, new mathematical tools, primarily convex analysis and algebraic topology. Convex analysis complemented real vector analysis, through separation theorems, as a standard technique for obtaining implicit prices. Moreover, one of the most famous examples of algebraic topology in economic theory is von Neumann's 1937 generalization of Brouwer's fixed-point theorem (Neumann 1945). Letting aside the later reception of Game Theory within the economics profession, von Neumann and Morgenstern's work occupies a pivotal place in the history of postwar economic theory. Indeed, theoretical economists in the Forties found in it not only an axiomatic treatment of expected utility, but also "a clear, almost textbook-style presentation of topics such as the geometry of n-dimensional spaces, vector operations, hyperplanes and half-spaces, convex spaces." (Giocoli 2003a, p. 9) Through von Neumann and Morgenstern's work, a postwar generation of young economists, trained largely in U.S. economics departments and research foundations, assimilated new mathematical tools and encountered a new approach to mathematical economics and to economic theory.[7]

Game theory, also because of the role occupied by von Neumann (a mathematical genius deeply fascinated, at least in his earlier years, by Hilbert's formalist program), is among the most important examples of how the development of mathematics reshaped economic theory (Israel and Gasca 2009). Indeed, one hallmark of the "system of relations" image of economics is the emphasis on consistency at the expense of empirical truth. Such consistency can be demonstrated only by proving the absence of internal contradiction, in a manner closely analogous to mathematical proof.[8]

[7] An exception was the development of mathematical economics in the USSR, which followed a trajectory not dissimilar to that of the United States, though, unsurprisingly, its focus was not competitive markets but problems of decentralized production and planning. Nevertheless, from a mathematical standpoint, there was no technical difference.

[8] A constructive proof is a demonstration that outlines a procedure leading to the mathematical object whose existence is asserted (namely, calculability refers to the object under scrutiny). A non-constructive proof is a demonstration that works

Finally, the radical Bourbakist approach represented a further step. "Bourbaki" was the pseudonym adopted by a group of young French mathematicians beginning in the Thirties, whose aim was the complete refoundation of mathematics on a strict axiomatic basis. According to this collective, mathematics was not only a discipline concerned with formal axiomatic systems but, above all, an autonomous subject, separated from the outside world and from applications. In this sense, Bourbakism radicalized Hilbert's attempt to clarify the status of mathematical truth. Bourbakists aimed in particular to elaborate multivalent theories, achieving unity within mathematics by exploiting the full power of axiomatization. Philosophically, Bourbaki's mathematics also avoided many "foundational issues," thereby sidestepping the demise of Hilbert's formalist program following Kurt Godel's critiques (Weintraub and Mirowski 1994).

Bourbakism viewed mathematics as a storehouse of abstract forms, and this view, largely through Debreu, profoundly influenced mathematical economics in the second half of the twentieth century. Debreu's *Theory of Value* (1959) is perhaps the most striking example of Bourbakism in economics. In this short work, the French economist (who had personal connections to "Bourbaki") explicitly aimed to treat the theory of value "with the standards of rigor," stating that such allegiance "dictates the axiomatic form of the analysis, where the theory, in the strict sense, is *logically entirely disconnected* from its interpretations." (Debreu 1959, p. x, my italics; for Debreu's connections to the Bourbaki group, see Weintraub 2002; Düppe 2012)

Debreu also offered one of the most precise summaries of the postwar mathematical approach to economic theory (Debreu 2008). A rich mathematical structure can be developed to ground economic theory. The action of an economic agent, for instance, can be described by an input vector and an output vector for each commodity, that is, by a point in commodity space (a finite-dimensional real vector space). A social system can be divided into different states, each described by listing the actions chosen by each agent. Each agent selects an optimal action given the actions of all others. Listing these reactions yields a new state, and thereby defines a transformation of the set of states of the social system into itself. A state of the system is an equilibrium if and only if it is a fixed point of that transformation. The construction of a formal model can thus appear as "an inexorable process in which rigor, generality, and simplicity are relentlessly pursued." Debreu summarized the approach as follows:

> "An axiomatized theory first selects its primitive concepts and represents each one of them by a mathematical object. [...]The economic interpretation of the theorems so obtained

by contradiction (for instance, by showing that assuming the converse of what we are trying to prove leads to a contradiction). This difference is at the origin of the contrast between the so-called "formalists," who accepted both kinds of mathematical proof, and the "intuitionists," who rejected non-constructive proofs. In economics, there has been a progressive abandonment of constructive proofs in favor of non-constructive ones (Giocoli 2003b; Leonard 2010).

> is the last step of the analysis. According to this schema, an axiomatized theory has a mathematical form that is completely separated from its economic content. If one removes the economic interpretation of the primitive concepts, of the assumptions and the conclusions of the model, its bare mathematical structure must still stand." (Debreu 2008, p. 457)

This radical image was soon challenged, after its emergence in the Fifties, by a more applied view of mathematical economics (Weintraub 2008). These challenges notwithstanding, the Bourbakist-axiomatic image remained central for decades in shaping how economic theory was produced. Economists have been socialized into it, most notably through advanced training such as Ph.D. courses, thereby providing a striking illustration of a persistent difference between economics and other social sciences.

1.1.2 Institutions and Communities

The interwar and postwar changes also reshaped, in profound ways, the institutional and social history of economics. At the same time, these transformations were facilitated by specific institutional arrangements. In a sense, the mathematization of economics required the development of specialized institutions, courses, and curricula in order to become effective. In turn, the emergence of such structures strengthened the position that mathematical economics came to occupy within the discipline.

Whereas the late nineteenth century witnessed the definitive institutionalization of economics through the creation of new faculties, economics departments, journals, and national associations, the years from the Thirties onward saw the rise of the United States as the most important location for economic research. This "Americanization" of economics was certainly accelerated by the rise of Nazism in Germany (and the annexation of Austria in 1938), after which a large number of scholars (not exclusively Jews) sought refuge in the United States (Leonard 2010; Hagemann 2011). For instance, in Ingrao and Israel's view, the development of modern General Economic Equilibrium theory represents perhaps the clearest example of how Nazism dramatically impoverished Europe's scientific culture, contributing to the subsequent, largely uncontested, supremacy of the United States (Ingrao and Israel 1987, p. 245). After the Second World War, the United States assumed the role previously occupied by Great Britain, but with a crucial difference: the increasing mathematization of economics substantially reduced the distinctiveness of national traditions (the "national schools"), replacing them with a unified methodological orientation, neo-classical economic theory grounded in the modeling of individual behavior through concepts such as preferences and endowments.[9]

To this development one must add the rise of highly sophisticated statistical methods, namely econometrics. Thus, despite the persistence of

[9] The construction of economics as a discipline in Great Britain, between 1850-1950, including the differences with the developments in the United States, has been reconstructed in Tribe 2022

internal differences in policy stances or even in technical matters[10], from the second postwar onward economics increasingly represented itself as a cohesive discipline, leaving little room for earlier "verbal" controversies such as those concerning the meaning of 'utility.'

This radical transformation unfolded, at least, in two distinct steps. The first involved re-framing standard economic problems in formal terms and elaborating (often creating) new theories, or reshaping existing ones—and new methods to address them (for example, the formalization of General Economic Equilibrium as a topological problem, or the development of linear programming). The second step, which followed, concerned the training and diffusion of these new theories and methods.

This second process became particularly important from the late Fifties onward, especially through projects promoted by institutions such as the Social Science Research Council.

The development of mathematical economics in the interwar years, particularly in the Thirties, was therefore facilitated by the establishment of institutions and associations, often initially animated by only a few scholars, devoted quite exclusively to these scientific endeavors and supported by private funding. One cannot understand the "scientific" turn in economics without considering institutions such as the Cowles Commission, the RAND Corporation, the Econometric Society, and the National Bureau of Economic Research (NBER), as well as key sites such as Princeton's Institute for Advanced Study. Yet, iron curtains did not isolated these institutions from each other. On the contrary, they were frequently intertwined, whether through shared affiliates or through overlapping funding sources.

The Cowles Commission for Research in Economics was founded in 1932 through the funding of Alfred Cowles III, a very wealthy investor and businessman interested in stock market forecasting. Despite this practical origin, with the assistance of the mathematician Harold T. Davis, Cowles involved a broad range of academic and professional economists. The Commission's agenda was soon expanded beyond business forecasting to include issues such as employment and the development of mathematical methods and quantitative analysis in economics more generally. From its inception, the Commission was closely connected to the Econometric Society (still the most prestigious association of mathematical economists within the discipline). Among its early academic supporters were figures such as Fisher, president of the Society, and Charles Roos, a mathematician who served as secretary-treasurer of the Society (Dimand 2019; Christ 1952).

The Econometric Society, established in December 1930 during the joint annual meetings of American scholarly associations in Cleveland (Ohio), was the first international disciplinary association in economics (Bjerkholt 2015). Its creation was promoted chiefly by Roos, Fisher, and the Norwegian economist Ragnar Frisch (later the 1969 Nobel laureate).[11] Fisher served

[10] For instance, among econometricians, the proper relationship between theories and measurement (Dimand 2019). A point briefly addressed below.

[11] Frisch was also the first to coin and to use the term "econometrics," in 1926, to define the new discipline intermediate between mathematics, statistics, and economics.

as its first president, while the Society's Council included nine members, American and European, drawn from both mathematics and economics.[12] In the Constitution of the Econometric Society , adopted at the first meeting (1930) and published in the first issue of the Society's journal, *Econometrica* (1933), the association was defined as "an international society for the advancement of economic theory in its relation to statistics and mathematics [...] completely disinterested, scientific organization without political, social, financial, or nationalistic basis" whose "main object shall to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences." (Roos 1933, p. 106) The charter further stated explicitly that any activity intended to foster such unification between theoretical and quantitative studies fell within the Society's sphere of interest (ibidem). Thus, the Cowles Commission and the Econometric Society in parallel and, while formally distinct, frequently intersected. Alfred Cowles also financed the publication of *Econometrica* and served as treasurer and secretary of the association during the Thirties. As Robert Dimand notes, Cowles's role as a private benefactor of research in economic theory, mathematical economics, and econometrics in the Thirties United States is comparable to the better-known role of Alfred Loomis in American physics (Dimand 2019). Later, in the Forties and Sixties, the Commission also benefited from additional sources of funding, alongside Cowles's initial support, including the Rockefeller Foundation, the RAND Corporation, and the Office for Naval Research.

Initially, the Cowles Commission was based in Colorado Springs (CO), where it remained until 1940, before moving to Chicago (1940–1955) and later to Yale University, where it remains active as the Cowles Foundation for Research in Economics at Yale University. During the Colorado years, its principal research interest was the study of economic fluctuations and business cycles using advanced statistical tools. After the move to Chicago, where it was affiliated with the University of Chicago's Department of Economics, the Commission broadened its focus toward the foundations of economic theory. This shift was largely associated with the influence and leadership of the Russian Empire-born economist Jakob Marschak, director of Cowles from 1940 to 1948.[13] In a statement outlining the Commission's activities, Marschak emphasized the importance of developing statistical

[12] These were: Luigi Amoroso (University of Rome), Ladizlaus von Bortkiewicz (University of Berlin), Arthur Bowley (London School of Economics), Francois Divisia (École Nationale des Pontes et Chaussées, Paris), Frisch (University of Oslo), Roos (Smithsonian Institute), Joseph Schumpeter (University of Bonn), Edwin B. Wilson (Harvard University) and Wladimir Zawadzki (University of Wilno).

[13] Marschak (1898-1977) obtained his Doctoral Degree in Germany, in 1922, after escaping the Soviet Union (where, during the Civil War, he was a member of the Menshevik Party and, for a while, labor secretary of the Soviet Republic of Terek). From Germany, he moved to Great Britain after the nazis' rise to power and later to the USA. For an intellectual and biographical sketch, see Arrow 1991; Hagemann 2011

techniques distinct from those typically employed in other empirical disciplines, as well as new mathematical methods: "the available results of mathematical analysis are currently applied and tried out in econometric investigations; conversely, new situations arising in the course of practical work present new problems to the mathematician." (Christ 1952) Alongside the refinement of econometric techniques, Cowles also became an incubator for strictly mathematical treatments of theoretical economic problems in the late Forties and early Fifties. To that end, Marschak defended a shift away from an exclusively empirical and quantitative emphasis toward "the general theory of mathematical development." Among the theoretical strands that flourished at Cowles in this period were new formal approaches to General Economic Equilibrium, Social Choice Theory, and Rational Choice Theory.

In 1949, the Commission organized the well-known "Conference on Activity Analysis of Production and Allocation," whose importance, also from a historical and network, analysis perspective—should not be underestimated. As Düppe and Weintraub argue, "that conference defined more than any single event, the emergence of a new kind of economic theory growing from game theory, operation research, and linear programming and the related mathematical techniques of convex sets, separating hyperplanes, and fixed point theory." Moreover, "[t]he conference was the 'coming out party' of the community that would transform the practices of academic economists for decades to come," establishing "the historical conditions for economics to become a modeling science." (Düppe and Weintraub 2014a, p. 454)

Finally, in the Fifties, much of the major progress in the Theory of Games was produced by mathematicians, primarily at Princeton University. Yet Marschak was among the first to provide a comprehensive review of von Neumann and Morgenstern's theory, and to offer critical discussions of expected utility and rational choice during the Fifties (Marschak 1946; Herfeld 2018).[14]

The Cowles Commission was not the only institution committed to quantitative analysis in economics. In 1920, a group of scholars, most notably Wesley C. Mitchell, established, again with private funding, the National Bureau of Economic Research (NBER). Its most notable accomplishment was the first estimate of U.S. national income in 1934, followed by the construction of other indices of economic activity, including GDP (Fogel et al. 2013). Although devoted to broadly similar research concerns, the approaches of NBER and Cowles diverged sharply, especially in the Forties and Fifties. The divergence stemmed from the prominence that Cowles assigned to economic theory, to the point that, in a famous exchange with NBER affiliates Arthur Burns and Rutledge Vining, the Cowles economist (and future 1975 Nobel laureate) Tjalling Koopmans famously characterized NBER's approach as "measurement without theory." The controversy also involved, at least implicitly, differences in perceived policy orientations, with NBER often associated with more conservative positions and Cowles more open to debates over economic planning (Dimand 2019; Levy and

[14] For enjoyable (although necessarily historically incomplete) internalist histories of the mathematical economics at Cowles, see: Arrow 1983; Debreu 1983

Peart 2020).

If the Cowles Commission remained primarily an academic institution (especially after its affiliation with Chicago, and later Yale), other research centers that played a crucial role in shaping postwar formal approaches to economics and the social sciences did not. The most famous and influential is certainly the RAND Corporation, "the Think Tank Icon of Cold War America." (Amadae 2003) Accordingly, RAND's close connections with the U.S. military apparatus have frequently been used—often in a derogatory tone—to emphasize the intimate relationship between mathematical social science and Cold War politics (Mirowski 2002; Erickson et al. 2015). RAND was founded in 1946 as a private research enterprise, partly funded by the U.S. Army Air Force and the Douglas Aircraft Company. Its mission was to conduct research and development for military purposes, as well as for civilian applications (a comprehensive, though controversial, history of RAND's early years is provided in Amadae 2003). For the purposes of this work, two points deserve emphasis. First, despite its practical orientation, RAND also financed theoretical research; among its affiliates, at different times, were pure mathematicians such as John Nash and Lloyd Shapley. Kenneth Arrow's Ph.D. research on social choice also benefited from RAND funding, as well as from the connections between RAND and the Cowles Commission (Arrow 1951b, p. ix; Arrow 1983). Second, and more than at Cowles (and in a different manner from Princeton's mathematics department), RAND researchers developed applied implications of game theory, particularly concerning optimal strategic behavior and its use in military and international political contexts (Leonard 2010).

Finally, some attention must be devoted to the role of specific academic departments. For the development of game theory, the central location was certainly Princeton's Department of Mathematics and, to a lesser extent, Princeton's Institute for Advanced Study.[15] John von Neumann obtained his U.S. academic affiliation at the IAS when he moved permanently to the United States in 1933 (though he spent a relatively small amount of time in New Jersey, especially after the beginning of World War II). The Institute itself had been established in the same year through the efforts of the progressive educator Abraham Flexner. Despite its intended interdisciplinarity, it is best known for its School of Mathematics, which hosted internationally renowned figures such as Albert Einstein, Hermann Weyl, Oswald Veblen, and Kurt Gödel, in addition to von Neumann.[16] Yet, well before the establishment of the Institute, Princeton University

[15] Given the scope of this work, the focus is on the intellectual development of game theory, and then no emphasis will be given to such otherwise essential places for the development of mathematical Economics like Paul Samuelson's MIT Economics Department (Backhouse 2017; Weintraub 2014).

[16] This statement is not an undermining of the importance of the Institute for historical studies and social sciences. For instance, among the members in these fields, we find, since the second postwar, such people like the medieval historian Ernst Kantorowicz, the art historian Erwin Panofsky, the economist, and historian Albert O. Hirschman, the international relations scholar and former ambassador George Kennan, among the others.

already possessed a leading mathematics department, with strong interests in recent developments in pure mathematics—such as topology—and strong connections with continental European mathematicians and institutions, especially Göttingen, Hilbert's university.

Similarly, Oskar Morgenstern, after the Anschluss, obtained a faculty appointment in Princeton's Economics Department in 1938, where he spent most of his career (Leonard 2010). Thus, despite clear earlier influences from French and German-speaking mathematical traditions, game theory was, in an important sense, a product of Princeton. Some of its applications were extensively studied at RAND, and Cowles also devoted attention to it, even if Cowles affiliates soon shifted their focus toward the broader framework of general equilibrium. However, it was at Princeton in the Fifties—after Cowles theoreticians largely lost interest, and in parallel with RAND activity—that some of the most important results in game theory, and the foundations for later developments, were produced.

The most prominent example is the concept of Nash equilibrium, the topic of John Nash's Ph.D. dissertation in mathematics (1950). Among the scholars affiliated with Princeton's mathematics department who made pivotal contributions to game theory (and beyond) were Albert Tucker, Harold Kuhn, Lloyd Shapley, Nash, Sam Karlin, and David Gale.

Martin Shubik offered a first-person account of Princeton as a birthplace of Game Theory in the Fifties. Although enrolled as a graduate student in economics, Shubik developed close connections with Nash and Shapley (Shubik 1992) and he was among the few economists in the Fifties who expressed a genuine interest in the Theory of Games and were willing to employ it. In his account, Shubik emphasized the differences he perceived between the economics department, where he was formally affiliated, and the mathematics department. In economics, despite the presence of renowned theorists such as William J. Baumol and Jacob Viner and a broadly favorable attitude toward mathematical economics (especially on Baumol's part), Game Theory attracted remarkably little interest. As Shubik remarked:

> "[. . .] [G]ame theory apparently had little impact on the economics department. William Baumol raised questions about the value of the measurable utility assumption used in much game theory work at that time; outside of Princeton Karl Kaysen had questioned the worth of game theory in economics. The view was that in spite of favorable reviews of Leonid Hurwicz and others, this new mathematical bag of tricks was of little relevance to economics.[17] This view was put forward in particular by Jacob Viner, whose favorite comment on the subject was that if game theory could not even solve the game of chess, how could

[17] An example is Schumpeter's attitude, as reported in Giocoli 2003b, p. 355 It must be recalled that Schumpeter was among the founders of the Econometric Society, and his view toward mathematical economics (letting a part his effective managing of it) was extremely positive, up to the point that he exercised a great influence on young Samuelson, where the latter was Harvard Graduate student in economics, in the late Thirties (Backhouse 2017; Swedberg 1991).

> it be of use in the study of economic life, which is considerably more complex than chess." (Shubik 1992, p. 152)

More generally, the attitude of scholars such as Viner toward game theory resembled that of other senior figures (Viner was born in 1892), even when they were not hostile to the mathematization of economics as such. In Shubik's narrative, moreover, the difference extended beyond Game Theory and reflected a broader contrast in the intellectual orientation of the two departments. Thus:

> "The contrast of attitude between the economics department and the mathematics department was stamped on my mind soon after arriving at Princeton.[...] The contrast [...] at that time has some lessons to teach. Besides Morgenstern, there were some fine scholars in economics such as Viner and Baumol, but there was no challenge or apparent interest in the frontier of the science. Morgenstern was to some extent an inconvenience. To me, the striking thing at that time was not that the mathematics department welcomed game theory with open arms - but that it was open to new ideas and new talent from any source, and it could convey to all a sense of challenge and a belief that much new and worthwhile was happening." (Shubik 1992, pp. 61–2)

To conclude, the institutions and sites discussed above, namely, the Cowles Commission, the RAND Corporation, and Princeton's mathematics department, were among the most crucial settings (though not the only ones) in which the dramatic changes in economic theory, briefly outlined in the opening paragraph, took place. Yet, as noted, these developments were not the final stage but rather an early phase in the broader disciplinary transformation of economics. A further step was needed: the effective "conquest" of the discipline, that is, the broad adoption of these approaches by most scholars, students, and instructors.

This process was largely successful in economics. The situation was different in the other social sciences. There, the entry of quantitative analysis and statistical methods was comparatively successful and followed a path partly analogous to economics, though in different terms, especially in the second stage, owing to the role of the SSRC and the board for mathematical social sciences. However, a revolution in theory-building comparable to postwar neo-classical microeconomics (for instance, axiomatic general equilibrium) and to Game Theory did not occur. The closest attempt, yet far less successful than in economics, was that of Riker.

1.2 The Development of Political Science

1.2.1 Early debates on Political Science

Political science emerged in the late nineteenth and early twentieth centuries as a specialized field of study related to, but distinct from, disciplines such as constitutional law, history, and political philosophy. In particular, its

link with historical study was strong both intellectually and institutionally. For example, the American Political Science Association emerged from the American Historical Association. Moreover, until the Thirties, the overlap between history and political science remained substantial. In countries other than the United States, this close affiliation persisted into the second postwar period (Farr 2009).

Methodologically, history was regarded, until the Thirties and Forties, as the principal field through which political conceptions could be observed and tested. The obvious consequence was that political analysis remained inseparable from normative and prescriptive implications. Although anecdotal, evidence of this connection lies in the wide circulation (in both the United States and England) of the Oxford historian Edward A. Freeman's aphorism: "History is past Politics, and Politics is present History." This sentence became the motto of the Johns Hopkins University Studies in Historical and Political Science (Farr 2009, p. 68; Ross 2009).[18] This intertwining of political and historical analysis reflected the fact that the former was closely tied to liberal politics, through narratives of the historical development of modern polities often framed in evolutionary terms, with "whiggism" (in its political and historical sense) as a central tenet (Ross 2009). It also suggests that the discipline's institutional trajectory shaped its intellectual production, reinforcing a persistent tension between normative and positive theory (Adcock and Bevir 2010).

Within this section's deliberately simplified narrative, a few broad—and often intertwined—dichotomies can be advanced to delineate the development of "pre-behavioralist" political science. These include the (apparent) opposition between historical and empirical research, between normative and positive theory, and finally between competing conceptions of the proper object of inquiry, statism or pluralism. These distinctions remain very general, and in practice they frequently overlap.

The first dichotomy is not, strictly speaking, an opposition. By empiricism, one should not think primarily of sophisticated debates in the philosophy of science, but rather of a broader appeal to the "scientific method." Despite the prevalence of historicism, early political science was not devoid of methodological discussion concerning its scientific status, that is, what "science" could mean in the analysis of political phenomena. The emergence of political science was indeed conceived by its early proponents (including Lord Bryce, Lawrence Lowell, and others), both in the United States and Great Britain, as a "scientific" project whose model was the natural sciences. Consequently, central issues included the impartiality of the scholar—at least in a broad sense compatible with the normative implications of much work—and questions of "method." Method, in turn, encompassed the application to the moral sciences of the empirical approach articulated by John Stuart Mill in his *System of Logic* (1843), as well as

[18] Another anecdote is in the reminiscences of the political scientist Charles Hyneman, whose mentor at Indiana University in the Twenties, Frank G. Bates, used to say that a political scientist is a middle ground between a poor lawyer and a poor historian. (Baer, Jewell, and Sigelman 1991, p. 9)

the Rankean historical method based on archival research.

The influence of more sophisticated versions of positivism was limited, though not negligible. There were debates about how impartiality and objectivity might be secured through quantitative methods and statistical tools. In the early twentieth century, the growing use of such techniques—still rudimentary—became a distinctive feature of American political science. Yet significant exceptions existed, such as the British scholar Graham Wallas, a professor at the London School of Economics and Political Science.[19]

In discussing the method of political reasoning, Wallas explicitly rejected a priori deductive approaches grounded in "personifications and uniformities" of political abstractions and types, and in "large and untried generalizations," and defended instead the use of quantitative analysis (Wallas 1920, pp. 138–9). Wallas's case is instructive. In his major work, *Human Nature in Politics* (1908), he developed a critique of the "intellectualist fallacy" associated with a priori deduction, which, in his view, overstated the power of human reason and neglected psychological features such as "impulses and instincts." His argument drew on psychological traditions (notably William James and the "Psychologie de la foule" theorists such as Gabriel Tarde and Gustave Le Bon) as well as on evolutionary reasoning (following Darwin).

Although elements of Wallas's discussion are anecdotal (not least given his own inclination toward political engagement), the core of his argument is systematic. Against the long-standing tendency in political analysis to search for "[. . .] 'standard'. . . facts about man which should bear the same relation to politics which the fact that all things can be weighted bears to physics, and the fact that all things can be measured bears to geometry" (Wallas 1920, pp. 120–1), he proposed a method aimed at "finding as many relevant and measurable facts about human nature as possible," and making them serviceable for political reasoning.

This method, explicitly compared to that of the biologist, is threefold: first, the description of such facts; second, their quantitative analysis; and finally, the analysis—both descriptive and quantitative—of the environment into which individuals are born, and of its effects on behavior. For Wallas, descriptive analysis remained inseparable from psychological considerations. For this reason, he criticized works such as those of Moises Ostrogorski and Lord Bryce, arguing that both authors attempted to fit their observations of political reality in the United States and Great Britain to preferred conceptions of "ideal political men." He went so far as to claim that, in Ostrogorski's work, "one seems to be reading a series of conscientious observations of the Copernican heavens by a loyal but saddened believer in the Ptolemaic astronomy." (Wallas 1920, p. 125)

Wallas grounded the role of quantitative methods by connecting them, first, to recent developments in "mathematical biology," namely biometrics, drawing on Karl Pearson's contributions to statistical analysis (Wallas 1920, p. 132), and, second, to developments in contemporary economics. In the

[19] Wallas (1858-1932) was an English scholar, member of the Fabian Society, and among the founders, together with Sidney and Beatrice Webb and the playwright George Bernard Shaw, of the LSE, in 1895. Qualter 1980; Howson 2011

latter case, Wallas attached particular importance to the shift "from the abstract deductions about ideal economic men" (which he associated with classical political economy and linked to Walter Bagehot) toward a method based "upon the variety and not the uniformity of individual instances," which he associated with William Stanley Jevons and Alfred Marshall. Jevons, in particular, "[. . .] arranged the hours of labour in a working day, or the units of satisfaction from spending money, on curves of increase and decrease, and employs mathematical methods to indicate the point where one curve, whether representing an imaginary estimate or a record of ascertained facts, would cut the others to the best advantage." (Wallas 1920, pp. 141–2) For Wallas, this approach was no longer purely abstract, because it mirrored the process through which real individuals reached practical outcomes; moreover, it could be extended by statistical analysis.

Wallas's discussion of economics may appear at first glance to sit uneasily with his earlier emphasis on the psychological dimensions of human nature. Indeed, neo-classical economics is often criticized for excessive a priorism, abstraction, and deductivism in its account of individual behavior. Yet Wallas's focus was on the quantitative relations involved in political issues of very different kinds, from public finance to legislative politics. Thus, although his discussion is framed in an unmistakably marginalist language, it reflects, in my view, his adoption of a "psychological" interpretation of marginalism of the kind common among contemporary British economists.[20] For this reason, it would be misleading to treat Wallas as a precursor of an "economic approach" to political science. Nevertheless, his analysis anticipates several distinctive features of subsequent developments. Accordingly, despite being largely unsuccessful in Britain, his quantitative and "psychological" orientation became one of the trademarks of proto-behavioralist analysis (Merriam 1923; Adcock and Bevir 2010).

Methodological debates also encompassed disputes over the proper normative or positive content of political science. Political science, understood as a scientific study of politics and society, was also regarded as a tool either to defend the existing order (for instance, nineteenth-century British liberal institutions) or to advance reform programs. Many prominent scholars were themselves politically involved (as in the cases of Bryce, Ostrogorski, Wallas, and Charles Merriam, among others). Moreover, although the classical Weberian account of a "vocational" stance toward science entered the American scholarly community mainly in the second postwar period, questions concerning impartiality were already widely discussed, together with the appropriate balance between political engagement and political analysis.[21]

[20] for a comprehensive historical discussion about the different interpretations of marginal utility theories, and their relations with psychology, Moscati 2018

[21] James Bryce (1838-1922) was a member of the English parliament for the Liberal Party, Ambassador to the United States, and occupied important cabinet roles; Moises Ostrogorski (1854-1921) was elected in the first Duma of the Russian Empire, as a member of Constitutional Democrat (Cadets) party; Charles Merriam tried, unsuccessfully, to be elected as Major of Chicago for the Republican Party in 1911. On the reception of Weber scholarship among American Scholars, see: Scaff 2011)

For example, in a review of recent advances in political method, Charles Merriam, founder of the so-called "Chicago School of Political Science" (see below), explicitly distinguished between two attitudes: "practical political wisdom or prudence exhibited by men of the type of Hamilton, Madison, Adams and Jefferson; and on the juristic side by such masters as Marshall, Story, Webster, and Calhoun" and "systematic study of government," which, in his view, began only with the work of Francis Lieber. The first attitude belongs to political actors, the second to scholars; the two may occasionally coexist in the same individual, but they must remain distinct (Merriam 1923).

A further perspective on the early state of the discipline, its methodological concerns, and its central objects of inquiry is provided by Lawrence Lowell's presidential address to the "American Political Science Association" in 1910 (Lowell 1910).[22] According to Lowell, political studies suffered chiefly from a lack of precision and from the "imperfect development of the means of self-expansion." Although different aspects of political and social life could in principle be identified with precision, their fundamental importance was often neglected. This was particularly true, he argued, for political science, which he defined as the "physiology of politics," that is, the study of how government actually works. Accordingly, debates about "living topics as proportional representation, the referendum and initiative, and the reform of municipal government [...] are for the most part conducted in the air. They are theoretical treating mainly of what ought to happen, rather than what actually occurs; and even when they consented to deal with facts is usually on a limited scale with very superficial attention to the conditions under which the facts took place." (Lowell 1910, p. 3) At the same time, Lowell did not reject a normative purpose, namely the support of reform movements. In his view, only the scientific study of politics could provide the knowledge necessary for effective political action.

> "[...] [t]he ultimate object of political science is moral, that is the improvement of government among men. But the investigator must study it as a science, as a series of phenomena of which he is seeking to discover the causes and effects. He must not set out with prejudice for or against a particular institutions, or, indeed, regard politics from an immediate moral standpoint; for if he does he will almost inevitably be subject to a bias likely to vitiate his observation.[...] It is our province to discover the principles that govern the political relations of mankind and to teach those principles to the men who will be in a position to give effect to them hereafter".(Lowell 1910, pp. 4–5)

With respect to the object of analysis, in the years before the First World War and throughout the interwar period, American political science was shaped by two broad tendencies, commonly labeled "statism" and "pluralism." Here, too, the distinction should not be overstated: the field

[22] Lowell (1856-1943) was a scholar of comparative politics whose main famous work is *The Government of England* (1908). He also became president of Harvard University

was not organized around rigid partitions or exclusive analytical frames. Topics such as political parties, electoral mechanisms, voting procedures, and related issues were already explored to some degree (for an extensive list of relevant problems, see Lowell 1910, pp. 11–3). Still, the contrast between "statism" and "pluralism" reflects, on the one hand, the discipline's inheritance from constitutional law and, on the other hand, its growing interaction with adjacent social sciences. In this sense, neither statism nor pluralism constituted a unified intellectual movement with a single manifesto; rather, they were intellectual tendencies, articulated through multiple texts, often sharing broad aims and, at times, a common political agenda.

Statism was closely connected to a conception of the state derived from European legal and political philosophy, especially German *Staatwissenschaft*. During the so-called Progressive Era, this conception was developed within American political thought through historical analysis and empirical classification, often with a strongly normative purpose: to help establish a rationally organized state and political community. At times, political science aligned with progressive agendas on issues such as race, immigration, women's labor, eugenics, and others (Leonard 2016).

In reaction to what was perceived as the monism of statism, some authors, such as Arthur Bentley and Charles Beard, increasingly emphasized the role of groups and organizations with divergent interests. The existence of such groups suggested that the state was, in practice, a plural polity. With pluralist theories, the relation between normative and positive analysis took on new forms. For instance, scholars such as the British Harold Laski (prior to his conversion to Marxism in the Thirties) viewed pluralist analysis as a way to defend and justify social differentiation against the unifying aspirations of the modern state.[23]

At the same time, debates about pluralism encouraged some authors, while maintaining a normative role for the state as the locus of liberal democracy and as an instrument for solving social problems, to develop more systematic approaches to the study of group politics.

A particularly influential role was played by the so-called "Chicago School of Political Science," which developed from the Twenties to the Fifties around the central figure of Charles Merriam. Merriam's importance in shaping both the Chicago School and broader developments in political science cannot be overstated. In the Twenties, his contributions included methodological and historical reflections on the state of the discipline, the new direction of Chicago's political science department, and the creation of institutions and scholarly associations, notably the "Social Science Research Council," of which Merriam was the first chairman (1923), as well as the first National Conference on the Science of Politics (Merriam 1923). In 1926, Merriam also served as president of the American Political Science Association.

At Chicago, a group of scholars influenced by Merriam produced seminal

[23] On Laski's "normative pluralism" influence in American scholarship, see: Dryzek 2006; Gunnell 1995

work on voting behavior, African American politics, political psychology, urban politics, comparative politics, political parties, methodology, and related topics (Heaney and Hansen 2006). These studies employed advanced empirical techniques, including survey research, content analysis, field experiments, factor analysis, and innovative combinations of quantitative and qualitative methods. Chicago graduates later became among the principal forerunners of the Behavioral Revolution in the second postwar period, including V.O. Key Jr, Harold D. Lasswell, Gabriel Almond, Herbert Simon, and David B. Truman.

The distinctiveness of the Chicago approach can be further captured by a recurring theme in the oral histories of influential American political scientists: the contrast between Chicago and Harvard as alternative models of the scientific study of politics (Baer, Jewell, and Sigelman 1991). These were not the only graduate programs in political science during the interwar period. Yet, in many later narratives about the discipline's development, they came to symbolize two different orientations toward political inquiry.

On the one hand stood Chicago's social science milieu, shaped by Merriam and by scholars such as Harold Lasswell, Leonard White, and Harold Gosnell, all of whom had been Merriam's students. On the other stood Harvard's School of Government, commonly associated with a stronger historical and theoretical orientation, represented by figures such as the historian of political thought Charles McIlwain and the German scholar Carl Joachim Friedrich. A particularly clear reminiscence is offered by David Easton, who completed his Ph.D. at Harvard in the Forties but later obtained his first appointment at Chicago. In his words, speaking of Harvard, "it is difficult today to appreciate fully how inimical the whole atmosphere in the Department of Government was, at least among many of the senior professors, to the scientific method for the study of politics and society." (Interview to D. Easton, in Baer, Jewell, and Sigelman 1991, p. 198) At the same time, "you could not graduate from the program without being sensitive to the importance of theory in political research." (ibidem) Harvard shaped Easton's orientation toward theory grounded in empirical concerns; yet, in his own perception, it did not provide training in what he understood as political science. Easton ultimately remarked: "By the time I left Harvard, I just didn't know what political science was all about." (Baer, Jewell, and Sigelman 1991, p. 199)

By contrast, Easton described Chicago as "a place where 'a tremendous emphasis was placed not only on the solid empirical ideas but on the procedures and means that were used to attain these ideals, or, in common parlance, upon methods [...] the whole atmosphere and rhetoric was one of interdisciplinary research, the sense that all the social science were indeed." (Baer, Jewell, and Sigelman 1991, p. 201) Easton arrived in Chicago in the early Fifties, when, according to several accounts, many of the most distinctive features of the School were already beginning to fade, following Merriam's death and the departure of many of his students, who were replaced by scholars with markedly different orientations such as Hans Morgenthau and Leo Strauss (Heaney and Hansen 2006). Nevertheless, this decline coincided with the nationwide diffusion of many Chicago-style

themes and practices and with the Behavioral Revolution in political science, of which Merriam was undoubtedly among the principal forerunners.

1.2.2 The Behavioral Revolution

In the Thirties and Forties, American political scientists brought to completion the "epistemic shift" that had begun in the late nineteenth and early twentieth centuries. A further step occurred in the Fifties with the genesis and development of the "Behavioral Revolution." The growth of empirical work that distinguished interwar American political science provides an essential starting point for understanding the subsequent Behavioral Revolution. Behavioralism's transformative aspirations lay in the departures it prescribed in order to make political science more systematic. Political scientist Robert Dahl defined Behavioralism as a "protest movement" within the discipline, carried forward by scholars dissatisfied with the historical and juridical approach and seeking a more scientific analysis (Dahl 1961). Such a commitment was already present in authors such as Wallas and in the Chicago approach, and American political science began a significant shift in its disciplinary orientation and methodological commitments in the Forties. In this sense, Behavioralism can be interpreted as the culmination of a longer and more complex development, as shown in the previous section. However, it also introduced genuinely innovative elements, to the point that some practitioners described it as an instance of "Kuhnian revolution" (Truman 1965; Almond 1966). Although this view has been contested by contemporaries as well as later practitioners and historians of political science, it remains the case that leading proponents of the new approach were inclined to emphasize difference and to define themselves in explicitly revolutionary terms.[24]

The following pages do not aim to describe or contend with all these interpretations in detail. Rather, they will highlight some of the main features of Behavioralism, relying primarily on how its practitioners perceived it. The focus will be on the role of theory in Behavioralism.

In a well-known paper on the origins and characteristics of Behaviouralism, Robert Dahl pointed out one of its most important features, namely the absence of a precise definition: "[t]he behavioral approach [. . .] is rather like the Loch Ness monster: one can say with considerable confidence what it is not, but it is difficult to say what it is." (Dahl 1961, p. 249) Furthermore, according to another behavioralist, David Truman: "[...] it's a mistake to over standardize the definition of what [behavioral revolution] was. It was a kind of multifaceted expression of dissatisfaction with the constraint and the formalities of the conventional political science, which we had inherited." (Baer, Jewell, and Sigelman 1991, p. 148) Thus, for Dahl, Behavioralism can be understood simply as "[...] an attempt to improve our understanding of politics by seeking to explain the empirical aspects of political life using theories and criteria of proof that are acceptable according to the canons,

[24] See for instance Heinz Eulau's interview in Baer, Jewell, and Sigelman 1991, 178 et ss.

conventions, and assumptions of modern empirical science." (Dahl 1961, p. 256)

It is evident that such a definition is very broad, potentially encompassing almost any attempt to understand politics through empirical theories and approaches. Dahl further noted: "Those who were sometimes called 'behavioralist' [...] shared a mood: a mood of skepticism about the current intellectual attainments of political science, a mood of sympathy toward 'scientific' modes of investigation and analysis, a mood of optimism about the possibilities of improving the study of politics." (Dahl 1961, p. 255) He therefore concluded that " [...] 'the behavioral approach' might better be called the 'behavioral mood' or perhaps even the 'scientific outlook.'"(Dahl 1961, p. 258)

According to Adcock, what is now called the "behavioral movement" took shape as a loose grouping of scholars committed to disciplinary transformation and sharing, in broad outline, a common vision of a new political science. This vision stood in contrast not only to that articulated by figures such as Hans Morgenthau, Leo Strauss, or Eric Voegelin (often European by origin), but also to that of political scientists satisfied with the existing discipline (Adcock 2009, p. 188).

Dahl identified six factors behind the behavioral approach in American political science, all rooted in specific features of American culture. First, the role of Merriam and the influence of European emigration. Second, the Second World War, which pushed many scholars to participate in administrative and planning institutions. A third, and, in Dahl's view, even more significant, impetus, also related to the war, came from the Social Science Research Council, whose presidency was assumed by the political scientist Edward Pendleton Herring. In particular, Herring helped create the Committee on Political Behavior in the Social Science Research Council. This committee emphasized individuals' behavior in political situations by examining political relationships among individuals, with the aim of formulating and testing hypotheses about behavioral uniformities across different institutional settings. The remaining factors were the 1949 conference on political behavior, which connected several groups of scholars committed to these issues; the rapid development of survey methods and statistics, which increasingly presented themselves as "scientific"; and finally, the influence of institutions and private foundations such as Carnegie, Rockefeller, and Ford.

Political behavior became a central object of research in the Fifties. Although political scientists' attention to behavior pre-dated that decade, it was only then that it assumed a dominant place, aided by the consolidation of quantitative analytical techniques.[25] In this context, Behavioralism can be treated as a comprehensive approach that combined a research focus on political behavior, a methodological plea for science, a political message

[25] Up to the point that in a "state of the discipline" volume put together by the American Political Science Association in the Forties it was affirmed that political behavior had largely replaced legal structures as the cardinal point of emphasis among political scientists (Griffith 1948).

centered on liberal pluralism, and the organizing concept of a political system. Behavioralists emphasized individuals and groups and, according to some historians, can even be described as a relatively coherent group, with founding texts and a quasi-manifesto, such as Easton's *The Political System* (Easton 1953; Garceau 1951). Yet, as Easton stressed, the movement did not appear clearly defined to those who were labeled "Behavioralists." As he explained: "[...] It was more clearly definable by those who were opposed to it because they were describing it in terms of the things within the newer trends that they found objectionable. So some would define behavioralism as an attempt to apply the methods of natural sciences to human behavior. Others would define it as an excessive emphasis upon quantification. Others as individualistic reductionism." (Baer, Jewell, and Sigelman 1991, p. 207)

Despite this definitional ambiguity, a shared sense of Behavioralism as a transformative enterprise was common among its proponents—a "rallying cry to promote change." In this sense, the label functioned less as a description of an already accomplished intellectual shift than as an instrument intended to produce one. This point was explicitly recognized by Riker, who, in the Fifties, never joined the movement, while nonetheless sharing several of its critical impulses (Riker 1997).

In one of the early "post-behavioral" reconstructions of the discipline, Albert Somit and Joseph Tanenhaus listed a set of features that they described as the "key behavioralists articles of faith" (Somit and Tanenhaus 1967, pp. 177–9). These included an emphasis on prediction and explanation grounded in observation and data collection; the pursuit of interdisciplinarity and "self-conscious criticism" of methods and results; and an orientation toward "pure research," leaving aside normative aspirations to establish the "truth or falsity of values" such as democracy, freedom, or equality, which were regarded as beyond scientific validation. Theoretical development also played a central role in orienting and directing research. Behavioralists believed that systematic science depends on the cumulative interplay between theoretical innovation and empirical inquiry, and they set out to reshape both sides of this interplay. Therefore, according to Robert Adcock, the behavioralist agenda for disciplinary transformation comprised two strands. The first pursued an "agenda of empirical theory," in which empirical validation is embedded within theoretical development. Easton's work is perhaps the most important example of this strand. It also included works such as Lasswell and Abraham Kaplan's *Power and Society*, and Truman's *The Governmental Process* (Lasswell and Kaplan 1950; Truman 1951).

The second strand focused on the development and use of more sophisticated empirical research techniques. It involved a shift from a relatively "low-key" empiricism, with no strong commitment to quantification, toward a more methodologically ambitious approach. In doing so, it reshaped what it meant to be "scientific" in political science, namely, to be "systematic." This strand proved more successful than the attempt to build a cumulative and unifying political theory. It encompassed methods and analytical tools, often borrowed from disciplines such as sociology and psychology, for in-

stance, survey research applied to the study of voting behavior and public opinion (Dahl 2005; Campbell et al. 1980).

As noted above, the behavioral movement remains central to how political scientists conceptualize their discipline's past. For Adcock, Behavioralism was revolutionary in character and impact, but many changes were neither immediate nor as radical as simplified narratives suggest. For example, Dahl's emphasis on the empirical character of Behavioralism can be misleading. It obscures the importance attributed to theoretical analysis and the fact that empirical research existed well before the Fifties. More fundamentally, the core of the movement concerned systematization—how to study politics systematically—rather than a change in object of inquiry. In this sense, the most transformative aspects of Behavioralism lay in its call for a new kind of theoretical work and for more advanced empirical techniques. The movement's "revolutionary" character thus reduces, in large part, to the adoption of novel and more sophisticated research techniques (Adcock 2009). Yet, interpreting the Behavioral Revolution primarily through technical innovation risks obscuring the movement's more complicated—and often disappointing—impact on political theory. Behavioralism's conception of political theory rested on using "self-conscious" abstraction to produce analytical frameworks and generate scientific progress; empirical research was essential insofar as it was considered the key to cumulative advancement. However, the results were frequently judged unsatisfactory: no foundational general theory emerged, and neither a unified methodological nor a unified theoretical framework came to be accepted by most practitioners.

1.2.3 Political Theory and Political Science

David Easton's *The Political System* (1953) has been seen by some as the "manifesto" of the new intellectual tendencies toward exact reasoning and empirical analysis in political science during the Fifties (Dryzek 2006). However, Behavioralism does not occupy a large space in this work. Rather than offering a new comprehensive political theory or presenting novel methods for political analysis, Easton provided a detailed discussion of "the state of political science." His aim was "to define the terms of the dispute to show the behavioralists that there was an important theoretical component that they were missing and, at the same time, to show the anti-behavioralists that to be a behavioralist did not necessarily exclude an understanding of the importance of values or moral discourse." (Interview to Easton, in Baer, Jewell, and Sigelman 1991)

Accordingly, his analysis is filled with discussions about the history of the development of political science and about scholars' attitudes toward scientific debate. For instance, one of the most famous chapters of the book addresses "the decline of modern political theory" (Easton 1953, 233 et ss.). Easton attributed this decline to historicism, by which he meant the almost exclusive interest in the history of political doctrines that characterized American political theory (especially the work of Dunning, McIlwain, and George Sabine).[26]

Consequently, the "value aspects" of political theory, as well as its empirical orientation, were overshadowed by historical considerations about political ideas, with the result that this kind of political theory proved scarcely able to yield a valuable understanding of political realities. Clarifying the relationship between the "value aspects" of theory and their empirical basis lies at the core of Easton's 1953 work and of the subsequent development of his analysis.

Easton began by acknowledging the disappointment surrounding political science, ("a discipline already twenty-five hundreds of years old" [sic]) due to its failure to clarify the relationship between facts and political theory, as well as the "vital role in this partnership." (Easton 1953, p. 4) Therefore, "the search for reliable knowledge about political phenomena requires ultimately the construction of a systematic theory, the name for the highest order of generalization." (ibidem) Yet from the very first lines of his work, it is evident that Easton's attitude toward theoretical argument in politics was anything but unfavorable.

His concerns about theoretical developments in political science originated not from a possible "empirical drift" of social science away from theorizing, but rather from "the growing disillusionment about the whole of scientific reasoning as a way of helping us to understand social problems." (Easton 1953, p. 5) He explicitly defined this as a "flight from scientific reason." (Easton 1953, p. 6) Thus, in American political science: "[. . .] We have, in consequence, the peculiar condition among the members of this discipline of nominal acceptance of their role as scientists, with the rejection in the practice of the recognized logic and techniques of the scientific method. Today this historical reluctance to commit themselves to a scientific approach to social knowledge shows few signs of decreasing: indeed, because of the present intellectual mood in Western society as a whole, it is actually growing." (ibidem)

Easton identified two different but intertwined ways in which this anti-scientific attitude manifested itself. The first was a movement away from a rational attitude toward life and toward dispositions such as emotion, faith, or tradition. The second was an increasing tendency to develop critical arguments against scientific methods. He associated the first attitude with thinkers such as the English writer and political theorist Michael Oakeshott. According to the latter, any rational approach to understanding politics separated from the "act of politics" was useless, because reason was not merely a set of tools or techniques, but also embodied the "greater wisdom of prejudice, tradition and accumulated experience knowable largely through history." (Easton 1953, p. 19)

From these views derived a tendency to diagnose a crisis in the methods of the social sciences and to blame the scientific attitude for the failures to understand contemporary political facts. Unlike the repudiation of reason in the name of faith and tradition, these discussions focused on the alleged failure of the scientific method in the social sciences. Indeed, despite the efforts made by many scholars to define scientific reasoning in political

[26] This chapter is a reprise of a previous Easton's paper. Easton 1951

inquiry, many other social scientists and philosophers remained skeptical about the social sciences, their methods, and their scope.[27] Easton did not reject such criticisms altogether; rather, he rejected their most radical conclusion, namely the alleged impossibility of a reason-based analysis in political science.

In his view, the development of political science as a discipline had "misconstrued the nature of the tools required for the attainment of reliable, generalized knowledge," preferring the "accumulation of facts and the premature application of this information to practical situations" over the development of verifiable theories (Easton 1953, p. 37). He famously labeled this tendency "hyperfactualism" (Easton 1953, 66 et ss.), namely the accumulation of historical and "empirical facts" about politics without systematic generalization. Moreover, "[. . .] in becoming preoccupied almost exclusively with problems of applying this factual information, political science has impeded its movement towards a fundamental understanding of political life, a kind of knowledge that would place the relation of means to ends on a secure foundation." (Easton 1953, p. 89)

One of the most troubling aspects of the discipline's development, according to Easton, was the imprecision with which even its most critical concepts were defined, making it difficult for scholars to judge adequately between conflicting statements. To overcome this problem, he called for the "[...] gradual creation of a new meaningful vocabulary, to be distinguished from artificial and unnecessary jargon, the refinement of current concept, and the development of special techniques for observing and reporting data, collating and testing them." (Easton 1953, p. 46) An explicit conception of scientific verifiability, and of the possibility of cumulative knowledge, was therefore central to his program.

This "empirical" stance did not undermine the importance of theoretical orientation. By "theory," Easton did not mean normative judgments about values, customary in political philosophy (which he termed "value theory"), but rather attempts to identify relations among political facts. In this sense, he spoke of "causal theory." (Easton 1953, 52 et ss.)

The two are not opposed: one is implicated in the other, and the distinction between them concerns primarily their internal logic. "Causal theory" can be regarded as a proxy for the stage of development of any science, insofar as it enables cumulative knowledge through increasing insight into relationships among facts. However, given the complexity of social life, defining a "fact" itself presupposes theoretical reasoning. Facts therefore cannot be separated from theoretical assumptions about their definition: "A fact is a particular ordering of reality in terms of a theoretical interest." (Easton 1953, p. 53) This point is relevant because Easton's position parallels, in part, arguments advanced by scholars who denied a strict "distinction between facts and values," and thus questioned the possibility of a purely objective social science, such as Leo Strauss and his followers. Easton's position, however, was not to reject the scientific project

[27] among the earlier group, Easton inserted scholars like George Caitlin, Harold Lasswell, Abraham Kaplan, and Herbert Simon.Easton 1953, p. 22

in toto, but to defend it against what he regarded as its most damaging implications.

For Easton, the development of theory is central to the establishment of political science because "pure technical refinements", for instance, improved data collection or advances in statistics, are insufficient. "Political science today is confronted with the need to recognize that scientific understanding of political life is ultimately possible only by clarifying the broad theoretical premises of research." (Easton 1953, p. 63)

Easton identified three domains of problems confronting theoretical research: the basic concepts needed to orient political inquiry, the categories of data that must be considered, and the role of value judgment in theory construction. By "orienting concepts," he meant those concepts that allow one to distinguish political science from other social sciences (such as economics, sociology, or psychology). Perhaps the most significant contribution of the book falls within this category: the idea of a "political system" (pp. 96 et ss.).[28] Under this label, Easton included "all these kinds of activities involved in the formulation and execution of a social policy [...] the policymaking process... constitutes the political system." (Easton 1953, p. 129) This entails an explicit rejection of earlier theories grounded in overly simple conceptions of "State" or "power." Closely related is his famous definition of politics as "the authoritative allocation of values for a society." (Easton 1953, 29 et ss.), which he presented as a "convenient and rough approximation to a set of orienting concepts" for political analysis. Moreover, this definition opens the way to the task of specifying the meanings of concepts such as "policy," "authority," and "society."

The construction of theory also requires discussion of the types of data that must be examined. In the development of American political science, Easton distinguished between approaches emphasizing institutional aspects and those emphasizing psychological issues. Within the "institutionalist group," he further differentiated between scholars focusing on "governmental institutions" and those emphasizing non-governmental groups. The "behavioral" approach, by contrast, focused on "political behavior" and, in his view, represented the genuinely novel route toward political science as a systematic discipline.

Easton's third problem concerned the "moral foundations of theoretical research," namely the relation between objective facts and value judgments. For Easton, the impossibility of political research wholly free from values called for a thorough exploration of the "moral premises" at the core of scientific inquiry. This applied especially to political science, given its ethical inspiration: "Men want to understand the political system so that they can use this knowledge for their own purposes." (Easton 1953, p. 223) In this sense, the first part of the twentieth century and the second postwar period had witnessed a rejection of the positivist faith in the possibility of total moral neutrality. Yet, for Easton, "the mere statement [...] that

[28] This idea (with modifications) will become the backbone of the subsequent development of Easton's political science, but the premises are contained in this work. (Easton 1965)

values underlie all research does not in itself lead to the inevitable conclusion that these values must, by virtue of their presence, influence this research." (Easton 1953, p. 225)

Such influence, in his view, does not provide the criterion for evaluating scientific work, which instead depends on correspondence with reality. Nonetheless, since the relation between facts and values is inextricable, the task of social scientists is to improve the reliability of knowledge by making underlying values explicit, as a "moral prelude to our main empirical theme." (Easton 1953, p. 228) This requires what he termed the "attainment of moral clarity," which in turn "requires training and experience in the concepts and procedures of moral inquiry, the kind of analysis we usually associate in political science with the study of strict political (value) theory." (ibidem) This moral clarity does not consist only in formally stating relevant values; it also involves "the positive task of constructing an image of the political system flowing from these moral premises." (Easton 1953, p. 231) In this sense, Easton proposed not merely an appeal for transparency about values, but an attempt at constructive synthesis between values and facts, aimed at clarifying their full meaning.

The only discernible suggestion of a broad theoretical framework on the horizon of empirical research is, according to Easton, the "theory of political equilibrium." With this idea, he referred to two modes of analysis: first, a way to understand processes within a political system (what he called "general equilibrium"); second, a way to describe the system ("constitutional equilibrium"). Because general equilibrium, in his view, implicated both normative and descriptive issues, the key difference was that constitutional equilibrium could be defined more straightforwardly as an equilibrium determined by institutions, whereas general equilibrium concerned an equilibrium among political parties and actors. Thus, in Easton's words, "constitutional equilibrium [. . .] deserves those necessary conditions for the existence of a constitutional order within a nation and peaceful relations among nations." (Easton 1953, p. 293)

General equilibrium, by contrast, entailed two distinct ideas: the interdependence of all elements within a political system (already subsumed in the concept of "political system"), and the tendency of these elements to act and react upon one another until reaching a point of stability. If the first idea was not wholly novel (Easton traced its presence to the rise of pluralism and its emphasis on the multiplicity of social forces, especially groups), the second was oriented toward developing far-reaching conclusions in political analysis, including normative conclusions about outcomes within a given political system.

The main difficulty of these concepts lay in the entanglement of normative and descriptive premises and aspirations. From a descriptive standpoint, the question was whether the concept of general equilibrium could be developed into a full-fledged conceptual framework for political inquiry, comparable to its role in economics (even though Easton recognized that in economics the concept functioned primarily as an analytical tool rather than a "substantive description" of the system:(Easton 1953, p. 274). An adequate model of general equilibrium would require consistent quantitative

analysis as well as "mental operations" to reconstruct possible relations among interdependent political variables, in a manner analogous to economics.

Despite these difficulties, Easton argued that "the idea of a general equilibrium implicit in so much empirical work in political science [...] can help to perpetuate the notion that political activity is part of an empirical system and a process of change through time. These are insights which future attempts at theory construction can scarcely neglect." (Easton 1953, p. 306) By contrast, the advantage of "constitutional equilibrium" lay in its relative ease of use as a descriptive tool for characterizing political systems in terms of institutional arrangements.

In conclusion, Easton warned that a divide was emerging between different conceptions of political theory, namely between "political theory" and the "empirical part of political science." The way to overcome this division was the pursuit of "reliable knowledge" about political life. But such attainment "[...] depends upon the development of the kind of analytical tool we call a conceptual framework... a general theory provides just such a set of criteria. It seeks to identify the major variables significant for understanding political life and showing their most important relations. It provides some test for determining the significance of any piece of empirical research towards an understanding of the whole of political life; the empirical investigation, in turn, contributes to the continuing task of improving the correspondence to reality of any existing theory." (Easton 1953, pp. 317–8)

Easton's pages contain perhaps one of the most substantial discussions of the place of political theory in modern political science written by a scholar who, while pursuing originality, remained committed to the new empirical and quantitative developments of the discipline. This does not exhaust the issue of theory's place within the scientific orientation of political science—neither historically (for instance, debates also concerned the development of "comparative politics," Adcock and Bevir 2010; Almond 1966) nor methodologically and philosophically.[29] Nonetheless, Easton's work can be read as capturing a broader mood within the behavioral movement. Behavioralists were not strictly a-theoretical; rather, they favored developing a positive political theory. The main difference with respect to political theorists *à la* Strauss was their acceptance of empirical methods. Moreover, the difference between behavioralists and those authors (starting from Riker) who adopted the label "Positive Political Theory" lies primarily in their commitment to different analytical tools for political theory and political analysis.

1.2.4 The SSRC and the mathematical social sciences in the second postwar

As noted above, some scholars, most notably Dahl, have attributed considerable importance to the role played by the Social Science Research

[29] Moreover, the attitude shaped by Easton did not last, albeit in a central place, in the subsequent disciplinary development, wherein, the term "political theory" came to be identified with such strictly philosophical approaches like that of Strauss (and his followers) or Sheldon Wolin.

Council in shaping second postwar American political science, particularly emphasizing the executive leadership of the political scientist Pendleton Herring, who became its chairman in 1948 (Baer, Jewell, and Sigelman 1991).

The Council was established in 1923 by a heterogeneous group of academics and intellectuals affiliated with the American Political Science Association, the American Sociological Society, the American Economic Association, and the American Historical Association, and later joined by representatives of the American Statistical Association, the American Psychological Association, and the American Anthropological Association. The Council's purpose was to foster an institutional environment conducive to the development of systematic social science and to encourage cross-fertilization among disciplines, explicitly aiming to generate new insights into contemporary social problems. This ambition was primarily pursued through the improvement of research methods.

This aspiration was closely intertwined with the intellectual project of Merriam and the Chicago school of political science. As noted above, Merriam was among the founders of the Social Science Research Council. As explicitly stated by figures such as the economist Wesley C. Mitchell, the council endorsed a "union-of-science viewpoint," rejecting both conservative approaches to the social sciences and rigid distinctions between social and natural sciences. From this perspective, scholarly research was to be analytically separated from moral, social, and political goals, while at the same time acknowledging that contingent social and political problems provided essential inputs for social scientific inquiry.

The Second World War marked a decisive turning point in the history of American science. During and after the war, annual federal support for scientific research increased dramatically, reaching approximately 500 million dollars per year. Moreover, the second postwar period witnessed the establishment of the National Science Foundation as the primary federal funding agency for science education in the United States.[30]

With regard to the social sciences, the Council continued to pursue its dual commitment to interdisciplinarity and methodological refinement, although its activities became increasingly specialized and detailed.

Pendleton Herring articulated this vision of the social sciences in the inaugural issue of the Council official journal, *Items*, published in 1947. In this essay, Herring addressed fundamental questions concerning the nature of social science, the responsibilities of social scientists, their role in modern society, and the requirements for their further development. He adopted a deliberately general tone, speaking on behalf of the social sciences as a whole rather than distinguishing among individual disciplines. According to Herring, the social sciences "represent the approach to human

[30] The history of the often conflictual relationship between natural and social scientists over the role of the latter within the National Science Foundation has been the subject of detailed historical reconstruction. This history also includes internal debates among social scientists within the Social Science Research Council concerning the opportunities and risks associated with federal funding, particularly with respect to academic freedom. (See Solovey 2013, 27 et ss.).

relationship that emphasizes analysis rather than force" (Herring 1947, p. 2, italics in the original). Their importance thus lay in their capacity to enhance understanding of social forces and human relationships, enabling societies to confront real-world problems. Social sciences were not expected to deliver "early solutions for such an enormous range of problems," but rather to "provide an approach to these problems that enables the human skill released by factual inquiry, by experimentation, and by analysis to make their contribution" (Herring 1947, p. 3). To fulfill this role, Herring emphasized the need for conceptual clarification and "careful selection and rigorous training" of social scientists. Their task was not "to determine public purposes or humanistic objectives," yet "the work of social scientists can make great contributions to the commonweal" (Herring 1947, p. 4). At the same time, Herring explicitly acknowledged a central risk associated with these scientific ambitions, namely "the danger that social science, by perfecting manipulative skills, can be turned to anti-social purposes in the hands of unscrupulous leaders" (ibid.). Consequently, he stressed the necessity of reciprocal interaction and transparency between social scientists and the broader public. He concluded by outlining eight broad objectives for the "growing drive for sound scientific practice in the field of human relations" (Herring 1947, p. 6): (1) identifying the distinctive contributions of the social sciences; (2) encouraging scientific inquiry; (3) promoting acceptance of the social sciences among scholars, public authorities, and the general public; (4) fostering cooperation within and across disciplines; (5) focusing on methodological research problems; (6) applying social science to practical social problems ("social engineering"); (7) improving the training of social scientists; and (8) securing governmental subsidies, private support, and academic recognition.

The final objective was clearly connected to ongoing debates over federal funding. The preceding points, however, exemplify a broader postwar American attitude toward the social sciences that some authors have criticized as "scientism," with Hayek offering one of the most prominent critiques (see, e.g., Hayek 1952).

To advance these aims, the Council continued to operate through conferences and the establishment of specialized subcommittees, typically organized in a multidisciplinary fashion. Two of these are particularly relevant for the present analysis: the Committee on Political Behavior and the Committee on the Mathematical Training of Social Scientists.

An initial committee focused on political behavior was established in 1945 and included figures such as Herring, Charles Hyneman, and V. O. Key Jr.. However, the fully fledged interdisciplinary Committee on Political Behaviour was created only in December 1949, following a conference on political behavior held at the University of Michigan from August 27 to September 2 of that year, sponsored by the Council (Ranney 1974).[31] The conference brought together 29 social scientists: twenty political scientists, two sociologists, three anthropologists, three psychologists, and one

[31] A report on the conference was published in the final 1949 issue of *Items* (Heard 1949).

statistician. The topics discussed ranged from methodological issues in the study of political behavior to more specific subjects such as governmental organizations, social authority, community structures, and political history. Particular emphasis was placed on the psychological foundations of political behavior and on emerging research techniques, such as field inquiries, exemplified by V. O. Key's *Southern Politics* (Key 1949).

It is tempting to compare the significance of this conference with that of the contemporaneous "Conference on Activity Analysis of Production and Allocation," sponsored by the Cowles Commission. However, closer examination reveals important differences. The Cowles conference represented a genuine "who's who" of mathematical economics (Düppe and Weintraub 2014a), including contributors such as Arrow, Dantzig, Dorfman, Gale, Koopmans, Kuhn, Morgenstern, Samuelson, Simon, Tucker, and Hurwicz.[32] Moreover, the conference played a decisive role in shaping participants' intellectual trajectories and their understanding of disciplinary development (Arrow 1983).

By contrast, the Michigan conference included relatively few figures who would later become central to the historiography of political science, aside from Truman, Key, and the quantitative sociologist Paul Lazarsfeld, in addition to Pendleton Herring himself. This contrast may reflect the divergent paths pursued by economics and political science. The Cowles conference presented tools that were immediately applicable and technically transformative for economic research.[33] By contrast, the conference on political behavior was embedded in a scholarly environment primarily oriented toward reflecting on the premises, scope, and objectives of political inquiry. It neither represented a radical methodological rupture nor provided ready-made tools for advancing research. Rather, it signaled a broader intellectual mood within the discipline.

In other words, while economists and political scientists shared a common ambition, rendering their disciplines more scientific, they pursued this goal through different questions and along distinct trajectories. A young economist attending the Cowles conference in 1949 would have encountered novel theoretical frameworks and computational tools that contrasted sharply with prevailing practices in economics departments. A political scientist interested in systematic methods, by contrast, could often pursue such interests outside the Michigan conference, engaging with subcommunities that did not necessarily converge there.

The Committee on Political Behaviour, established in December 1949, was chaired initially by Key (1949–53) and subsequently by Truman (1953–64). Its central concern was "the development of theory and improvement in methods which are needed if social science research on the political process is to be more effective" (quoted in Ranney 1974). Among its concrete outcomes were major initiatives in survey research on U.S.

[32] This group included five future Nobel laureates in economics, as well as foundational contributors to mathematics such as Dantzig, Kuhn, and Tucker.

[33] Perhaps the most notable outcome was Dantzig's first proof of the simplex algorithm for solving linear programming problems.

presidential elections, beginning with the 1952 election and extending retrospectively to earlier contests, as well as efforts to refine analytical methods through conferences and summer seminars. The committee also played a key role in promoting comparative politics, which in 1954 became the focus of a separate committee, the "Committee on Comparative Politics".

While the Committee on Political Behavior concentrated on a substantive domain within political science, the Council also devoted considerable attention to the training of social scientists. In the early Fifties, this commitment took the form of a renewed emphasis on mathematical education, culminating in the establishment of the Committee on the Mathematical Training of Social Scientists in 1952.[34] The committee's activities and objectives were later reconstructed by Frederick Mosteller in a retrospective account published in 1974 (Mosteller 1974). In addition to organizing conferences and summer schools, the committee issued policy recommendations aimed at improving mathematical training for social scientists.

A policy statement published in *Items* in 1955 identified three strategies for addressing the mathematical needs of social scientists: the creation of specialized mathematics curricula for social science students; the introduction of dedicated sections within existing mathematics courses; and the reform of general undergraduate mathematics curricula to provide a common foundation for all students (Scientists 1955). The rationale for enhanced mathematical training lay both in the capacity to model social problems formally and in the ability to assess the usefulness and limitations of mathematical methods.

Focusing mainly on the first approach, the Committee members recognized that "the traditional undergraduate curriculum in mathematics - college algebra, trigonometry, analytic geometry, and calculus - does not afford satisfactory preparation to social scientists" (Scientists 1955, p. 14). Accordingly, they proposed some recommendations for undergraduate mathematical training. Moreover, some procedures for integrating mathematical and social sciences were suggested, like focusing on the social science training of mathematicians and interdisciplinary works. In fact, to them, " [...] social scientists will get better advice from mathematicians who participate than from those who are merely consulted. The most difficult question is often that of the mathematical formulation of social science problems. The social scientist should avoid both limiting the mathematician and having the mathematician limit him." (Scientists 1955, p. 16) The final section of the statement also addressed the issue of mathematical training for graduate and postdoctorate students. One possible solution was identified in the organization of summer institutes.

Focusing mainly on specialized curricula, the committee argued that "the traditional undergraduate curriculum in mathematics—college algebra, trigonometry, analytic geometry, and calculus—does not afford satisfactory preparation to social scientists" (Scientists 1955, p. 14). Alternative training

[34] The original members were William G. Madow (mathematician), E. P. Hutchinson (sociologist), Jacob Marschak, George A. Miller (psychologist), Frederick Mosteller (statistician), and Robert M. Thrall (mathematician) (Scientists 1955).

pathways were then proposed and it was encouraged closer integration between mathematics and the social sciences, including greater mathematical literacy among social scientists and greater social science exposure among mathematicians. In fact, to them, " [...] social scientists will get better advice from mathematicians who participate than from those who are merely consulted. The most difficult question is often that of the mathematical formulation of social science problems. The social scientist should avoid both limiting the mathematician and having the mathematician limit him." (ibidem)

The final section of the statement also addressed the issue of mathematical training for graduate and postdoctorate students. One possible solution was identified in the organization of summer institutes. The Council sponsored three summer institutes on mathematics in the social sciences in 1953, 1955, and 1957: the first at Dartmouth College, the second divided between the University of Michigan and Stanford, and the third, focused on advanced training, at Stanford.[35] Participants were drawn primarily from economics and psychology, with more limited representation from sociology and very few from political science. The committee's activities continued into the Sixties, increasingly concentrating on specialized topics (Orozco Espinel 2020).

Mosteller's 1974 retrospective can be read as a declaration of "mission accomplished," opening and closing with the observation that the mathematical training of social scientists was the product of a long and deliberate process rather than organic evolution:

> "The current level of mathematical training for social scientists in this country was not quickly achieved, nor did it grow by itself through natural evolution; instead, it has come about through a long, fairly deliberate process that has depended upon the ideas and contributions of a great many people and organizations [...] In 1953, in addressing the Council, I explained that the results would look meager for a long time, but that 'the carefully thought out program of the Council's Committee on the Mathematical Training of Social Scientists may move us slowly to a place we will be proud of in 20 years.' I think that has happened." (Mosteller 1974, p. 24)

This assessment underscores the decisive role played by the SSRC in early postwar American social science, not only in fostering interdisciplinary collaboration but also in reshaping the very image of the social sciences. The case of mathematics is emblematic in this regard. The activities of the Committee on Mathematical Training paralleled, and were partially reinforced by, the development of mathematical economics at institutions such as the Cowles Foundation and RAND. Although mathematical economics was already far more advanced than parallel developments in other disciplines, the initiatives and summer institutes of the Social Science Research

[35] For a detailed overview, see Mosteller 1974; Orozco Espinel 2020.

Council contributed to the broader diffusion of mathematical methods within economics as well (Orozco Espinel 2020).

Yet a striking feature of the committee's activities in the Fifties is the marginal role played by political science. Few political scientists appear to have participated in these training programs, at least as suggested by available reports. This absence may be explained by the simultaneous engagement of the discipline's most active figures in the Behavioral Revolution, which pursued the ideal of systematic social science along paths other than formal mathematical modeling. At the same time, this context renders those political scientists who did choose to pursue formal approaches to political analysis all the more distinctive, and their intellectual efforts correspondingly more deserving of historical investigation.