Galileo's Mathematical Errors

Viktor Blåsjö

Abstract: Galileo's abilities as a mathematician were far below that of many of his contemporaries. He made numerous technical mistakes — including several high-profile, mathematically erroneous applications of his own law of fall — that were swiftly spotted and corrected by the leading mathematicians of the day. Many aspects of Galileo's work can be viewed as consequences of this limited technical proficiency in mathematics. For example, he ignores Kepler's work and dismisses comets as a chimerical atmospheric phenomena: decisions that are difficult to justify on scientific grounds but which make sense if we grant that Galileo wanted to avoid technical mathematics at all costs. Instead he drops rocks, looks through tubes, rails against Aristotelian philosophers, and expounds at length about basic principles of scientific method: all of which can be seen as dwelling on precisely those parts of the mathematician's worldview that do not require any actual mathematics.

Keywords: Galileo, cycloidal area, orbital speeds, extrusion by terrestrial whirling, atmospheric theory of comets.

1. Cycloid

The cycloid is the curve traced by a point on a rolling circle, like a piece of chalk attached to a bicycle wheel. Many mathematicians were interested in the cycloid in the early 17th century, including Galileo. What is the area under one arch of the cycloid? That was a natural question in Galileo's time. Finding areas of shapes like that is what geometers had been doing for thousands of years. Archimedes for instance found the area of any section of a parabola, and the area of a spiral, and so on. Galileo wanted nothing more than to join their ranks. The cycloid was a suitable showcase. It was a natural next step following upon the Greek corpus, and hence a chance to prove oneself a "new Archimedes."

There was only one problem: Galileo just wasn't very good at mathematics. Try as he might, he could not for the life of him come up with one of those clever geometrical arguments for which the Greek mathematicians were universally admired. All those brilliant feats of ingenuity that Archimedes and his friends had blessed us with, it just wasn't happening for Galileo.

Perhaps out of frustration, Galileo turned to the failed mathematician's last resort since time immemorial: trial and error. Unable to crack the cycloid with his intellect, he attacked it with his hands. He cut the shape out of thick paper and got his scales out to have this instrument do his thinking for him. As best as he could gather from these measurements, Galileo believed that the area under the

Viktor Blåsjö, Utrecht University, Netherlands, v.n.e.blasjo@uu.nl, 0000-0001-8494-4567

Referee List (DOI 10.36253/fup_referee_list)
FUP Best Practice in Scholarly Publishing (DOI 10.36253/fup_best_practice)

Viktor Blåsjö, *Galileo's Mathematical Errors*, © Author(s), CC BY 4.0, DOI 10.36253/979-12-215-0266-4.07, in Marco Faini, Marco Sgarbi (edited by), *Errors, False Opinions and Defective Knowledge in Early Modern Europe*, pp. 87-103, 2023, published by Firenze University Press, ISBN 979-12-215-0266-4, DOI 10.36253/979-12-215-0266-4

cycloid was somewhere near, but not exactly, three times the area of the generating circle (Drake 1978, 19, 406).

This was no way to audition for the pantheon of geometers. Galileo was left red-faced when mathematically competent contemporaries solved the problem with aplomb while he was fumbling with his cutouts. These actual mathematicians proved that the cycloid area was in fact exactly three times the area of the generating circle, even though Galileo had explicitly concluded the contrary on the basis of his cardboard diorama. (The correct result was proved by Roberval in 1634. See Struik 1969, 232–8, Whitman 1943, Kline 1972, 350.)

When Galileo heard of others working on the cycloid challenge, he sought help on this "very difficult" problem from his countryman Bonaventura Cavalieri, a competent mathematician. "I worked on it fruitlessly," lamented Galileo. "It needs the mind of a Cavalieri and no other," he pleads, tacitly acknowledging his own unmistakably inferior mathematical abilities. (Galileo to Cavalieri, 24 February 1640, Drake 1978, 406. Cavalieri did not take up the problem—"I too left it aside" (Freguglia and Giaquinta 2016, 34)—but Torricelli solved it soon thereafter.)

It is interesting to contrast this with the very different reaction to the same problem by Galileo's contemporary René Descartes, the famous philosopher who was also a vastly better mathematician than Galileo. When Descartes heard of the problem he immediately wrote back to his correspondent that "I do not see why you attribute such importance to something so simple, that anyone who knows even a little geometry could not fail to observe, were he simply to look." (Descartes to Mersenne, 27 May 1638, AT.II.135, Jullien 2015, 171.) He then immediately goes on to give his own proof of the result composed on the spot. Descartes is not famous for his humility, but the fact of the matter is that a number of mathematicians solved the cycloid problem with relative ease, while Galileo was fumbling about with scissors and glue.

In the case of the cycloid, it is an unequivocal fact that Galileo used an experimental approach because he lacked the ability to tackle the problem as a mathematician. If Galileo could have used a more mathematical approach he would unquestionably have done so. I suggest that what is so glaringly obvious in this case holds for Galileo's science generally. Galileo's celebrated use of experiments in science is not a brilliant methodological innovation but a reluctant recourse necessitated by his shortcomings in mathematical ability.

The cycloid case also makes it clear *why* the mathematically able prefer geometrical proofs to experiments: the latter are notoriously unreliable. By relying on experiments unchecked by proper mathematics, Galileo got the answer wrong, and not for the first time nor the last. "Do not think that I am relying on experiments, because I know they are deceitful," said Huygens (*Oeuvres*.XI.115, Palmerino and Thijssen 2004, 189), and all other mathematicians with him. It had always been obvious that mathematics and science can be explored using experiment and observation. As Galileo says: "You may be sure that Pythagoras, long before he discovered the proof [...], had satisfied himself that the square on the side opposite the right angle in a right triangle was equal to the squares on the other two sides" (Galileo, *Dialogue*, OGG.VII.75, Wootton 2010, 85)—presumably by making numerical measurements on various concretely drawn triangles. But able mathematicians had always known that haphazard trial and error had to be superseded by rigorous demonstration for a treatise to be worth the parchment it is written on. It is this—and not ignorance of "the scientific method"—that explains why you don't see experimental and numerical data defiling the pages of masterpieces of ancient mathematics and science such as those of Archimedes.

2. Planetary Speeds

Galileo stated the correct law of fall, as every high school physics student knows. However, he made numerous fundamental mistakes when trying to apply this law in a range of situations. One such error is what has been called Galileo's "Pisan Drop" theory of planetary speeds (Heilbron 2010, 116). The planets orbit the sun at different speeds. Mercury has a small orbit and zips around it quickly. Saturn goes the long way around in a big orbit and it is also moving very slowly. Galileo imagines that these speeds were obtained by the planets falling from some faraway point toward the sun, and then being somehow deflected into their circular orbits at some stage during this fall (Figure 1). That supposedly explains why the planets have the speeds they do.

Galileo expounds on this hypothesis in the *Dialogue,* and claims to have checked it mathematically and found that empirical orbital data "agree so closely with those given by the computations that the matter is truly wonderful" (Galileo 1953, 29). Galileo was so proud of this erroneous argument that he repeated it in his second major work, the *Discourse,* as well (Galileo 1974, 233, OGG.VIII.284). In both places he omits the details, however. Galileo has one of the characters in his dialogue say that "making these calculations [. . .] would be a long and painful task, and perhaps one too difficult for me to understand," whereupon Galileo's mouthpiece in the dialogue confirms that "the procedure is indeed long and difficult" (Galileo 1953, 30).

Mathematically competent contemporaries did not find it too "difficult" to check Galileo's theory, however. Mersenne immediately ran the calculations and found that Galileo must have messed his up, because his scheme doesn't work. (Marin Mersenne, *Harmonie Universelle,* II.103–7, Galileo 1974, 233, note 22. Later Newton made the same observation; Newton 1999, 144.) There is no such point from which the planets can fall and obtain their respective speeds. Galileo's precious idea is so much nonsense, which evidently must have been based on an elementary mathematical error in calculation.

3. Centrifugal Force

Galileo wished to refute the following ancient argument: "The earth does not move, because beasts and men and buildings" would be thrown off (Galileo 1989, 220). Picture an object placed at the equator of the earth, such as a rock lying on the African savanna. Imagine this little rock being "thrown off" by the earth's rotation. In other words, the rock takes the speed it has due to the rotation of the earth, and shoots off with this speed in the direction tangential to its motion.

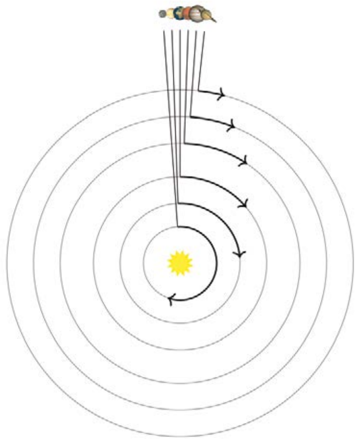

Figure 1 – Galileo's erroneous theory that the orbital speeds of the planets are equal to the speeds they would have acquired through free fall if dropped from a common height.

This is not what happens to an actual rock, because gravity is pulling it back down again. The rock stays on the ground since gravity pulls it down faster than it rises due to the tangential motion. How can we compare these two forces quantitatively? Since we know the size and rotational speed of the earth, it is a simple task (suitable for a high school physics test) to calculate how much the rock has risen after, say, one second. This comes out as about 1.7 centimeters. We need to compare this with how far the rock would fall in one second due to gravity. Again, this is a standard high school exercise (equivalent to knowing the constant of gravitational acceleration *g*). The answer is about 4.9 meters. This is why the rock never actually begins to levitate due to being "thrown off:" gravity easily overpowers this slow ascent many times over.

But this conclusion depended on the particular size and speed and mass of the earth. We could make the rock fly by spinning the earth fast enough. For example, if we run the above calculations again assuming that the earth rotates 100 times faster, we find that, instead of rising a measly 1.7 centimeters above the ground in one second, the rock now soars to 170 meters in the same time. The fall of 4.9 meters due to gravity doesn't put much of dent in this, so indeed the rock flies away.

These things were calculated correctly in Galileo's time (by Mersenne; Bertoloni Meli 2006, 113). But Galileo, alas, gets all of this horribly wrong. Even though we are supposed to celebrate Galileo as the discoverer of the law of fall, it is apparently too much to ask that he work out this basic application of it.

In fact, Galileo claims to "prove" that the rock will never be thrown off regardless of the rotational velocity. "There is no danger," Galileo assures us, "however fast the whirling and however slow the downward motion, that the feather (or even something lighter) will begin to rise up. For the tendency downward always exceeds the speed of projection." Thus Galileo proudly offers "a geometrical demonstration to prove the impossibility of extrusion by terrestrial whirling." (Galileo 1953, 197–8.)

Galileo's so-called "demonstration" is shown in Figure 2. (Galileo 2001, 231–4. The errors in Galileo's argument have been analysed by Chalmers and Nicholas 1983, Hill 1984.) It is indeed a qualitative argument that ostensibly rules out all possible cases of centrifugal projection, regardless of the rotational speed of the earth V, the radius of the earth R, or the magnitude of gravitational acceleration g. It is true, as Galileo says, that the ratio

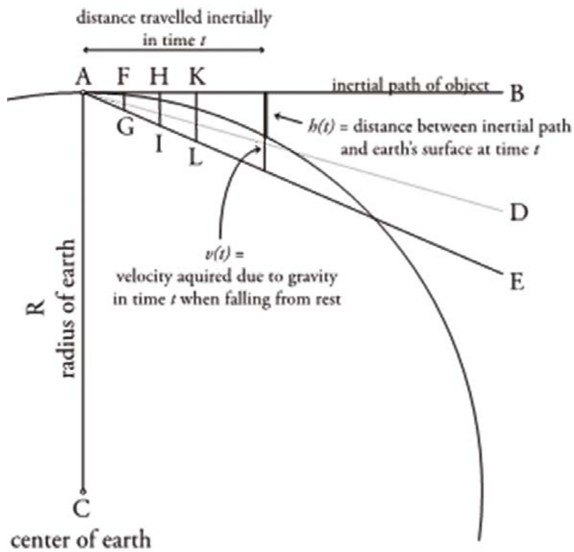

Figure 2 – Galileo's "proof" that centrifugal projection can never hurl objects off the earth. If gravity stops acting on an object at A, it would move inertially in the tangential direction AB. Since inertial motion has uniform speed, it would reach the equally spaced points $AFHK$ in equal time intervals. If the object had instead been dropped from rest, it would have acquired a certain downward speed in those same time intervals. These speeds are represented in the diagram by FG, HI, KL. Since the velocity acquired in free fall is proportional to time, $AGILE$ is a straight line. The slope of the line depends on the magnitude of gravitational acceleration, but for the purposes of this argument this value does not matter; in other words, we could just as well consider the speeds to be determined by some other line AD. The impossibility of centrifugal projection follows, according to Galileo, from the fact that as we consider smaller and smaller time intervals (that is to say, as we zoom it at A), the distance $h(t)$ required to catch up with the earth shrinks very rapidly to zero, while the speed $v(t)$ acquired from fall shrinks only linearly to zero. Therefore, says Galileo, the speed of fall $v(t)$ will, for some small enough t, be more than enough to cover the distance $h(t)$ and then some. In other words, the object will never get off the ground.

$v(t)/h(t)$ goes to infinity as t goes to zero. But this is obviously comparing apples to oranges, namely a velocity with a distance. The relevant comparison is between $h(t)$ and the distance $d(t)$ covered by free fall in this time. Galileo evidently felt that since in small time intervals $v(t)$ is overwhelmingly larger than

$h(t)$, then $d(t)$ must surely be larger than $h(t)$ as well. But this is false. Instead, the limit of $d(t)/h(t)$ as t goes to 0 is gR/V^2. In other words, $d(t)$ does not always overpower $h(t)$, as Galileo mistakenly believes. Rather, whether $d(t)$ is greater or smaller than $h(t)$ for small t depends on the specific parameters of the situation in question. A strong gravitational acceleration g, or a big radius of the rotational path R, makes it easier for the object to "catch up" with the surface of the earth, while a big rotational speed V makes it harder. Whether the object catches up with the surface or flies away depends on the relation between these parameters.

4. Circular Path of Fall

A rock dropped from the top of a tower falls in a straight line to the foot of the tower. But its path of fall is not actually straight if we take into account the earth's rotation. Seen from this point of view—that is to say, from a vantage point that doesn't move with the rotation of the earth—what kind of path does the rock trace? Galileo answers, erroneously,

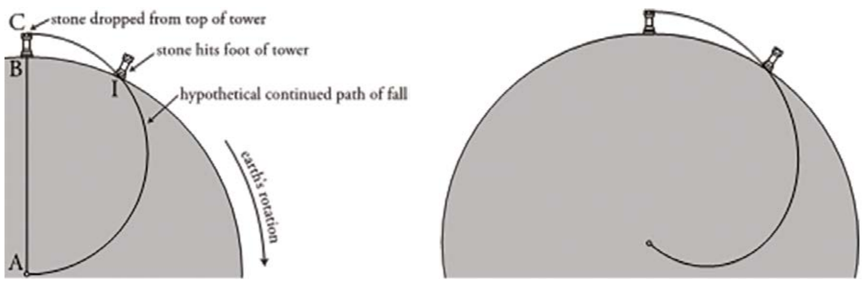

Figure 3 – Left: Galileo's erroneous conception of the path of fall of a rock dropped from a tower. "*AB* [is the radius of] the terrestrial globe. Next, prolonging *AB* to *C*, the height of the tower *BC* is drawn. The semicircle *CIA* [...], along which I think it very probable that a stone dropped from the top of the tower *C* will move, with a motion composed of the general circular one [due to the rotation of the earth] and its own straight one [due to gravity]." Galileo 2001, 192, OGG.VII.191. Right: From Galileo's assumptions it follows that the path should be a spiral rather than a semicircle.

that it will be a semicircle going from the top of the tower to the center of the earth (Figure 3):

If we consider the matter carefully, the body really moves in nothing other than a simple circular motion, just as when it rested on the tower it moved with a simple circular motion. [...] I understand the whole thing perfectly, and I cannot think that [...] the falling body follows any other line but one such as this [...]. I do not believe that there is any other way in which these things can happen. I sincerely wish that all proofs by philosophers had half the probability of this one (Galileo 2001, 192–3, OGG.VII.191).

This is "so obviously false (and besides incompatible with his own theory of uniformly accelerated motion of falling bodies) that one may wonder that Galileo did not see it himself" (Koyré 1955, 335). Once again Galileo doesn't understand basic implications of his own law. Mersenne readily spotted Galileo's error, whereupon Fermat observed that the path should be a spiral, not a semicircle (Koyré 1955, 336, 342, Engelberg and Gertner 1981, Galileo 2001, 556). This would be the right answer given Galileo's assumptions, namely that the path is generated by composing uniform angular motion with uniformly accelerated radial motion toward the center of the earth. (As stated in Galileo 2001, 192, and again later when he admitted Fermat's correction (Koyré 1955, 343).) This implies that the path of fall is $r = r_0 - a\theta^2$ in polar coordinates, which is indeed a spiral. This is still not the true path of fall, since Galileo's assumption that his law of fall remains unchanged in the interior of the earth is itself false. But I am not concerned here with criticising Galileo on such anachronistic grounds. Much worse is the fact that he got the wrong answer even if we grant his own assumptions.

When his embarrassing error was pointed out to him, Galileo replied that "this was said as a jest, as is clearly manifest, since it is called a caprice and a curiosity." (Galileo to Pierre Carcavy, 5 June 1637, OGG.XVII.89, Shea 1972, 135.) But in reality "it is hard to believe that Galileo had really meant his solution of the trajectory of the falling body to be merely a joke" (Koyré 1955, 343). If Galileo truly meant his argument to be taken merely in jest, then why did he say that he "considered the matter carefully" and "sincerely wished that all proofs by philosophers had half the probability of this one" and so on? Many of Galileo's errors come with these kinds of bombastic claims where Galileo is editorialising about how remarkably convincing his own arguments are. It is advisable and sobering for any reader of Galileo to always keep this in mind.

5. Projectile Motion

Pick up a rock and throw it in front of you. The path of its motion makes a parabola. Galileo is famous for this result but in fact he only asserts it—he does not offer a proof. Even Galileo's own follower Torricelli acknowledged this: the

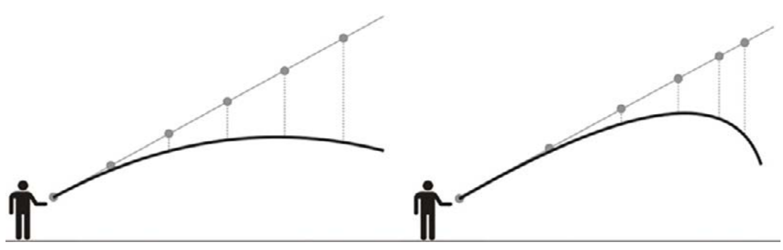

Figure 4 – Left: Correct conception of projectile motion. The dots indicate uniform inertial motion in the firing direction. Right: Erroneous conception of projectile motion, as drawn by Galileo in unpublished manuscripts. The dots indicate a decelerating motion in the firing direction, as if the projectile was struggling to ascend the incline. In both cases the rectilinear motion is composed with an independent vertical motion according to the law of fall. Based on Schemmel 2012, 94, 96.

result is "more desired than proven," as he says, very diplomatically (Torricelli, 1644, Damerow et al. 2004, 275). And the reason why Galileo doesn't prove this result is a revealing one. It is due to a basic misunderstanding.

The right way to understand the parabolic motion of projectiles like this is to analyse it in terms of two independent components: the inertial motion and the gravitational motion. If we disregard gravity, the rock would keep going along a straight line forever at exactly the same speed. That's the law of inertia. But gravity pulls it down in accordance with the law of fall. The rock therefore drops below the inertial line by the same distance it would have fallen below its starting point in that amount of time if you had simply let it fall straight down instead of throwing it. A staple fact of elementary physics is that the resulting path composed of these two motions has the shape of a parabola.

Galileo does not understand the law of inertia, and that is why he fails on this point. If the projectile is fired horizontally, such as for instance a ball rolling off a table, then Galileo does prove that it makes a parabola. He proves it the right way, the way just outlined, by composition of inertial and gravitational motion (Galileo 1989, 217, 221–2, OGG.VIII.269, 272–3).

But if you throw the rock at some other angle, not horizontally, then Galileo doesn't dare to give such an analysis. "Although [Galileo's] *Discorsi* takes it for granted that the trajectory for oblique projection is a parabola, no derivation of this proposition is presented." "At the point in the systematic treatment of projectile motion in the *Discorsi* where oblique projection is actually dealt with and correctly stated to yield a parabolic trajectory, there is simply a gap in the argumentation, and no derivation is offered for this claim." (Damerow et al. 2004, 237.)

Galileo's failure is quite clearly due to his not daring to believe in uniform inertial motion in any other direction than along the horizontal. He seems to fear that the law of inertia is perhaps not true for such motions. He is worried that the rectilinear component of the projectile's motion should be seen not as uniform but rather as gradually slowing down, like a ball struggling to roll up a hill or an inclined plane. In the latter case the trajectory is still a parabola, though not an "upright" one. See Figure 4. Indeed, more generally, "neither in the *Discourses* nor in the *Dialogue* does Galileo anywhere assert the eternal conservation of rectilinear motion" (Koyré 1978, 175). On the contrary, he explicitly rejects it: "Straight motion cannot be naturally perpetual." (Galileo 1953, 32.) "It is impossible that anything should have by nature the principle of moving in a straight line." (Galileo 1953, 19.)

In his final account, Galileo correctly "postulated upright parabolas for all angles of projection. Galileo's reasoning for this shape is, however, untenable in classical mechanics. What is more, Galileo was unable to derive it from the consideration of two component motions." "Galileo was [...] confronted with a contradiction between the inclined-plane conception of projectile motion and his claim that the trajectory is an upright parabola for all angles of projection, a contradiction he was never able to resolve." (Schemmel 2008, 234.) Since he only trusted the horizontal case, Galileo tried to analyse other trajectories in terms of this case. To this end he assumed, without justification, that a parabola traced by an object rolling off a table would also be the parabola of an object fired back up again in the same direction (Galileo 1989, 245, OGG.VIII.296. Schemmel 2008, 234, Damerow et al. 2004, 227, 236). In other words, "he takes the converse of his proposition without proving or explaining it," as Descartes—a mathematically competent reader—immediately pointed out (Descartes to Mersenne, 11 October 1638, AT.II.387. Drake 1978, 391.)

Figure 5 – Left: The catenary, or shape of a hanging chain, which Galileo erroneously believed to be a parabola. Right: The catenary (dotted) compared to a parabola (solid) of equal arc length between the same endpoints.

Instead, "it was Galileo's disciples who first derived the parabolic trajectory for oblique projection, although they present it merely as an explication of Galileo's *Discorsi*," which it is not (Damerow et al. 2004, 7). Indeed, "even before Galileo's *Discorsi* appeared in print, Bonaventura Cavalieri published a derivation of the parabolic trajectory that is consistent with classical mechanics and is not restricted to horizontal projection." (Damerow et al. 2004, 284). Cavalieri was Galileo's countryman and in some sense disciple, and was very generous in deferring credit to Galileo.

The failures of Galileo's treatment of projectile motion confirms his misconception that inertia is limited to horizontal motion, which, as we have seen, was already independently suggested by other passages. Some have tried to argue that "if Galileo never stated the law [of inertia] in its general form, it was implicit in his derivation of the parabolic trajectory of a projectile" (Drake 1964, 602). This would have been a good argument if Galileo had treated parabolic trajectories correctly. But he didn't, so the evidence goes the other way: Galileo's bungled treatment of parabolic motion is yet more proof that he did not understand inertia.

Even apart from the above errors and omissions, the mathematical details of Galileo's presentation of projectile motion are very clumsy. Galileo's "calculations are unnecessarily complicated, and were greatly simplified by Torricelli in [...] 1644, a complete revision and enlargement [...] which [...] makes Galileo's demonstrations and procedures obsolete" (Buchwald and Fox 2013, 53). Once again Galileo's text bears the marks of an amateur mathematician, in other words. And once again his followers almost immediately cleaned up his mess in more mathematically able works that were full of deference to Galileo. "While [...] inspired by veneration of Galileo, Torricelli is more logical in his treatise." (Hall 1952, 91.) Hence later mathematicians who used Torricelli's better but reverential account rather than Galileo's original for the mathematical details could easily be left with a much more flattering impression of the mathematical quality of "Galileo's" theory than if they had studied Galileo's own treatise in detail. Perhaps it is not so strange, then, that posterity got a bit confused and attributed much more to Galileo than he actually earned.

6. Catenary

The shape of a hanging chain (Figure 5) looks deceptively like a parabola. It is not, but Galileo fell for the ruse: "Fix two nails in a wall in a horizontal line [...] From these two nails hang a fine chain [...] This chain curves in a parabolic shape." (Galileo 1974, 143, OGG.VIII.186). More competent mathematicians proved him wrong: Huygens demonstrated that the shape was not in fact parabolic (Bukowski 2008; Truesdell 1960, 45). Admittedly, Huygens's proof is from 1646, four years after Galileo's death. So one may consider Galileo saved by the bell on this occasion, since he was proved wrong not by his contemporaries but only by posterity. It is not fair to judge scientists by anachronistic standards. On the other hand, Huygens was only seventeen years old when he proved Galileo wrong. So another way of looking at it is that a prominent claim in Galileo's supposed masterpiece of physics was debunked by a mere boy less than a decade after its publication.

In any case, Galileo thus ascribed to the catenary the same kind of shape as the trajectory of a projectile. He considered this to be no coincidence but rather due to a physical equivalence of the forces involved in either case (Galileo 1989, 256, OGG.VIII.309). Indeed, Galileo made much of this supposed equivalence and "intended to introduce the chain as an instrument by which gunners could determine how to shoot in order to hit a given target" (Renn et al. 2001, 118).

Galileo also tried to test experimentally whether the catenary is indeed parabolic. To this end he drew a parabola on a sheet of paper and tried to fit a hanging chain to it. His note sheets are preserved and still show the holes where he nailed the endpoints of his chain (Renn et al. 2001, 39). The fit was not perfect, but Galileo did not reject his cherished hypothesis. Instead of questioning his theory, he evidently reasoned that the error was due merely to a secondary practical aspect, namely the links of the chain being too large in relation to the measurements. Therefore he tried it with a longer chain, and found the fit to be better. In this way he evidently convinced himself that he was right after all (Renn et al. 2001, 92–104).

The catenary case thus undermines two of Galileo's main claims to fame. First it brings his work on projectile motion into disrepute. The composition of vertical and horizontal motions that we are supposed to admire in that case looks less penetrating and perceptive when we consider that Galileo erroneously believed it to be equivalent to the vertical and horizontal force components acting on a catenary. Secondly, Galileo's reputation as an experimental scientists par excellence is not helped by the fact that his experiments in this case led him to the wrong conclusion, apparently because his pet hypothesis led him to a biased interpretation of the data and a sweeping under the rug of an experimental falsification.

7. Moons of Jupiter

The moons of Jupiter were probably the most surprising new discovery made when telescopes were first pointed at the sky. An anecdote related by Kepler conveys some of the excitement: "My friend the Baron Wakher von Wachenfels drove up to my door and started shouting excitedly from his carriage: 'Is it true? Is it really true that he [i.e., Galileo] has found stars moving around stars?' I told him that it was indeed so, and only then did he enter the house." (Kepler to Galileo, 1610, Santillana 1955, 10.) It seems Galileo was indeed the first to observe the moons of Jupiter, but only by the smallest possible margin: Simon Marius independently observed them the very next day (Gaab and Leich 2018, Chapter 5, Pasachoff (2015)).

Galileo's mathematical ineptitude is on display in this case as well. "Galileo's first calculations [of the orbital periods of Jupiter's moons] were geocentric, not heliocentric. Galileo was treating Jupiter as if it revolved around the Earth, not the Sun. How he ever came to make such an error is an interesting question." (Drake 1999, 421. See also Shea 2009, 35. Galileo eventually realised his error when his calculations didn't match observations.)

Kepler and Marius, meanwhile, understood the matter perfectly and realised at once that this was another good argument against the Ptolemaic system (Drake 1999, 422). One Galileo supporter offers a very charitable interpretation: "this throws in doubt the view that by 1611 Galileo was already a Copernican zealot anxious to find every possible argument for the Earth's motion" (Drake 1999, 429). A more plausible explanation, in my opinion, is that Galileo was simply not very competent as a mathematical astronomer. It was not lack of desire to prove the earth's motion that made Galileo miss the point, it was lack of ability.

8. Comets

"Have you seen the fleeting comet with its terrifying tail?" (Drake and O'Malley 1960, 4.) This was the question on everyone's lips in 1618, following the appearance of a comet "of such brightness that all eyes and minds were immediately turned toward it." "Suddenly, men had no greater concern than that of observing the sky [...]. Great throngs gathered on mountains and other very high places, with no thought for sleep and no fear of the cold." (Drake and O'Malley 1960, 6.) "That stellar body with its menacing rays was considered as a monstrous thing" (Drake and O'Malley 1960, 4, 6), and, according to many, surely a cosmic omen foretelling imminent disasters.

Some urged a more dispassionate approach, arguing that "the single role of the mathematician" is merely to "explain the position, motion, and magnitude of those fires." (Drake and O'Malley 1960, 6–7.) Indeed, "the mathematician" had been so engaged for generations. Tycho Brahe, for instance, had worked extensively on comets, and in Galileo's time the task was taken up in depth by Kepler and others.

But entering this game would have required more mathematical skill and diligence than Galileo was used to displaying. Not coincidentally, Galileo offered an argument for why one should ignore the serious mathematical astronomy of comets, namely that such accounts are hopelessly inconsistent:

Observations made by Tycho and many other reputable astronomers upon the comet's parallax [...] vary among themselves [...]. If [...] complete faith [...] be placed in them, one must conclude either that the comet was simultaneously below the sun and above it, [...] or else that, because it was not a fixed and real object but a vague and empty one, it was not subject to the laws of fixed and real things (Galileo, *Assayer* 1623, Drake and O'Malley 1960, 257–8).

Kepler was flabbergasted that someone calling himself a geometer could be so dismissive of the excellent work of mathematically able astronomers such as Tycho:

> Galileo [...], if anyone, is a skilled contributor of geometrical demonstrations and he knows [...] what a difference there is between the incredible observational diligence of Tycho and the indolence common to many others in this most difficult of all activities. Therefore, it is incredible that he would criticize as false the observations of all mathematicians in such a way that even those of Tycho would be included (Kepler, appendix to *Hyperaspistes* 1625, Drake and O'Malley 1960, 351).

This paradox disappears if one recognises that Galileo is not a skilled geometer after all.

Unlike serious mathematical astronomers (and perhaps precisely in order to avoid having to engage with their mathematically advanced works), Galileo maintained that comets were not physical bodies travelling through space at all, but rather a chimerical atmospheric phenomenon. (It happens that Aristotle too had held that comets were sublunary, but tradition was clearly not the reason for Galileo to adopt his theory, as Galileo argues vehemently against the Aristotelian theory and the principles on which it is based (Galileo (1957), 263, 266, 270–3).)

According to Galileo's theory of comets, "their material is thinner and more tenuous than fog or smoke" (Galileo, *Assayer* 1623, Galileo 1957, 254). "In my opinion," says Galileo, comets have "no other origin than that a part of the vapour-laden air surrounding the earth is for some reason unusually rarefied, and [...] is struck by the sun, and made to reflect its splendour" (Shea 1972, 81, OGG.VI.94).

Galileo's vapour theory of comets is inconsistent with basic observations, as he himself admits. If comets are nothing but "rarefied vapour"—that is to say, some kind of pocket of thin gas—then you'd imagine that their natural motion would be straight up, like a helium balloon. Indeed Galileo does propose that comets have such paths. But then he at once admits that this doesn't fit the facts:

"I shall not pretend to ignore that if the material in which the comets takes form had only a straight motion perpendicular to the surface of the earth [...], the comet should have seemed to be directed precisely toward the zenith, whereas, in fact, it did not appear so. This compels us either to alter what was stated, [...] or else to retain what has been said, adding some other cause for this apparent deviation. I cannot do the one, nor should I like to do the other." (Shea 1972, 82–3, OGG.VI.98.) Bummer, it doesn't work. But Galileo sees no way out, so he just leaves it at that.

Galileo's contemporaries were not impressed. "[Grassi's] criticism of Galileo is on the whole penetrating and to the point. He was quick to spot Galileo's inconsistencies. Grassi produced an impressive array of arguments to show that vapours could not explain the appearance and the motion of the comets [as Galileo had claimed]." (Shea 1972, 84.) For instance, the speeds of comets do not fit Galileo's theory. According to Galileo's theory, the vapours causing the appearance of comets rise uniformly from the surface of the earth straight upwards. Therefore the comet should appear to be moving fast when it is close to the horizon, and then much slower when it is higher in the sky. Just imagine a red helium balloon released by a child at a carnival: it first it shoots off quickly, but soon you can barely tell if it's rising anymore, even though it keeps going up at more or less the same speed, because your distance and angle of sight is so different. But comets do not behave like that. Detailed observations of the comet of 1618 showed a much more constant speed than Galileo's hypothesis requires.

Galileo also offered another poorly considered argument against the correct view of comets as orbiting bodies, namely that their orbits would have to be unrealistically big: "How many times would the world have to be expanded to make enough room for an entire revolution [of a comet] when one four-hundredth part of its orbit takes up half of our universe?" (Galileo, Shea 1972, 77.) This is a poor argument, because the universe must indeed be very big and then some according to Copernican theory, in order to explain the absence of stellar parallax. Since the earth's motion is observationally undetectable, the orbit of the earth must be minuscule in relation to the distance to the stars. That means there is plenty of room for comets. But Galileo conveniently pretends otherwise in his argument against comets. Evidently, Galileo "was so intent on refusing Tycho that he failed to notice that he was pleading for a universe in which there would be no room for the heliocentric theory" either (Shea 1972, 88).

In sum, Galileo's completely erroneous theory of comets was roundly and rightly criticised by contemporaries. It is difficult to see why Galileo nevertheless found it so attractive, except perhaps for the fact that it conveniently alleviated him of having to do any actual mathematical astronomy of comets.

9. Conclusion

Galileo made numerous mistakes that were corrected by his mathematically superior contemporaries. It is time to abandon the persistent myth of "Galileo's mathematical genius" (Costabel and Lerner 1973, I.41). Historians will never see Galileo's true colours as long as they keep taking it for granted that Galileo was "the greatest mathematician in Italy, and perhaps the world" in his time (Heilbron 2010, 303). In reality, tell-tale signs of mathematical mediocrity permeate all of Galileo's works.

Galileo's mathematical shortcomings can be seen as a consistent theme intertwined with many aspects of his career. Galileo's celebrated adoption of empirical experiments and the telescope are grateful avenues of research for someone ill equipped to make a contribution on mathematical grounds. Likewise, it is easier to rhapsodise about the mathematical design of the universe and expound the basic principles of scientific method than to engage with advanced mathematical science ("those who can't do, teach"). In physics, as Descartes put it, Galileo "did not need to be a great geometer" for the purposes that he set himself: "he is eloquent to refute Aristotle, but that is not hard" (Drake 1978, 390). In astronomy, the very title of Galileo's *Dialogue Concerning the Two Chief World Systems: Ptolemaic and Copernican* reveals how antiquated and irrelevant to mathematical astronomers his framing of the issue of heliocentrism was, since "the Ptolemaic system already had been set aside, at least among mathematical astronomers" (Magruder 2009, 208), because, as Kepler said, there was "practically no one who would doubt what is common to the Copernican and Tychonic hypotheses" (Jardine 1984, 147) already well before Galileo had entered the picture. Regarding his conflict with the church, "if Galileo spoke only as a mathematician he would have nothing to worry about" (Drake 1978, 249), he was told by church authorities in 1615. Perhaps things would have turned out differently if Galileo's ability to advance science "as a mathematician" had not been so limited.

Galileo's errors also call for reassessing his good points. Apollo 15 astronauts performed an experiment on the moon. They dropped a hammer and a feather and found that they fell with the same speed. "Galileo was correct," they concluded in a famous video recording still often shown in science classrooms today. Lucretius was correct, they could have said instead, since he predicted that this would happen in the absence of air well over a millennium before Galileo (*De rerum natura*, II: 225–39). Meanwhile, Galileo was wrong, because he considered it "obvious" that the moon had an atmosphere (Shea 2009, 93). If the astronauts wanted to test Galileo's theory they should not have dropped a hammer and a feather. They should have taken off their helmets and suits and tried to breathe. That would have showed you how "right" Galileo really was. It is easy to be a hero of science if you are allowed a hundred guesses and people only remember the few that worked. If there had been air on the moon, the astronauts would have hailed Galileo for this "discovery" instead.

Posterity has chosen to remember only Galileo's successes while forgetting his numerous errors. Galileo made many erroneous claims that would have earned him not a little credit if they had been correct. It is dangerous to start with what we know and ask of history only who was the first to say it. Such selective retrospection is bound to reward careless scientists who made a hundred wild guesses instead of those who weigh evidence carefully before making any rash judgements. Galileo is indeed excessively and erroneously assertive where he should have been much more cautious and aware of the limitations of his evidence in many cases. In this way Galileo is undermining his right to claim credit for the things he did get right: his accounts of his correct discoveries may sound very convincing and emphatic, but knowing that he was equally sure of a long list of errors gives us reason to suspect that some of the things he got right are to some extent guesswork propped up with overconfident rhetoric in the hope that readers will mistakenly think his case is stronger than it is. Only by paying attention to Galileo's errors can we gain a sound perspective on his truths.

References

OGG: *Le Opere di Galileo Galilei*, Antonio Favaro (ed.), 20 volumes, Florence, 1890-1909, later reprinted with additions.

Bertoloni Meli, Domenico. 2006. *Thinking with Objects: The Transformation of Mechanics in the Seventeenth Century*. Baltimore, MA: Johns Hopkins University Press.

Buchwald, Jed Z. and Robert Fox (eds.). 2013. *The Oxford Handbook of the History of Physics*. Oxford: Oxford University Press.

Bukowski, John. 2008. "Christiaan Huygens and the Problem of the Hanging Chain." *College Mathematics Journal* 39, 1: 2–11.

Chalmers, Alan, and Richard Nicholas. 1983. "Galileo on the Dissipative effect of a Rotating Earth." *Studies in History and Philosophy of Science* 14, 4: 315–40.

Costabel, Pierre, and Michel Pierre Lerner (eds.). 1973. "Introduction." In *Les nouvelles pensées de Galilée*, 2 vols., Paris : J. Vrin.

Damerow, Peter, Gideon Freudenthal, Peter McLaughlin, and Jürgen Renn. 2004. *Exploring the Limits of Preclassical Mechanics: A Study of Conceptual Development in Early Modern Science: Free Fall and Compounded Motion in the Work of Descartes, Galileo, and Beeckman*. Second edition. Berlin: Springer.

Drake, Stillman, and C. D. O'Malley. 1960. *The Controversy on the Comets of 1618*. Philadelphia: University of Pennsylvania Press.

Drake, Stillman. 1964. "Galileo and the Law of Inertia." *American Journal of Physics* 32, 8: 601–8.

Drake, Stillman. 1978. *Galileo at Work*. Chicago: University of Chicago Press.

Drake, Stillman. 1999. *Essays on Galileo and the History and Philosophy of Science*. Volume 1. Selected and introduced by N. M. Swerdlow and T. H. Levere. Toronto: University of Toronto Press.

Engelberg, Don, and Michael Gertner. 1981. "A Marginal Note of Mersenne Concerning the Galileian Spiral." *Historia Mathematica* 8, 1: 1–14.

Freguglia, Paolo, and Mariano Giaquinta. 2016. *The Early Period of the Calculus of Variations*. Birkhäuser: Springer.

Gaab, Hans, and Pierre Leich (eds.). 2018. *Simon Marius and His Research*. Berlin: Springer.

Galilei, Galileo. 1953. *Dialogue Concerning the Two Chief World Systems*, translated by Stillman Drake. Berkeley: University of California Press.

Galilei, Galileo. 1957. *Discoveries and Opinions of Galileo*, translated with an introduction and notes by Stillman Drake. New York: Anchor Books.

Galilei, Galileo. 1974. *Two New Sciences*, translated by Stillman Drake. Madison, WI: University of Wisconsin Press.

Galilei, Galileo. 1989. *Two New Sciences,* translated by Stillman Drake, second edition, Wall & Emerson. Toronto, University of Toronto Press.

Galilei, Galileo. 2001. *Dialogue Concerning the Two Chief World Systems,* translated by Stillman Drake. New York: Modern Library.

Hall, A. Rupert. 1952. *Ballistics in the Seventeenth Century*. Cambridge: Cambridge University Press.

Little Heath, Thomas (ed.). 2002. *The Works of Archimedes*. Mineola, NY: Dover Publications.

Heilbron, J. L. 2010. *Galileo.* Oxford: Oxford University Press.

Hill David K. 1984. "The Projection Argument in Galileo and Copernicus: Rhetorical Strategy in the Defence of the New System." *Annals of Science* 41, 2: 109–33.

Jardine, Nicholas. 1984. *The Birth of History and Philosophy of Science: Kepler's A Defence of Tycho against Ursus with Essays on its Provenance and Significance*. Cambridge: Cambridge University Press.

Jullien, Vincent (ed.). 2015. *Seventeenth-Century Indivisibles Revisited*. Birkhäuser.

Kline, Morris. 1972. *Mathematical Thought from Ancient to Modern Times*. Oxford: Oxford University Press.

Koyré, Alexandre. 1955. "A Documentary History of the Problem of Fall from Kepler to Newton." *Transactions of the American Philosophical Society* 45, 4: 329–95.

Koyré, Alexandre. 1978. *Galileo Studies*, translated by John Mepham. Hassocks: Harvester Press. [First published as *Etudes Galiléennes*, 1939].

Magruder, Kerry V. 2009. "Jesuit Science After Galileo: The Cosmology of Gabriele Beati." *Centaurus* 51, 3: 189–212.

Montesinos, José, and Carlos Solis (eds.). 2001. *Largo campo di filosofare: Eurosymposium Galileo 2001*. La Orotava: Fundación Canaria Orotava de Historia de la Ciencia.

Newton, Isaac. 1999. *The Principia: Mathematical Principles of Natural Philosophy,* translated by I. Bernard Cohen and Anne Whitman, preceded by "A guide to Newton's Principia" by I. Bernard Cohen. Berkeley: University of California Press.

Palmerino, Carla Rita, and J. M. M. H. Thijssen (eds.). 2004. *The Reception of the Galilean Science of Motion in Seventeenth-Century Europe*. Berlin: Springer.

Pasachoff, Jay M. 2015. "Simon Marius's *Mundus Iovialis*: 400th Anniversary in Galileo's Shadow." *Journal for the History of Astronomy* 46, 2: 218–34.

Poupard, Paul C. (ed.) 1987. *Galileo Galilei: Toward a Resolution of 350 Years of Debate*. Pittsburg, PA: Duquesne University Press.

Renn, Jürgen, Peter Damerow, and Simone Rieger. 2001. "Hunting the White Elephant: When and How did Galileo discover the law of fall?" In Jürgen Renn (ed.), *Galileo in Context,* 29-149. Cambridge: Cambridge University Press.

de Santillana, Giorgio. 1955. *The Crime of Galileo*. Chicago: University of Chicago Press.

Schemmel, Matthias. 2008. *The English Galileo: Thomas Harriot's Work on Motion as an Example of Preclassical Mechanics*, Volume 1: Interpretation. Berlin: Springer.

Schemmel, Matthias. 2012. "Thomas Harriot as an English Galileo: The Force of Shared Knowledge in Early Modern Mechanics." In Robert Fox (ed.), *Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy on Early Modern England*, 89-112. Farnham: Ashgate, Surrey.

Shea, William. R. 1972. *Galileo's Intellectual Revolution*. London: Macmillan.

Shea, William R. 2009. *Galileo's Sidereus Nuncius or A Sidereal Message*. Translated from the Latin by William R. Shea. Introduction and notes by William R. Shea and Tiziana Bascelli. Sagmore Beach MA: Science History Publications.

Struik, Dirk Jan. 1969. *A Source Book in Mathematics, 1200-1800*. Cambridge, MA: Harvard University Press.

Truesdell, Clifford. 1960. *The rational mechanics of flexible or elastic bodies 1638-1788*. In *Leonhardi Euleri Opera Omnia*, Series II, Volume XI, Part 2. Zürich: Füssli.

Whitman, E. A. 1943. "Some Historical Notes on the Cycloid." *American Mathematical Monthly* 50, 5: 309–15.

Wootton, David 2010. *Galileo: Watcher of the Skies*. New Haven: Yale University Press.