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GALILEO, VIVIANI AND THE TOWER OF PISA

> . . . when the larger [ball] has reached the ground, the other is short of it by two fingers-breadth. Galileo Galilei†

Introduction: The Problem

THE STORY of the leaning tower experiment — one of the most famous anecdotes in the history of science — relates that sometime around the year 1590 Galileo climbed to the top of the leaning tower of Pisa and from there dropped two objects of different weights, in order to disprove Aristotle's law of fall, which claimed that the speed of fall of bodies is proportional to their weight. By letting the two objects fall simultaneously — so the story tells — and showing that they reached the ground simultaneously, Galileo demonstrated to the professors and students gathered round the tower that Aristotle was wrong.

In theory this story should have very little importance either for science or for its history. The experiment certainly had no impact on Galileo's thought; if it occurred, it was only a public performance and Galileo would not have climbed to the top of the tower without knowing the result beforehand. It should be of little interest, particularly to those historians of science who hold that science is primarily an abstract intellectual enterprise, insulated from social and political circumstances. After all, it was a social event, not an organic part of his scientific work.

Yet the leaning tower demonstration has often been regarded as a turning-point in the history of science, and many authors who believe that Galileo's science was mainly empirical have produced it as a classical example of the superiority of empirical science over *a priori* science. This is so despite the fact that the leaning tower story, unlike the story of Newton's apple — which is reported to have been narrated by Newton himself — is never mentioned in

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† *OG* 8, 109. Quotation taken from G. Galilei, *Dialogues Concerning Two New Sciences*, H. Crew and A. De Salvio (trans.) (New York, 1914), p. 65.

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any of Galileo's writings, nor is there any evidence that he ever narrated it.[1] It was reported 12 years after his death by one of his closest pupils and collaborators, Vincenzio Viviani (1622–1703) as part of a biography of Galileo written in 1654 and published posthumously for the first time in 1717.[2] And from the end of the last century onwards, historians of science have become more and more aware that Viviani might have presented a distorted image of Galileo, and that his biography is unreliable for the purposes of the modern history of science. The story of the leaning tower experiment, in particular, became famous as a legend after 1935, when Lane Cooper (1875–1959), a professor of English at Cornell, wrote his famous book showing how Viviani's story was not supported by evidence, and ridiculing those who repeated it.[3] Yet whereas, thanks to Cooper, the arguments against the story are by now well known, the arguments in its favour, which are perhaps just as valid, are much less known, and there are historians — including today's leading Galilean scholar, Stillman Drake — who are inclined to believe that Viviani's leaning tower story is true.[4]

Is the story of the leaning tower experiment true or false? The purpose of the present article is to present the story, the arguments for and against it, and to propose a new way of looking at the whole problem by considering it not as a problem of *Galileo's science*, but rather as a problem of *Viviani's "history" of science*, relying on Viviani's, rather than on Galileo's, papers. Some of these papers — like most of Viviani's papers — are still unpublished. They do not offer any solution to the problem, but suggest that, perhaps, the question above is not appropriate to the context in which Viviani was writing. Let me therefore begin by presenting the story in Viviani's own words.

The Story

In his life of Galileo, Viviani relates that when Galileo was a professor at the University of Pisa, between 1589 and 1592 (in 1592 Galileo was appointed to the chair of Mathematics in Padua and left Pisa), he had controversies with Aristotelian philosophers:[5]

> And then, to the dismay of all the philosophers, very many conclusions of Aristotle were by him [Galileo] proved false through experiments and solid demonstrations and discourses, conclusions which up to then had been held for absolutely clear and indubitable; as, among others, that the velocity of moving bodies of the same

[1] On the sources of the story of Newton's apple see R. S. Westfall, *Never at Rest: A Biography of Isaac Newton* (Cambridge, 1980), pp. 154–155.

[2] V. Viviani, 'Racconto istorico della vita del Sig. Galileo Galilei . . .', in: *Fasti consolari dell'Accademia Fiorentina*, S. Salvini (ed.) (Florence, 1717), pp. 397–431; *OG*, 19, 597–632.

[3] L. Cooper, *Aristotle, Galileo, and the Tower of Pisa* (New York: Ithaca, 1935).

[4] S. Drake, *Galileo at Work: His Scientific Biography* (Chicago, 1978), p. 415.

[5] *OG* 19, 606. Translation from Cooper, 26.

> material, of unequal weight, moving through the same medium, did not mutually preserve the proportion of their weight as taught by Aristotle, but all moved at the same speed.

Then comes the description of the experiments:

> demonstrating this with repeated experiments from the height of the Campanile of Pisa in the presence of the other teachers and philosophers, and the whole assembly of students.

Not a word more.

Although this short description is not supported by any evidence, for nearly two centuries nobody ever thought to question it. Indeed, Viviani had all the credentials of a reliable biographer: few men of letters had had the opportunity to know Galileo better than he; he had been Galileo's assistant during the three last years of Galileo's life; and Galileo regarded him as an adopted son. Viviani later also became an important and influential person — he practically took Galileo's place as court scientist — and what he said was regarded as very authoritative. His life of Galileo was therefore republished in 1744, 1808, 1826, 1864, and again many times during the twentieth century.[6]

Not everybody, however, agreed with the story. The first to doubt it were two of the greatest early modern Galilean scholars: Raffaello Caverni (1837–1900), a Florentine priest, and Emil Wohlwill (1835–1912), a German historian of science.

Caverni's and Wohlwill's Doubts

Caverni's and Wohlwill's arguments against the leaning tower story relied on some of Galileo's writings which they believed to have been written in the period when he was professor in Pisa and which contradict Viviani's report.[7] Galileo, at that time, had not yet published anything, but among his papers is a collection of Latin notes dealing with motion in a medium, which were probably written in the same period. Some of these notes were published for the first time only in the middle of the nineteenth century (1854) by Eugenio Albèri (1817–1878), an Italian scholar who edited a collection of Galileo's work. The whole manuscript was published by Antonio Favaro (1847–1922), the outstanding editor of the most complete "National Edition" of Galileo's

[6] I took this information from A. Favaro, 'Sulla veridicità del "Racconto istorico della Vita di Galileo", dettato da Vincenzio Viviani', *Archivio Storico Italiano*, tomo 73, Vol. 1, disp. 2 (1915), (Florence, 1916), p. 4.

[7] R. Caverni, *Storia del metodo sperimentale in Italia*, 6 Vols (Florence, 1891–1900; reprinted, Bologna, 1970), Vol. 4, chap. 5, see, in particular, p. 276. E. Wohlwill, 'Über einen Grundfehler aller neueren Galileibiographien', *Münchener medizinische Wochenschrift* **50** (1903), 1850–1851; Die Pisaner Fallversuche', *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften* **4** (1905), 229–248.

writings, under the title *De motu* (1890).[8] In his *De motu* Galileo says repeatedly and explicitly that bodies of different weights fall with *different* speeds. On one occasion Galileo even says that he dropped two different bodies, one of lead and one of wood, from the top of a high tower: "the lead moves far out in front. *This is something I have often tested*" (*sic*). These statements certainly did not escape Caverni's and Wohlwill's critical eyes.[9]

Caverni's doubts were presented in his outstanding study of Galileo's work, written between 1891 and 1900 and having an "anti-Galilean" orientation, claiming among other things that Galileo had claimed priority for discoveries by others, including Viviani. In the specific case of the leaning tower story too, Caverni blamed Galileo — not Viviani — for having lied; Caverni may have assumed that Viviani reported what Galileo told him. Wohlwill, on the other hand, directly blamed Viviani, and pointed out that there is no evidence for many other details in Viviani's life of Galileo, and thus they may never have occurred.

Caverni's and Wohlwill's claims are perhaps strengthened by the fact that Galileo presented the earliest formulation of the law of free fall only about 15 years later, in 1604, in a famous letter to the Venetian philosopher Paolo Sarpi (1552–1623). And even on this occasion, Galileo seems not to have had a very clear notion of free fall, since he derived his law from the false premise that the speed of falling bodies is proportional to the distance covered. In reality, the speed of falling bodies is proportional to the *time* of fall, and Galileo obtained the correct result only because he made a mathematical mistake which cancelled out his false premise.[10]

Caverni and Wohlwill seemed to have produced a very strong point against the leaning tower story, whether it was invented by Galileo or by Viviani. Yet Favaro objected, and argued that the evidence produced by Caverni and Wohlwill was unreliable.

Favaro's Reply

Wohlwill's claims — and implicitly also Caverni's — were discussed by Favaro in two long essays published in 1916 and 1917. In discussing the

[8] *OG* 1, 251–419. Translated by I. E. Drabkin in *On Motion and on Mechanics* (Madison, 1960), pp. 3–131, and *Mechanics in Sixteenth-Century Italy: Selections from Tartaglia, Benedetti, Guido Ubaldo & Galileo* (Madison, 1969), pp. 329–387. This Ms was already mentioned by Viviani in his work *Quinto libro degli Elementi d'Euclide...* (Florence, 1674), 104–105. It was, however, published only by E. Albèri under the title 'Sermones de motu gravium', in: *Le Opere di Galileo Galilei*, Vol. 11 (Florence, 1854), pp. 1–80.

[9] Experiments from a tower are mentioned in Drabkin's first translation (*ibid.*) on pp. 27, 38, 87, 101, 107, 127 and in the second on p. 374. The quotation is from p. 107.

[10] *OG*, 10, 115–116. Galileo's error in deduction is discussed by A. Koyré in his *Études Galilé ennes* (Paris, 1939; reprinted, 1966); translated into English by J. Mepham under the title *Galileo Studies* (Hassocks, Sussex, U.K., 1978), pp. 67–78.

leaning tower story, Favaro argued that Wohlwill's objections relied on papers that Galileo had never published and hence were not reliable evidence.[11]

Favaro had a point. Firstly, the fact that Galileo did not publish his notes on motion could indicate that he was not entirely satisfied with them. Secondly, Caverni and Wohlwill assumed that Galileo's *De motu* was compiled between 1589 and 1592, a dating which is far from being entirely certain. In theory, the passages contradicting Viviani's story could have been written earlier, and Galileo might have had time to change his mind, reach the conclusion that different bodies fall with equal speed, and perform the leaning tower demonstration. More recent research (1960) indicates that at least some of Galileo's remarks may indeed have been written earlier than Caverni and Wohlwill thought, thus strengthening Favaro's argument.[12]

And indeed, in his *De motu*, Galileo also objects to, and even ridicules, Aristotle's law of fall:

> But how ridiculous this view is, is clearer than daylight. For who will ever believe that if, for example, two lead balls, one a hundred times as large as the other, are let fall from the sphere of the moon, and if the larger comes down to the earth in one hour, the smaller will require one hundred hours for its motion? Or that, if two stones, one twice the size of the other, are thrown from the top of a *high tower* at the same moment, the larger reaches the ground when the smaller is only halfway down from the top of the tower?[13]

Here — as well as on several other occasions in the treatise — Galileo also mentions the possibility of performing experiments from a high tower (without, however, presenting any precise experimental description), which also speaks in favour of a possible leaning tower experiment, and partly confirms Viviani's story.

In his *De motu*, Galileo also presents for the first time the famous thought experiment, repeated in the later *Two New Sciences*, in which he argues that Artistotle's law leads to a self-contradiction. Assuming that two bodies of the same material but of different volume fall, as Aristotle believed, at different speeds, i.e. that the larger moves faster, then, if the two bodies are united, the smaller should slow down the greater. But the two united bodies form a greater one which, according to Aristotle, should fall faster than the two single ones. Hence Aristotle's law is self-contradictory, and in Galileo's words: "bodies of the same material but unequal volume move [in natural motion] with the same

[11]A. Favaro, 'Sulla veridicità . . .', pp. 20–34 on the leaning tower, and 'Di alcune inesattezze nel "Racconto Istorico della Vita di Galileo" dettato da Vincenzio Viviani', *Archivio Storico Italiano*, tomo 74, Vol. 2, disp. 3 e 4 (1916) (Florence, 1917). Favaro argued that Wohlwill's claims were in general exaggerated, although he admitted that Viviani had on some occasions distorted facts.

[12]I. E. Drabkin, 'A Note on Galileo's *De Motu*', *Isis* **51** (1960), 271–277.

[13]*OG* 1, 263, Drabkin, 26–27. The italics are mine.

speed".[14] It is therefore not unlikely that Galileo had already reflected sufficiently in Pisa to carry out the leaning tower experiment.

Although Favaro may be right in arguing that *De motu* cannot be used as evidence against the leaning tower experiment, there remains, nevertheless, no direct evidence that the leaning tower demonstration did occur. At this point, however, Favaro becomes rather partisan and produces a rhetorical argument. He says: "nobody had ever doubted [Viviani], until, regrettably, in the past few years, one of the most authoriative and profound Galilean scholars [Wohlwill], argued against him."[15]

Why "regrettably"? Is it against scholarship to suggest that Viviani may have altered facts? The driving spirit behind Favaro's argument seems to be an (irrational) resentment, as if, by claiming that Viviani had distorted Galileo's image, Wohlwill had broken a taboo. Favaro's argument becomes even more surprising if one considers that he was the first to point out that Viviani's life of Galileo contained inexactitudes: in 1887, he had discovered that Viviani had varied the report of Galileo's date of birth by four days — from 15 February to 19 February 1564 — in order to make it coincide with Michelangelo's death — which occurred on 18 February 1564. This argument against Viviani was repeated by Wohlwill, and Favaro could not object to it.[16]

Favaro's remark may be partially understood if one looks at Viviani's papers in the Galilean Collection of manuscripts kept in the National Library in Florence — especially those collected in volume 11, containing the documents that Viviani used to compile his life of Galileo. In Favaro's day, many of these documents were still unpublished (some are still unpublished today). Favaro and Caverni knew these papers very well, whereas Wohlwill, in Germany, may never have seen them (the era of microfilm had not yet begun). Although they do not contain any indication of the leaning tower experiment, the way Viviani documented himself certainly shows that, in general, he was a conscientious historian and biographer, and would hardly have invented facts; his variation of the report of Galileo's date of birth appeared to have been an exception. Thus one can well understand why Favaro remarked: "It is not enough to say that Viviani's claims are not documented; one should prove that they contradict truth".[17]

Favaro concludes:

> The fact of the experiments on the fall of bodies, performed from the height of the tower of Pisa in order to demonstrate the new truths he had arrived at, is affirmed by

[14] *OG* 1, 265, Drabkin 29. The same argument is repeated in his *Discorsi e dimostrazioni matematiche, intorno a' due nuove scienze attenenti alla mecanica & i movimenti locali*, *OG* 8, 107–108. Crew and de Salvio, 63.

[15] Favaro, 'Sulla veridicità . . .', p. 6.

[16] A. Favaro, 'Sul giorno della nascita di Galileo', *Memorie del R. Istituto Veneto di scienze, lettere ed arti* **22** (1887), 701–711.

[17] Favaro, 'Sulla veridicità . . .', p. 23.

> Viviani, who must have had it from Galileo's own lips, [affirmed] in a manner so sure that it cannot be called into question, much less be flatly denied because no confirmation of it is found in contemporaneous documents.[18]

In other words, Viviani's reliability, according to Favaro, is beyond discussion.

Favaro's reply to Wohlwill silenced the issue for the next 20 years. But Caverni's and Wohlwill's doubts remained, and were raised again, dramatically, by Lane Cooper in 1935.

Cooper

In a small, by now very famous book, *Aristotle, Galileo and the Tower of Pisa*, Cooper repeated Wohlwill's doubts concerning the leaning tower experiment. Cooper's aim was merely to assemble the literature relevant to the leaning tower story for the use of English-speaking readers and show that it does not confirm Viviani's story. Cooper pointed out that the whole story had been presented too schematically, disregarding its historical context, which may have been much more complex. Cooper also said that what Aristotle had stated concerning fall is not entirely clear and we do not know how exactly Galileo understood it, and that there is also no evidence that Galileo was as anti-Aristotelian as Viviani had claimed. Furthermore, many philosophers before and after Galileo occupied themselves with the problem and even performed very similar experiments, and Galileo's alleged demonstration was by no means as exceptional as it is normally presented.

Cooper also produced additional evidence, strengthening Caverni's and Wohlwill's thesis. He pointed out that in the year 1641 Vincenzio Renieri (1606–1647), a follower of Galileo and the professor of mathematics at the University of Pisa, wrote two letters to Galileo which contradicted Viviani's description of the leaning tower experiment.[19]

Renieri's first letter, written on 13 March 1641, reports two experiments performed from the top of the leaning tower. In a first experiment, a lead ball and a wooden ball were dropped from the tower, the former reaching the ground first by at least three ells (about 1.75 metres). The second experiment involved two lead balls, one the size of a cannonball and the other of a musket bullet, the larger reaching the ground first by a palm (about 22 cm).

Galileo must have replied to Renieri, since on 20 March Renieri wrote a second letter to Galileo acknowledging its receipt. In Renieri's second letter one finds the following remark: "but that two heavy bodies, unequal in weight but of the same material, falling from the same height perpendicularly have to arrive with *different* velocity and in different time at the centre, this I think I

[18]The quotation is from Cooper, 53.
[19] *OG*, 305–306, 310.

have heard or read from you". Renieri's experimental results then contradict Viviani's description.[20]

So much for the evidence against Viviani produced by Cooper. Yet his book was not directed so much against Viviani as it was against the literature which presented the leaning tower demonstration as a dramatic turning point in the history of science, in other words against the literature that relied blindly on Viviani. Cooper showed that many authors not only disregarded Caverni's and Wohlwill's warning that the story may not have been true but, worse, amplified it with imaginary details that Viviani had never stated. What would have happened, asks Cooper, if Viviani's original report were indeed invented?

Cooper's book thus had the merit of presenting the leaning tower story as an important issue in the *historiography* of science, rather than in the *history* of science.

Cooper's humorous presentation, and his writing during the period in which the American public was becoming increasingly interested in the history of science, gave the whole issue a particular resonance. Still, this resonance hardly justifies the outrageous objections which came in its wake.

Shortly after its appearance, Cooper's book was reviewed by two leading scholars, Aldo Mieli (1879–1950) — the founder (in 1928) of the International Academy of the History of Sciences — and Harold Cherniss — one of the leading classicists of the century. Both reviewers, just like Favaro 20 years earlier, express criticism. Yet both argue mainly against a secondary claim by Cooper — his presentation (or misrepresentation) of Aristotle's law of fall — and practically disregard Cooper's main claim, namely that Viviani's story is not documented. They argue that Cooper had misunderstood Aristotle and the complex relation between Aristotle, Aristotle's commentators, and Galileo. This argument may be correct, but it weakens by very little — if at all — Cooper's claim concerning Viviani.[21]

Mieli was particularly scornful. What seemed to bother him most of all was the fact that Cooper was not a physicist (Sarton says in his obituary of Mieli that the latter was more of an administrator of culture than a historian of science).[22] In his four-page review he scornfully calls Cooper no less than six times "the professor of English" (with additional derisive comments), and says that had Cooper known more physics he might have understood better the context of the law of free fall. Mieli also blames Cooper for not being well read

[20]*OG* 18, 310. Translation from Cooper, 33. The italics are mine.

[21] A. Mieli, *Archeion, Archivio di Storia della Scienza* **17** (1935), 303–307. H. Cherniss, *Modern Language Notes* **51** (1936), 184–186. There were also more positive reviews of Cooper's book: R. T. Gunther, *Nature* **136** (1935), 6–7; A. S. Eve, 'Galileo and Scientific History: The Leaning Tower and Other Stories', *Nature* **137** (1936), 8–10. Cooper replied to some of his reviewers in 'Galileo and Scientific History', *The Scientific Monthly* **43** (1936) 163–167. This article was included in Cooper's collection of articles, *Aristotelian Papers* (New York: Ithaca, 1939), pp. 90–100.

[22]G. Sarton, *Archives Internationales d'Histoire des Sciences* **30** (1951), 340.

and invites him to read the works of Favaro and Duhem on the subject. Mieli, however, does not say how a better knowledge of physics — even of Aristotelian physics — could have affected Cooper's argument that there is no evidence that Galileo ever performed the leaning tower experiment; Favaro and Duhem certainly did not produce this evidence in any of the writings mentioned by Mieli.[23]

Mieli's review may have inspired Guiseppe Boffito (1869–1944), the bibliophile priest who in 1940 updated the very useful Galilean bibliography, to add the following remark after the entry on Cooper's book: "The author is a philologist, not a physicist: this deficiency is evident throughout the book."[24]

Is a degree in physics a necessary prerequisite to argue that an anecdote in history is not well documented (or perhaps even to write history of science)?

The disproportionate and irrational reaction to Cooper's book confirms that although the leaning tower story may not be important to the history of science, it is indeed an important case study in the historiography of science. If Cooper's reviewers did not have a logical argument against him, what was it, then, that triggered their reaction? Before considering the story from a more historiographical point of view, let us consider briefly the studies related to the leaning tower story that were carried out after Cooper.

Recent Research

Shortly after the publication of Cooper's book, Alexander Koyré (1892–1964), suggested in 1939 in his *Galilean Studies* that experiment did not play any essential role in Galileo's work and that Galileo may never have performed some of the experiments he describes in detail. Koyré's claims could implicitly strengthen Caverni's, Wohlwill's and Cooper's view that a leaning tower experiment may never have taken place. Yet Koyré's conclusions are not related to this specific experiment. Koyré argued in general that conceptual issues, as opposed to purely empirical ones, predominate in modern science, or at any rate, that this was how an historian would approach it. Koyré was writing about the role of experiment in modern science in general, and did not consider the historiographical question of whether or not Viviani had reported the truth.[25]

In the half century that has since elapsed, Galilean studies have made considerable progress. The study of the context of Galileo's work has permit-

[23]P. Duhem, *Le Système du monde: histoire des doctrines cosmologiques de Platon à Copernic*, 10 vols (Paris, 1913–1959).

[24]Galileo's bibliography was compiled by A. Carli and A. Favaro, *Bibliografia galileiana (1568–1896)* (Rome, 1986). G. Boffito updated it, *Bibliografia galileiana, 1896–1940, primo supplemento* (Rome, 1943), p. 198. Cooper's book, incidentally, was listed under the letter "L" for "Lane", rather that "C" for "Cooper".

[25]Koyré, *Galileo Studies* and, 'An Experiment in Measurement', *Proceedings of the American Philosophical Society* **97** (1953), 222–237. Reprinted in Alexandre Koyré, *Metaphysics and Measurement* (London, 1968), pp. 89–117.

ted a better understanding of his contribution to science. New Galilean manuscripts have been published and studied, and particular attention has been paid to Galileo's early works. But none of these studies, even the study of the context of the discovery of the law of free fall, produced any further evidence, either in favour of or against Viviani's leaning tower story.

One trend in Galilean studies, mainly in reaction to Koyré, has been the repetition of the few experiments Galileo describes in his writings with similar tools and procedure, to prove that these experiments could indeed have been performed. No attempt has been made, to the best of my knowledge, to repeat the leaning tower experiment. Indeed, the experiment is hardly repeatable, since Viviani did not report any experimental data. And, as we have already seen, when similar experiments were performed by Vincenzio Renieri, he obtained results that were different from those reported by Viviani. Later in this paper we shall have the opportunity to consider additional occasions on which the experiment was performed in Galileo's time and always gave negative results.[26]

Many of the results obtained in the past 50 years are summarized by Stillman Drake in his recent biography of Galileo. Despite the lack of evidence, Drake is convinced that the leaning tower demonstration did take place. Drake repeats Favaro's argument that Viviani had heard the leaning tower story "from Galileo's own lips", when Galileo received Renieri's letter. Viviani was then Galileo's amanuensis, and Drake conjectures that he had read Renieri's letters to Galileo and written his reply. It is on this occasion, according to Drake, that Galileo recalled for Viviani the incident which occurred some 50 years earlier, and had Galileo's letter to Renieri been preserved, it would have provided definite evidence that Galileo's leaning tower experiment did take place.[27]

But Galileo's letter to Renieri is lost, and Drake's argument, like Favaro's, relies on "Viviani's accuracy". The main argument in favour of or against the leaning tower story, then, remains Viviani's reliability. Whereas Favaro and Drake believe that Viviani was reliable, Wohlwill and Cooper think that he was not.

I therefore propose to replace the question — *Did Galileo perform the leaning tower experiment*? with the question — *Why did Viviani think it important to report such an experiment*? Answering the second question does not imply an immediate solution to the problem since, as Caverni implicitly suggested,

[26]See my survey 'The Role of Experiment in Galileo's Physics', *Archive for History of Exact Sciences* **23** (1980), 227–252. Although many works mentioned the leaning tower experiment, none attempted to solve the question of the truth of the story. Let me quote two such works; E. A. Moody, 'Galileo and Avempace: The Dynamics of the Leaning Tower Experiment', *Journal of the History of Ideas* **12** (1951), 163–193, 375–422; D. C. Lindberg, 'Galileo's Experiments on Falling Bodies', *Isis* **56** (1965), 352–354.

[27]Drake, *Galileo at Work*, 415.

Viviani may have been misled by Galileo, but it certainly might help to put the problem in its correct historiographical context.

Perhaps something more can be learnt concerning the context in which Viviani was writing from volume 11 of the Galilean collection of manuscripts, containing the documents used by Viviani to write his life of Galileo. Gal. MS 11 contains, *inter alia*, two drafts of Viviani's life of Galileo, which Favaro labelled *A* (folios 72–118) and *B* (folios 22–68) — *A* being an earlier copy, slightly different from *B* — and many other documents concerning Galileo's life.[28]

Many of these documents were published by Favaro in Vol. 19 of the National Edition, or elsewhere. A number of documents remained unpublished, evidently because Favaro thought that they did not add anything to a better understanding of Galileo. Yet, what was published, was published with Galileo in mind, and not Viviani. Seen from the point of view of Viviani, one may get a different picture.

Can we learn anything new from these documents?

Viviani's Literary Context

Viviani initially wrote his life of Galileo for the earliest collection of Galileo's works, published in Bologna in 1655/6. But finally it was not included in this collection and Viviani saved it and improved it in order to publish it as part of his own collection of Galileo's works, which he planned but never published.[29]

Viviani was writing during an age in which a biography had to follow certain standards, much as those adopted by Giorgio Vasari (1511–1574), the Mannerist painter and architect who wrote the *Vite*, the most famous collection of biographies in the history of art.[30]

[28] *OG* 19, 597–598. The detailed contents of Gal. Ms 11 are presented by A. Procissi in *La Collezione Galileiana della Biblioteca Nazionale di Firenze*, 2 Vols (Rome, 1959–1985), Vol. 1, pp. 13–16.

[29] *Opere di Galileo Galilei . . . In questa nuova edizione insieme raccolte, e di vari trattati dell'istesso autore non più stampati accresciute. . .* (Bologna, 1655–1656). The story of the edition is reported by Viviani in a long letter written in 1656 to Elia Diodati, a friend of Galileo in Paris, published by P. Galluzzi and M. Torrini in *Le Opere dei discepoli di Galileo Galilei: carteggio, 1642–1656*, 2 vols (Florence, 1975–1984), Vol. 2, pp. 301–308.

[30] P. L. Rose, in *The Italian Renaissance of Mathematics: Studies on Humanists and Mathematicians from Petrarch to Galileo* (Geneva, 1975), chap. 11, pp. 243–279, and N. Jardine, in *The Birth of History and Philosophy of Science. Kepler's* A Defence of Tycho against Ursus *with Essays on its Provenance and Significance* (Cambridge, 1984), chap. 8, pp. 258–286, have shown how important rhetoric was for early modern biographies of scientists. The first edition of Vasari's work was published in Florence in 1550 under the title *Le vite de più eccellenti architetti, pittori e scultori italiani, da Cimabue insino a' tempi nostri*. A second edition followed in 1568, under the title *Le vite de' più eccellenti pittori, scultori, et architettori. . .* . Many other editions, reprints and translations followed. I used the following works, *La vita di Michelangelo*, P. Barocchi (ed.) (Milan, 1976), and *Le Vite de' più eccellenti pittori, scultori e architettori nelle redazioni del 1550 e 1568*, R. Bettarini (ed.), annotated by P. Barocchi (Florence, 1966). *Lives of the Artists*, a selection translated by G. Bull, 2 vols (Penguin Books, 1965, 1971; reprinted, 1988). For a more detailed treatment of the relations between Viviani and Vasari, see my 'Viviani's Life of Galileo', *Isis* **80** (1989).

Vasari's *Vite*, published in 1550, and, in a second, enlarged edition in 1568, are a typical example of how biographies were written in those days. Viviani relied on Vasari: Gal. MS 11, 168, contains excerpts from Vasari.[31] And one does not need to be an expert in Italian literature to see that his biography is very similar in style and form to a Vasarian biography — as if it were written by Vasari himself. On one occasion, at least, when describing Galileo's youth, Viviani uses the exact expressions used by Vasari to describe Giotto's youth. Vasari, says of young Giotto: "... mostrando in tutti gli atti ancora *fanciulleschi* una *vivacità* e prontezza d'*ingegno*"; and Viviani says "... ne' prim'anni della sua *fanciullezza* a dar saggio della *vivacità* del suo *ingegno* ..."[32]

Vasari's *Vite*, like other biographies written in the same period, have been the subject of many studies and found to contain recurring elements. One of them is the embellishment of the artist's image by means of anecdotes—often invented. Vasari invented stories, such as the famous story of Giotto's perfect 'O'.[33]

Thus Viviani's story of the leaning tower experiment is to be understood as belonging to a literary style in which truth had less importance than it has in modern biographies. What was important then, was to embellish Galileo's image, even by means of invented stories. But even this embellishment was subject to certain rules, dictated by the tastes of Viviani's audience.

Literary Tastes of Viviani's Audience

Viviani's intended audience included not only philosophers but also the general educated public; Giordano Bruno and Galileo himself already wrote in Italian for this public, which in those days might have included many of the educated nobility, the learned clergy and of course, the universities.

This educated public, in Viviani's day, used to meet in the many literary academies. There exists an interesting document stating precisely the expectations of the general educated public and, in particular, of the members of a literary academy, as far as science was concerned. It is a letter written in 1642 by Bonaventura Cavalieri (1598–1647) to Evangelista Torricelli (1608–1647).

Both Cavalieri and Torricelli were followers of Galileo. In 1642, Torricelli was admitted to the *Accademia della Crusca*, and Cavalieri wrote him a letter

[31]In Viviani's handwriting, taken from p. 774 of the 1568 edition of Vasari's life of Michelangelo. Barocchi, Vol. 1, p. 116. This is Vasari's statement on Michelangelo's death: "*con conoscimento grandissimo ... a miglior vita*". Bull, Vol. 1, pp. 417–418. In folio 168v Vasari is mentioned again, but the handwriting is not that of Viviani.

[32]Vasari: p. 139 in the 1568 edition; Bettarini, Vol. 2, p. 96. Bull, Vol. 1, p. 57, translates as follows: "....Giotto showed in all his boyish ways such unusually quick intelligence and liveliness...", Viviani, *OG* 19, 601.

[33]The classical Renaissance notions of biography of artists are described by E. Kris and O. Kurz, *Legend, Myth, and Magic in the Image of the Artist: A Historical Experiment* (New Haven, 1979).

of congratulations. Cavalieri also gave Torricelli advice about how to speak to the members of this illustrious academy, and mentioned the expectations of the members:

> I hear that they expect physical rather than mathematical things, and perhaps they are right for the former resemble more the chaff [*crusca*], whereas the latter is the flour — the true food and nutriment of the intellect. It is advisable to meet their expectation, and more than that, the *universal* expectation that has little esteem for mathematics, unless it sees some applications, . . .[34]

The members of the *Accademia della Crusca*, as well as the "universal" public in general, apparently preferred to hear about practical, "tangible", physical things rather than abstract mathematical ones. Cavalieri regrets this, but advises Torricelli to please his public, by avoiding topics which are not popular.

Thus Torricelli and Viviani had to adapt themselves to these expectations and try to present an image of science which was as practical and "tangible" as possible. And a description such as the leaning tower experiment, whether true or not, was exactly what Viviani's audience would have liked to hear.

There is an interesting correction in Viviani's hand-written drafts related to the leaning tower experiment confirming all this. In draft *A* Viviani ends the story by saying that "all this is treated extensively by him in his *last Dialogues* concerning two new sciences." In *B*, one finds the same remark with a small variation "all this is treated extensively by him in the *said Dialogues* concerning two new sciences". Why?[35]

The *Dialogues Concerning Two New Sciences* are divided into four Days, the first two being devoted to the strength of materials, and the third and fourth to kinematics. By saying "last dialogue", Viviani probably meant this last part of the book, which also covers free fall. Initially Viviani may have thought that Aristotle's law of fall was refuted in this part of the book. But then he realized that, amazingly, this was done in the *First Day* (dealing with the resistance of bodies) and corrected "last" to "said". Yet in the *First Day*, Aristotle's law of fall is refuted by the famous thought experiment, already described in Galileo's *De motu*. This is not the kind of "physical", "tangible" argument that Viviani's audience would have wanted to hear. Viviani therefore cancelled in *B* the entire remark, leaving the more dramatic description of the leaning tower experiment, which may not have been entirely true and was perhaps less "scientific", but certainly more "convincing", considering Viviani's audience (Fig. 1).

[34]Galluzzi and Torrini, *Carteggio*, Vol. 1, p. 18 (the italics in the English translation are mine).
[35]*A*: Gal. Ms 11, 83: ". . . che tutto si vede poi diffusamente trattato da lui nelli ultimi Dialoghi delle due nuove Scienze". *B*: Gal. Ms 11, 34: ". . . che tutto si vede poi diffusamente trattato da lui trattato nelli sudetti Dialoghi delle nuove Scienze". Cf. *OG* 19, 606.

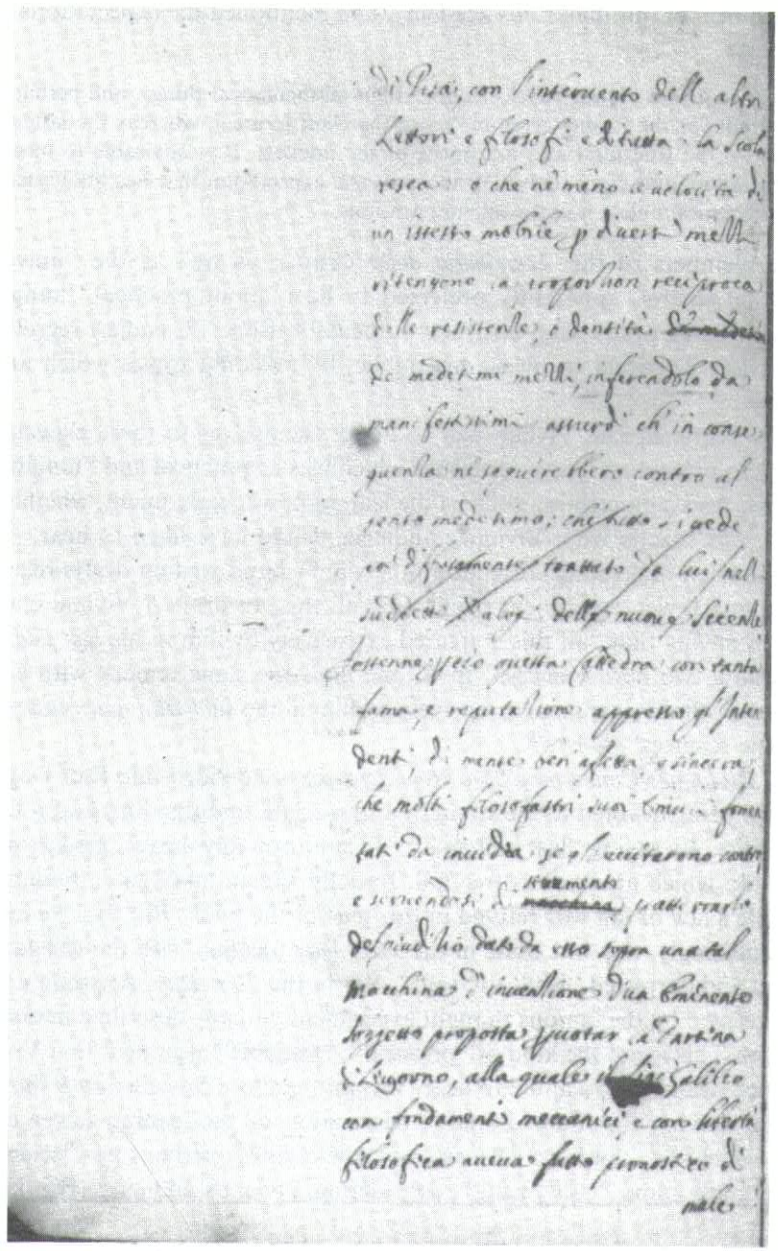

Fig. 1. *Gal. Ms* 11, 34. *In the middle, one can clearly see how Viviani deleted the sentence relating the leaning tower experiment to Galileo's* Two New Sciences (*courtesy of the Biblioteca Nazionale Centrale, Florence*).

One can now see the importance, to Viviani, of reporting the leaning tower experiment: it was part of the literary requirements of his day. There remains the question: is the story of the leaning tower experiment true?

The Truth of the Story

Viviani's anecdotes in his life of Galileo, as in any contemporary biography, contain an element of truth, and an element of embellishment. One instance is Viviani's variation of the report of the date of Galileo's birth. Another instance is Viviani's alteration of the reported age at which Galileo entered university; according to the documents in the archives of the University of Pisa, Galileo began his studies in 1581, when he was over seventeen and a half. And indeed, in draft *A* of his biography Viviani wrote eighteen, but in *B* he cancelled it and wrote seventeen, and finally cancelled seventeen also and wrote sixteen. There are many more such instances. In general, Viviani's variations are small, clearly intended to embellish Galileo's image, and he takes care not to get too far away from the truth. In other words, most of what Viviani says is to a large extent true, with a small degree of fiction — an embellishment imposed on him by the literary conventions of his day.[36]

The leaning tower description fits within this scheme. Let us see what is true and what is invented in this story. We have seen that in his *De motu*, written around 1590, Galileo repeatedly produced the example of a body falling from a tower. When Viviani wrote his biography of Galileo, *De motu* was still unpublished, but we know for certain that the manuscript was among Viviani's papers, and Viviani may have been inspired by it.

Furthermore, although Galileo may not have performed the leaning tower experiment, he most probably did perform experiments from *a* tower. This tower may even have been a leaning tower, or even *the* leaning tower (there are several leaning towers in Pisa), since a leaning tower is better suited to this type of experiments than a normal tower. Also, this is an experiment that requires at least two experimenters, one to drop the objects and the other to observe them reaching the ground (Viviani speaks of a crowd below the tower). If Galileo performed the experiment, he was certainly helped by some assistants or students. Galileo does not describe all this, but the very fact that he mentions experimenting from a tower makes this possibility very plausible.

There is also evidence, as Cooper points out, that Galileo did climb to the top of a famous tower to perform an important demonstration. It was not the leaning tower of Pisa but the Tower of San Marco in Venice. On 21 August 1609, Galileo made a demonstration of his telescope to the Venetian authorities from the top of this tower.[37]

[36] *A*: Gal. Ms 11, 77. *B*: Gal. Ms 11, 28. Cf. *OG* 19, 32 and 602.
[37] Cooper, 29.

Lastly, during the seventeenth century there was a vogue for stories about experiments performed by Pisan professors in Pisa from the top of the leaning tower. Renieri's is one instance, and in the writings of Galileo and his followers there is additional evidence.

In 1597, Jacopo Mazzoni (1548–1589), a professor of philosophy in Pisa and Rome and a friend of Galileo's, wrote a work in which he claimed, just like Galileo, that Aristotle's Law of Free Fall was experimentally proved to be wrong. Mazzoni did not produce any detailed experimental description but a few years later, in 1612, Giorgio Coresio, a professor of Greek in Pisa, wrote another book objecting to Mazzoni's claims. Coresio argued that Mazzoni had performed experiments from his window, that the window was not high enough and that he (Coresio) had repeated it from the top of the leaning tower. Coresio reported that his bodies behaved exactly according to Aristotle's law (*sic*), namely, the heavier a body was, the faster it fell.[38]

Additional evidence can be found among the writings of the *Accademia del Cimento* — the scientific academy active at the Tuscan court between 1657 and 1667. In 1667 this Academy published its official publication, the *Saggi di naturali esperienze*.[39] Before these *Saggi* were published, the president of the Academy, Prince Leopold de' Medici (1617–1675), sent drafts of the work to a number of referees. One of them was Carlo Rinaldini (1615–1698), an Aristotelian professor of philosophy at the University of Pisa. Commenting on an experiment related to Torricelli's tube, Rinaldini says that he tried to measure the differences in air pressure at the top and bottom of the tower.[40]

Viviani, then, needed but little imagination to present an anecdote which would be sufficiently realistic while conforming to the scheme of the biography he had to write. He had a manuscript by Galileo speaking of experiments from a tower; he might have heard reports of Galileo experimenting from a tower and, in any case he knew for sure that Galileo had performed a demonstration at the top of the well known tower of S. Marco in Venice; and it was fashionable in his days to perform experiments from the top of the leaning tower of Pisa. He put two and two together and obtained a little more than four.

Of course such a construction is not acceptable as modern history of science. But it was very common, and even a "must" in this type of seventeenth-century prose. The leaning tower experiment is simply a classical anecdote in a classical biography of those times. And Viviani, on this occasion, was not writing as a scientist or as a historian of science, but as a man of letters,

[38] *OG* 4, 242.

[39] L. Magalotti (ed.), *Saggi di naturali esperienze fatte nell'Accademia del Cimento...* (Florence, 1667).

[40] G. Abetti and P. Pagnini (eds), *Le Opere dei discepoli di Galileo Galilei, edizione nazionale, vol. I, L'Accademia del Cimento. Parte Prima* (Florence, 1942), p. 341.

addressing an audience interested in literature. He should only be praised for having succeeded in producing such a piece of prose, with so much truth in it. And, as Cooper made clear, he is probably more reliable as a historian of science than many modern historians of science who amplified what he wrote.

The story of the leaning tower experiment, therefore, should not be a matter for consideration by historians of science, but rather by historians of literature. Yet it remains important as a historiographical case study. On the one hand, it may help us understand the sixteenth-century historiography of science. But, on the other hand — more a question related to modern historiography of science — it would be helpful to understand why leading historians insist so much on the truth of the story; somehow it is disquieting when important scholars such as Antonio Favaro or Aldo Mieli make use of their prestige and of rhetorical arguments — rather than rational ones — to "refute" the views of their opponents.

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