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72. We wish to thank Dr. H. Fernández-Morán, Mixter Laboratories for Electron Microscopy, Massachusetts General Hospital, Boston, for the electron micrograph of the mitochondria of retinal rods and cones shown on the cover. For the techniques of high-resolution electron microscopy used to obtain this micrograph see H. Fernández-Morán, *J. Appl. Phys.* **30**, 2038 (1959), *Ann. N. Y. Acad. Sci.* **85**, 689 (1960). Dr. Fernández-Morán has also examined preparations of the electron transport particle (ETP) from our laboratory, and has observed a predominant type of particle 150 to 200 A in diameter with indications of substructure of the order of 15 to 20 A (private communication).

An Experiment in the History of Science

With a simple but ingenious device Galileo could obtain relatively precise time measurements.

Thomas B. Settle

On the "Third Day" of his *Discorsi* (*1*) Galileo described an experiment in which he had timed a ball accelerating along different lengths and slopes of an inclined plane. With it he believed he had established the science of naturally accelerated motion. To get a better appreciation for some of the problems he faced I have tried to reproduce the experiment essentially as Galileo described it. In the process I found that it definitely was technically feasible for him, and I think I gained a good idea of the type of results he probably looked for and of how well they turned out.

He described the experiment because, in his words: "in those sciences where mathematical demonstrations are applied to natural phenomena, as is seen in the case of perspective, astronomy, mechanics, music, and others [,] the principles, once established by well-chosen experiments, become the foundations of the entire superstructure" (*1*, p. 171). In this case his aim was to establish a science based on two principles: (i) a general definition of uniform acceleration, "such as actually occurs in nature" (*1*, p. 154), as that motion in which equal increments of velocity are added in equal times and (ii) an assumption that "the speeds acquired by one and the same body

The author is a graduate student in the history department of Cornell University, Ithaca, N.Y.

moving down planes of different inclinations are equal when the heights of these planes are equal" (*1*, p. 163). Though he could not test these assumptions directly, he claimed that he tested consequences of them which, to us, seems to carry the same weight.

This is relatively straightforward. Though Galileo did not give us a sampling of his data, he did tell us what equipment he used, and did state explicitly that his results were very good. Since we know his "principles" were correct theoretically, we should have no reason, on the face of it, to doubt any of the particulars.

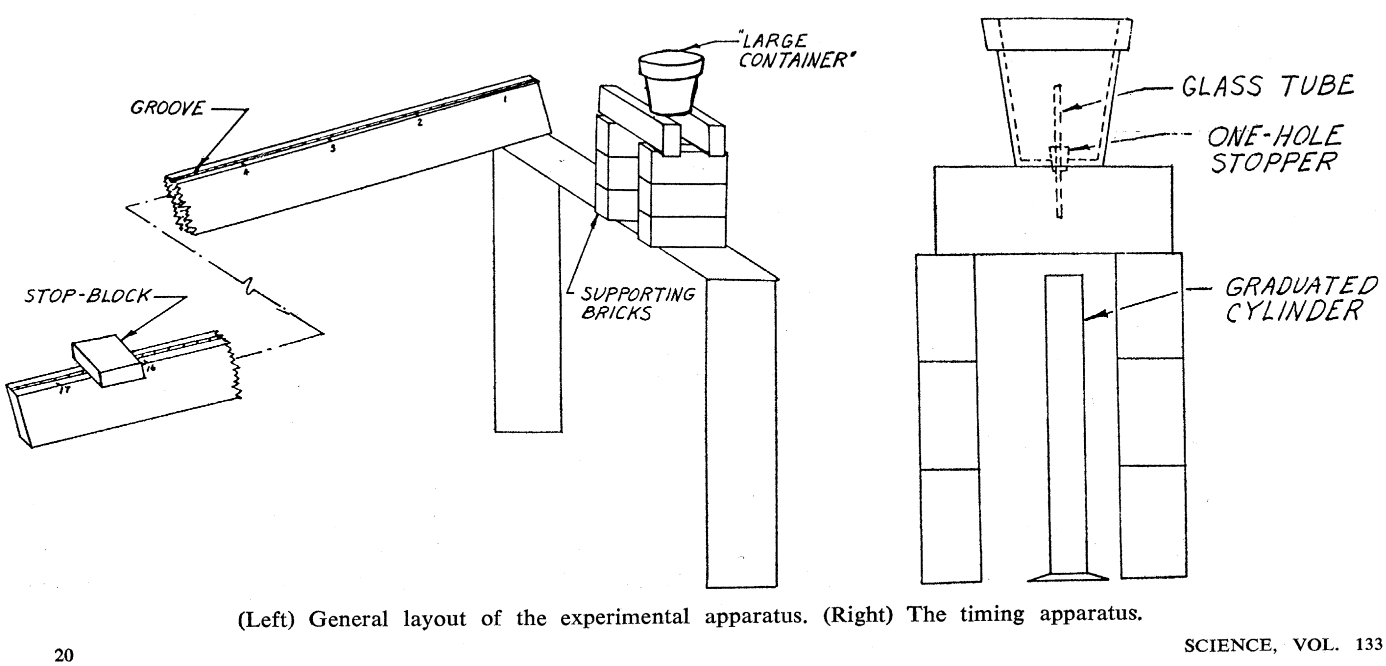
Yet they have been doubted. Before the publication of the *Discorsi*, Marin Mersenne had seen references to the experiment which lacked experimental detail. From these he had tried to perform the experiment; and because, probably, of a combination of conceptual and experimental errors, which we need not explore here, he concluded: "Ie doute que le sieur Galilee ayt fait les experiences des cheutes sur le plan, . . . l'experience n'est pas capable d'engendrer vne science" (*2*). Perhaps taking his cue from Mersenne, Alexandre Koyré has recently commented on the "amazing and pitiful poverty of experimental means at his [Galileo's] disposal": "A bronze ball rolling in a 'smooth and polished' wooden groove! A vessel of water with a small hole through which it runs out and which one collects in a small glass in order to weigh it afterwards and thus measure the times of descent (the Roman water-clock, that of Ctesebius, had been already a much better instrument): what an accumulation of sources of error and inexactitude!" (*3*).

An interesting conclusion, but I think a bit premature. To my knowledge no one has ever tried to perform an experiment equivalent to the one Galileo described. The laws of acceleration have been demonstrated many times with more sophisticated techniques; but no one, including Mersenne, has ever tried to find out if Galileo's wooden channel and water timing device actually worked, or what sort of results he accepted as the foundations of his new science. If these questions were merely of antiquarian interest we could leave them to the mercy of each individual's philosophic predisposition. But they are more; they weigh heavily upon, and are in fact basic to, any adequate evaluation of the logico-scientific status of Galileo's exposition of naturally accelerated motion, his real contributions to science, or his views on the nature of science and the need for experiment.

I hope to show that this experiment, once conceived and brought to full maturity, is simple, straightforward, and easy to execute. Thus far I can only reproduce the end product of a process of evolution (in Galileo's own mind) which may have covered 20 years. There is, in addition, a fascinating and vastly important body of knowledge concealed in the "conceiving" and "bringing to maturity" of both the theoretical and empirical aspects of this experimentation, just as in most other significant departure points in the history of experimental science. Eventually we would like to know the actual evolution of Galileo's thought in time as well as logic. For each step of original work we would like to know the mistakes and dead ends, the contributions and limitations of the existing technology and mathematics, the many conceptual aids as well as hindrances inherited from his contemporaries, and the nature and significance of his own predispositions. This could, we would hope, give us broader insights into the formative stages of any new discipline. But for now our aims are more limited.

First, let us see what Galileo himself says of the experiment (*1*, pp. 171–72):

> A piece of wooden moulding or scantling, about 12 [braccia (*4*)] long, half a [braccio] wide, and three finger-breadths thick, was taken; on its edge was cut a channel a little more than one finger in breadth; having made this groove very straight, smooth, and polished, and having lined it with parchment, also as smooth and polished as possible, we rolled along it a hard, smooth, and very round bronze ball. Having placed this board in a sloping position, by lifting one end some one or two [braccia] above the other, we rolled the ball, as I was just saying, along the channel, noting, in a manner presently to be described, the time required to make the descent. We repeated this experiment more than once in order to measure the time with an accuracy such that the deviation between two observations never exceeded one-tenth of a pulse beat. Having performed this operation and having assured ourselves of its reliability, we now



(Left) General layout of the experimental apparatus. (Right) The timing apparatus.

rolled the ball only one-quarter the length of the channel; and having measured the time of its descent, we found it precisely one-half of the former. Next we tried other distances, comparing the time for the whole length with that for the half, or with that for two-thirds, or three-fourths, or indeed for any fraction; in such experiments, repeated a full hundred times, we always found that the spaces traversed were to each other as the squares of the times, and this was true for all inclinations of the plane, i.e., of the channel, along which we rolled the ball. We observed that the times of descent, for various inclinations of the plane, bore to one another precisely that ratio which, as we shall see later, the Author had predicted and demonstrated for them.

For the measurement of time, we employed a large vessel of water placed in an elevated position; to the bottom of this vessel was soldered a pipe of small diameter giving a thin jet of water, which we collected in a small glass during the time of each descent, whether for the whole length of the channel or for a part of its length; the water thus collected was weighed, after each descent, on a very accurate balance; the differences and ratios of these weights gave us the differences and ratios of the times, and this with such accuracy that although the operation was repeated many, many times, there was no appreciable discrepancy in the results.

Then let us recognize what, exactly, Galileo sought, so that we will demand no more of his work than he did himself. Galileo thought in the language and form of Euclidean geometry. He had neither the apparatus of functional mathematics nor the interdefined system of standard weights and measures which would allow him to work with such a formula as $s = \frac{1}{2}gt^2$. He designed his equipment for less sophisticated use. In substance, he only asked it to show that: (i) for a given inclination of the plane, the distances a ball travels are in direct proportion to the squares of the time intervals (*5*):

$$S_1/S_2 = T_1^2/T_2^2 \qquad (1)$$

and (ii) for planes of different inclinations, the times of descent are proportional directly to the distance of travel and inversely to the square root of the vertical height of fall (*6*):

$$T_1/T_2 = (L_1/L_2)\ (H_2/H_1)^{\frac{1}{2}} \qquad (2)$$

This is important for at least three reasons. We must not ask him to give us a value for the acceleration due to gravity as we understand the term. Our "*g*" only came much later, after a great deal of further development in physics and mathematics (*7*). Nor should we expect him necessarily to give determinations that might be interpreted as an early form of the same thing. In addition, we see there is little justice in Koyré's criticism that Galileo failed to account for rotational inertia (*3*). Not only did the problem not exist in his mind, but it was irrelevant to the proof of his laws. The functional equivalent for

$$s = \tfrac{1}{2}gt^2$$

for a ball on an inclined plane is

$$s = \tfrac{1}{2}\,(5/7)(a/c)gt^2,$$

a/c being the ratio, for a given slope, of the vertical height of fall to the slope length. The factor 5/7 accounts for rotational inertia; being constant, it does not affect the proportionalities given above. Finally, because he could work entirely with ratios, Galileo could be completely arbitrary in his choice of measures.

Reproducing the Experiment

The most difficult part of executing the experiment lay in the necessity of choosing equipment and procedures which were available to Galileo or which were inherently no better than those he could muster. In making a plane, for instance, I assumed that he would have had excellent craftsmen at his disposal but that the work would have been done essentially by hand. Nonetheless, after choosing a 2- by 6-inch pine plank 18 feet long, with a straight grain and few knots, I had a ¼-inch rectangular groove cut in one edge with a circular saw (*8*). This done, I hand-sanded the surfaces, applied wood filler, and thoroughly rubbed in wax, making the rolling edges of the groove hard and smooth. Even so, there were irregularities where knots or the grain crossed the groove. But I made no further attempt to make the edges exactly parallel over the whole length.

I used both a standard billiard ball and a steel ball bearing, respectively about 2¼ inches and ⅞ inch in diameter.

For time measurement I used an ordinary flowerpot as a water container and threaded a small glass pipe through its bottom hole for the outflow. In all the live runs this pipe was 4½ inches long and had an inside diameter of about 0.18 inch. Its upper end was positioned high enough for me to cover it easily with a finger while my palm rested on the rim of the pot. Instead of collecting the water and then weighing it on a balance, I collected it in a graduated cylinder and "weighed" it by reading its volume in milliliters.

Then, for each reading, I placed a wooden block at a predetermined distance down the slope; filled the pot with water while holding a finger over the inside end of the pipe; filled the pipe by letting the water flow briefly; took an initial reading of the water level in the graduated cylinder; placed the ball at the starting position on the plane with my free hand; released the ball and lifted my finger simultaneously; replaced my finger at the sound of the ball striking the block; and took a final reading of the graduated cylinder.

How good was all this? From a study of the ratios we know that Galileo had to make only three measurements: slope length, vertical height of fall, and time. The first was easy; I marked off the plane in even foot lengths, using a 1-foot architect's scale. Actually, all either I or Galileo needed was a compass sufficiently large to mark off convenient unit lengths and sufficiently rigid to do it accurately. Then ratios of length turn out to be rational fractions.

Galileo did not mention how he measured vertical height, but water-level techniques for various purposes had been used in the building trades for centuries, and measuring heights would have presented no serious problem. I took a long piece of flexible tubing, fixed a short length of glass pipe in either end, and filled it with water. Placing the meniscus in one pipe at a mark near the lower end of the plane, I could measure vertically from the meniscus of the other pipe to a mark near the upper end. For each inclination we need only one such measure to compare with the distance between marks. The scales do not even have to be to the same base.

Of the three measurements, the measurement of time is the most controversial and the most difficult. With a little thought we find that it has two crucial aspects: we want the flow from the pipe to be uniform for at least the period of our longest readings, and we need to practice so that we can actually release the ball and the water flow at the same time and stop the flow at the strike of the ball without anticipation or delay.

First, we must remember that the operator is an integral part of the apparatus. He must spend time getting the feel of the equipment, the rhythm of the experiment. He must consciously train his reactions. And each day, or at the end of each break, he must be allowed a few practice runs to get warmed up. Galileo accomplished all this by repeating the experiment "many, many times."

Table 1. Sample of experimental results and calculations which confirm Eq. 2.

Distance	Time (ml of water) (Exp.)	(Av.)	(Cal.)
15	88	90+	90+
	91		
	91		
	90		
	90		
	90		
	90		
	90		
	89		
	90		
13	84	84	84
	84	base	
	84		
	84		
	84		
	84		
	84		
10	72	72+	74−
	73		
	72		
	72		
	72		
	72		
7	62	62−	62−
	61		
	62		
	61		
	62		
	62		
	62		
5	53	52	52+
	53		
	53		
	53		
	53		
	52		
	53		
	51		
	51		
	52		
	53		
	51		
3	40	40	40+
	40		
	40		
	41		
	39		
	41		
	40		
1	26		
	17		
	25		
	24		
	23		
	25		
	23	23.5	23+
	24		
	23		
	23		
	24		
	24		
	24		
	23		
	23		

Then we must remember that this is not a water clock; it is what it is and no more—a container for water with a pipe of small diameter in its bottom and with no dials, falling weights, or gear trains. All we are interested in, we find, is maintenance of a constant flow in the pipe for a maximum of 8 seconds. How can we test this? Galileo mentions a "pulse beat." Is it possible that he checked his own flow rate against a beating pendulum, a *pulsilogia?* On this hunch I made a simple pendulum out of a piece of thin wire and the billiard ball. Since a 1-meter pendulum has a beat of about 1 second, I made this pendulum somewhat less than a meter long so that it would beat at about pulse rate. By watching the shadow of the bob against vertically lined paper I could accurately lift and reset my finger in the timer at the end of a beat. I found, after collecting water at intervals of 2, 4, 6, 8, and 10 beats, that the flow was indeed constant within the limits of precision discussed below (*9*).

As a matter of interest, using the second-hand on my watch and timing for 5- and 10-second intervals, I made a rough determination of the rate of flow and found it to be 19.5 milliliters per second. It followed that, if I could measure a definite interval to within 2 milliliters, my apparatus would be precise to almost 1/10 second. In fact, it was very common to get sets of points well within this limit, to 1 milliliter or about 1/20 second. Is this better than Galileo could have done? My flowerpot was probably smaller than his "large vessel," giving me a greater fall of head for each reading. If my flow was "constant," his certainly was. Then the only thing in doubt is the "weighing." From Agricola we learn that early 16th century assayers could weigh with precision to the equivalent of 0.2 grams (*10*). My cylinder was graduated to 2 milliliters, and I read to 1 milliliter—a measurement five times as crude as the one that Galileo could have commanded.

We note further that Galileo, though presenting his results as valid for all slopes, only claimed to have successfully tested relatively shallow ones.

Table 2. Experimental data obtained with the billiard ball for the bases of three slopes, and times computed from one of the other slopes. *L*, slope length; *a*, vertical height; *T*, time.

Slope	Experimental data L	*a*	T	Calculated data T
a	12	2.92	117	118− (from b)
b	13	6.25	84	85− (from c)
c	9	11.47	52	51+ (from a)

Whether this was the result of experimental insight alone or of poor results obtained at steeper inclinations we do not know. But the reasons are obvious. The theoretical results are only valid if there is no slippage between the ball and the plane and since the errors in the time readings are fixed, the accuracy decreases with the shorter intervals. So I followed Galileo's example, nor did I think it particularly worthwhile to try to find a maximum practicable slope.

Experimental Results

As I have intimated, all this turned out quite well. Table 1 gives a representative sample of some experimental results and calculations which confirm Eq. 1 above. This particular run involved the billiard ball on a slope;

$$a/c = 6.25/(8 \times 12) \text{ inches,}$$

or about 3°44′. The distances are given in Table 1, column 1.

Column 2 gives, for each distance, the several observed times in milliliters of water. In this case all except the last set were recorded one evening, this last being recorded the following morning. Here we see the process of warming up; only after the first six readings did I begin to take the results seriously.

Column 3 merely gives the sight-averages of the good readings of column 2. They serve as specific times for the distances where these are needed in further calculations or comparisons.

Column 4 shows calculated times. Whereas Galileo struggled simultaneously with two unknowns, the validity of the laws and the worth of the equipment, I was really using known and accepted laws to determine the latter. As a result I have chosen to focus on the most ticklish part of the work, the time measurements, by comparing the experimental and theoretical determinations. For each run I chose the sight-average time for one of the middle-to-long distances as a base. Then, using the equation

$$T_1 = (S_1/S_2)^{\frac{1}{2}} \times T_2,$$

I calculated times for the other distances. Actually, we are comparing experimental points with points on a parabola passing through one of them.

This comparison needs little comment. Even the maximum deviation, at distance 10, is less than 2 milliliters, or 1/10 second. Elsewhere, by and large, the deviations are considerably less.

The check of Eq. 2 turns out just as well. To fit my data and purposes I reduced it to

$$T_1 = [(L_1/L_2)\ (a_2/a_1)]^{\frac{1}{2}} \times T_2,$$

a being a unit measure of vertical height. Table 2, columns 1–4, shows the pertinent experimental data, obtained with the billiard ball, for the bases of three slopes. Column 5 shows times computed, as noted, from one of the other slopes.

The results of the tests made with the steel ball were just as good, but I found that they were not comparable with those made with the billiard ball. For instance, on the shallowest slope, the billiard ball made the 16-foot mark in 136 milliliters but the steel ball took 4 milliliters longer. This seemed odd; theoretically, neither the mass nor the radius should affect the acceleration. By the correct formula we can calculate that both balls should have traversed the distance in 132 milliliters. Actually, because the balls run on the two edges of the groove, their "running" circumferences are slightly less than their real ones, so they require more revolutions, and more time, to cover the same distance. A rough calculation shows that this fact probably accounts for most of the discrepancies. Had Galileo noticed similar differences between results for balls of different size, he probably would have ascribed them to frictional retardation. In any case, it appears that they would not have controverted his proportionalities.

Conclusion

I have tried to emphasize the simplicity and ease with which these results were obtained. The only extended effort put into the equipment was with respect to the plane, and then only to the limits already mentioned. And except for the effort involved in developing my own ear-hand coordination, I maintained a deliberately cavalier attitude towards the procedures and measures. For instance: the striking block and the starting position were located at the marks on the slope only by eye; the vertical height reading was not taken as finely as more time and patience would have allowed; and, I am sure, the time measure was not brought to as high a polish as a larger pot, a smaller pipe, and a finer "balance" would have made possible. But with no more precise knowledge of Galileo's tools than what can be learned in the passage cited, I wanted to give "error and inexactitude" every reasonable chance to accumulate. And yet they did not.

What of this? When I said that Galileo worked with two unknowns, I meant it only from a logical point of view. By the time both the theory and the experiment had evolved to the level implicit in the *Discorsi,* Galileo would have had sufficient confidence in the worth of each independently, irrespective of their mutual confirmation. And the fact that they coincided so nicely added one more to the list of those sciences in which mathematical demonstration is appropriate to physical phenomena. But it was not as simple then as it seems now. Science could only grow on the bones of one of the deepest prejudices of the Middle Ages, one which regarded all here below as corrupt and innately lacking the perfection, mathematical or otherwise, of the real world. At one place in Galileo's other major work, the *Dialogo,* Simplicio is made to express this opinion by saying: "In physical science there is no occasion to look for mathematical precision of evidence" (*11*). By finding this excellent approach to perfection in the physical world, Galileo took a long and important step in this early phase of experimental science.

References and Notes

1. G. Galilei, *Dialogues Concerning Two New Sciences*, H. Crew and A. de Salvio, trans. (Northwestern University, Evanston and Chicago, 1946).
2. M. Mersenne, *Harmonie Universelle: Livre Second: Des movvemens de tovtes sortes de corps* (Paris, 1636–37), p. 112. Mersenne, a Minim friar and close friend of Descartes, corresponded regularly with many of the leading figures of his day and was a physicist-mathematician in his own right. When he did the work described in *Harmonie* he had access to Galileo's published material—for instance, the *Dialogo* (see *11*)—and to circulating manuscripts, but probably not to the *Discorsi*, which was only published in 1638. Although Galileo had alluded to the experiment in several places, nowhere had he given the descriptive detail present in the *Discorsi*. We may even guess that the detail given there may have been in answer to Mersenne's criticism.
3. A. Koyré, "An Experiment in Measurement," *Proc. Am. Phil. Soc.* **97**, 224 (1953).
4. Galileo used the term *braccio*; it has been translated variously as *cubit* and *yard*, to neither of which it accurately corresponds. In the *Discorsi* (p. 16) Galileo spoke of the fact that "it was not possible, either by pump or by any other machine working on the principle of attraction, to lift water a hair's breadth above 18 braccia." If 34 feet is assumed to be the equivalent in our system, 1 braccio should be close to 22.7 inches, a figure a shade higher than that usually given.
5. See G. Galilei, *Discorsi,* pp. 167–168, theorem II, proposition II.
6. See ———, *ibid.*, p. 181, theorem V, proposition V.
7. Conceptually, the acceleration due to gravity is considerably more advanced than the distance a body falls in the first second—a measurement which several of Galileo's contemporaries did try to perform. The first fits into a functional relationship with all the attendant transformation provided by the calculus; the second can only be used in calculation involving ratios of quantities subject to the same effects. The first is regarded as a special case of universal gravitation and is quite easily seen as a function of latitude, altitude, the presence of massy formations, and so on; the second, Galileo and his contemporaries generally regarded as constant over the surface of the earth and extending, only hypothetically if at all, undiminished to such locales as the moon. Galileo had not abstracted the notions of velocity and acceleration as quantitative entities in themselves (instantaneous velocity and acceleration), notions which "fell out in the wash" with the development of the calculus. He thought of motion in terms of total distance as compared to total time, and of acceleration as the uniform increase of "motion" from one large segment to another—that is, he talked only of total distance and time and of ratios of them.
8. Galileo did not describe in detail the shape of the groove he used or how the ball (or balls) fitted into it. I assume that he rolled the ball on the edges of the groove, as I did.
9. Each time, in filling the pot, I referred the water level to a definite mark, making it somewhat lower for the short runs and slightly higher for the longer ones. I believe, though without definite proof, that this sort of compensation was not beyond the level of Galileo's capacities.
10. G. Agricola, *De Re Metallica,* translated by H. C. Hoover (Dover, New York, 1950), appendix C.
11. G. Galilei, *Dialogue Concerning the Two Chief World Systems—Ptolemaic & Copernican*, S. Drake, trans. (Univ. of California Press, Berkeley and Los Angeles, 1953), p. 230.