AN EXPERIMENT IN MEASUREMENT

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HISTORIANS of modern science,[1] when trying to determine its essence and structure, and thus to oppose it to the medieval and classical ones, insist, as often as not, in contradistinction to the abstract and bookish character of the latter, upon the empirical and concrete character of the former. Observation and experience leading a vigorous—and victorious—assault upon tradition and authority: such is the image, itself traditional, that we are usully given of the spiritual revolution of the seventeenth century, of which modern science is, in the same time, the root and the fruit.

This picture is by no means wrong. Quite on the contrary: it is perfectly obvious that modern science has unmeasurably—and even beyond measure—enlarged our knowledge of the world, increased the number of "facts"—all kinds of facts—that it has discovered, observed and collected. Besides, it is just in this way that some of the founders of modern science have seen and understood themselves and their work. Gilbert and Kepler, Harvey and Galileo—they all extoll the admirable fecundity of experience and direct observation, as they oppose it all to the sterility of abstract and speculative thought.[2]

Yet, whatever the importance of the new "facts" discovered and brought together by the *venatores,* a simple amount of "facts," that is, a mere collection of observational or experiential data, does not constitute a science: they have to be ordered, interpreted, explained. In other words, it is only when subjected to theoretical treatment that a knowledge of facts becomes science.

Besides, observation and experience—in the meaning of brute, common-sense observation and experience—had a very small part in the edification of modern science;[3] one could even say that they constituted the chief obstacles that it encountered on its way. It was not *experience*, but *experiment* that had nourished its growth and decided the struggle: the empiricism of the modern science is not *experiential*; it is *experimental.*

I certainly don't need to insist here upon the difference between "experience" and "experiment." Yet I would like to stress the close connection between this latter and the building of theory. Far from being opposed to each other, experiment and theory are bound together and mutually interdetermined, and it is with the growth of precision and refinement of theory that grow the precision and refinement of the experiments. Indeed, an experiment—as Galileo so beautifully has expressed it—being a question put before nature, it is perfectly clear that the activity which results in the asking of this question is a function of the elaboration of the language in which it is formulated. Experimentation is a teleological process of which the goal is determined by theory. The "activism" of modern science, so well noticed—*scientia activa, operativa*—and so deeply misinterpreted by Bacon is only the counterpart of its theoretic development.

We have to add, moreover—and this determines the characteristic features of modern science—that, for its theoretical work, it adopts and develops the pattern of thinking of the mathematician. This is the reason why its "empiricism"

[1] I shall use the term "modern science" for the science of the seventeenth and eighteenth centuries, i.e., for the period which goes, roughly, from Galileo to Einstein. This science is sometimes called "classical" science in contradistinction to the contemporary one; I will not follow this usage and will reserve the designation "classical science" to the science of the classical world, chiefly to that of the Greeks.

[2] *Cf.* for instance W. Whewell, *History of the inductive sciences,* 3 v. London, T. W. Parker, 1837; E. Mach, *Die Mechanik in ihrer Entwicklung, historisch-kritisch dargestellt,* Leipzig, F. A. Brockhaus, 1883; 9th ed., Leipzig, F. A. Brockhaus, 1933; in English under the title: *The science of mechanics,* Chicago, Open Court, 1883; 5th ed., La Salle, Open Court, 1943.

[3] As recognized already by Tannery and Duhem, the Aristotelian science is in much better accordance with common experience than that of Galileo and Descartes. *Cf.* P. Tannery, Galilée et les principes de la dynamique, in *Mémoires Scientifiques* **6**: 400 sq., Toulouse, E. Privat, 1926; P. Duhem, *Le Système du Monde* **1**: 194–195, Paris, Herrman, 1913.

differs *toto caelo* from that of the Aristotelian tradition:[4] "the book of nature is written in geometrical characters" declared Galileo; this implies that in order to reach its goal modern science is bound to replace the system of flexible and semi-qualitative concepts of the Aristotelian science by a system of rigid and strictly quantitative ones. Which means that modern science constitutes itself in substituting for the qualitative or, more exactly, for the *mixed* world of common-sense (and Aristotelian science) an Archimedian world of geometry made real; or—which is exactly the same thing—in substituting for the world of the more-or-less of our daily life a universe of measurement and precision. Indeed this substitution implies automatically the exclusion from—or the relativation in—this universe of everything that cannot be subjected to exact measurement.[5]

It is this research of quantitative precision, of the discovery of exact numerical data, of these "numbers, weights, measures" upon which God has built the world that forms the goal, and thus determines the very structure of the experiments of modern science. This procedure is not co-extensive to experimentation in general: neither the alchemy, nor Cardano, nor Giambatista Porta—nor even Gilbert—is looking for numerical results. This because they think the world as an ensemble of qualities much more than as an ensemble of magnitudes. Quality, indeed, is repugnant to the precision of measure.[6] Nothing is more significant in this respect than the fact that Boyle and Hooke, both of them experimenters of the first rank, men who know the value of precise measurement, make a purely qualitative study of the spectral colors. Nothing reveals better the incomparable greatness of Newton than his ability to transcend the realm of quality and to break through into the realm of physical, that is quantitatively determined, reality. But besides the theoretical (conceptual) and psychological difficulties that hinder the application of the idea of mathematical rigor to the world of perception and action, the actual performance of a correct measurement encounters in the seventeenth century technical difficulties of which, living as we do in a world overcrowded with, and dominated by, precision instruments, we have, I am afraid, a very bad understanding. Even historians, who—as Professor I. Bernard Cohen pointed out—only too often present us with the decisive experiments of the past not as they *were* actually performed *then,* but as they *are* performed *now* in our laboratories and classrooms, do not realize the real conditions, and therefore the real meaning, of experimentation in the heroic epoch of modern science.[7] And it is in order to bring a contribution to the history of the constitution of the experimental methods of science that I will try, today, to tell the story of the first conscious and sustained attempt of an experimental measurement. The measurement of a universal constant, the constant of acceleration of bodies in their free fall.

Everybody knows the historical importance of the law of fall, the first of the mathematical laws of the new dynamics developed by Galileo, the law which established, once and forever, that "motion is subjected to the law of number."[8] This law presupposes that gravity, though by no means an essential property of bodies (and of which, moreover, we ignore the nature), is, nevertheless, their universal property (all bodies are "heavy" and there are no "light" ones); besides, for every one of them it is an invariable and constant property. It is only on these conditions that the Galilean law is valid (in the vacuum).

Yet, in spite of the mathematical elegance and physical plausibility of the Galilean law, it is obvious that it is not the only possible one.[9] Besides,

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[4] It is an empiricism that the Aristotelian tradition opposes to the abstract mathematism of the Galilean dynamics. *Cf.,* on the empiricism of the Aristotelians, J. H. Randall, Jr., Scientific method in the School of Padua, *Jour. Hist. of Ideas* **1**: 177–206, 1940.

[5] This applies, of course, only to the so called "exact sciences" (physico-chemical) in contradistinction to "natural science" or "history" (sciences dealing with the "natural" world of our perception and life) which does not—and perhaps cannot—discard quality and substitute a world of exact measures for the world of the "more-or-less." In any case neither in botany nor in zoology, nor even in physiology and biology did exact measurements play any role; their concepts are still the non-mathematical concepts of the Aristotelian logic.

[6] Quality can be ordered, but not measured. The "more and less" we are using in respect to quality enable us to build a scale, but not to apply exact measurement.

[7] *Cf.* I. Bernard Cohen, A sense of history in science, *Amer. Jour. Physics* **18** (6): 343 sq., 1950.

[8] *Cf.* Galileo Galilei, *Discorsi e Dimostrazioni matematiche intorno a due nuove scienze, Opere,* Edizione Nazionale, **8**: 190, Firenze, 1898.

[9] Thus G. B. Baliani proposes a law according to which traversed spaces are *ut numeri* and not *ut numeri impares;* Descartes and Torricelli discuss the possibility of the spaces being in cubical and not in quadratic proportion to the time; in the Newtonian physics acceleration is a function of attraction and therefore not constant. Moreover, as Newton himself does not fail to point out, the inverse square law of attraction is by no means the only possible one.

we are not in the vacuum but in the air, and not in the abstract space, but on the earth, and even, perhaps, on a moving one. It is quite clear that an experimental verification of the law, as well as of its applicability to bodies falling in our space, *in hoc vero aere,* is indispensable. Just as is indispensable the determination of the concrete value of the acceleration (of g).

It is well known with what extreme ingenuity, being unable to perform direct measurements, Galileo substitutes for the free fall the motion on an inclined plane on one hand, and that of the pendulum on the other. It is only justice to recognize his immense merit and genial insight, which are not diminished by the fact that they are based on two wrong assumptions.[10] But it is justice too to turn our attention to the amazing and pitiful poverty of the experimental means at his disposal.

Let us learn from himself his *modus procedendi*:[11]

A piece of wooden moulding or scantling, about 12 cubits long, half a cubit 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 cubits 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 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 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 also 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 has predicted and demonstrated for them.[12]

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 the ratios of these weights gave us the difference and the ratios of the times, and this with such accuracy that although the operation was performed many, many times there was no appreciable discrepancy in the results.

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!

It is obvious that the Galilean experiments are completely worthless: the very perfection of their results is a rigorous proof of their incorrection.[13]

[10] Galileo's experiments are based on the assumptions (*a*) that the motion of a ball *rolling* down on an inclined plane is equivalent to that of a body *gliding* down (without friction) on the selfsame plane and (*b*) that the pendular motion is perfectly isochronous. This isochronism being a consequence of his law of fall, an experimental confirmation of the former would therefore confirm the latter. Unfortunately, no direct measurement of consecutive oscillation-periods is possible: just because there are no clocks with which we could measure them. Galileo, therefore—and one cannot but admire his experimental genius—substitutes for the direct measurement the comparison of the motion of two different pendula (of equal length), the bobs of which, though having performed oscillations of different amplitudes, arrive nevertheless at the same moment at their position of equilibrium (the lowest point of the curve); the same experiment made with pendula, the bobs of which are constituted by bodies of different weight, demonstrates experimentally that bodies heavy and light (individually as well as specifically) fall with the same speed. *Cf. Discorsi,* 128 sq.

[11] *Cf. Discorsi,* 212 sq. I am quoting the translation of Henry Crew and Alfonso de Savio, *Dialogues concerning two new sciences,* 178 sq., N. Y., Macmillan, 1914; reprinted, N. Y., Dover Publications, 1952.

[12] The speed of the descent is proportional to the sine of the angle of inclination. *Cf. ibid.,* 215, 219; pp. 181, 185 of the translation.

[13] Modern historians, accustomed to see the Galilean experiments performed for the benefit of students in our school laboratories, accept indeed this astonishing report as gospel truth and even praise Galileo for having thus experimentally established not only the empirical validity of the law of fall, but even this law itself. (*Cf.,* among countless others, N. Bourbaki, *Eléments de mathématique* **9**, première partie, livre IV, chap. I–III, Note historique, p. 150 (Actualités scientifiques et industrielles, N **1074**, Paris, Herrman, 1949)). *Cf.* Appendix 1.

No wonder that Galileo who, doubtlessly, is fully aware of all that, refrains, as far as possible (thus in the *Discourses*), from giving a concrete value for the acceleration; and that whenever he gives it (as in the *Dialogue*), it is completely and utterly false. So false that Father Mersenne has been unable to hide his surprise: "He supposes," writes he to Peyresc,[14] "that a bullet falls one hundred cubits in 5″ [seconds]; wherefrom it follows that the bullet will fall not more than four cubits in one second, though I am certain that it will fall from a greater height."

Indeed, four cubits—not even seven feet[15]—is less than the half of the true value; and about half the value that Father Mersenne will establish himself. And yet, that the figures given by Galileo are grossly inaccurate is by no means surprising; quite the contrary: it would be surprising, and even miraculous, if they were not. What is surprising, that is the fact that Mersenne, whose experimental means were not much richer than those of Galileo, could have obtained so much better results.

Thus modern science finds itself at its beginnings in a rather strange and even paradoxical situation: it has precision for principle; it asserts that the real is, in its essence, geometrical and, consequently, subject of rigorous determination and measurement (*vice-versa,* mathematicians as Barrow and Newton see in geometry itself a science of measurement[16]); it discovers and formulates (mathematically) laws that allow it to deduce and to calculate the position and speed of a body at each point of its trajectory and at each moment of its motion, and it is not able to use them because it has no ways to determine a moment, nor to measure a speed. Yet, without these measures the laws of the new dynamics remain abstract and void. In order to give them a real content it is indispensable to possess the means of measuring time (space is easy to measure), that is *organa chronou, horologia,* timekeepers as Galileo has called them; in other words: reliable clocks.[17]

Time, of course, cannot be measured directly but only through something else in which we find it embodied. That is either (*a*) a constant and uniform process, such for instance as the constant and uniform motion of the heavenly sphere or the constant and uniform outflow of water in the water-clock of Ctesebius;[18] or (*b*) a process which, though not uniform in itself, can be repeated, or repeats itself, automatically; or finally (*c*) a process which, though not repeating itself as completely identical, employs for its completion the same amount of time, presenting us thus, so to say, an atom or unity of duration.

It is in the pendular motion that Galileo found such a process. Indeed a pendulum, provided of course all external and internal impediments, such, for instance, as friction or the resistance of air, were eliminated, would reproduce and repeat its oscillations, in a perfectly identical manner, till the end of time. Moreover, even *in hoc vero aere* where its motion is continuously retarded and where no two oscillations are strictly identical, the period of these oscillations remains constant.

Or to put it in Galileo's own words:[19]

> First of all one must observe that each pendulum has its own time of vibration so definite and determinate that it is not possible to make it move with any other period than that which nature has given it, and which depends neither on the weight of bob, nor on the amplitude of the oscillation, but only and solely on the length of the pendulum.

[14] F. M. Marin Mersenne, *Lettre à Peyresc* of 15 January 1635; *cf.* Tamizey de Larroque, *La Correspondance de Peyresc* **19**: 112, Paris, A. Picard, 1892; *cf. Harmonie Universelle* **1** (2): 85, 95, 108, 112, 144, 156, 221, Paris, 1636.

[15] The florentine cubit, doubtlessly used by Galileo, contains 20 inches, i.e., 1 foot, 8 inches; and the Florentine foot is equal to the Roman one, that is equal to 29.57 cm.

[16] *Cf.* Isaak Barrow, *Lectiones Mathematicae* of 1664–66 (*The Mathematical Works of Isaac Barrow, D. B.,* ed. by W. Whewell, Cambridge, C. U. P. 1860), 216 sq; Isaac Newton, *Philosophiae naturalis principia mathematica, preface,* London, 1687.

[17] The unreliability of the clocks of the sixteenth and seventeenth centuries is well known; precision clocks are byproducts of scientific development (*cf.* Willis I. Milham, *Time and timekeepers,* N. Y., Macmillan, 1923; L. Defossez, *Les savants du XVIIe siècle et la mesure du temps,* Lausanne, ed. *Journal Suisse d'horlogerie,* 1946), yet their building is usually explained by the urge of solving the problem of longitude, i.e., the pressure of practical needs of navigation, the economical importance of which grew considerably since the circumnavigation of Africa and the discovery of America (*cf.* for instance Lancelot Hogben, *Science for the citizen,* 2nd ed., 7th impression, 235 sq.; London, G. Allen and Unwin, 1946). Without denying the importance of practical needs or economic factors on the development of science, I believe this explanation, which combines Baconian and Marxist prejudices for *praxis* against *theoria,* to be at least 50 per cent false: the motives for building correct time measuring instruments were, and still are, immanent to the scientific development itself. *Cf.* my paper, Du monde de l'à peu près à l'univers de la précision, *Critique,* n. 28, 1946.

[18] *Cf.* its description in H. Diels, *Antike Technik,* 3rd ed., Leipzig, Teubner, 1924.

[19] *Cf.* Galileo Galilei, *Discorsi e dimostrazioni,* 141; English translation, 141.

This great discovery, by the way, has been made by Galileo not by gazing at the oscillations of the great candelabra of the cathedral of Pisa and stating their isochronism by comparing them with the beats of his pulse, as following Viviani is still told in the textbooks,[20] but by extremely ingenious experiments in which he compares the oscillations of two pendula of the same length but with bobs of different matter and thus of different weight (cork and lead),[21] and first and foremost by hard mathematical thinking. Thus says Salviati: [22]

And first, as to the question whether one and the same pendulum really performs its vibrations, large, medium and small, all in exactly the same time, I shall rely upon what I have already heard from our Academician. He has clearly shown that the time of descent is the same along all chords, whatever the arcs which subtend them, as well along an arc of 180° [i.e., the whole diameter] as along one of 100°, 60°, 10°, 2°, ½° or 4′. It is understood, of course, that these arcs all terminate at the lowest point of the circle, where it touches the horizontal plane.

If now we consider descent along arcs instead of chords then, provided these do not exceed 90°, experiment shows that they are all traversed in equal times; but these times are greater for the chord than for the arc, an effect which is all the more remarkable because at first glance one would think just the opposite to be true. For since the terminal points of the two motions are the same and since the straight line included between these two points is the shortest distance between them, it would seem reasonable that the motion along this line should be executed in the shortest time; but this is not the case, for the shortest time—and therefore the most rapid motion—is that employed along the arc of which this straight line is the chord.

As to the times of vibration of bodies suspended by threads of different lengths, they bear to each other the same proportion as the square roots of the lengths of the threads; or one might say the lengths are to each other as the squares of the times; so that if one wishes to make the vibration-time of one pendulum twice that of another, he must make its suspension four times as long. In this manner, if one pendulum has a suspension nine times as long as another, this second pendulum will execute three vibrations during each one of the first; from which it follows that the lengths of the suspending chords bear to each other the (inverse) ratio of the squares of the number of vibrations performed in the same time.

One cannot but admire the depth of the Galilean thinking which shows itself in its very error: the oscillations of the pendulum are of course, not isochronous; and the circle is not the line of the quickest descent; but, to use the terms of the eighteenth century, the "brachistochrone" curve, and the curve upon which oscillations are performed in the same time, or the "tautochrone" one, are recognized by Galileo to be the same line.[23]

It is rather strange that, having discovered the isochronism of the pendulum—the very basis of all modern chronometry—Galileo, though he tried to achieve with its help a time keeper, and even to construct a mechanical pendulum-clock [24]—never used it in his own experiments. It seems that it was Father Mersenne who first got this idea.

Mersenne, as a matter of fact, does not tell us *expressis verbis* that he employed the pendulum as a means of measuring the time of descent of heavy bodies in the experiments he reports about in his *Harmonie Universelle*.[25] But as in the same work he gives a careful description of the motion of the semi-circular pendulum and insists upon the vari-

[20] The famous candelabra was put into the Cathedral of Pisa three years after Galileo's departure from this city; at the time, in which Viviani places the discovery, the cupola of the Pisan Cathedral was still bare and void. *Cf.* E. Wohlwill, Ueber einen Grundfehler aller neueren Galilei-Biographien, *Münchener medizinische Wochenschrift,* 1903, and *Galilei und sein Kampf für die Copernicanische Lehre* **1**, Hamburg und Leipzig, L. Voss, 1909; R. Giacomelli, *Galileo Galilei Giovane e il suo "De Motu."* Quaderni di storia e critica della scienza, **1**, Pisa, 1949.

[21] *Cf. supra,* n. 10.

[22] *Cf.* Galileo Galilei, *Discorsi e dimostrazioni,* 139; English translation, 95.

[23] The times of descent on all the chords being equal and the motion along the (circular) arc being quicker than that along the chord, it was reasonable for Galilei to assume that the descent along the arc was the quickest possible and that the motion of the pendulum was, therefore, isochronous. That it is not the case was discovered experimentally by Mersenne in 1644 (*cf. Cogitata Physico-Mathematica, Phenomena Ballistica,* Parisiis, 1644, propositio XV, septimo, p. 42), and theoretically by Huygens who, in 1659, demonstrated that the "tautochrone" line of descent is the cycloid and not the circle (the same discovery was made independently by Lord Brounker, in 1662). As for the cycloid being at the same time the curve of the quickest descent ("brachistochrone"), this was demonstrated by J. Bernoulli in 1696, and independently—answering the challenge of Bernoulli—by Leibniz, de l'Hopital and Newton.

[24] This clock, or, more exactly, its central regulating mechanism, was constructed by Viviani; *cf. Lettera di Vincenzio Viviani al Principe Leopoldo de' Medici intorno al applicazione del pendolo all' orologio,* in Galileo Galilei, *Opere,* Ed. Naz., **19**: 647 sq., Firenze, 1907; *cf.* equally E. Gerland-F. Traumüller, *Geschichte der physikalischen Experimentierkunst,* 120 sq., Leipzig, W. Engelmann, 1899; L. Defossez, *op. cit.,* 113 sq.

[25] *Cf. Harmonie Universelle* **1**: 132 sq., Paris, 1636.

ous utilizations of the same in medicine (for the determination of the variations in speed of the pulse beats), in astronomy (for the observation of the eclipses of the moon and the sun), etc.,[26] it is practically certain, and moreover confirmed by another passage of the *Harmonie Universelle,* not only that he did use a pendulum but even that this pendulum was three and a half feet long.[27] It is indeed of such a pendulum that, according to Mersenne, the period is exactly equal to one second of the prime mobile.[28]

The results of Mersenne's experiences, "performed more than 50 times," are quite consistent: the falling body traverses 3 feet in half a second, 12 in a second, 48 in two, 108 in three, and 147 in three and a half. Which is nearly twice as much (80 per cent) as it should be according to the figures given by Galileo.

Thus Marsenne writes:[29]

> But concerning the experiences of Galileo, one cannot imagine where the great difference that one finds here in Paris and in the surroundings concerning the times of the falls, which have always appeared to us to be much smaller than his, comes from: not that I should like to reproach such a great man for little care in his experiences, but we have made ours many times from different heights, in the presence of many persons and they always succeeded in the same way. Therefore if the cubit which Galileo has used has only one foot and two thirds, that is twenty inches of the royal foot which we use in Paris, it is certain that the bullet descends more than one hundred cubits in 5 seconds.

Indeed, explains Mersenne, "the hundred cubits of Galileo" are equal to $166\frac{2}{3}$ of "our" feet.[30] But Mersenne's own experiments "repeated more than fifty times" have given quite different results; according to them, in 5″ a heavy body will traverse not 100, but 180 cubits or 300 feet.

Mersenne does not tell us that he has actually dropped heavy bodies from the altitude of 300 feet: it is a conclusion that he draws by applying the "duplicate proportion" to the experimental data at his disposal. Yet as these data "demonstrate" that a heavy body falls three feet in half a second, twelve in a second, forty-eight in two, 108 in three and 147 in three and a half[31]—figures that are in a perfect accord with the duplicate proportion—Mersenne feels entitled, and even bound, to assert that a heavy body will fall $166\frac{2}{3}$ feet in only $3\frac{18}{25}$ seconds, and not in five. Moreover, he adds, from Galileo's figures, it would follow that a

[26] *Ibid.,* 136: "Quoy qu'il en soit, cette manière d'Horologe peut servir aux observations des Eclypses de Soleil, & de la Lune, car l'on peut conter les secondes minutes par les tours de la chorde, tandis que l'autre fera les observations, & marquer combien il y aura de secondes, de la première à la troisiesme observation, etc.

"Les Médecins pourront semblablement user de cette methode pour reconnoitre de combien le poux de leur malades sera plus viste ou plus tardif à diverses heures, et divers iours, et combien les passions de cholere, et les autres le hastent ou le retardent; par exemple qu'il faut une chorde de trois peids de long pour marquer la durée du poux d'aujourd'hui par l'un de ses tours, et qu'il en faille deux, c'est à dire un tour et un retour pour le marquer demain, ou qu'il ne faille plus qu'une chorde longue de 3/4 de pied pour faire un tour en mesme temps que le poux bat une fois, il est certain que le poux bat fois plus viste."

[27] *Ibid.,* 220, Corollaire 9: "Lorsque j'ay dit que la chorde de 3 pieds & demy marque les secondes par les tours ou retours, je n'empesche nullement que l'on n'accourcisse la chorde, si l'on trouve qu'elle soit trop longue, et chacun de ses tours dure un peu trop pour une seconde, comme i'ay quelquefois remarqué, suivant les différentes horologes communes ou faites exprez: par exemple le mesme horloge commun, dont i'ay souvent mesuré l'heure entière avec 3600 tours de la chorde de 3 pieds & demy, n'a pas fait d'autresfois son heure si longue: car il a fallu seulement faire la chorde de 3 pieds pour avoir 900 retours dans l'un des quarts d'heure dudit horologe: et i'ay experimenté sur une monstre à rouë faite exprez pour marquer les seules secondes minutes, que la chorde de 2 pieds & demi ou environ faisoit les tours esgaux ausdites secondes. Ce qui n'empesche nullement la vérité ny la iustesse de nos observations, à raison qu'il suffit de sçavoir que les secondes dont ie parle, sont esgales à la durée des tours de ma chorde de 3 pieds & demy: de sorte que si quelqu'un peut diviser le jour en 24 parties esgales, il verra aisément si ma seconde dure trop, et de combien est trop longue." For his subsequent experiments reported in *Cogitata Physico-Mathematica, Phenomena Ballistica,* 38 sq., Mersenne used a pendulum of three feet only. He had noticed, indeed, that the three and a half feet one was a bit too long, though the difference was practically imperceptible; *cf. Cogitata,* 44.

[28] "One second of the prime mobile" is the time in which the "prime mobile," i.e., the skies, or the earth, describes a rotation of one second.

[29] *Cf. Harmonie Universelle* 1: 138.

[30] As a matter of fact, the foot used by Galileo is shorter—29.57 cm.—than the "royal" foot used by Mersenne—32.87 cm.; the difference of their respective data is therefore even much greater than it is assumed by the latter.

[31] As a matter of fact, Mersenne obtained 110 and not 108 feet on one hand, and 146½ on the other. But Mersenne does not believe in the possibility of reaching exactitude by experiment—considering the means at his disposal, he is perfectly right—and thus assumes that he is entitled to correct the experimental data in order to fit them to the theory. Once more he is perfectly right, as long, of course, as he remains (and he does) on this side of the margin of the experimental errors. Needless to say that Mersenne's procedure has been followed by science ever since. *Cf.* Appendix 2.

heavy body would fall only one cubit in half a second, and four cubits, that is about 6⅔ feet in one second, instead of the twelve feet which it descends in fact.

The results of the Mersennian experiments—the figures obtained by him—of which he is very proud, and of which he makes use for calculating the times with which bodies would fall from all possible altitudes up to the moon and the stars,[32] and the length of all kinds of pendulums with periods up to 30″—constitute, undoubtedly, a progress in respect to those of Galileo. Yet they imply a rather awkward consequence, opposed not only to common sense and the fundamental teachings of mechanics, but also to Mersenne's own calculations: namely that the descent on the periphery of the circle is quicker than that on the perpendicular.[33]

Mersenne seems not to have noticed this consequence (nor did anybody else) at least for several years. In any case he does not mention it before the *Cogitata Physico-Mathematica* of 1644, where, resuming anew the discussion of the law of the fall and of the properties of the pendulum, he states it, though in somewhat attenuated form, together with that of the non-isochronism of the great and small oscillations.[34]

Thus, having explained how strange it is that a three-foot pendulum (which he uses now instead of the three-and-a-half-foot one which he employed formerly) makes his semi-oscillation in exactly half a second (that is, descends three feet), when free-falling bodies traverse twelve feet in a second (that is equally three feet in a semi-second), whereas according to calculations made already in the *Harmonie Universelle* it should traverse in the time of a semi-oscillation 11/7 of the semi-diameter [35] (i.e., 33/7 or 5 feet) he continues,

> this implies a very great difficulty, because both [these facts] have been confirmed by numerous observations, namely that falling bodies traverse on the perpendicular twelve feet only, and that the three feet pendulum descends from *C* to *B* in half a second: which cannot occur but if the globe [of the pendulum] descends from *C* to *B* on the circumference in the same time as a similar globe [falls] on the perpendicular *AB*. Now as this one should descend 5 feet in the time in which the globe comes from *C* to *D*, I do not see any solution.

One could of course assume that bodies fall quicker than it has been admitted: but this would be against all observations. We have therefore, states Mersenne, either to accept that bodies fall on the perpendicular with the same speed as they descend on the circle, or that the air resists more strongly the motion downwards than the oblique one, or finally, that bodies traverse in free fall more than 12 feet in one second, and more than 48 in two, but that, because of the difficulty of ascertaining exactly, by attending to the sound of the percussion of the body on the pavement, the precise moment of this occurrence, all our observations concerning this question are utterly faulty.[36]

It must have been rather hard for Mersenne to admit the fallaciousness of his, so carefully made, experiments and the meaninglessness of the long calculations and tables based upon them. Yet it was unavoidable. Once more he had to recognize that precision couldn't be achieved in science and that its results were only approximately valid. Thus it is not surprising that in his *Reflexiones Physico-Mathematicae* of 1647 he tries in the same time to perfect his experimental methods—thus by holding the bob of the pendulum and the descending body (similar leaden spheres) in one and the same hand in order to insure the simultaneity of the beginning of their motions,[37] and by fixing his pendulum to a wall in order to insure the simultaneity of the end of these motions by the merging together of the two sounds produced by the hit of the pendulum upon the wall and by that of the falling body upon the ground; and to explain, at a considerable length, the lack of certainty of the results,[38] which, by the way, confirm those of his former investigations: the body seems to fall 48 feet in about 2″, and 12 in 1″. Yet, insists Mersenne, it is impossible to determine exactly the length of the pendulum of which the period would be precisely a second, nor is it possible to perceive, by hearing, the exact coincidence of the two sounds. A couple of inches or even feet more or

[32] *Cf. ibid.,* 140. In his calculations, Mersenne assumes—as Galileo—that the value of the acceleration is a universal constant.

[33] The ball descends on the quadrant of the circle as quickly as on the radius, if this radius is equal to 3 feet, or even quicker if the radius is equal to 3½ feet.

[34] *Cf. Cogitata Physico-Mathematica, Phenomena Ballistica,* 38 and 39; see Appendix 3.

[35] *Cf. ibid.,* 41.

[36] It is interesting to note that in his experiments, Mersenne determines the moment of arrival of the falling body to the earth not by sight, but by hearing; the same method will be followed by Huygens, doubtlessly under Mersenne's influence.

[37] *Cf. Reflexiones Physico-Mathematicae* **18**: 152 sq., Parisiis, 1647.

[38] *Cf. ibid.* **19**: 155: *De variis difficultatibus ad funependulum et casum gravium pertinentibus.*

less does not make any difference. Thus, he concludes, we have to content ourselves with approximations and not ask for more.

Nearly at the same time in which F. M. Mersenne performed his experiments, another experimental research of the laws of fall, linked together with an experimental determination of the value of g, was made in Italy by a team of Jesuit scientists led by the famous author of the *Almagestum Novum,* R. P. Giambattista Riccioli,[39] who, strangely enough, was perfectly independent from, and even wholly ignorant of, the work of Mersenne.

Riccioli has a rather bad reputation with the historians of science—a reputation not quite merited.[40] Yet one must confess that he is not only a much better experimenter than F. M. Mersenne, but even a much more intelligent one, and that he has an infinitely deeper understanding of the value and meaning of precision than the friend of Descartes and Pascal.

It was in 1640, when he was professor of philosophy in the *Studium* of Bologna, that Riccioli started a series of investigations of which I shall give here a brief account,[41] and I would like to stress the carefully thought out and methodical way in which he proceeds with his work. He does not want to take anything for granted and, though, as a matter of fact, he is firmly convinced of the value of Galileo's deductions, he first tries to establish, or better to say, to verify, whether the thesis of the isochronism of pendular oscillations is exact; then, whether the relation asserted by Galileo between the length of the pendulum and its period (period proportional to the square root of the length) is confirmed by experience; finally, to determine, as precisely as possible, the period of a given pendulum in order to obtain in this way a time-measuring instrument fit to be used for the experimental research of the speed of fall.

Riccioli starts by preparing a convenient pendulum: a spherical metallic bob suspended to a chain [42] attached to a metallic cylinder turning freely in two, equally metallic, sockets. A first series of experiments aims at the verification of the Gallilean assertion of the constancy of the period of the pendulum by counting the number of its oscillations in a given time. The time is measured by the means of a water-glass and Riccioli, revealing a deep understanding of the empirical conditions of experimentations and measurement, explains that it is the double process of running out and filling again of the water-glass that is to be taken as the unit of time. The results of this first series confirm the assertion of Galileo.

A second series of experiments for which Riccioli uses two pendulums, of the same weight, but of different length ("height"), namely of one and of two feet, confirms the square root relation established by Galileo: the number of oscillations in the unit of time is, respectively, 85 and 60.[43]

Mersenne would probably stop at this point. Not Riccioli; he understands quite well that even by using his method of turning the water-glass upside down one is still far away from real precision: for this we have still to look elsewhere, that is to the skies, to the only really exact *horologium* existing in this world, to the *organa chronou* provided by nature, the motions of the heavenly bodies and spheres.

Riccioli realizes full well the tremendous importance of the Galilean discovery: the isochronism of the pendulum enables us to achieve a *precise* time keeper. Indeed, the fact that large and small oscillations are performed in the same time entails the possibility of maintaining its motion as long as we want by counteracting its normal and spontaneous slowing down; for instance, by giving it a new push after a certain amount of beats; [44] thus

[39] The report on these experiments is included in the *Almagestum Novum, Astronomiam veterem novamque complectens observationibus aliorum et propriis, Novisque Theorematibus, Problematibus ac Tabulis promotam . . . auctore P. Johanne Baptista Riccioli Societatis Jesu . . . ,* Bononiae, 1651. The work had to have three volumes, but only the first one, in two parts, has been published. This "first volume" is, indeed, 1504 pages long (in folio).

[40] Riccioli is, of course, an anti-Copernican and, in his great works—*Almagestum Novum* of 1651 and *Astronomia Reformata* of 1665—he heaps arguments upon arguments in order to refute Copernicus, which is, indeed, regrettable, but after all rather natural for a Jesuit. On the other hand, he does not hide his great admiration for Copernicus and Kepler and gives a surprisingly correct and honest account of the astronomical theories he is criticizing. He is immensely learned and his works, especially the *Almagestum Novum,* are an invaluable source of information. This makes his ignorance of the works of Mersenne so much more surprising.

[41] *Cf. Almagestum Novum* **1** (1), bk. II, ch. XX and XXI: 84 sq. and **1** (2), bk. IX, sect. IV, 2: 384 sq. I presented a report on the experiments of Riccioli to the *XXII Congrès international de Philosophie des Sciences* which met in Paris, in 1949.

[42] *Cf. Almagestum Novum* **1** (1), bk. II, ch. XX: 84,

[43] *Cf. ibid.,* ch. XXI, prop. VIII: 86.

[44] This pushing of the pendulum is by no means easy and implies a long training.

any number of atoms of time can be accumulated and added together.

It is clear, however, that in order to be able to use the pendulum as a *precise* instrument for measuring time, we have to determine *exactly* the value of its period. This is the task to which, with an unyielding patience, Riccioli will devote himself. His aim is to manufacture a pendulum of which the period would be exactly one second.[45] Alas, in spite of all efforts he will not be able to reach his goal.

To begin with he takes a pendulum weighing about one pound and three feet and four inches (Roman) [46] "high." The comparison with the water-glass has been satisfactory: nine hundred oscillations in a quarter of an hour. Riccioli proceeds, then, to a verification by the means of a sundial. For six consecutive hours, from nine o'clock in the morning to three o'clock in the afternoon, he counts (he is aided by the R. P. Francesco Maria Grimaldi) the oscillations. The result is disastrous: 21,706 oscillations instead of 21,660. Moreover, Riccioli recognizes that for his aim the sundial itself lacks the wanted precision. Another pendulum is prepared and "with the aid of nine Jesuit fathers," [47] he starts counting anew; this time—the second of April 1642—for twenty-four consecutive hours, from noon to noon: the result is 87,998 oscillations whereas the solar day contains only 86,640 seconds.

Riccioli makes then a third pendulum, lengthening the suspension chain to 3 feet, 4.2 inches. And, in order to increase the precision even more, he decides to take as a unit of time not the solar, but the sidereal day. The count goes on from the passage through the meridian line of the tail of the Lion (the twelfth of May 1642) till its next passage on the thirteenth. Once more a failure: 86,999 oscillations instead of 86,400 that there should have been.

Disappointed yet still unbeaten, Riccioli decides to make a fourth trial, with a fourth pendulum, somewhat shorter this time, of 3 feet, and 2.67 inches only.[48] But he cannot impose upon his nine companions the dreary and wearisome task of counting the swings. Father Zeno and Father F. M. Grimaldi alone remain faithful to him to the end. Three times, three nights, the nineteenth and the twenty-eighth of May and the second of June 1645, they count the vibrations from the passage through the meridian line of the Spica (of Virgo) to that of Arcturus. The numbers are twice 3,212 and the third time 3,214 for 3,192 seconds.[49]

At this point Riccioli seems to have had enough of it. After all, his pendulum, the period of which is equal to 59.36‴, is a perfectly usable instrument. The transformation of the number of oscillations into seconds is easy. Besides, it can be facilitated by precalculated tables.[50]

Still, Riccioli is rather worried about his lack of success. He tries, therefore, to calculate the "height" of a pendulum which would swing in exactly a second: arriving at the result that it should be 3 feet, 3.27 inches.[51] He confesses, however, not having actually made it. On the other hand he has certainly manufactured much shorter pendulums in order to achieve a greater refinement in measuring time intervals: one of 9.76 inches with the period of 30‴; another, still shorter, of 1.15 inches of which the period is only 10‴.

"It is such a pendulum that I employed," says Riccioli, "for measuring the speed of the natural descent of heavy bodies" in the experiments performed in this same year 1645 at the Torre degli Asinelli, in Bologna.[52]

Now it is obviously impossible to use so rapid a pendulum simply by counting its swings; one has to find out some means of summing them up. In other words, one has to construct a clock. Actually it is a clock, the first pendulum clock, that Riccioli has built for his experiments. Yet it would be difficult to consider him a great clockmaker, a forerunner of Huygens and Hooke. His clock, indeed, had neither weight nor spring, nor even hands or dial. As a matter of fact, it was not a mechanical clock, but a human one that he built.

In order to sum up the beats of his pendulum

[45] Riccioli, as we shall see, is not as easily satisfied as Mersenne.

[46] A Roman foot is equal to 29.57 cm.

[47] *Cf. Almagestum Novum, loc. cit.*, 86. The names of these fathers ought to be preserved as examples of devotion to science; here they are (*cf.* 1 (2): 386): Stephanus Ghisonus, Camillus Rodengus, Jacobus Maria Palavacinus, Franciscus Maria Grimaldus, Vicentius Maria Grimaldus, Franciscus Zenus, Paulus Casarus, Franciscus Adurnus, Octavius Rubens.

[48] *Cf. ibid.*, 87.

[49] *Cf. ibid.*, 85. As the motion of the pendulum is not isochronous the exquisite concordance of the results of Riccioli's experiments can be explained only if we assume that he made his pendulums perform practically equal and *small* oscillations.

[50] Riccioli gives these tables in the *Almagestum Novum* 1 (1): bk. 2, ch. XX, prop. XI: 387.

[51] *Cf. ibid.* and 1 (2): 384.

[52] *Cf. ibid.* 1 (1): 87.

Riccioli imagined a very simple, and a very elegant device: he trained two of his collaborators and friends, "gifted not only for physics but also for music, to count *un, de, tre* . . . (in the Bolognese dialect in which these words are shorter than in Italian) in a perfectly regular and uniform way, as are wont to do those who direct the execution of musical pieces, in such a way that to the pronunciation of each figure corresponded an oscillation of the pendulum." [53] It is with this "clock" that he performed his observations and experiments.

The first question studied by Riccioli concerned the behavior of "light" and "heavy" bodies.[54] Do they fall with the same, or with different, speeds? A very important, and very controversial, question, to which, as we know, ancient and modern physics gave different answers. Whereas the Aristotelians maintained that bodies fall so much quicker as they are heavier, Benedetti had taught that all bodies, at least all bodies possessing an identical nature, i.e., specific gravity, fell with the same speed. As for the moderns, such as Galileo and Baliani, followed by the Jesuits Vendelinus and N. Cabeo, they asserted that all bodies, whichever their nature or weight, fell always with the same identical speed (in the *vacuum*).[55]

Riccioli wants to settle this problem once and forever. Thus on the fourth of August 1645 he proceeds to work. Spheres of equal size but of different weight, made, respectively, of clay and of paper, covered with chalk (this is in order to make their motion along the wall, as well as their bursting when reaching the pavement, easier to observe), were dropped from the summit of the Torre dei Asinelli, particularly convenient for this kind of experiment [56] and sufficiently high—312 Roman feet—to make such differences in speed perceptible in their effects. The results of the experiments, which Riccioli repeats fifteen times, are indubitable: heavy bodies fall quicker than light ones. Yet their lagging behind, which, depending on the weight and the dimension of the balls varies from 12 to 40 feet, does not contradict the theory developed by Galileo: it is to be explained by the resistance of the air and has been foreseen by him. On the other hand the observed facts are perfectly incompatible with the teachings of Aristotle.[57]

Riccioli is intensely conscious of the originality and of the value of his work. Accordingly he pokes fun at the "semi-empiricists" who don't know how to make a really conclusive experiment and who, for instance, assert—or deny—that bodies fall with the same speed because they are unable to determine the precise moment when the body strikes the pavement.[58]

The second problem investigated by Riccioli is even more important. He wants to ascertain the proportion with which the falling body accelerates its motion. It is, as it is taught by Galileo, a "uniformly difform" (uniformly accelerated) motion, that is a motion in which the spaces traversed are *ut numeri impares ab unitate* or, as Baliani wants it, a motion in which these spaces are a series of natural numbers? As for the speed, is it proportional to the duration of the fall, or to the space traversed.[59]

Aided by R. P. Grimaldi, Riccioli manufactures a number of balls made of chalk, of identical dimensions and weight, and, after having established by direct measurement of the times of their falling from different stories of the Torre dei Asinelli that they follow the Galilean law,[60] he proceeds to the verification of this result (nothing is more characteristic than this inversion of the procedure) by dropping these balls from previously calculated, determined, altitudes, using to this purpose all the churches and towers of Bologna of which the heights are appropriate, namely, those of St. Peter, St. Petronio, St. James, and St. Francis.[61]

The results are concordant in all details. Indeed their accord is so perfect, the spaces traversed by the balls (15, 60, 135, 240 feet) confirm the Galilean law in so rigorous a manner that it is quite obvious that the experimenters have been convinced of its truth before starting. Which, after all, is not surprising, as the experiments with

[53] *Cf. ibid.* **1** (2) : 384.

[54] Riccioli, a hundred years behind his time, still believes in "lightness" as an independent quality correlated with, and opposed to, "heaviness."

[55] *Cf. ibid.*, 387.

[56] The Torre degli Asinelli possess vertical walls and stands on a rather large and flat platform.

[57] *Cf. ibid.*, 388.

[58] *Cf. ibid.* and **1** (1) : 87.

[59] *Cf. ibid.* It is interesting to note that Riccioli uses the old scholastic terminology and, quite correctly, identifies the "uniformly difform" (*uniformiter difformis*) motion with the uniformly accelerated (or retarded) one.

[60] He tells us, indeed, that he thought about the problem since 1629 and adopted the relation 1, 3, 9, 27, before having read Galileo in 1634, having been allowed to do so by his superiors. It is interesting to note that before having read Galileo the very learned Riccioli did not identify the *uniformiter difformis* motion with that of the fall.

[61] *Cf. ibid.*, 387. The experiments were continued from 1640 to 1650.

the pendulum have already given to it a full confirmation.

Yet even if we admit—as we must—that the good fathers corrected somewhat the actual results of their measurements, we have nevertheless to acknowledge that these results are of a quite surprising precision. Compared to the rough approximations of Galileo himself, and even to those of Mersenne, they constitute a decisive progress. They are certainly the best ones that could be obtained by direct observation and measurement and one cannot but admire the patience, the conscience, the energy, and the passion for truth of the R. P. Zeno, Grimaldi, and Riccioli (as well as of their collaborators) who, without any other instrument for measuring time than the human clock into which they transformed themselves, were able to determine the value of the acceleration, or, more exactly, the length of the space traversed by a heavy body in the first second of its free fall through the air, as being equal to 15 (Roman) feet. A value which Huygens alone, by using the mechanical clock invented by him, or better to say, by applying indirect methods which his mathematical genius enabled him to discover and to use in the very construction of his clock, will be in a position to improve.

It is very interesting, and very instructive, to study the *modi procedendi* of the great Dutch scientist to whom we owe our watches and clocks. Their analysis enables us to witness the transformation of the still empirical or semi-empirical experiences of Mersenne and Riccioli into a truly scientific experiment; it imparts to us, too, a very important lesson, namely, that in scientific investigations the direct approach is by no means the best nor the easiest one, and that empirical facts are to be reached only by using a theoretical circuit.

Huygens starts his work by repeating, in 1659 (the twenty-first of October), the (last) experiments of Mersenne, as described by the latter in his *Reflexiones* of 1647; and once more we are obliged to stress the appalling poverty of the experimental means at his disposal: a string-pendulum attached to the wall; its bob, a leaden ball, and another, similar, leaden, globe are held in the same hand; the simultaneity of the arrival of the two globes, respectively, to the wall and to the ground is determined by the coincidence of the two sounds produced by the hits. Strangely enough, using exactly the same procedure as Mersenne, Huygens obtains better results; according to him, the body falls 14 feet.[62]

On the 23rd of October 1659, Huygens repeats the experiment, using this time a pendulum the semi-vibration of which is equal not to a half second, but to three-quarters of it. During this time the leaden sphere falls 7 feet, 8 inches. It follows that in one second it would fall about 13 feet, 7½ inches.[63]

On the fifteenth of November, 1659, Huygens makes a third trial. This time he improves somewhat his procedure by attaching both bob and the leaden sphere to a thread (instead of holding them in the same hand) by the cutting of which they are released. Moreover, he puts parchment on the wall and the ground to make the perception of the sounds more distinct. The result is about 8 feet, 9½ inches. Yet, just as Mersenne before him, Huygens is obliged to admit that his result is valid only as an approximation, because three or even four inches more or less in the height of the fall cannot be distinguished by the means employed by him: the sounds seem to coincide. It follows, therefore, that an exact measure cannot be obtained in this way. But the conclusion he draws therefrom is by no means the same. Quite on the contrary. Whereas Mersenne renounces the very idea of scientific precision, Huygens reduces the role of the experiment to that of verification of theoretically arrived at results. It is enough when it does not contradict them, as in this case where the observed figures are perfectly compatible with those deduced from the analysis

[62] *Cf.* Ch. Huygens, *Œuvres* **17**: 278, La Haye, M. Nijhof, 1932: "II D. 1. Expertus 21 Oct. 1659. Semisecundo minuto cadit plumbum ex altitudine 3 pedum et dimidij vel 7 pollicum circiter. Ergo unius secundi spatio ex 14 pedem altitudine."

[63] *Cf.* Huygens, *Œuvres* **17**: 278. "II D. 2. Expertus denuo 23 Oct. 1659. Pendulum adhibui cujus singulae vibrationes 3/2 secundi unius. unde semivibratio qua usus sum erat 3/4." Erat penduli longitudo circiter 6 p. 11 unc. Sed vibrationes non ex hac longitudine sed conferendo eas cum pendulo horologij colligebam. Illius itaque semivibratione cadebat aliud plumbum simul e digitis demissum ex altitudine 7 pedum 8 unc. Ergo colligitur hinc uno secundo casurum ex altitudine 13 ped. 7 1/2 unc. ferè.

"Ergo in priori experimento debuissent fuisse non toti 3 ped. 5 poll.

"Sumam autem uno secundo descendere plumbum pedibus 13. unc. 8. Mersenne 12 ped. paris. uno secundo confici scribit. 12 ped. 8 unc. Rhijnland. Ergo Mersenni spatium justo brevius est uno pede Rhijnl.

A Rheinland foot is equal to 31.39 cm.

of the motion of the circular pendulum, i.e., about 15 feet, 7½ inches per second.[64]

As a matter of fact, the analysis of the pendular motion gives, as we shall see, even better results.

I have already mentioned the paradoxical situation of modern science at the time of its birth: possession of exact mathematical laws combined with the impossibility of their application because of the inability of performing a precise measurement of the fundamental magnitude of dynamics, that is, of time.

Nobody seems to have felt it more strongly than Huygens, and it is certainly for that reason, and not for practical considerations such as the necessity of good clocks for navigation—though he, by no means, neglected the practical aspect of the question [65]—that, at the very beginning of his scientific career, he applied himself to the solution of this fundamental and preliminary problem: the perfecting, or better, the building of a perfect time-keeper.

It is in the year 1659, the same year in which he made the measurements I have just reported, that he reached his goal by constructing an improved pendulum clock; [66] a clock which he used in order to determine the exact value of the oscillation of the pendulum that he had employed in his experiments.

In the history of the scientific instruments Huygens' clock occupies a very important position: it is the first apparatus that embodies in its construction the laws of the new dynamics; it is the result not of empirical trial and error, but of careful and subtle theoretical investigation of the mathematical structure of circular and oscillatory motions. Thus the very history of the pendulum-clock gives us a good example of the value of the roundabout way in preference to the direct one.

Huygens, indeed, is perfectly aware that, as already discovered by Mersenne, small and large oscillations of the pendulum are not performed in the same time. In order to construct a perfect time-keeper one has, therefore, (*a*) to determine the truly isochronous curve, and (*b*) to find out the means to make the bob of the pendulum to move along this line and not along the periphery of the circle. It is well known that Huygens succeeded in solving both problems (though for doing it he had to devise a completely new geometrical theory),[67] and to achieve a perfectly isochronous motion, the motion along the cycloid; moreover, that he succeeded in fitting his cycloidal pendulum into a clock.[68]

He was now in position to proceed, with an infinitely better equipment—a mechanical clock instead of a human one—and therefore a much better chance of reaching precision, to experiments on the line of those of Riccioli. Yet he never tried to perform them. This because the construction of the pendulum-clock put into his hands a much better procedure.

As a matter of fact, he had not only discovered the isochronism of the motion along the cycloid, but also—something that Mersenne had tried (but failed) to find out for the circle—the relation between the time of descent of a body along the cycloid to that of its fall along the diameter of its

[64] *Cf. ibid.*; 281: "II D. 4. 15 Nov. 1659. Pendulum AB semivibrationi impendebat 3/4 unius secundi; filum idem BDC plumbum B et glandem C retinebat, deinde forficubus filum incidebatur, unde necessario eodem temporis articulo globulus C et pendulum moveri incipiebant. plumbum B in F palimsesto impingebatur, ut clarum sonum excitaret. globulus in fundum capsae GH decidebat. simul autem sonabant, cum CE altitudo erat 8 pedum et 9 1/2 unciarum circiter. Sed etsi 3 quatuorve uncijs augeretur vel diminueretur altitudo CE nihilo minus simul sonare videbantur. adeo ut exacta mensura hoc pacto obtineri nequeat. At ex motu conico penduli debebant esse ipsi 8 pedes et 9 1/2 uniciae. unde uno secundo debebunt peragi a plumbo cadente pedes 15. unc. 7 1/2 proxime. Sufficit quod experientia huic mensurae non repugnet, sed quatenus potest eam comprobet. Si plumbum B et globulum C inter digitos simul contineas ijsque apertis simul dimittere coneris, nequaquam hoc assequeris, ideoque tali experimento ne credas. Mihi semper hac ratione minus inveniebatur spatium CE, adeo ut totius interdum pedis differentia esset. At cum filum secatur nullus potest error esse, dummodo forfices ante sectionem immotae teneantur. Penduli AB oscillationes ante exploraveram quanti temporis essent ope horologij nostri. Experimentum crebro repetebam. Ricciolus Almag. 1. 9 secundo scrupulo 15 pedes transire gravia statuit ex suis experimentis. Romanos nimirum antiquos quos a Rhenolandicis non differre Snellius probat."

[65] Member of a maritime nation, Huygens was fully aware of the value and importance of a good timekeeper for navigation, as well as of the financial possibilities of the invention of a marine clock. It is well known that he tried to have his clock patented in England. *Cf.* L. Defossez, *op. cit.,* 115 sq.

[66] The first pendulum clock was constructed by Huygens in 1657; it contains already curved jaws ensuring the isochronism of the (flexible) pendulum. Yet these jaws were not yet mathematically determined, but formed only on the basis of empirical trial and error procedure. It was only in 1659 that Huygens discovered the isochronism of the cycloid and the means of making the bob of the pendulum move along a cycloid.

[67] That of the evolutes of geometrical curves.

[68] *Cf.* L. Defossez, *op. cit.,* 65; on the contemporary attempts of R. Hooke, *cf.* Louise D. Patterson, Pendulums of Wren and Hooke, *Osiris* **10**: 277–322, 1952.

generating circle: these times are to each other as the semi-circumference is to the diameter.[69]

Thus, if we could make a (cycloidal) pendulum, swinging in precisely one second, we would be able to determine the exact time in which a heavy body would descend along its diameter, and therefrom—the spaces traversed being proportional to the squares of time—to calculate the distance of its fall in one second.

The length of such a pendulum—which, besides, needn't be a cycloidal one, because, as Huygens will point it out to Moray,[70] small oscillations of a common (perpendicular) pendulum are performed in practically the same amount of time as those of the cycloidal one—can be easily calculated as soon as we have succeeded in determining the period of a given cycloidal one.

But, as a matter of fact, we do not need to bother ourselves with the actual fabrication of such a pendulum. This because the formula devised by Huygens,

$$g = \frac{4\pi^2 r^2 l}{3600^2} \quad \text{or} \quad T = \pi\sqrt{\frac{l}{g}}$$

has a general value and determines the value of g as a function of the length and the speed of whatever pendulum we may be using. Indeed, it is a rather short and quick pendulum that Huygens has used, a pendulum only 6.18 inches long, and making 4,464 double oscillations per hour. Accordingly, Huygens concluded that the value of g is 31.25 feet, i.e., 981 cm., which is the value that has been accepted ever since.[71]

The moral of this history of the determination of the acceleration constant is thus rather curious. We have seen Galileo, Mersenne, Riccioli endeavoring to construct a time-keeper in order to be able to make an experimental measure of the speed of the fall. We have seen Huygens succeed, where his predecessors had failed, and, by his very success, dispense with making the actual measurement. This, because his timekeeper is, so to say, a measurement in itself; the determination of its exact period is already a much more precise and refined experiment than all those that Mersenne and Riccioli have ever thought of. The meaning and value of the Huygensian circuit—which finally revealed itself as a shortcut—is therefore clear: not only are good experiments based upon theory, but even the means to perform them are nothing else than theory incarnate.

APPENDIX

1. M. Mersenne, *Harmonie Universelle,* Paris, 1636, p. 111 sq.:

Or il faut icy mettre les experiences que nous avons faites très exactement sur ce suiet, afin que l'on puisse suivre ce qu'elles donnent. Ayant donc choisi une hauteur de cinq pieds de Roy, et ayant fait creuser, et polir un plan, nous luy avons donné plusieurs sortes d'inclinations, afin de laisser rouler une boule de plomb, et de bois fort ronde tout au long du plan: ce que nous avons fait de plusieurs endroits différents suivant les différentes inclinations, tandis qu'une autre boule de mesme figure, et pesanteur tombait de cinq pieds de haut dans l'air; et nous avons trouvé que tandis qu'elle tombe perpendiculairement de cinq pieds de haut, elle tombe seulement d'un pied sur le plan incliné de quinze degrez, au lieu qu'elle devroit tomber seize poulces.

Sur le plan incliné de vingt cinq degrez le boulet tombe un pied 5 demi, il devroit tomber deux pieds, un pouce un tiers: sur celuy de trente degrez il tombe deux pieds: il devroit tomber deux pieds et 1/23 car il feroit six pieds dans l'air, tandis qu'il tombe deux pieds 1/2 sur le plan, au lieu qu'il ne devroit tomber que cinq pieds. Sur le plan incliné de 40 degrez, il devroit tomber trois pieds deux pouces 1/2: et l'experience très exacte ne donne que deux pieds, neuf pouces, car lorsqu'on met le boulet à deux pieds dix

[69] *Cf.* Ch. Huygens, *De vi centrifuga* of 1659, *Œuvres* **16**: 276, La Haye, M. Nijhof, 1929.

[70] *Cf.* Christiaan Huygens, Lettre à R. Moray 30 decembre 1661, *Œuvres complêtes,* publiées par la Société hollandaise des sciences **3**: 438; La Haye, M. Nijhof, 1890: "Je ne trouve pas qu'il soit nécessaire d'égaler le mouvement du pendule par les portions de le Cycloide pour determiner cette mesure, mais qu'il suffit de le faire mouvoir par des vibrations fort petites, lesquelles gardent assez près l'églite des temps, et chercher aussi quelle longueur il faut pour marquer, par exemple, une demie seconde par le moyen d'une horologe qui soit déjà en train de bien aller, et ajustée avec la Cycloide.

[71] *Cf.* Ch. Huygens, *Œuvres* **17**: 100: "Het getal van de dobbele slaegen die het pendulum in een uyr doen moet, gegeven sijnde, quadreert het selve, en met het quadraat divideert daer mede 12312000000. ende de quotiens sal aenwijsen de lenghde van het pendulum. te weten als men de twee laetste cijffers daer af snijt, soo is het resterende het getal der duijmen die het pendulum moet hebben; de 2 afgesnedene cijffers beteijckenen, het een, de tienden deelen van een duijm die daer noch bij moeten gedaen werden, het ander, de 100ste deelen van een duym, van gelijcken daer bijte doen. Rhynlandse maet.

"bij exempel Een horologe te maecken sijnde diens pendulum 4464 dobbele slagen in een uijr doen sal, het quadraet van 4464 is 19927296, waer mede gedeelt sijnde 12312000000, komt 6/18 ontrent. dat is 6 duijm 1/10 en 8/100 van een duijm. Indien het getal van de heele duijmen meer is als 12 soo moet het door 12 gedeelt werden om te weten hoe veel heele voeten daer in sijn."

pouces loin de l'extrémité du plan le boulet qui se meut perpendiculairement chet le premier; et quand on l'éloigne de deux pieds huit pouces sur le plan, il tombe le dernier: et lorsqu'on l'éloigne de deux pieds neuf pouces, ils tombent instement en mesme temps, sans que l'on puisse distinguer leur bruits.

Sur le plan de quarante cinq degrez il devroit tomber trois pieds et 1/2 un peu davantage, mais il ne tombe que trois pieds, et ne tombera point trois pieds 1/2, si l'autre ne tombe cinq pieds 3/4 par l'air.

Sur le plan de cinquante degrez il devroit faire trois pieds dix pouces, il n'en fait que deux et neuf pouces: ce que nous avons repeté plusieurs fois très exactement, de peur d'avoir failly, à raison qu'il tombe en mesme temps de 3 pieds, c'est à dire de 3 pouces davantage sur le plan incliné de 45 degrez: ce qui semble fort estrange, puisqu'il doit tomber dautant plus viste que le plan est plus incliné: Et néanmoins il ne va pas plus viste sur le plan de 50 degrez que sur celuy de 40: où il faut remarquer que ces deux inclinations sont également éloignées de celle de 45 degrez, laquelle tient le milieu entre les deux extremes, à sçavoir entre l'inclination infinie faite dans la ligne perpendiculaire et celle de l'horizontale: toutefois si l'on considère cet effet prodigieux, l'on peut dire qu'il arrive à cause que le mouvement du boulet estant trop violent dans l'inclination de 50 degrez, ne peut rouler et couler sur le plan, qui le fait sauter plusieurs fois: dont il s'ensuit autant de repos que de sauts, pendant lesquels le boulet qui chet perpendiculairement, avance toujours son chemin: mais ces sauts n'arrivent pas dans l'inclination de 40, et ne commencent qu'après celle de 45, insques à laquelle la vitesse du boulet s'augmente toujours de telle sorte qu'il peut toujours rouler sans sauter: or tandis qu'il fait trois pieds dix pouces sur le plan incliné de cinquante degrez, il en fait six 1/2 dans l'air au lieu qu'il n'en devroit faire que cinq.

Nous avons aussi experimenté que tandis que la boule fait 3 pieds 10 pouces sur le plan incliné de 50 degrez, elle fait 6 pieds 1/2 par l'air, combien qu'elle ne denst faire que cinq pieds. A l'inclination de 40, elle fait quasi 7 pieds dans l'air, pendant qu'elle fait 3 pieds 2 pouces 1/2 sur le plan; mais l'expérience reiteree à l'inclination de 50, elle fait 3 pieds sur le plan, quoy que la mesme chose arrive à 2 pieds 9 pouces: ce qui monstre la grande difficulté des experiences; car il est très difficile d'appercevoir lequel tombe le premier des deux boulets dont l'un tombe perpendiculairement, et l'autre sur le plan incliné. J'ajoûte néanmoins le reste de nos experiences sur les plan inclinez de 60 et de 65 degrez: le boulet éloigné de l'extremité du plan de 2 pieds, 9 pouces, ou de 3 pieds, tombe en mesme temps que celuy qui chet de cinq pieds de haut perpendiculairement, et néanmoins il devroit cheoir 4 pieds 1/3 sur le plan de 60, et 4 pieds 1/2 sur celuy de 65. Sur le plan de 75 il devroit faire 4 pieds 10 pouces, et l'experience ne donne que 3 pieds 1/2.

Peut estre que si les plans ne donnoient point plus d'empeschement aux mobiles que l'air, qu'ils ne tomberoient suivant les proportions que nous avons expliqué: mais les experiences ne nous donnent rien d'asseuré, particulièrement aux inclinations qui passent 45 degrez, parce que le chemin que fait le boulet à cette inclination, est quasi égal à celuy qu'il fait sur les plans de 50, 60, et 65; et sur celuy de 75 il ne fait que demi pied davantage.

F. Mersenne allows himself even to doubt the actual performance by Galileo of some of the experiments mentioned by the great scientist. Thus, referring to the experiments on the inclined plane described by Galileo in his *Dialogo* (not to those described in the *Discorsi,* which I have quoted), he writes (*Harmonie Universelle,* 112, corr. I):

Je doute que le sieur Galilée ayt fait les experiences des cheutes sur le plan puisqu'il n'en parle nullement, et que la proportion qui donne contredit souvent l'experience: et desire que plusieurs esprouvent la mesme chose sur des plans differens avec toutes les précautions dont ils pourront s'aviser, afin qu'ils voyent si leurs experiences respondront aux notres, et si l'on en pourra tirer assez de lumiere pour faire un Theoreme en faveur de la vitesse de ces cheutes obliques, dont les vitresses pourroient estre mesurees par les differens effets du poids, qui frappera dautant plus fort que le plan sera moins incliné sur l'horizon, et qu'il approchera davantage de la ligne perpendiculaire.

2. *Ibid.* 138:

Mais quant à l'expérience de Galilée, on ne peut ni imaginer d'où vient la grande différence qui se trouve icy à Paris et aux environs, touchant le tems des cheutes, qui nous a toujours paru beaucoup moindre que le sien: ce n'est pas que je veuille reprendre un si grand homme de peu de soin en ses expériences, mais on les a faites plusieurs fois de différentes hauteurs, en présence de plusieurs personnes, et elles ont toujours succédé de la mesme sorte. C'est pourquoy si la brasse dont Galilée s'est servy n'a qu'un pied et deux tiers, c'est à dire vingt poulces de pied du Roy dont on use à Paris, il est certain que le boulet descend plus de cent brasses en 5″.

Cecy étant posé, les cent brasses de Galilée font 166 2/3 de nos pieds, mais nos expériences répétées plus de cinquante fois, jointes à la raison doublée, nous contraignent de dire que le boulet fait 300 pieds en 5″, c'est à dire 180 brasses, ou quasi deux fois davantage qu'il ne met: de sorte qu'il doit faire les cent brasses, ou 166 pieds 2/3 en 3″ et 18/25, qui font 3″, 43‴, 20⁗, et non pas 5″; car nous avons prouvé qu'un globe de plomb pesant environ une demie livre, et que celuy de bois pesant environ une once tombent de 48 pieds en 2″, de 108 en 3″, et de 147 pieds en 3″ et ½. Or les 147 pieds reviennent à 88 et 1/5 brasses; et s'il se trouve du mesconte, il vient plustot de ce que nous donnons trop peu d'espace aux dits temps, qu'au contraire, car ayant laissé cheoir le poids de 110 pieds, il est iustement tombé en 3″, mais nous prenons 108 pour régler la proportion; et les hommes ne peuvent observer la différence du temps auquel il tombe de 110, ou de 108 pieds. Quant à la hauteur de 147 pieds, il s'en fallait un demi-pied, ce qui rend la raison double très-iuste, d'autant que le poids doit faire 3 pieds en une demie seconde, suivant cette vistesse, 12 pieds dans une seconde minute; et conséquemment, 27 pieds en 1″ et ½, 48 pieds en 2″, 75 en 2″ et ½, 108 pieds en 3″ et 147 pieds en 3″ et ½, ce qui revient fort bien à nos experiences, suivant lesquelles il tombera 192 pieds en 4″ et 300 en 5″, pendant lequel Galilée ne met que 166 pieds ou 100 brasses, selon lesquelles il doit faire une brasse en une demie seconde, 4 en 1″, ce qui font près de 6 pieds 2/3, au lieu de 12 que le poids descend en effet.

3. F. M. Mersennus, *Cogitata physico-mathematica,* Phenomena ballistica, Parisii, 1644. Propositio XV. Grauium cadentium velocitatem in ratione duplicata temporum augeri probatur ex pendulis circulariter motis, ipsorúmque pendulorum multifarius usus explicatur, 38–44.

Certum est secundò filum à puncto C ad B cadens temporis insumere tantundem in illo casu, quantum insumit in ascensu à B ad D per circumferentiam BHFD; sit enim filum AB 12 pedum, docet experientia globum B tractum ad C, inde ad B spatio secundi minuti recidere, & alterius secundi spatio à B versus D ascendere. Si verò AB trium pedum fuerit, hoc est praecedentis subquadruplum, spatio dimidij secundi à C descendet ad B, & aequali tempore à B ad D vel S perueniet; ad D si filum & aër nullum afferant impedimentum, cùm impetus ex casu C in B impressus sufficiat ad promouendum globum pendulum ad D punctum.

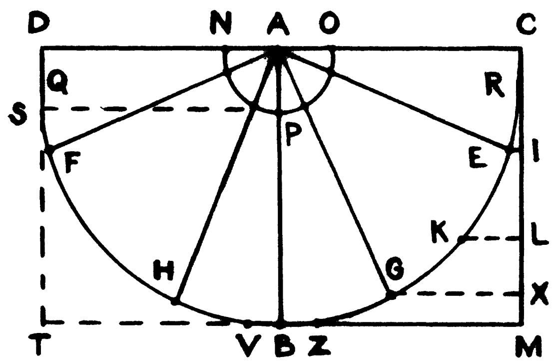

FIG. 1.

Globus igitur spatio secundi percurret dimidiam circumferentiam CBD, & aequali tempore à D per B versus C recurret; donec hinc inde vibratus tandem in puncto B quiescat, siue ab aëris & fili resistentiam vnicuique cursui & recursui aliquid detrahentem, siue ob ipsius impetus naturam, quae sensim minuatur, qua de re postea. Nota verò globum plumbeum vnius vnciae filo tripedali appensum, non priùs quiescere postquam ex puncto C moueri coepit, quàm trecenties sexagies per illam semicircumferentiam ierit; cuius postremae vibrationes à B ad V sunt adeo insensibiles, vt illis nullus ad obseruationes vti debeat, sed alijs maioribus, quales sunt ab F, vel ab H ad B.

Certum est tertiò filum AP fili AB subquadruplum vibrationes suas habere celeriores vibrationibus fili BA; esséque filum AB ad PA in ratione duplicata temporum quibus illorum vibrationes perficiuntur, atque adeo tempora habere se ad filorum longitudines vt radices ad quadrata; quapropter ipsae vibrationes sunt in eadem ac tempora ratione.

Sextò, filum tripedale potest alicui iustò videri longius ad secundum minutum qualibet vibratione notandum, cùm enim in linea perpendulari AB graue cadens citiùs ad punctum B perueniat, quam vbi ex C vel D per circumferentiae quadrantem movetur, quandoquidem AB linea breuissimè ducit ad centrum grauium, & tamen ex observationibus grauia cadentia tripedale duntaxat interuallum ab A ad B semisecundo, & 12 pedes secundo conficiant, illud filum tripedale minus esse debere videtur: Iamque lib. 2. de causis sonorum, corrollario 3. prop. 27. monueram eo tempore quo pendulum descendit ab A, vel C and B per CGB, posita perpendiculari AB 7 partium, graue per planum horizonti perpendiculare partes vndecim descendere.

Quod quidem difficultatem insignem continet, cùm vtrúmque multis observationibus comprobatum fuerit, nempe grauia perpendiculari motu duodecim solummodo pedes spatio secundi, globum etiam circumferentiae quadrantem, cuius radius tripedalis, à D ad B semisecundo percurrere; fieri tamen nequeunt nisi globus à C ad B per circumferentiae quadrantem descendat eodem tempore quo globus aequalis per AB: qui cùm pedes 5 perpendiculariter descendat eo tempore quo globus à C ad D peruenit, nulla mihi solutio videtur; nisi maius spatium à graui perpendiculariter cadente percurri dicatur quam illud quod hactenus notaueram, quod cùm ab vno quóque possit obseruari, nec vlla velim mentis anticipatione praeiudicare, nolui dissimulare nodum, quem alius, si potis est, soluat. Vt vt sit obseruatio pluries iterata docet tripedale filum nongentesies spatio quadrantis horae vibrari, ac consequenter horae spatio 3600: quapropter si per lineam perpendicularem graue 48 pedes spatio 2 secundorum exactè percurrat, vel fatendum est graue aequali tempore ab eandem altitudine per circuli quadrantem, ac per ipsam perpendicularem cadere, vel aërem magis obsistere grauibus perpendiculariter, quàm obliquè per circumferentiae quadrantem descendentibus, vel graue plures quàm 12 pedes secundi spatio, aut plusquam 48 duobus secundi descendere, in eo fefellisse observationes, quòd allisio, grauium ad pauimentum aut solum ex audito sono indicata fuerit, qui cùm tempus aliquod in percurrentis 48 pedibus insumat, quo tamen graue non ampliùs descendit, augendum videtur spatium à grauibus perpendiculariter confectum.

Septimò, globus B ex C in B cadens paulò plus temporis quàm ab E, & ab E quàm à G insumit, adeout fila duo equalia, quorum vnum à C, aliud à G suas vibrationes incipiat, quod à G incipit, 36 propemodum uibretur, dum quod à C incipit 35 duntaxat vibratur, hoc est vnam vibratiorem lucretur quod à G cadit, à quo si quamlibet vibrationem inciperet, & aliud suam quamlibet a puncto C, longè cituis illam vibrationem lucraretur. Quantò verò breuiori tempore globus leuoir, verbi gratia suberis, suas vibrationes faciat, quantòque citiùs vibrationum suarum periodum absoluat, lib. 2. de causis sonorum prop. 27 & alijs harmonicorum nostrorum locis reperies.

Duodecimò, pendulorum istorum vibrationes pluribus vsibus adhiberi possunt, vt tractatu de horologio vniversali, & harmonicorum tum Gallicorum 1. 2. de motibus, & alijs pluribus locis, tum Latinorum etiam 1. 2. de causis sonorum à prop. 26. ad 30. dictum est . . . Tantùm addo me postea deprehendisse fili tripedalem longitudinem sufficere, quae sua qualibet vibratione minutum secundum notet, cùm praedictis locis pedibus 3 1/2 vsus fuerim: sed cùm vnusquisque debeat experiri, cum horologio minutorum secundorum exactissimo, filum quo deinceps in suis vtatur obseruationibus, non est quod hac de re pluribus moneavi: adde quòd in mechanicis filum illud siue tripedale, siue pedum 3 1/2 satis exactè secunda repraesentet, vt experientia conuictus fateberis: hinc in soni velocitate reperienda, quae secundo 230 hexapedas tribuit, hoc filo vsus sum, quo medici possint exporare varios singulis diebus aegrotorum, sanorúmque pulsus.