I

PRE-GALILEAN MECHANICS*
(An Example: Leonardo da Vinci and the Law of the Lever)

1. Historical Background

All too commonly, the simplistic picture is presented of the origin of rational mechanics (and indeed, of "modern" science) in a sudden confrontation with, and defiance of entrenched authority buttressed by ancient wisdom. Galileo is the great knight-in-armour; and the established Church and the Schools and, lurking behind them, Aristotle and his venerable disciples, intellectual descendents and interpreters, represent the dragons. In contrast to the cultural indebtedness to classical antiquity evident in our language and literature and art, in our philosophy, logic, and mathematics, in ethical ideals and political norms, in science all we owe to the ancients is a dubious legacy of errors and omissions, whose rectification may have helped in finding the true path to knowledge. The Greeks were too contemptuous of manual labor to grasp the significance of experimental science; the Romans, too busy imposing their own Law and Order to concern themselves seriously with Nature's. And when, in the Middle Ages, classical scholarship is re-discovered, it is, apparently, just those features most inimical to scientific inquiry that become blended with Christian doctrine to provide the scholastic metaphysics: the very antithesis of natural science. Such is the modern dogma, and like all dogmas, a simple and ultimately misleading picture of black and white.

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*This subject, at least as a part of experimental physics, is a rather unorthodox and neglected one. Its roots are deep, widespread and variegated--they lie in a sort of pre-history of "modern" science. To trace these historical origins, and to exemplify them by laboratory work is no easy matter, nor is success readily vouchsafed. For these reasons a rather fuller (although still drastically simplified) explanation seems called for here, than in the succeeding accounts of more familiar experimental physics.

The roots of natural science, and of mechanics in particular, are deep and varied. Great as were the contributions of Galileo's outstanding genius, these owe as much, or perhaps more, to his study of the masters of the subject who preceeded him as to his rebellion against the authorities who opposed him. Mechanics had a long history when Galileo entered on the scene; Galileo was--directly and indirectly--heir to this knowledge, and he exploited and built on it.

The examples, chosen to illustrate both the historical development and the enduring principles discovered, centre on the law of the lever, which in one guise or another, played a central and recurrent role in mechanical ideas from Aristotle and Archimedes up to the 17th century. Today, these studies of the lever would be classified as 'statics'; but the 'theoretical' distinction between this class of phenomena and dynamics, or mechanics in general, is quite recent. Earlier, one can certainly distinguish--indeed, the separation was almost absolute amongst the practitioners--between practical mechanics as exemplified in the technology of the day, and theoretical mechanics which developed through mathematical and philosophical speculation and astronomical observation. It was, what we would now term statics that formed, to a large degree, the scientific basis for much of the technology that developed steadily from classical through medieval and renaissance times. This technology, extraordinarily advanced though it was, was largely empirical, and the science that emerged from it was rooted in experience and practical experimentation. In contrast, early dynamics owed more to theorizing, and later to the accumulated body of astronomical observation, but neither of these brought this science really close to 'experiment', in the sense of empirical generalization and its test in practical trial. Statical mechanical principles could be--and repeatedly were--tested, generalized and improved, by practice. Dynamics received little such support; the primitive instrumentation of time measurement, in particular, made experimentation here far more elusive than in statics*

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*This distinction is emphasized by Newton who makes it clear from the outset (in the preface to the Principia) that he will be concerned with "rational" rather than "practical mechanics [to which] all manual arts belong, from which mechanics took its name...I consider philosophy rather than the arts, and write not concerning manual but natural powers." Statics figures little in the Principia. To Galileo, the distinction was not yet so clearly made. Of "Dialogues Concerning Two New Sciences" half is statics and half dynamics.

Not surprisingly then, in contrast to the dramatic change in Kinematics and Cosmology in the 16th and 17th centuries, statics shows a more continuous development from Archimedes to the present time. And even Dynamics, far from uprooting all that went before, inherited much from the statics that grew up with practical mechanics; and not a little from the vast legacy of ideas, scholarship and analysis that range from the speculations of the pre-Socratic Greeks, the practical science of the Alexandrian School, the Arab commentaries of the Middle Ages, the incisive criticisms and advances beyond Aristotelian physics made by the "scholastics" at Paris and Oxford in the 14th century and the transmission of these ideas, theories and methods to the scholars who flourished in Italy in the 15th and 16th centuries.

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Leonardo da Vinci (1453-1519) was a supreme master of the arts, crafts and technology of his day. He was also fully familiar with the inherited body of scientific knowledge, either directly or from his teachers and contemporaries. Not surprisingly then, mechanics--in the sense of the principles and practice of machines, and therefore, largely 'statics'--occupied prominent place in his ideas and activities. By bringing together, perhaps as closely as any other activity, theory and practice, vision and technique, knowledge, imagination, and invention, mechanics seemed to provide an ideal outlet for his particular genius (or part of it!) Da Vinci was assuredly no great scholar, certainly not a mathematician of high order, but his notebooks on mechanics provide us with an interesting impression of the state of mechanical knowledge in his day, embellished always with his own fertile imagination.

There is no systematic treatment of mechanics in Leonardo's (unpublished) writings: notes, ideas, conjectures, questions, sketches, and plans abound beyond number. When these writings were made public and studied (in the 19th century) it seemed that da Vinci had anticipated some of the major ideas which blossomed out a century later (Stevinus, Galileo). Later scholarship (especially the pioneer work of P. Duhem) casts the matter in a different light. It seems far more likely that some of these apparently "revolutionary" ideas, were in fact part of the stock of knowledge, transmitted from the great scholars of the earlier schools (Jordanus, Buridan, Oresme of the Paris school, William of Ockam, Heytesbury and Swineshead of Oxford, etc.) to the Renaissance thinkers of Italy. In the science of mechanics, Leonardo is a brilliant mirror rather than a luminary, but none the less interesting for that.

2. Experiments

These centre on Leonardo's ideas on levers, pulleys, and the inclined plane--all key components of the mechanical devices of his time, and all, theoretically, illustrative of the sustained attempts to exploit the principle of the lever. The basic account is provided by Chapter IV--Leonardo da Vinci's Mechanics: Statics (Ivor B. Hart).

Our primary interest is to exhibit the mutual influence between experience or practice and theoretical ideas at this early, embryonic stage in the development of mechanics. Time and again one witnesses the struggle to extract abstract principles from practical demonstrations; or conversely, to use familiar experience to confirm what is already surmised.

i) The law of the symmetrical lever (going back to the axioms of Archimedes--Ref.2) provides the starting point. (Logic or Experience?)

ii) The asymmetric lever; the lever with more than one weight; combined and distributed weights; the Centre of Gravity (again Archimedes).

iii) The lever with bent arms; the lever with "forces" applied non-perpendicularly; da Vinci's formulation of the problem: the product of 'force' (or weight) and 'perpendicular' distance; the use of strings and pulleys to create non-perpendicular forces, or generally to change the direction of a force.

iv) The pulley interpreted as a 'lever' problem. Compound pulleys, the windlass, etc.

v) The emergence of the principle of virtual velocities (or displacements); the idea of mechanical advantage, generalized from the simple lever; its ambiguous implications for dynamics!

vi) The struggle with the principle of the inclined plane; the relationship between motion and equilibrium problems with inclined planes.

vii) Abstract and practical realization of a principle (of the inclined plane): e.g., the relationship of the three possibilities; (a) sliding blocks (b) slipping spheres (c) rolling 'carriages'.

What is their relationship? (Experimentally, theoretically, practically). The role of friction (in each case).

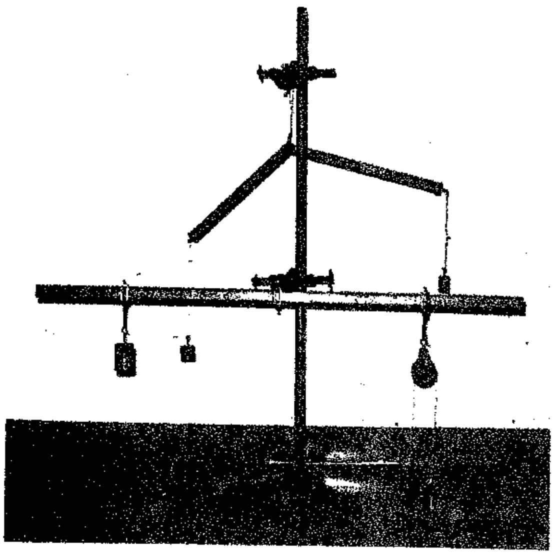
viii) A venture into the realm of dynamics, lever-balances with <u>moving</u> masses. The difficult problem of 'force' on <u>moving</u> objects.

3. Apparatus

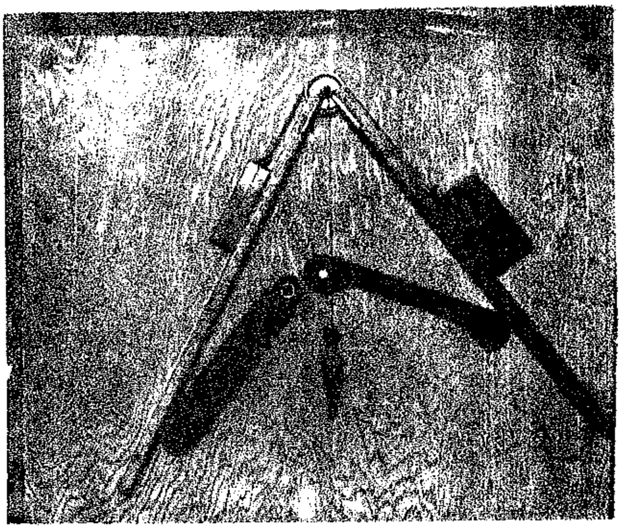
As befits the portrayal of an age when specifically experimental apparatus and instrumentation was almost non-existent, the laboratory equipment is easily improvised and wholly unsophisticated. It is patterned on common experience.

Figure (i) shows a simple arrangement to investigate the straight lever, the bent lever, and the lever with moving (accelerated!) weights.

Figure (ii) shows an arrangement made to investigate the properties of weights on inclined planes.



(i)



(ii)

4. General Questions and Problems

To what extent are the laws of the lever intuitive, i.e., simple generalizations of common experience? Are these real experiments, or simply demonstrations? Does (and did) experimentation lead to generalization of the laws to less familiar circumstances? and less obvious results? Can the laws be formulated more precisely (accurately?) in mathematical than ordinary language? And how, in various cases, can the analysis in terms of the laws of the lever be related to an analysis in terms of the current (Newtonian) principles of mechanics (Laws of equilibrium of bodies and forces)?

5. Bibliography

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+denotes reproduced abstracts are made available

*+1) The Mechanical Investigations of Leonardo da Vinci. Ivor B. Hart, University of California Press (1963); especially Introduction to Second Edition and Chapter VI.

*2) The Works of Archimedes. Edited by T.L. Heath, Dover Publications, New York (1953); especially pp.189-195.

+3) The Medieval Science of Weights. E. Moody and M. Claggett, University of Wisconsin Press (1960).

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5) Etudes sur Leonard da Vinci. Pierre Duhem, Paris (1906-1913).

6) I. Libri di Meccanica. (Italian) Illustrated reproductions of da Vinci's notebooks; especially IV: De Pesi.

+7) Essays in the History of Mechanics. C. Truesdell, Springer Verlag, New York (1968); especially pp.7-9:"Did Leonardo Experiment?"and pp. 25-34:"Birth of the History of Mechanics."

8) A History of Mechanics. (English translation) R. Dugat, Central Book Company, Inc., New York (1905); especially Chapters I-IV.

References (1) and (7) provide further extensive bibliography.

II.

Galileo: La Bilancetta (The Little Balance)

This is a charming little experiment that spans nearly 2,000 years of history, from Archimedes (287-212 B.C.) to Galileo (1564-1642). It is a nice illustration of both scientific principles and method, and Galileo's early approach to both. The apparatus is extremely simple. (A translation of Galileo's short and easily read paper--he was 22 when he published it--with commentary, is given as appendix to Galileo and the Scientific Revolution, L. Fermi and G. Bernardini, Ref. 1).

The problem at issue is how to estimate the proportions of two substances (e.g., gold and silver) in a mixture of arbitrary, complex shape (Heiro's crown). It goes back to the familiar old tale of Archimedes, in his bath, pondering on this problem of Hiero's crown, and, as recorded by the Roman architect Vitruvius* (Ref. 3), joyously exclaiming "Eurika! Eurika!" and leaping from the bath with the answer. It is, apparently, to infer the density (or, more precisely, specific gravity) of the object from its volume and its weight. The difficult matter is the assessment of the volume; the solution is to weigh the displaced water.

Weighing was probably already quite a refined operation in Archimedes' time, so that the limitations in precision would be in the volume measurement. Vitruvius supplies some details of how he believes this was done--not very convincingly, if the method is to be of any practical value.** Galileo is likewise unconvinced; and his admiration for Archimedes leads him to surmise that he (Archimedes) was more astute than Vitruvius' portrayal implies. Surely, he could--and probably did--use his famous "Archimedes' Principle" of buoyancy (Ref. 2) to infer the volume (and hence density), rather than the crude displacement method. Galileo's little

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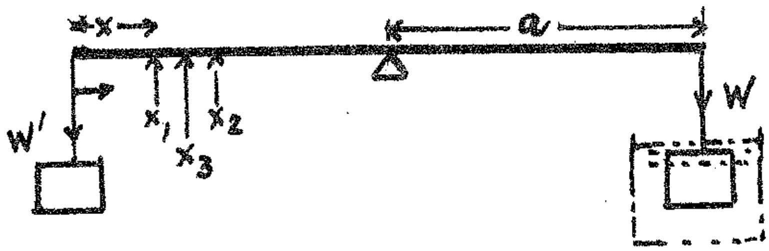
* Marcus Vitruvius Pollo, 85-26 B.C.

** One can easily 'invent' much more accurate versions of Archimedes' displacement method than that sketched by Vitruvius. The 'modern' specific-gravity bottle is an illustration of such.

balance is a neat, practical way of accomplishing this. It exploits both Archimedes' Principle and the principle of the lever.

It can be emphasized that since one is only attempting to determine a ratio--the proportions of Lead (in lieu of Gold), and Tin (in lieu of Silver), in the mixture--no absolute scales of mass or length are required; nor should they be used!



An object of weight W, specific gravity ρ, (Volume = W/ρ), is counterbalanced by an equal weight W' on a symmetrical balance. When W is immersed in water, the same weight W' has to be moved a distance, x, to restore the balance.

Then, $(x-a)/a = (\rho-1)/\rho$, or $x/a = 1/\rho$.

For fixed a then, the displacement, x, is a measure of ρ, irrespective of the actual weight, W. For any three samples, W_1 of material (1), W_2 of material (2), and $W_{1,2}$ of some mixture, one obtains, then:

$$\rho_1 = x_1/a; \quad \rho_2 = x_2/a; \quad \rho_{1,2} = x_{1,2}/a.$$

(Since only the ratios of ρ_1, ρ_2 and $\rho_{1,2}$ are significant, the actual magnitude of a is not required. If the proportions of (1) and (2) in the sample of the mixture (1,2) are expressed in fractions by weight: f_1, f_2 ($f_1+f_2 = 1$) then, we easily get:

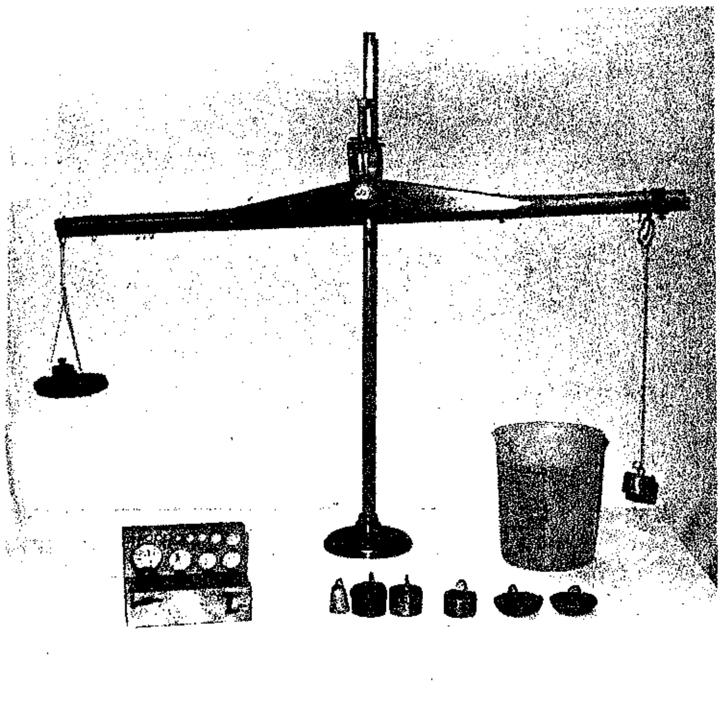
$$f_1/f_2 = (x_2-x_1)/(x_1-x_{1,2}).$$

Thus the "scale" between x_1 and x_2 acts as measure of the ratio f_1/f_2. All that we require is relative position of $x_{1,2}$ on this scale. Neither absolute lengths, nor distances of the x's from W' are required--only the differences and their ratios--as Galileo clearly asserts without detailed argument.

2. <u>Apparatus</u>: (Simple straightforward construction, following Ref. 1).

The "wire scale" is made of 24 gauge (0.02" dia.) copper wire. To facilitate counting of the wires, every fifth arc is painted white. An ordinary pin can be used in place of Galileo's "stilleto"! The knife-edge of the balance is simply made, of steel (or brass). Its location, with respect to the center of gravity of the beam (about 1/4" above) is a suitable compromise between stability and deflection sensitivity.

The balance is suspended from a bracket and the indicating arm points upwards, both apparently common practices of the day. The need for a stable platform is, in principle, thereby eliminated. Perhaps such balances were commonly (in the market place?) used where such amenities were not available!



3. <u>Experiment</u>:

i) The balance is checked <u>without</u> W, W' (using the adjustable sliding balance weight).

ii) A buoyancy measurement (x_1) should be made with several pieces of, say, lead, of different size, to check reproducibility of x_1/a (i.e., the assessment of the specific gravity of ${}_1$ of lead). Likewise, x_2 for the tin, ${}_2$.

iii) $x_{1,2}$ now is measured for <u>different</u> samples of mixture. (These may be samples of different size of <u>same</u> mixture, and also (marked!) samples of different mixtures.

4. <u>Questions</u>:

i) What factors determine the precision/sensitivity of the method?

ii) Is the balance used well designed? Why is it suspended? How small a displacement can be observed? Is this limited by the coarseness of the wire "scale" or the registration of "balance"? Are these two features well matched?

iii) What features determine the angle of deflection of the balance, for a given difference of weights or their positions? (General problem of stability versus sensitivity for a beam balance).

iv) Is there an optimum size of the balance in relation to the sizes of the samples to be examined?

v) Is this a <u>practical</u> method of assaying metals for purity? (See below).

5. <u>Comments</u>:

What were Galileo's motives and/or intentions? These might have been anything from a youthful display of ingenuity or an illustration, for pedagogic purposes, of mechanical principles, to an aspiration to make (and sell!) an instrument of commercial value! (It would not be the only example of such an enterprise by Galileo to alleviate his pecuniary difficulties!)

Realistically, we must recall that chemical/metallurgical techniques of assaying, particularly of gold and silver, were quite highly developed in Galileo's day (although not in Archimedes' time!). The method of "the little balance" does have the merit that it is a simple, "non-destructive" one.

Vannoccio Biringuccio's "Pirotechnia", published in 1540 and well known at the time Galileo published his "Little Balance" (1586), gives extensive accounts of methods of metal-assaying and a remarkably full picture of the technology of the period. Quite small samples (fractions of one ounce) could be used, and weighing was, apparently, quite a refined technique: Biringuccio refers to a "small assay balance... with beam lifting device." (See figure). The design of Galileo's "little balance" clearly reflects the techniques of his time. But it is most doubtful whether it was a development that was of practical value to the assayer concerned with detecting small amounts of base metals in nobler ones.

An assaying laboratory, showing balances, muffle furnace for cupeling, ingot mould, etc.

6. Bibliography

1) Galileo and the Scientific Revolution. Laura Fermi and G. Bernadini, Basic Books, New York (1961); Appendix: "The Little Balance."

2) The Works of Archimedes. Edited by T.L. Heath (1897), Dover Reprints (1953);"On Floating Bodies," Book II, Prop. 7 (pp.258-61).

3) De Architect. Vitruvius, Translated in J.R. Newman, The World of Mathematics, pp.185-6, Simon & Schuster, New York (1956).

4) The Pirotechnia of Vannoccio Biringuccio (1540). English translation with notes by C.S. Smith and M.T. Gnudi (1942), M.I.T. Press Paperback Edition (1966).

III

GALILEO: Motion on the Inclined Plane

1. Historical Background

> My purpose is to set forth a very new science dealing with a very ancient subject. There is in nature, perhaps nothing older than motion, concerning which the books written by philosophers are neither few nor small; nevertheless I have discovered by experiment some properties of it which are worth knowing, and which have not hitherto been either observed or demonstrated...
> ....
> so far as I know no one has yet pointed out that the distances transversed, during equal intervals of time, by a body falling from rest, stand to one another in the same ratio as the odd numbers beginning with unity.
>
> Galileo (1566-1642)

So Galileo opens his dialogue of the third day (De Motu Locali, p.41). To be sure, the problem is an ancient one. Philosophers, astronomers, mathematicians and logicians had wrestled with the elusive nature of change and motion for two millenia at least, from the time of Thales, Pythagoras, and Zeno to the scholars of Galileo's own day. Aristotle had put his finger on the heart of the problem when he declared that to measure motion one must first measure time, but time can only be measured if one understands motion.[1] To arrest the fleeting moment; to understand instantaneous change or to measure instantaneous time; to grasp the logical, mathematical problems of the instant and the infinitesimal; to master the practical problems of observing and describing the evanescent--all demanded, as we now know, subtlety and sophistication of the highest order. Many made contributions to one or other aspect of the question, but Galileo's was the first full mastery of the problem with a breadth that justifies regarding his work as the beginning of the modern era of dynamics.

In what sense was Galileo's treatment novel? Was its basis truly experimental discovery? or experimental confirmation of mathematical abstractions based on broader philosophical principles (of economy, simplicity, beauty), and on the cumulative impressions of general and indistinctly defined experience? Is this truly an archetypical example of the famous empirical-hypothetico-deductive pattern of scientific discovery, which, if Galileo did not invent, at least did so much to promote and develop?

The celebrated account of his experiments and reasoning which occupy the third and fourth day in the Dialogues were, one must remember, written (published 1638) when Galileo was about 70 years old. He had been studying, thinking, writing, arguing about, and experimenting with the mechanics of motion for some 50 years. His assured, logical formulations in the Dialogues represent his final views, arrived at after many changes and redevelopments; and without doubt influenced by much of the work of others which, whether consciously or not, must have contributed to his mature understanding. Indeed, in many of the individual components of Galileo's theory we can recognize (even if Galileo himself did not!) the ideas and the language of some predecessor. It is no less a tribute to his genius to appraise his achievement as a masterly selection and exploitation from the whole legacy of writings--right and wrong--of just those elements which could be welded together to form a comprehensive theory of motion.

First, there was the solution of the problem of measuring time: the oft-told story of the swinging lamp in the Duomo at Pisa; Galileo's use of his pulse; his discovery of the isochronism of the pendulum, and hence a practical, sensible, if not strictly logical way of breaking Aristotle's vicious circle! There was his profound, passionate distrust of authority and dogma; and its natural corrolary--the appeal to, and reliance on, observation and experiment as well as the logic and mathematics of which he was a master. But however iconoclastic and intellectually independent or even rebellious, Galileo did not create mechanics *ab initio*. He had teachers, and though he rebelled against their teachings, they provided him with the foundations on which he built.

Galileo's earlier investigations of motion--especially of "free-fall" (from his work, *De Motu*, 1592, to his letter to Scarpi, 1604) clearly exhibit the Aristotelian character of his point of departure and of his initial ideas. Aristotelian notions of 'natural' and 'violent' motion are blended with the Archimedean principle of buoyancy, to provide a theory of what is essentially the terminal velocity of a body falling in a 'resistive' and buoyant medium. One easily recognizes the emergence of the steady but determined struggle for abstraction--to conceive the ideal motion--free fall, without the complication of the 'accidental' resistive medium. Alongside these "non-Aristotelian" notions, there is the more general and ever more pronounced departure from the peripatetic and scholastic outlook in the insistance on asking "How?" rather than "Why?"

In the steps of his development from the Galileo of Pisa and the early days in Padua, to the final work of the *Dialogues*, Galileo retraces, perhaps unknowlingly, many steps that had been taken more than two centuries earlier. The challenge to Aristotle had certainly been made by William of Occam (1300-1350), and probably by some earlier (Arabic) writers. Jean Buridan (c.1300-1360) had given a remarkably perceptive argument (the persistent motion of a top or grindstone) to vitiate the Aristotelian argument about the role of the surrounding air. He had also initiated the alternative 'impetus' theory, with which Galileo's own idea of '*virtus impresa*' (impressed force) is essentially equivalent.

The ideas of changing motion, and in particular of uniformly changing velocity ("uniform-difform" change in the terminology of the time) had already been elaborated by Albert of Saxony (fl. c. 1350-60), and Nicole Orseme (d.1382). The latter, a pupil of Buridan, had also developed a mathematical-geometrical representation of such non-uniform acceleration; that the distance travelled was the same as if the velocity were constant and equal to the mean of its initial and final values (c.f., Galileo, Theorem I, Proposition I; p. 173*). To arrive at this result the concepts of 'mean' (with respect to time) and 'uniform acceleration' (uniform increase of velocity with time) had first to be clarified; and here we notice that Galileo, just like Albertus before him, arrives at this classification only after abandoning the inapplicable alternative of uniform increase with distance.

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*Unspecified page references are to Reference #1.

A little later, Heytesbury (fl. 1370) and Swineshead ('the calculator') of Oxford independently enunciated the theorem; "If the motion of a body is uniformly accelerated and starts from rest with a value zero, the body will travel three times further in the second half of the time than in the first half." Here we see the first two terms: 1, 3, of the famous Galilaen sequence: 1, 3, 5, 7,...! (c.f., Extract from Dialogues quoted above).

But Galileo not only resumes and repeats these logical-mathematical-analytical kinematical deliberations of more than two centuries earlier: he combines them with experimental demonstration and phenomenal inquiry, he extends them to a far wider range of circumstances, to motion on an inclined plane*, to the pendulum, to the flight of projectiles and also to some less successful speculations. In doing so, he initiates the transformation from abstract kinematics to the real domain of dynamics.

2. The Experiments (Unspecified References are to the Dialogues, Reference #1).

Whatever may or may not have been Galileo's experience with objects let fall from high towers (cf. Ref. 2), his exploitation of the inclined plane--a 'controlled' experiment from the results of which are inferred the properties of the less testable 'natural' system--is surely the more original contribution to science, and is the main interest here. The apparatus is made to Galileo's prescription, insofar as this is given (Ref. 1, pp.178-179). Several arrangements have been used to correlate the times of start and finish of the motion with the start and finish of the simple water clock. Some automatic mechanical contraptions, of the type Leonardo might have invented, have been used (see Figure); but the simplest way, and a quite effective one, is to start and stop the water with one's finger and correlate this with motion 'by ear'.

*Here Galileo's considerations of 'forces' are reminiscent of those of Leonardo da Vinci and of Simon Stevin (1548-1620) in which in turn we can trace the ideas of the Paris school (Jordanus) of the 13th century. See also historical note to Experiment I: Pre-Galilean Mechanics.

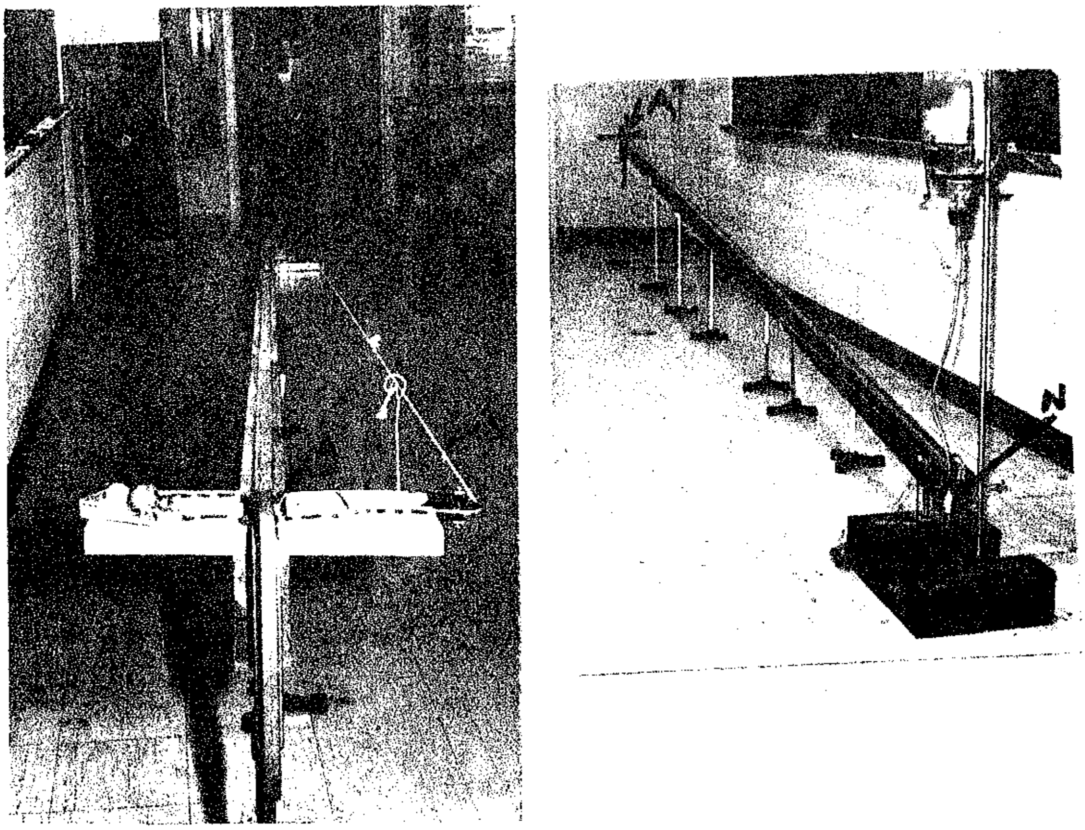


Figure: <u>The inclined-plane arranged for 'automatic' timing</u>. Cutting the string (S) simultaneously releases the sphere (at A) and moves the nozzle, (N) so that water enters the graduated vessel. When the ball reaches the bottom, the nozzle is deflected so that the water enters a standby container.

i) Preliminary experiments are made to check the accuracy/reproducibility of the water-clock timing method (p.179). The 'clock' might also be compared with the simple pendulum or pulse (both used on different occasions by Galileo for timing).

ii) Direct check of Theorem II, Proposition II (p.174):

$$\text{Distance proportional to (time)}^2$$

for several different inclinations. Deduction from this of the 1, 3, 5, 7,... Theorem (pp.175-177).

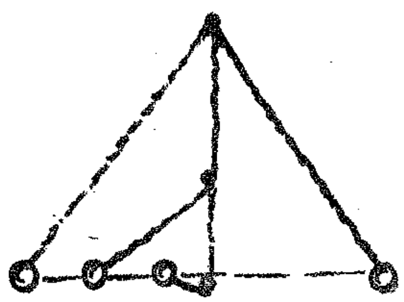
iii) Experimental verification of the relation

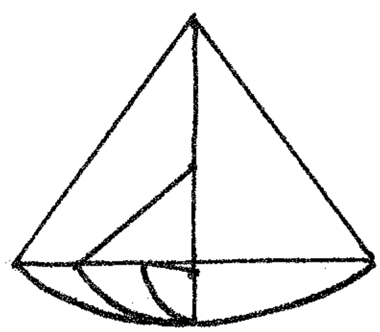
$$t \propto 1/\sqrt{H}$$

for the time to traverse a path of fixed length but varying inclination (i.e., H, the difference in height of the two ends; Theorem IV, Proposition IV).

iv) Observations and measurements should be made with rolling spheres of different materials, and different diameters. Also, the limiting case of motion on a plane of minimum inclination should be examined.

v) Supplementary Experiment with Swing Pendulum. This demonstration of the relationship between height and velocity.





Galileo (pp.170-171) refers to this before the inclined plane experiments, but it clearly lends support to his later arguments. It is a simple demonstration but well worth careful repetition.

3. Some Questions:

i) Galileo claims (p.179) to have predicted at least some of the experimental results-- e.g., the law:

$$t \propto 1/\sqrt{H}$$

quoted in (iii) above. And in the numerous theorems, propositions, and lemmas that follow his account of the experiments, he indeed appears to derive these results from "mathematical" arguments. But since we are dealing with a physical phenomenon--free 'fall' down inclined planes, there must, presumably, be some initial premise(s) or assumption(s). What are these? Is the postulate of uniform acceleration made *a priori* from considerations of simplicity? (Galileo discusses this first in the context of free fall vertically for which no real 'measurements' are described)

ii) How is the property of free fall related to motion in the inclined plane? Is the single basic assumption (given on p.169) that:"

> The speeds acquired by one and the same body moving down planes of different inclinations are equal when the heights of these planes are equal.

a sufficient basis for all the theorems (pp.185 *et. seq.*)?

iii) What is the role of the supplementary experiment (iv), in relation to this 'theorem'? Is this an attempt to give a physical (dynamical) explanation or justification of the basic kinematical assumption, in terms of forces ('forza', 'impeto', 'momenti')? How convincing is it? And to what extent does this explanation rely on experiment or experience? (Notice here the dubious connection with statics, and the 'principle of virtual velocities' first elaborated by Jordanus).

iv) Suppose it had been possible (with the timing techniques available) for Galileo to compare the times of descent down an inclined plane with those of directly vertical free fall through the same heights, would he have in fact confirmed his Theorem III, Proposition III:

> If one and the same body, starting from rest, falls along an inclined plane and also along a vertical, each having the same height, the times of descent will be to each other as the lengths of the inclined plane and the vertical.(p.185)

Do the theorems which follow (pp.186 et.seq.) really depend on the truth of this Theorem III?

4. Some Comments

It seems pretty clear that in Galileo's measurements with the inclined plane it was rolling that he examined (not sliding). His remarks about the smoothness of the plane notwithstanding, it is most unlikely that he was able to so reduce the friction as to be able to study sliding down inclines gentle enough for this technique to be meaningful (this might be checked experimentally). How 'smooth and polished' then should the 'parchment' be (p.178)? Could it be too smooth for rolling? In any event, the range of inclines for which the law, $t \propto 1/\sqrt{H}$, applies must be limited. Would it be different for spheres of different sizes and/or materials? (This point can also be nicely illustrated with solid and hollow spheres--or more easily with cylinders).*

If--and only if--the relationship between rolling and free-fall is understood, can the acceleration of free fall be inferred from the measurements on the inclined plane. The result might then have been compared with the period of oscillations of a simple pendulum. But there is no evidence that Galileo fully understood the precise relationship between rolling, on the one hand, and frictionless sliding or free-fall on the other. Nor did he, although he studied its properties, possess a sufficient theory of the simple pendulum. This was accomplished some decades later by Huygens, Newton, and others.

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*It is of obvious value for the student to re-work the analysis of a rolling sphere (or cylinder) on an inclined plane, and to exhibit the relationship between the critical angle of inclination for rolling (versus slipping) and the coefficient of friction.

And even with an adequate theory, in order to translate the experimental measurements of the inclined plane experiments to acceleration in terms of some 'standard' unit of time (day, hour, etc.), some "calibration" of his crude water clock would have been necessary. One can conceive how this might have been accomplished, but there is no evidence that Galileo actually made the attempt. At this early, nascent stage of the science of mechanics, the idea of measuring a quantity like 'acceleration of free fall' in 'standard', universally accepted units was still unborn.

Precisely what Galileo's experiments "proved" may be--and indeed has been--the subject of much discussion and controversy (c.f., for example, Ref. 2). But there is little doubt that much was learnt from them. As the publisher remarks to the Reader in the preface to the original Dialogues:

> "...Sight can teach more, and with greater certainty in a single day than can precept even though repeated a thousand times."

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(See also comments on Experiments I, II).

IV

NEWTON'S INVESTIGATIONS OF THE OSCILLATIONS OF FLUIDS

1. Historical Background

Every student learns how Isaac Newton formulated the "Three Laws of Motion" and so laid down the basis for the 'classical' mechanics, and indeed the classical physics, which was to develop in the succeeding two centuries. The major historical theme, the one (and often the only one) which is so familiar, concerns Newton's theory of gravitation, which, together with his Laws, provides an explanation of Kepler's laws of planetary motion.

Unquestionably, this was the major triumph of the Newtonian theory. The predictions were precise and unequivocal in this direct application of the theory to essentially 'point' masses. The extension of Newtonian theory to finite objects--especially to large spherical objects, as is required to include the Earth's gravity in the Newtonian scheme--is also a well known part of the broad historical picture. But not all objects are 'points' or 'spheres', and Newton and his contemporaries (and even more so, his immediate successors) were also concerned with the other, less ideal, mechanical phenomena. How could the Laws formulated for 'point' particles be applied in such more general and complex situations? What were the known phenomena which demanded explanation; and what attempts were made to devise experiments to test the theories or to extend them?

For this aspect of Newton's physics we can turn to the second book of the Principia, "The Motion of Bodies in Resistive Mediums", where we find both subject matter and procedures not a little different from the contents of the other two books. In dealing with celestial phenomena, Newton has at his disposal the accumulated data of centuries--much of it of high precision. The motions are those of 'ideal' objects moving in empty space. Only here and there ~~is~~ there a question of repairing gaps in knowledge by new observations; and the possibility of new experiments does not arise. For the motion in resistive mediums, which is the subject of fluid mechanics in embryo, the situations is very different. The physical situation is far more complex, the theories are at times quite tentative, and the range of known and carefully studied phenomena is quite limited. Theory now not only needs the test of observation; it also can benefit from the hints which derive from further experiment. In Newton's development of the subject, we find frequent appeal to experiment, which Newton both devises and executes.

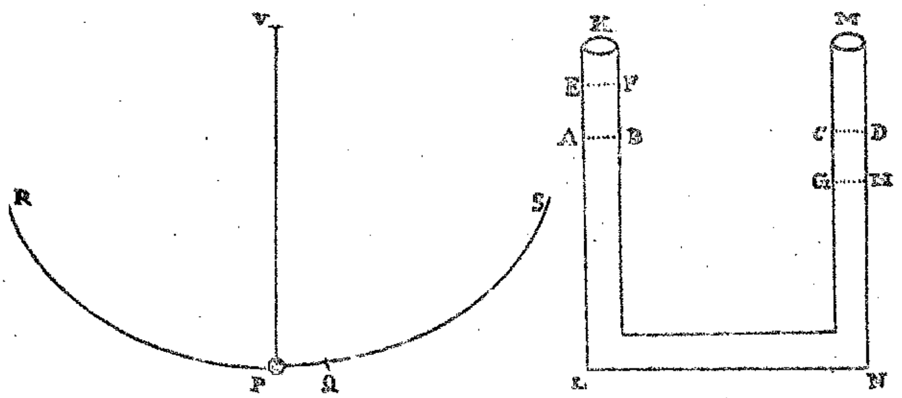
Prior to Newton's work, the mechanics of fluids as developed between the time of Archimedes (c.250 B.C.) to that of Torricelli, 1577-1644, and Pascal, 1623-1662, --"Treatise on the Equilibrium of Liquids and the Heaviness of the Air"--was almost entirely limited to statics. (The Alexandrian and Roman hydraulic technology appears to have been an almost entirely practical art). In extending to fluids his own mechanical laws, Newton is in fact creating a new science, and he is led step by step from one topic to another: from motion through fluids to motion of fluids, from motion to oscillations, from oscillations in pipes to waves on the surface of water, from waves on the surface to waves in the bulk medium, and finally to a mechanical theory of the propagation of sound in air. The successive steps may follow one another, naturally--or give this impression in the orderly sequence that is laid out in the Principia. But there is certainly not here the same formal rigour as characterizes Newton's celestial mechanics. Indeed, there is much guesswork: and some notable wrong guesses. It is a triumph as much of the power of imagination as of analysis.

The simple theorem:Proposition XLV, Theorem XXV, (Reference #1 and reproduced on the next page) about the oscillations in a U-tube is a most interesting link in the Newtonian development between the formal rigorous 'particle' mechanics which forms the basis of his "World Systems" and the application of Newtonian principles to the more complex problem of the motion of bulk matter. It might have been just a thought-experiment. Although Newton would have had no difficulty in setting up the experiment and testing the theorem, he would hardly have had any doubt as to its outcome.

PROPOSITION XLIV. THEOREM XXXV

If water ascend and descend alternately in the erected legs KL, MN of a canal or pipe; and a pendulum be constructed whose length between the point of suspension and the centre of oscillation is equal to half the length of the water in the canal: I say, that the water will ascend and descend in the same times in which the pendulum oscillates.

I measure the length of the water along the axes of the canal and its legs, and make it equal to the sum of those axes; and take no notice of the resistance of the water arising from its attrition by the sides of the canal. Let, therefore, AB, CD represent the mean height of the water in both legs; and when the water in the leg KL ascends to the height EF, the water will descend in the leg MN to the height GH. Let P be a pendulous body, VP



the thread, V the point of suspension, RPQS the cycloid which the pendulum describes, P its lowest point, PQ an arc equal to the height AE. The force with which the motion of the water is accelerated and retarded alternately is the excess of the weight of the water in one leg above the weight in the other; and, therefore, when the water in the leg KL ascends to EF, and in the other leg descends to GH, that force is double the weight of the water EABF, and therefore is to the weight of the whole water as AE or PQ to VP or PR. The force also with which the body P is accelerated or retarded in any place, as Q, of a cycloid,* is (by Cor., Prop. LI, Book I) to its whole weight as its distance PQ from the lowest place P to the length PR of the cycloid.* Therefore the motive forces of the water and pendulum, describing the equal spaces AE, PQ, are as the weights to be moved; and therefore if the water and pendulum are quiescent at first, those forces will move them in equal times, and will cause them to go and return together with a reciprocal motion. Q.E.D.

*With a simple pendulum of small amplitude, (which is sufficiently isochronous for these experiments!) the 'cycloid' reduces to the arc of a circle.

Cor. I. Therefore the reciprocations of the water in ascending and descending are all performed in equal times, whether the motion be more or less intense or remiss.

Cor. II. If the length of the whole water in the canal be of $6\frac{1}{9}$ feet of *French* measure, the water will descend in one second of time, and will ascend in another second, and so on by turns *in infinitum;* for a pendulum of $3\frac{1}{18}$ such feet in length will oscillate in one second of time.

Cor. III. But if the length of the water be increased or diminished, the time of the reciprocation will be increased or diminished as the square root of the length.

Verification of his theory may have lent some support to, and given Newton some confidence in, his method of theorizing about fluids; but the assurance with which he starts out is replaced by a far more tentative style when he has extended himself to the furthest ramifications of his theory.

Newton's work on fluid dynamics exhibits the whole range of his powers: from the masterful mathematical-philosopher to the imaginative experimental explorer. But why was Newton so engrossed in this subject at all? It hardly holds the central position in his philosophy or world-system that it occupies in the middle of the Principia! Nor, although it indicates the power and range of Newtonian methods, is it an unambiguous demonstration of their success. Possibly Newton's concern here is as much with rival philosophical cosmological systems--particularly the Cartesian--as with his own; eager to defend his own system by mastering his opponents'. Newton's own system of cosmical dynamics and motion in a void becomes far more compelling if he can show that the alternative, with its vortices and motions in and of fluids, cannot possibly be valid.

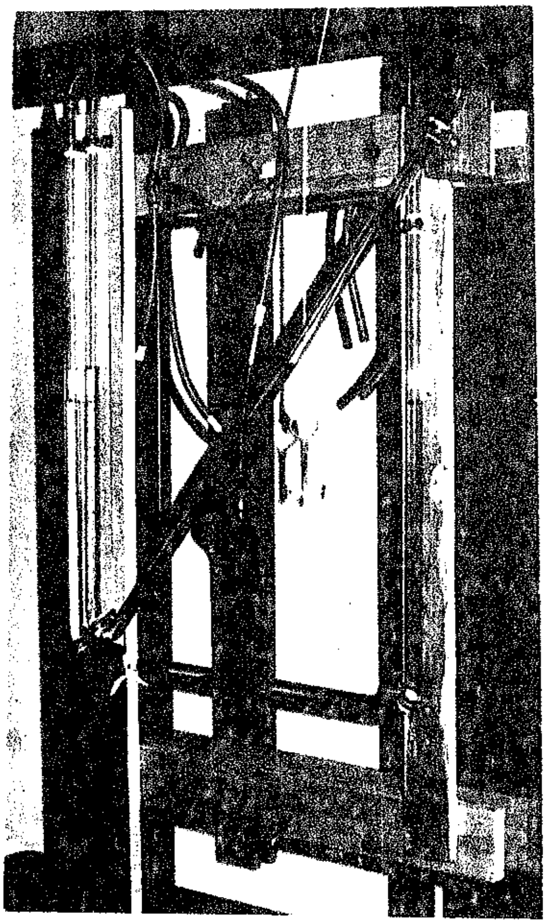
Whatever Newton's motives at the outset, in the end he lays a foundation for the further development of hydrodynamics. His influence here may not have been so profound as was his great work in mechanics and cosmology but it is none the less real. In his celebrated work on Hydrodynamics, published some 50 years after the Principia, Daniel Bernoulli writes:

> These are the things which have been communicated to the public up to this time on the oscillations of fluids, and certainly first by Newton, in order to show the nature of waves...

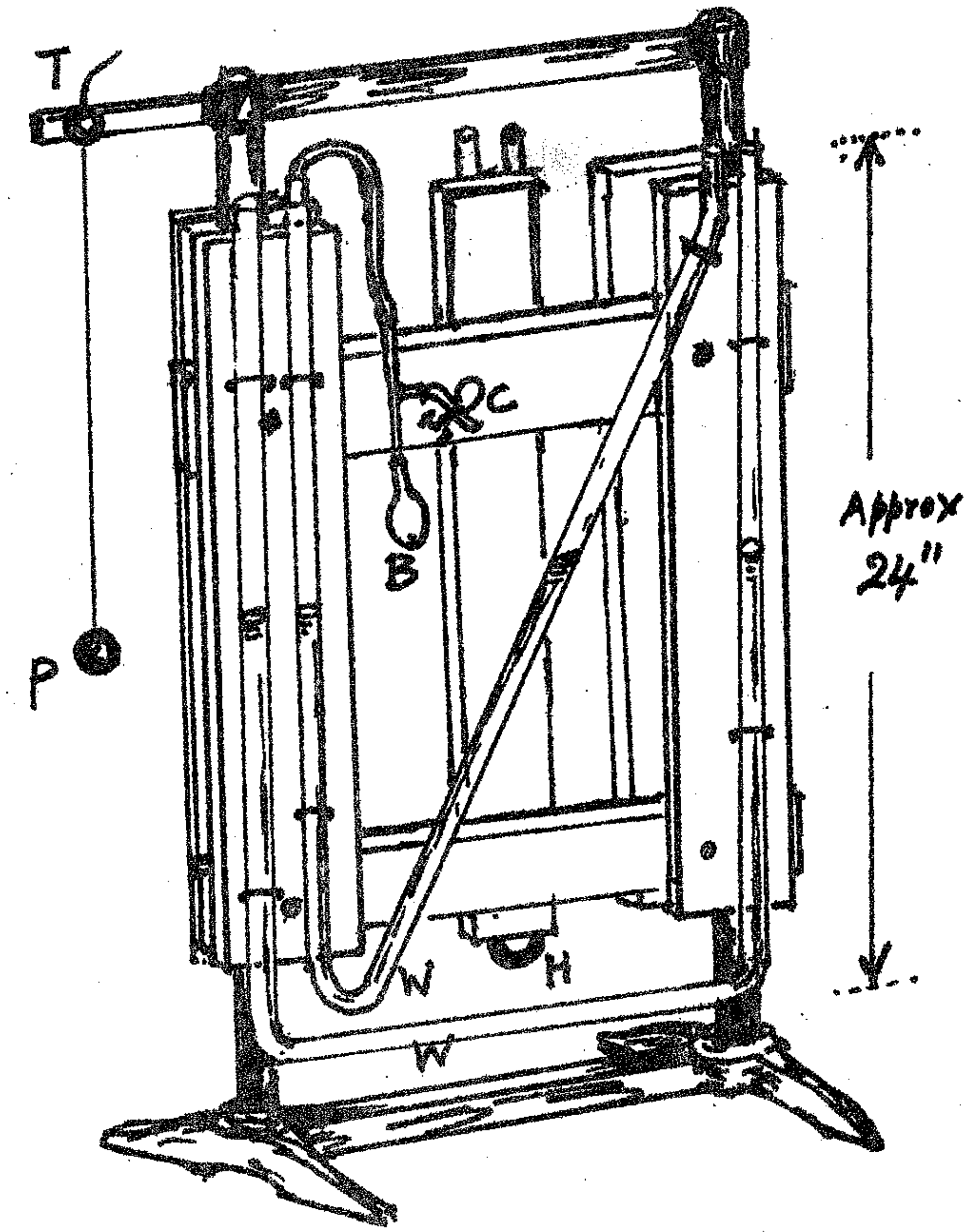
2. Experiments

These comprise a verification of variants and extensions of Newton's Theorem XXXV. A variety of U-tubes can be used, with water, mercury, etc..

Oscillations are compared with those of a simple pendulum of variable length. By use of some simple beat-technique (no time-piece required) the condition of isochronism can be established easily and with precision. A simple arrangement for mounting the U-tube is shown in the illustrations.



In another arrangement, a U-tube is mounted on a separate board which can be rotated as a whole (in a vertical plane). The dependence of the "effective length" (i.e., twice the length of the isochronous pendulum) can be explored as function of angles, etc..



Apparatus for Newton's Oscillations of Fluids: The Apparatus is mounted on a "standard" laboratory frame. Vertical slats of wood on each side are secured simply by bolts passing through. The water-filled U-tubes, W (on side shown), are of approximately 1/2" I.D. glass; the mercury filled U-tubes, H (on reverse side), are approximately 1/4" I.D. glass.
A small atomiser bulb (B) together with a spring pinch-cock (C) provides a convenient means of starting the oscillations.
P is a simple pendulum of adjustable length (using brass "terminal" T). A little color-dye is added to the water.

3. Some Questions and Comments

i) The theorem is true for any fluid. What Newtonian principle(s) does this illustrate? Is this--or was it in Newton's time--a particularly stringent test of these principles? What better ones were available?

ii) What are the features essential for isochronism? Straightness of tubes? Uniformity of bore? Lack of appreciable 'damping'?

iii) Newton's Theorem is extended by the Bernoullis, father (Johann) and son (Daniel), (Reference #3, p.129): first for a tube with uniform bore, but with limbs at arbitrary angles; second for a "leather bag or a conduit of any given shape whatever, full of water, termination at either end in two cylindrical conduits--inclined to the horizon in any way whatever..." For this latter case, with the two conduits of equal bore, (a), and water columns of length l_1, l_2, respectively, Bernoulli gives for the length of the isochronous simple pendulum an expression of the form (p.133):

$$L_{equiv.} = [l_1+l_2+(l_1 l_2/a)MN]/(\text{Cos}\ \theta_1+\text{Cos}\ \theta_2)$$

Here, M is the mass and N, a particular property of the dimensions, etc., of the flow in the irregular connecting bag. Its value is derived from Bernoulli's general treatment of flow of fluids in pipes (Ref.#3, Chap.III). It is of interest to examine what basic properties of fluids it is necessary to assume in order to obtain a definite prediction for $L_{equiv.}$; and how far such predictions were or could have been tested. In sum, what aspects of fluid motion are tested in the verification of Newton's theorem and the Bernoullis' generalization of them? Were (simplifying) assumptions implicit in their analysis, and how far were they justified in the explication of these phenomena?

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V

LIGHT AND COLOUR:
Newton's Experimentum Crucis

1. Historical Background

The study of light was one of Newton's earliest researches (1666-1672); it was also the subject of his last scientific writings--the famous book 'Opticks' (1704, 1717, 1721, 1730). The half-century or more in which he worked, wrote, developed, expounded, and defended his views on optics was one of the most significant in the development of physics; and in the evolution of Newton's ideas on the nature of light, one sees mirrored the whole development of a Natural philosophy whose principles and methods had, for two centuries or more, such a profound influence on science. One can study, as is so often done, Newton's specific contributions to optics: his experiments and theories; the vigorous controversies and heated polemics with his contemporaries; the triumph, in his own time, of his ideas over those of his rivals; the commanding and perhaps stultifying authority of the Newtonian theory--as propounded by Newtonian disciples--for the century or so after his death; and the eventual 'overthrow' of what became the hardened Newtonian 'corpuscular theory'. But all this, illuminating though it may be, treats only a limited aspect of the matter. Newton's concern with light--as indeed that of his age, of Kepler, Descartes, Huygens, Boyle, Hooke--was an ineluctable part of the whole nascent natural-experimental-mathematical philosophy of the 17th century. The exploration of any particular phenomenon, and its interpretation, both exemplified and in turn helped to shape the new philosophical methods. Nowhere is this clearer than in Newton's work on light and optics, which, as his ideas developed, became increasingly part of a whole outlook which embraced the great range of natural, physical phenomena which were, in Newton's time, the subject of Natural Philosophy.

The immediate period in which Newton began his researches was one of very lively activity in optics. In 1663, James Gregory (Optica Promota) had described the first reflecting telescope; in 1665, Father Grimaldi

(Physico-mathesis) reported his observations of colored fringes accompanying shadows (diffraction); Robert Hooke's Micrographia, 1665/7, gave extensive accounts of the colors of thin plates and flakes; and in 1669 the Danish philosopher, Erasmus Bartholin, discovered the 'double refraction' of Iceland-spar. Prior to these seminal discoveries, major comprehensive works on optics had been written by Kepler (Dioptrice, 1611) and Descartes (Dioptique, and Météores, 1638), and it was against the background of these that the newer discoveries were analyzed and interpreted.

Descartes' views especially, and the general philosophic framework in which they were expounded, exercised great authority. Light was a force, or potential motion, exerted on and propagated through a plenum--a space filled with material 'particles' in close mutual interaction. It was in Descartes' famous phrase an example, as were all phenomena, of 'matter and motion'. It was part of this dramatic new philosophy to eschew all peripatetic (Aristotelian) categories such as Quality, Substance, Sensible Qualities, etc., in the analysis of light no less than in mechanics proper. Indeed, light was to be treated, as were more palpable phenomena, in mechanical terms of 'bodies, modes, and actions,'--and it was an easy matter for the followers of Descartes to attack any rival theory as a relapse into a discredited Aristotelianism. Newton's early views on the nature of light and color were in fact so attacked and he makes the following very cogent defense:

> Through an improper distinction which some make of mechanical Hypotheses, into those where light is put a body, and those where it is put the action of a body, understanding the first of bodies trajected through a medium, the last of motion or pression propagated through it, this place may be by some unwarily understood of the former: Whereas light is equally a body or the action of a body in both cases. If you call its rays the bodies trajected in the former case, then in the latter case they are the bodies which propagate motion from one to another in right lines till the last strike the sense. The only difference is, that in one case a ray is but one body, in the other many....The bodies in both cases must cause vision by their motion.
>
> (1672; v. Ref. 3, p.244)

Newton's primary optical researches (1666-1672) are, however, essentially in experimental philosophy; and he repeatedly and emphatically displays a desire to avoid both detailed "hypothesis" about the exact nature of light, as well as wider philosophical and epistemological issues. (These become much more interwoven with his ideas on light in later years, when the whole frame of Newtonian philosophy has been developed). It is these great experiments of Newton's and what they taught, even if they did not prove, about the nature of light, that is the subject of concern here. In the barest outlines, one may summarize Newton's (and subsequently the generally accepted) conclusions thus: (Ref.#1, p.lxi-lxxii)

i) White light (sunlight, in particular) is a mixture of 'colored light' of many sorts (e.g., the colors displayed in the rainbow).

ii) If the different rays, of which white light is the composite, are "by any means... separated from one another", those of least refrangibility "beget a sensation of Red colour," those of greatest refrangibility "of a deep violet and the the intermediate ones, of intermediate colours." As distinct from the direct separation of sunlight itself--by a prism--the separation may also be affected by reflection or diffusion from a (Colored) object, which preferentially reflects light of some sorts (colors) rather than others. This is the relationship between the 'color of light' and the 'color of an object'.

iv) Newton associated--in analogy with sound--a different frequency with each color; and rays of light of different color "excite vibrations of different types in the aether" which provide the correspondingly different sensations (of colors), when they reach the eye. (But this is, admittedly, a hypothesis, which transcends the inferences from experiment.) Much of the early debate centered on the question: How much did Newton's experiments really prove? Were alternative, and less revolutionary, interpretations (than the composite nature of white light) possible?

2. The Experiments

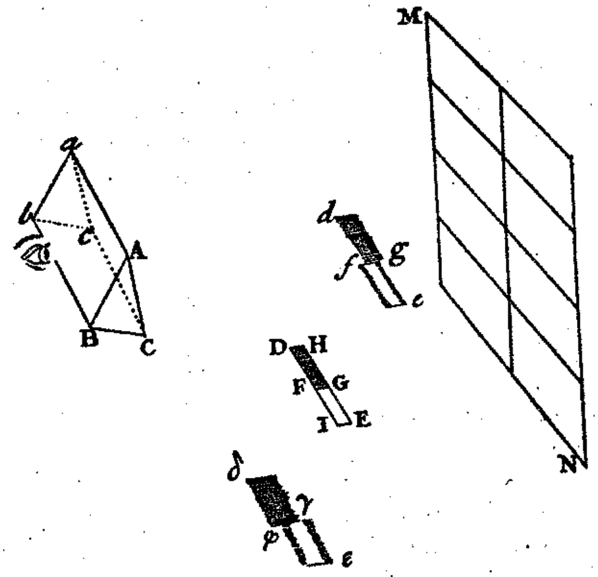
Newton experimented with light from a very early age. His most famous experiments on the colors of light seem to have been made in 1666 (when he was 23), but were not published until 1671/72. They display Newton as first and foremost an experimenter, and a most practical one. It seems likely that his experiments with prisms were inspired by the problem of chromatic aberration encountered in refracting telescopes; for in his very first publication Newton goes on to discuss the construction of a new type of reflecting telescope.

Although somewhat elaborated, and set-out as a more logical sequence, the style of experimentation described by Newton much later in his Opticks is not greatly different from what was actually done in 1666-1672. Here, then, we follow the sequence in Opticks. The experiments are all clearly described in Newton's own words: Book One, Part I (pp.20-62) and Book One, Part II, Prop.XI, Prob. V (p.186 ff). All references below are to Opticks, Fourth Edition (Ref. 1).

i) Prop. I, Theorem I (pp.20-66), Exp't. 1,2:

'Coloured light' is here produced by reflection of (diffuse) sunlight from differently painted surfaces. The difference in refrangibilities are observed directly by viewing the colored surfaces through a glass prism (v. Figure, below). That the same difference in refrangibilities accounts for the different focusing properties of a glass lens for differently colored light (Chromatic aberration) is shown in Experiment 2.

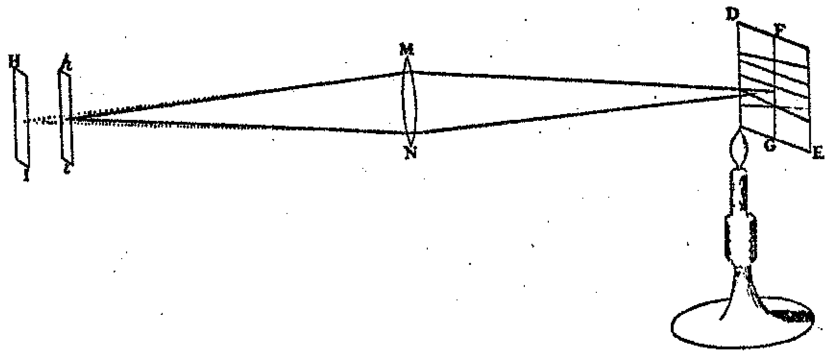
Exp. 1



ii) Prop. II, Theorem II, Exp'ts 3-10:

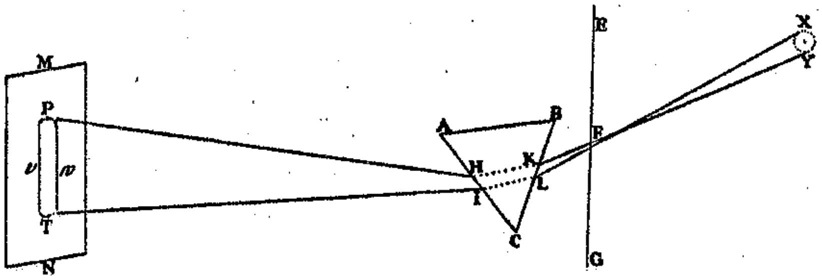
Here, Newton analyzes the "Light of the Sun" and for this, direct beams of sunlight are necessary. If this is not practicable, there are several alternatives. The closest is to use a plane mirror to direct light into a darkened room, and then proceed as Newton does. (Notice, however, Newton's Proposition III, Theorem III, p.63).

Exp. 2



If an artificial 'white light' source is to be used, in a meaningful way, one must remember that many of the detailed controversies (e.g., with Pardies and Lucas, c.f., Ref.#4) concerning Newton's experiments and their interpretation can be attributed to the simple, unrefined optical techniques Newton used, techniques which in the hands of those with less skill and insight--or with different motives!--could easily give different and even misleading results.

Exp. 3



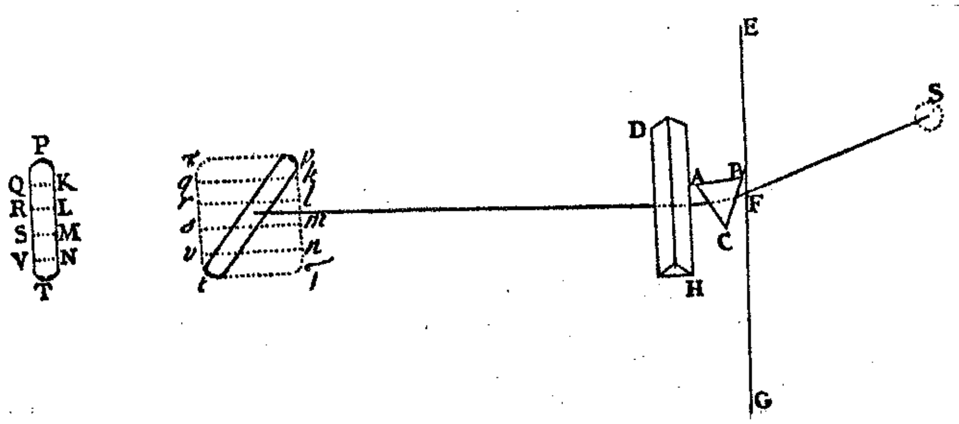
The problem is one of collimation of light. The sun's angular diameter is about 1/100, and the angular divergence of the beams from this source is not at all dependent on the various apertures used in Newton's optical arrangements. To simulate this situation one would need a diffuse 'uniformly' illuminated area sufficiently large and sufficiently far from the first aperture, so that the angular divergence of the light was not governed primarily by the stop size. In practice--in a typical room with, say 10 feet, available--this means scaling down the dimensions (apertures, spacings, etc.) of Newton's layout by a factor of 3 or so.

Note also that the luminosity and whiteness of any artificial source may be appreciably different from the sun's (as 'filtered' through the atmosphere). However, with a properly darkened room, meaningful experiments not too different from Newton's can be made (as did Newton) to 'focus' (and to brighten) the images. But these involve additional features, which are best avoided at the beginning.

Exp't. 3,4 (pp.26-34):
Notice how Newton establishes the important condition of "minimum deviation". (Why is this so important?) The character of the colored image does not depend on the length of the path in the prism (only the angle), on the nature of the surface of the glass, nor on the material (the use of water-filled prism): all evidence against the notion that the colors are 'manufactured' by the prism.

Exp't. 5 (pp.34-45):
A most convincing and striking demonstration that when the light is 'dispersed' the different 'colours' are each refracted by the prism without further 'dispersion'.

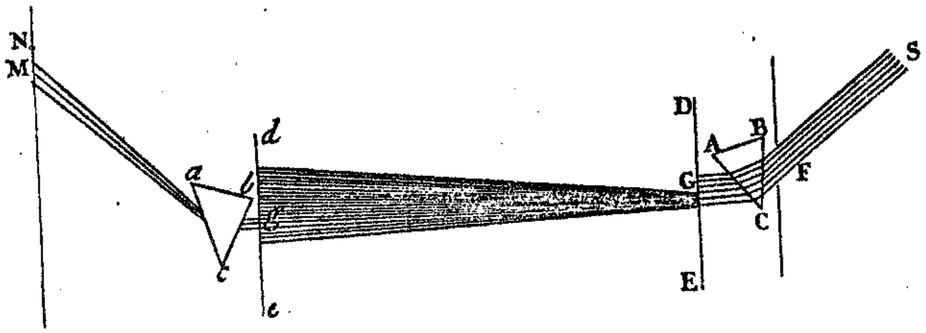
Exp. 5

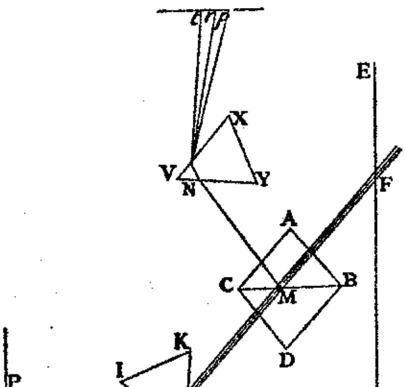


Exp't. 6 (pp.65-68):

This, together with Exp't. 5, comprises, in the various forms that have been recorded, the famous "Experimentum Crucis"* of Newton: the 'proof' that white light is composite--that is to say, that the different colored components into which it can be analyzed are not capable of further decomposition. 'Composite' can only be meaningful in terms of simple, (elementary), entities. For Newton, these are the "rays of definite refrangibility" and experiments (with prisms, etc.) certainly justify this claim that the individual 'colours' are the simplest elements. Experiments 7, 8 and 9 are elaborations of this basic theme.

Exp. 6



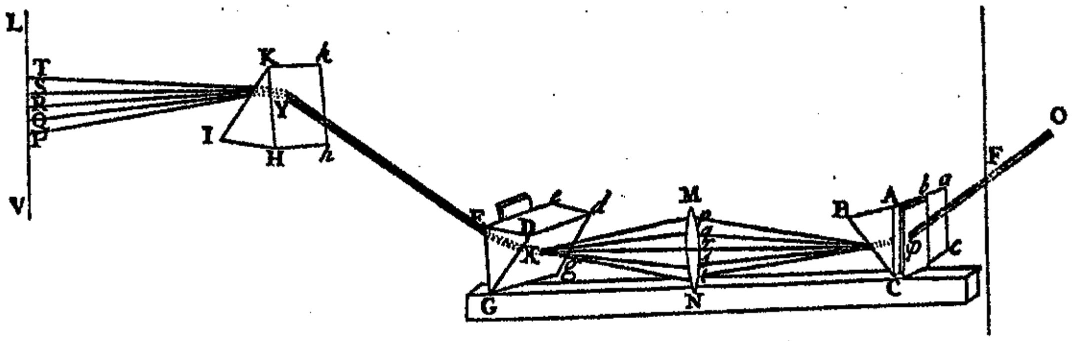


Exp't. 10 (pp.57-63)
A beautiful experiment which vividly demonstrates that 'total internal reflection', related as it is to refrangibility, exhibits the same separation of white light into its elementary colors.

*There are Baconian echoes in the phrase. In his 'Novum Organum', F. Bacon stresses the importance of "crucial instances (Instantiae Crucis) and luciferous experiments" in arriving at the truth (Ref.#4).

iii) Prop. XI, Problem VI (pp.186-191):

Here is the demonstration of the reverse of the foregoing analysis--the synthesis of white light from the constituent colors. Notice the use of prisms both in same direction, with an intervening lens to provide the reversal of angles.



3. Comments

The few experiments mentioned above do scant justice to the powerful mastery of experiment which Newton already displays in his earliest work (no wonder that 1666 has been described as his "annus mirabilis"; experiments were not his only preoccupation in that year). Nor do they do justice to the much wider range of optical investigations and discoveries described so vividly in the Opticks. Many of the commentaries that have subsequently been written have focused on the general theories in which Newton framed his interpretations; they sometimes lose sight of the actual discoveries and the experimental methods developed. It is of greatest interest and value to examine Newton's contributions and evaluate them under quite distinct headings:

1) Discoveries about the nature of light: color, refraction, polarization.

2) Experimental method: method of analysis, inference, etc..

3) Practical techniques: analysis of light, reflecting telescope, method of fringes, etc..

4) Attempts to make a comprehensive theory of light to embrace all the observed phenomena (Newton's successes and failures in optics).

5) Newton's attempt to incorporate his whole theory of optical phenomena, within the context of a comprehensive Natural Philosophy (his aims, preconceptions, and prejudices).

All too often, Newton's contribution to Optics is remembered as his advocacy of the 'corpuscular hypothesis'. Yet, some 150 years after Newton, one finds Augustin Fresnel, the pioneer of the 'wave-theory' of light, using Newton's measurements of the wavelength of light (the first ever made) to explain the precise details of the first fringe-systems he examines!

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