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# THREE OF GALILEO'S DISCOVERIES

## EMPIRICAL PARADIGMS FOR THE LOGIC OF SCIENCE

The logician interested in an account of science that is faithful to the actual practice of science has a number of problems, not the least of which are the following: first, the problem of avoiding psychologism, and second, the problem of having historical sources that are illuminating about the logical turns characterizing a piece of research that ended in discovery.

As to psychologism, or the danger of confusing merely social and psychological causation for a discovery with the considerations constituting the evidence for it, it is important not to court the equal danger of neglecting the logical conditions influencing the beginning of an inquiry and the course of it. The typical piece of scientific investigation usually begins with an hypothesis for which something that the scientist already knows appears to provide some evidence. The scientist's mind at the beginning of an investigation is not empty, and what there is in it cannot but be logically influential. For example, what the scientist takes himself already to know in common with his peers, the knowledge that he had to give behavioral evidence of having mastered just to enjoy recognition for competence to do research and, therefore, some license to be heard, determines in advance that hypotheses inconsistent with this knowledge will not be likely to occur to him, or, if they do occur, will not be likely to get his serious consideration. And this is minimal. The consistency of an hypothesis with already settled or received scientific opinion is not enough, since all this means is that nothing considered to be settled opposes the possibility that the hypothesis is true. A graduate student in science obliged to undertake a piece of research usually must satisfy his mentors or judges before he is allowed even to begin, not only that his hypothesis is not inconsistent with already acquired knowledge, but that this knowledge or some part of this knowledge tends actually to support his hypothesis or makes it plausible to the extent, at least, that research to test its truth seems worth undertaking. This may make it appear that the discoveries that scientists are satisfied have already been made have such a grip logically on scientists engaged in research that scientific progress must be linear and never revisionary. But this is not so, as it overlooks the equally influential power from a logical point-of-view of phenomena that become noticed either casually or in the course of research and that, by their inconsistency, give a sign that the hypothesis is false or that in some particular respect the settled scientific ideas that inspired it must be revised.[1] Of course, there is psychology and social psychology involved in all this, but it is not psychologism. A kind of causation is noticed by saying these things, but the way in which settled opinion is logically influential is in determining what receives attention, and the influence of received knowledge is what we notice.

The second problem concerns sources that are trustworthy for perceiving the threads of logic attending a piece of research that ended in discovery. Here it is important to distinguish exactly what use and relevance contemporary textbook expositions, almagests, elements, and summae have, and what use they do *not* have, for understanding research activity. Such sources help us to understand the accepted scientific beliefs that existed and, as a result of these, the possibly influential logic biases, before the start of scientific activity eventuating in discovery. For this reason,

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[1] This touches on a problem too complex to be dealt with satisfactorily here, and is therefore unavoidably oversimplified. Cf. P. Duhem, *La théorie physique*, Paris, Marcel Rivière & C$^{ie}$, 1914, pp. 278-285. Cf. alto Bronowski, J., "Humanism and the Growth of Knowledge," *The Philosophy of Karl Popper*, ed. Paul A. Schilpp, LaSalle, Ill., Open Court Publishing Co.; 1974, pp. 618-628.

the logician interested in research activity cannot neglect them but, at the same time, he cannot be content with them because they will not tell him what he wants to know. For example, an account of the chemistry of combustion would be structured to show the role of oxygen and never discuss in any detail, or even mention, phlogiston or the role in combustion which that hypothetical element at one time was imagined by scientists to have. On the other hand, to perceive the logic of scientific activity when this activity led from belief in phlogiston to its downfall and replacement by belief in oxygen, the logician would seem to be in need of sources showing the scientific mind engaged, not in textbook exposition, but in reaching beliefs not yet actually known or acceptable to it. But now, where are such sources to be found? Scientists rarely write diaries that reveal the development of their thought in the course of some piece of research, that tell us not only what beliefs they started with and what beliefs they ended with, but what considerations that were logically persuasive excited the occurrence of the change.

Meyerson offered a helpful answer: the logician who wants to understand the logic of scientific change needs opportunities to hear scientists *argue*; one might say he needs opportunities to see a scientist locked in logical combat, with himself primarily while his research is still in progress, but with his peers otherwise who must be convinced that he has made a discovery after he has convinced himself.[2] In logical situations of this kind, the scientist who believes he has made a discovery is on his mettle to persuade peers with attitudes similar to his before he began his research, people ready to ask if other hypotheses were considered by him or why he disqualified other hypotheses if he did consider them. *His* mind has been changed, but *their* minds are not yet changed, and difficulties must have occurred to him exactly like difficulties which they feel; alternatives must have, or should have, occurred to him

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[2] "And if one wishes to understand the motivations to which the scholar has adhered in analyzing what he has done it will obviously be particularly useful to listen to what arguments he will bring to bear when he has to defend his method of analysis against contestations and attacks. This is exactly what happens during the discussions at scientific congresses - such as the Solvay Congress, for example, whose proceedings, as my readers know, have given us some singularly precious insights." - Emile Meyerson, "Etudes des produits de a pensée," *Essais*, Paris, J. Vrin, 1936, p. 139.

like alternatives they can think of, so that his logic in debate with his peers must be a logic that enables them to reproduce at least somewhat the scientific change that he underwent himself.

This is not yet satisfactory, mainly because Meyerson was thinking of the Solvay Congress, the debates which he could exploit for his logical and psychological purposes, not because transcripts of the debate aspects of such proceedings are any more available than scientific diaries a logician might desire to have, but because Meyerson personally attended the Solvay Congress. Nevertheless, Meyerson offered a useful suggestion, especially for books such as Galileo's *Discourses Concerning Two New Sciences*, Darwin's *Origin of Species*, and Harvey's *On the Motion of the Heart*, where the writer is arguing for ideas he knows to be revolutionary, and has some disposition to play not only his own role as protagonist of new ideas, but the role of the scientific critics he would have if the format were a scientific conference given to an extensive debate or dialogue. The latter role, in such books, is a role the writer can play because, but for his research and the effect it had of changing his scientific opinions, it is a role that he would be psychologically and logically disposed to play as his peers are prepared to play it. He knows where his peers are because he has been there, and he knows how they need to be satisfied about the merits of his discovery because he needed to be satisfied exactly or nearly exactly as they need to be.

It is hoped that these remarks will suffice to satisfy critics fearful of psychologism or of insufficiently supportive sources for the account which follows about the logic of three of Galileo's discoveries: the law of inertia, the law of freely-falling bodies, and the law that a projectile has a parabolic path. Galileo's *Discourses Concerning Two New Sciences* is not a diary that unfolds, as it was taking place, the mental process that led to these discoveries nor is it a record of debate at an actual scientific conference. Nevertheless, as I hope to show, the logically influential elements advanced by Galileo in the *Discourses* for the persuasion of his peers were probably not afterthoughts useful for this purpose, but some of the most important of the logically influential elements in the mental process that eventuated in his own persuasion, so that, in this sense, the *Discourses* may be taken to supply a record of the logic of scientific change as useful as Meyerson believed a record of debates at a scientific conference might be to the logician of science who needs a description of a process of change to support his usually normative or prescriptive purpose. The important point is that, since works like the *Discourses* invite attention to a need for revolutionary change, the elements of debate necessary for discerning the properties of the logic of discovery are hardly suppressible in them in comparison with research reports which, though proposing the adoption of new ideas, do not invite the adoption of revolutionary ones.

Discoveries like those on dynamics defended by Galileo in the *Discourse* have another logical value that should be mentioned. A scientific discovery is sometimes an accident, something unsought, a happening to which no particular methodology was directed because no problem was influential and no hypothesis existed that might have shaped or influenced the actions eventuating in the discovery. Herschel's discovery of Uranus was of this kind. He was doing routine reconnaissance of stars already familiar and catalogued when he came upon it and realized that no one had noted it before. Such discoveries provide nothing of logical interest beyond the trivial fact that another observer of Uranus, not knowledgeable in astronomy, could not have appreciated that Uranus was a planet about which, up to the time, no one had any knowledge; calling them "scientific" only means that they were made by a scientist, and it does not mean that a logical process was involved that is of special interest for understanding how most scientific discoveries are made. In contrast, most scientific discoveries involve calling upon some knowledge one already has for an hypothesis that might resolve a problem and, having recourse to experience thereafter, for satisfaction that it does resolve the problem. One's hypothesis determines in such cases what experience or experiences shall be deemed relevant for testing it, and also what behavior or behaviors might be suited (for example, going to Peru and making a measurement of a degree of latitude there, or doing a particular experiment with particular materials and equipment) so that the investigator is enabled to have the experience required. Consequently, if there is such a thing as a "logic of discovery", instances of scientific discovery in which investigative behavior was shaped by an hypothesis offer the most suitable paradigms for discerning what its aspects are. The three laws on dynamics which Galileo defends in the *Discourses Concerning Two New Sciences* are paradigms of this logically useful kind.

## CONTEXT AND ARGUMENT

In Galileo's youth, the consensus of opinion among physicists as to the motions of terrestrial or sublunary bodies was as follows. A terrestrial body either (1) moved straight up or straight down, depending upon the element of earth, air, fire, or water that predominated in this makeup, or (2) it moved in a different manner owing to the added influence of an external force which some other body supplied or transmitted, or (3) it was at rest, being at a place in space that its material composition disposed it to be, or being impeded from making progress to such a place by some external force. Corresponding examples are (i) the behavior of an unsupported stone, (ii) an arrow in flight, and (iii) the whole earth at the center of the universe, or a stone at rest on the surface of the earth.

Research in dynamics was concentrated on working out a satisfactory theory of *immanent force* and the manner of transmission of *external or applied force*. Thus, bodies such as flames tending upward or stones moving downward were considered to betray an immanent tendency to get to places in space where it was their material nature to belong and where it would be natural for them to be at rest if they got there. A stone would drop straight down if it were let go, owing to the same immanent tendency, but it could be made to move upward by a suitably directed blow overcoming its natural tendency, which, however, woul ultimately prevail and make the stone slow down to a stop and drop. As for arrows and similar projectiles, one problem was: what made them keep going once separated from the bow or other instrument that supplied the impulse? Did they carry a cargo of directed force that gradually was spent? Was space full of some transparent attenuated material through which the projectile had to plow and which, as the projectile did so, rushed into the places the projectile left behind, thus supplying new but progressively smaller blows until the natural tendency of the projectile body took over completely and made it come to the ground?[3]

A reader may be tempted to disdain these ideas because they are strange to him or because he knows they are now obsolete. But the logician cannot allow such considerations to influence him. They were not strange to Galileo, nor disdained by him before he made his discoveries, and their strangeness to a reader now only proves that the reader was exposed to different scientific indoctrination carrying forward not only the influence of Galileo's revolutionary work in dynamics but the influence of Newton's ideas on gravitation as well. What is significant about them for our purpose in this paper is that physicists in Galileo's youth were mainly interested in the nature, source, and manner of action of the forces accounting for the motions of terrestrial bodies.

As to the dynamical discoveries themselves, it is worthy of note that at the very beginning of *Discourses Concerning Two New Sciences*, Galileo pointedly makes it clear that he had no intention of making inquiry into the nature of the forces at work that cause unsupported stones to fall or projectiles to fly through the air. His intention, he declares, was simply to determine what is regular about the motions of falling bodies and projectiles, whatever the cause of their motion might be.[4] Some writers have read into this a manifesto that the business of science is to ascertain *how* phenomena occur and not *why* they occur. But a better interpretation seems to be that Galileo suspected that theories as to forces, immanent or external, were unsettled and unsatisfactory because they were premature.[5]

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[3] The best general work, both for its lucid presentation and its excellent bibliography of basic relevant sources and studies, on the context of scientific thinking against which Galileo's advances in dynamics took place, may be A. C. Crombie's *Medieval and Modern Early Science*, 2 vols., Garden City, New York, Doubleday, 1959 (originally *Augustine to Galileo: The History of Science A. D. 400-1650*, Cambridge, Harvard University Press, 1953). Specifically relevant are: M. R. Cohen & I.E. Drabkin, *A Source Book in Greek Science*, New York, 1948; A. Mansion, *Introduction à la physique aristotelicienne*, 2nd ed., Louvain, 1946; M. Clagett, *The Science of Mechanics in the Middle Ages*, Madison, Wisconsin, 1959; J. A. Wiesheipl, *The Development of Physical Theory in the Middle Ages*, London, 1959.

[4] "At present it is the purpose of our Author merely to investigate and to demonstrate some of the properties of accelerated motion, whatever the cause of acceleration may be...," Galilei, G., *Discourses Concerning Two New Sciences*, tr. Henry Crew and Alfonso de Salvio (Reprinted by arrangement with Northwestern University Studies in *Great Books of the Western World*, Ed. R. M. Hutchins, Vol. 28, 1952), p. 202.

[5] For example, Galileo does not hesitate to advance a causal theory to

Forces being left out of account, the variables that are significantly related in all instances of bodies in motion are three: velocity, time and distance traversed ($v$, $t$, and $s$ hereafter). The relations between these variables are simplest in the case of any body moving throughout a certain interval of time with constant velocity, i.e.,

$$s = vt$$

Though this equation expresses a law holding for bodies moving with constant velocity, it should be noted that it would be true even if, in case there were no phenomenal instances of movement with constant velocity, it were not true in physics. It expresses a truth of rational dynamics, a law that experiment is not needed to establish, since its being true is provided by definition of the bodies for which it is valid as bodies whose velocity is constant. As we shall see, it has logical importance also that the equation has *analogy* to the equation for the area of any rectangle: area = length x width, since this means that Galileo could count on his peers appreciating without debate that the rectangle lends itself for use as a graph in which the area represents distance traversed, the length time, and the width constant velocity.

It was not a problem, therefore, for Galileo or for any student of dynamics before him, what the relation of $s$ is to $v$ and $t$ in bodies moving with constant velocity. But the following was still mathematically problematic when Galileo turned his attention to it, apparently simply because the question had never been put before either as a question in physics or as a question in pure mathematics whose answer might not have a physical application: what is the relation of $s$ and $v$ and $t$ in bodies starting from rest and moving for a specified period of time with *constantly accelerated velocity?* And the problem was not just to find an expression comparable in simplicity to $s = vt$. The problem was a physical one with possible practical applications because free-falling bodies seemed to be bodies whose velocity might undergo acceleration constantly.

explain the heat of a body as function of the motions and the impact on each other of constituent miniscule bodies. Cf. Galilei, G., *The Assayer*, in *Discoveries and Opinions of Galileo*, tr. Stillman Drake, New York, Doubleday, 1957, pp. 277-278.

Galileo begins by defining constantly accelerated velocity. He defines a constantly accelerated body as a body that receives equal increments of velocity in equal times. The definition is analytically true. Maybe free-falling bodies, whose behavior Galileo hopes to understand, are not bodies whose velocity is constantly accelerated, but, if they are, then by definition they have to be bodies that receive equal increments of velocity in equal times.

Although Galileo does not immediately proceed to it, it is better for our purpose to take up at this point the first theorem he proves:

> *Theorem I*: The time in which any space is traversed by a body starting from rest and uniformly accelerated is equal to the time in which that same space would be traversed by the same body moving with a uniform speed whose value is the mean of the highest speed and the speed just before acceleration began.[6]

This is logically interesting for the geometrical analogy to $s = vt$ which has been mentioned and which it was evidently Galileo's hope to apply. It is important for understanding one logical source of inspiration for the hypothesis which, on confirmation by recourse to experiment, became the law of free-falling bodies. Otherwise, it might be pretended that we have a mathematician merely putting a geometrical problem irrespective of any application the solution might have: given a rectangle, construct a right triangle that has a common side and that is equal in area. When

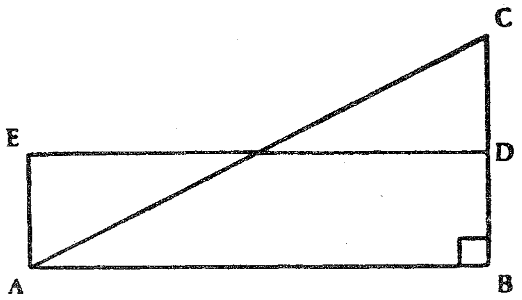

Fig. 1

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[6] *Discourses Concerning Two New Sciences*, p. 205.

the appropriate figure is drawn as follows (Fig. 1), with the common side AB representing equal times, the hypotenuse AC representing constantly accelerated velocity, and BC made double BD to represent the different final velocities required, it is easy for Galileo to prove that the areas which represent the distances traversed must be equal.

The step that has been taken establishes that the areas of right triangles may be allowed to represent distances traversed by constantly accelerated bodies in just the same way as the areas of rectangles may be allowed to represent distances traversed by bodies moving with constant velocities. But the fundamental analogy has more fertility, as the next step which Galileo takes proves:

> *Theorem II*: The spaces described by a body falling from rest with a uniformly accelerated motion are to each other as the squares of the time-intervals employed in traversing these distances.[7]

Again, geometrical elements are applicable which have analogy and with which, as mathematics, none of Galileo's peers could disagree: the areas of similar triangles are to each other as the squares of their corresponding sides and, if the corresponding sides are made to represent different times, the areas will be distances related to each other as the squares of the corresponding times (Fig. 2).

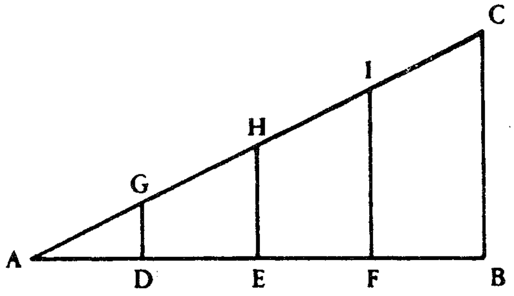

Fig. 2

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[7] *Ibid.*, p. 206.

Although we recognize in Theorem II the familiar law of free-falling bodies, it is evident that the law is nothing more at this stage than an hypothesis dependent entirely on three considerations: (1) its seeming reasonable that, whatever its nature, the force that makes an unsupported body start to fall ought to continue to be operating and ought to remain the same in magnitude so long as some other force does not interfere, (2) a definition of "continuously accelerated" that is analytically true, and (3) relevant Euclidean theorems. Galileo mentions one phenomenon that appeared to support the first consideration, *viz.*, that a body falling from a greater height (and therefore taking more time to fall) drives a stake farther into the ground than the same body falling from a lesser height, but that is all the experience invoked and by itself it would not support what the theorem precisely claims. (Indeed, he mentions that this phenomenon at first made him think that the velocities of a free-falling body at different times in the course of its fall varied directly as the distance fallen, so that at double the distance it was going twice as fast, at triple the distance three times as fast, etc., which is a notion entirely antithetical to the concept of continuous acceleration due to an identical constantly operating force).[8] There is no recourse yet to experiment of any kind that might be said to constitute a test of his hypothesis, and at this point it is only Galileo's expectation that, when there is recourse to experiment, this will show that his hypothesis, so far only mathematically true, is materially true.

Moreover, recourse to experiment is not easy because Galileo's theorem pertains to the behavior of unsupported bodies, the characteristics of whose falling are due exclusively to the unknown force that makes them start to fall, a condition that requires other forces such as wind, atmosphere, or contact with other bodies to be somehow isolated. How was this to be done? The vacuum pump had not yet been invented, and one widespread physical doctrine, suggested earlier in reference to theoretical speculation as to the motion of projectiles, maintained that space was a plenum and that breaches in this plenum were not naturally possible (nature abhors a vacuum). Besides which, bodies falling from any height, whether affected by the atmosphere or not, fall too fast for the ratios of the distances traversed at different times to have

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[8] *Ibid.*, p. 203.

been ascertainable directly with instrumentation available in Galileo's time.

Galileo's answer to these experimental difficulties is a classic example of both direct and indirect experiments calculated to show that impure experimental examples could nevertheless provide strong evidence of the truth of an hypothesis for the pure but experimentally unattainable case. For instance, instead of using bodies dropped from a certain height and plummeting straight down, he resorts to bodies rolling down inclined planes after being let go at the top, because these bodies must owe their commencing to roll down and their increasing velocity to the action of the same force hypothecated to account for the behavior of bodies that have merely been dropped. The inclination of the plane can be varied so as to change the magnitude of the equal increments of velocity in equal times for which the hypothecated constant force is supposed to be responsible; that is, the progress of the rolling body could be made advantageously slow by making the angle of incline small enough, but the ratios of the distances traversed should be the same, whatever the angle of inclination, so that the hypothesis can thus be tested for the ideal case, *viz.*, vertical fall itself. A second example is the sort of experiments, conceptual and actual, to which Galileo resorts to test the *law of inertia* implied in his hypothesis. By his hypothesis, a body acted on constantly by the same force that made it commence to fall or roll down in the first place can only receive equal increments of velocity in equal times so long as no other force operates. Therefore, it cannot lose, but must conserve, velocity already acquired and, in case at some moment the single force were suddenly and completely withdrawn, the body must thereafter move constantly with the velocity and in the direction it had at the moment. In connection with both the hypothesis for free-falling bodies and the notion of inertia involved, the experiments which Galileo suggests that the reader must complete with him *mentally* are, as other writers have observed, remarkable.

First, imagine a string (Fig. 3) suspended from a nail at A, with a lead bullet tied to it at other end B. On the wall behind the string, draw a horizontal line CD that subtends the arc CBD which the bullet would describe if it were set swinging. Now imagine a nail driven into the wall at E so that when the bullet is moved to point C and let go the thread is intercepted at E. Then the arc described by the bullet after it reaches B is changed from BD to BG. Next, if a nail is placed at F, the arc described by the bullet after it reaches B is changed to BI. And, finally, if a nail is placed at a point J, the string, upon interception by it, winds itself around the nail. And, Galileo concludes, the fact that the points D, G, and I all lie on the same line shows that the momentum of the bullet at B at each of the trials was such as to make the bullet travel upward along any arc of no matter what radius so long as the arc had the same height as the height of the bullet's fall. Here Galileo means to show that balls made to roll down planes of different inclination would have the same final velocity at the end of their respective planes so long as the planes are of equal height, and he means to show that the force which makes them acquire that velocity is the same force that causes them to diminish in velocity to zero as they rise up planes of equal height. The advantage of using the pendulum instead of oppositely placed planes of equal height is that it enables him to eliminate the effect of such influences on acceleration as a bump at the point where the bottom of one inclined plane touched the bottom of the other. With the exception of friction from the atmosphere, which explained for him why the bullet did not *quite* reach the points D, G, and I, he could be satisfied that the final velocity acquired by the bullet at B would be conserved but for

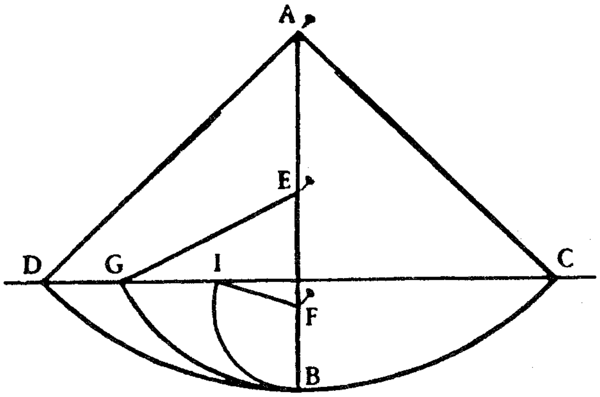

Fig. 3

the continued action of the same force upon the bullet as it turns and travels upward to D, G, or I.

Similar considerations govern Galileo's reasoning as to the behavior of a ball coming off an inclined plane on to a horizontal one. If the horizontal plane could be made infinitely long and perfectly straight, the velocity and direction of the ball after the bump (and diminishment of velocity) it undergoes upon meeting the horizontal plane should be constant, except for the influence of friction with the air through which the ball must pass. Galileo could not remove the air, and infinitely long and perfectly straight planes exist only in the world of Euclidean geometry. So Galileo proceeds indirectly by exploiting the idea that the same momentum acquired during fall down the same inclined plane should make the same ball go longer and longer distances on the horizontal plane as residual unevenness on the latter plane is reduced by more and more lapping and polishing. The analogue to this with the pendulum would be to let the bullet traverse the arc from C to B and imagine that at point B the bullet is separated from the string as it moves on to a horizontal plane tangent to B.

The result of these mental experiments is that Galileo is now prepared to justify using a ball rolling down an inclined plane in order to test his hypothesis as to the ratios of the distance traversed by free-falling bodies in equal times. He cannot remove forces checking acceleration such as air; he can only diminish, not wholly remove, other forces that would do the same, such as the unevenness of any surface he might use; and finally he cannot actually test his hypothesis with bodies whose fall is vertical. Nevertheless, he can still now claim that if he finds his hypothesis to be true for impure instances of bodies in free fall, such as balls rolling down inclined planes, it will also have been proved true for the pure, ideal, and experimentally unattainable case of bodies plummeting through absolutely empty space.

For the actual laboratory experiment Galileo used a board several yards long on which he had made a straight smooth groove from one end to the other. For each angle of inclination of the board he then determined with notches the distance expected to be traversed by a smooth brass ball in one unit of time, in two, in three, etc., in every case verifying that the distances did vary as the squares of the number of units of time elapsed. It is understandable if this result is anticlimatic for the logician: the reasoning is over.

Galileo next attacked the question of the path taken by projectiles. Its being answered required no further experimenti but simply deduction from the law of inertia and the law of free-falling bodies that Galileo had already proved experimentally to his satisfaction. The law of inertia guaranteed that if a body at rest on a horizontal plane were set in motion on the plane by a blow and if no forces whatever acted upon the body thereafter its velocity and direction would be conserved or unchanged. And the law of free-falling bodies provided that if the support provided by the horizontal plane should be withdrawn another constantly acting force would betray its own influence causing the body to fall in such a way as to traverse distances varying as the squares of the times. Consequently, the body in our example has to be imagined to be moving with two velocities and in two directions at once: a velocity which, being constant and not influenced by any other force, causes the body to cover equal distances in equal times horizontally, and a velocity which, increasing by equal amounts in equal times owing to a constantly acting force, causes the body to fall so as to cover distances varying as the squares of the times. The result, Galileo takes pride in announcing, is that the loci of the points constituting the path of the projectile must necessarily be related to each other as the loci of the points that constitute the parabola are known to be. It is sheer deduction of a conclusion from premises known to be true because they have been tested experimentally, hence a conclusion of whose truth, as well as logical validity, Galileo has the right to believe that we can feel confident.[9]

## CONCLUSION

Some of the particular elements of logical activity that evidently were influential for Galileo's persuasion that contemporary dynamical doctrines required important change have been noticed in course. Conspicuously noteworthy, however, are logical lessons about the peculiar relationship of pure mathematics to phenomena and about the determining role of scientific tradition in connection with discoveries that even subvert scientific tradition and

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[9] *Ibid.*, pp. 238-240.

change it in important ways. The ancient Greek mathematicians whose work on conic sections led to the discovery of the ellipse and the parabola were conscious only of having made discoveries in pure mathematics and could not have guessed that their formulas for these curves needed only to be known to men immersed in empirical studies like Kepler and Galileo to be changed into descriptions of important phenomena. It is also clear, from this example, that a rigid standard of practical usefulness, applied for deciding what should and what should not be investigated, what should and what should not be preserved and transmitted, what should and what should not be taught and learnt, overlooks the role that complacency and ignorance of the future must inevitably play in setting up the standard.

In Plato's *Republic,* Socrates, when challenged to show that his description of the ideal state is true, replies that the ideality and actual unattainability of the state that has been described does not disturb him because truth is of such a sort that theory must always come closer to truth than practice.[10] The powerful appeal of such a point of view with respect to the nature of scientific truth cannot be denied when we remember examples of scientific work like the experiments which Galileo asks us to cooperate in completing with him mentally, since they were experiments that evidently could not be completed in any other way. And this appeal is enhanced when we consider how often in the history of science mathematical ideas and formulas developed independently of experience have needed to be applied to phenomena to make phenomena yield their scientific secret or significance. These remarks are worth making because they bear importantly on the directive role of hypotheses in scientific investigations and on the resources of logic, imagination, and antecedent experience and knowledge of every kind that are called upon in the invention of hypotheses.

One might hope for more from a natural history of Galileo's mental development pertinent to the discoveries that have been discussed, had Galileo written such a thing instead of the *Discourses Concerning Two New Sciences.* But, unless this were merely psychologically interesting, it is hard to see that insights of any use to the logician would be different *in kind.*

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[10] *The Republic,* tr. G.M.A. Grube, Indianapolis, 1974, Bk. V, 273a, p. 132.