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Galileo and the Concept of Inertia

The origin of the law of inertia, and Galileo's role in it, involve questions more intricate than is generally supposed, being still subjects of study and debate today. And there are few questions more fascinating in the whole history of physics. One need only try to conceive of a science of physics without the concept of inertia in order to perceive that the introduction of this fundamental notion must have produced a revolution in physical thought as profound as any that have occurred since. One can then hardly fail to wonder who it was that introduced this idea, and how it came to occur to him. I shall try to indicate roughly the part that Galileo played in this great revolution in physical thought.

Newton implied in his *Principia* that Galileo, being in possession of the first two laws of motion, thereby discovered that the descent of falling bodies varied as the square of the elapsed time.[1] Historically, and biographically with regard to Galileo, this remark leaves much to be desired, though it is interesting autobiographically; that is, as a clue to Newton's own source of the concept of inertia and to his method of thought. Because Newton easily perceived that the law of free fall followed directly from a correct understanding of his first two laws together with the assumption that gravity exerted a constant force, it was natural for him to assume that his great Italian predecessor had actually made these discoveries in that orderly fashion. In point of fact, however, Galileo arrived at the law of free fall long before he gave any explicit statement of his restricted law of inertia, and though he was first to recognize the true physical significance of acceleration, he never did formulate the force law. With Galileo's law of free fall, as with Kepler's laws of planetary motion, Newton was able to produce mathematical derivations and demonstrations of results that

his predecessors had derived only from long study of sometimes chaotic observational data, assisted by flashes of insight rather than by mathematical or even logical deduction. It is interesting that Galileo had already perceived this to be the normal order of events in science; in reference to one of Aristotle's ideas and its proof, he wrote:

> I think it certain that he first obtained this by means of his senses, by experiments and observations . . . and afterwards sought means of proving it. This is what is usually done in the demonstrative sciences. . . . You may be sure that Pythagoras, long before he discovered the proof for which he sacrificed a hecatomb, was sure that in a right triangle the square on the hypotenuse was equal to the squares on the other two sides. The certainty of a conclusion assists not a little in the discovery of its proof.[2]

The historical question is whether and to what extent Galileo is entitled to credit for the anticipation of Newton's first law of motion. Technical priority for the first complete statement of the law of inertia belongs to Descartes, who published it in 1644, two years after Galileo's death, supported by a philosophical argument. But if Galileo never stated the law in its general form, it was implicit in his derivation of the parabolic trajectory of a projectile, and it was clearly stated in a restricted form (for motion in the horizontal plane) many times in his works. A modern physicist reading Galileo's writings would share the puzzlement—I might say the frustration—experienced by Ernst Mach a century ago, when he searched those works in vain for the general statement that (he felt) ought to be found there. It would become evident to him, as it did to Newton and Mach, that Galileo was in possession of the law of inertia, but he would not then be able to satisfy those historians who demand a clear and complete statement, preferably in print, as a condition of priority.

To physicists, if not to historians, it is ironical that this particular law should be credited to Descartes, whose physics on the whole operated to impede the line of scientific progress begun by Galileo and continued by Newton. They both possessed in a high degree one special faculty that Descartes lacked; that is, the faculty of thinking correctly about physical problems as such, and not always confusing them with related mathematical or philosophical problems. It is a faculty rare enough still, though much more frequently encountered today than it was in Galileo's time, if only because nowadays we all cope with mechanical devices from childhood on. In Galileo's day, thinking was not continually brought to bear in this (or any other) way on mere physical processes. There were of course skilled technicians, craftsmen, and engineers, but their impressive achievements had been derived rather from the accumulation of practice and tradition than by the deductive solution of physical problems. Thinkers as a class, then comprising roughly the university population, were concerned mainly with medicine, law, theology, and philosophy. Formal instruction in physics consisted largely in the exposition of Aristotle.

A cardinal tenet of Aristotle's physics was that any moving body must have a mover other than itself, and since this notion also appeals to experience and common sense, it stood as a formidable obstacle to the discovery of the principle of inertia. It was difficult, of course, to explain under Aristotle's rule the continuance of motion in objects pushed or thrown. The first man to override that rule and to suggest that a force might be impressed on a body, and endure in it for some time without an outside mover, was probably Hipparchus. This idea was developed and advocated by Johannes Philoponus, a brilliant sixth-century commentator on Aristotle. During the Middle Ages a few daring philosophers developed this thesis further into the concept of *impetus,* largely in opposition to *antiperistasis,* an idea mentioned (though not clearly accepted) in two forms by Aristotle. This was the view that the separate mover for a projectile is the medium through which it travels. Aristotle preferred the form in which it is supposed that the mover of the object imparts to the medium the power of continuing its motion. In the other form, the argument was this: As a body moves, it tends to create a vacuum in its wake; since nature abhors a vacuum, the surrounding medium rushes in, striking the object from behind and thus impelling it further. It is easy to see why the theory of impetus gained ground against that of antiperistasis—which was ridiculed by Galileo, incidentally, in virtually the same manner as that which we should employ today. After his time little more was heard of it, or of the medieval impetus theories that had never entirely succeeded in displacing it.

From the end of the nineteenth century until recent years, historians of science tended to regard Galileo's inertial concept as a natural and logical outgrowth of medieval impetus theory. That view is now undergoing review and modification, thanks to a more careful analysis of the actual writings of medieval philosophers, as well as to the complete accessibility of Galileo's own papers, including his long unpublished early studies on motion and mechanics. Indeed, if inertia were nothing more than a simple and logical outgrowth of impetus theory, developed and debated over a period of several centuries by astute philosophers, then this outgrowth might be expected to have developed much earlier in the game as a way out of various difficulties inherent in impetus theory that conservative Aristotelians had always been quick to point out. The belief that inertia grew naturally out of an earlier theory was plausible, as we shall see, but turns out to have been historically unsound. It is at best a half-truth, and as Mark Twain said, a half-truth is like a half-brick; it is more effective than the whole thing because it carries further. I shall try to put matters in a new perspective by explaining the sense in which impetus theory opened a road for acceptance of the law of inertia, though it did not thereby suggest that law, and by indicating the steps that Galileo actually took in arriving at his concept of inertia, which went along a quite different road.

Aristotle's idea that every motion requires a moving force, and ceases when that force stops acting, appeals to common sense because it is roughly borne out by experience. In most cases the relatively short persistence of motion in an object after the propelling action has ceased is not nearly so impressive as the effort required to set or even to keep the object in motion. Perhaps that is why many philosophers did not feel the need of any stronger force to account for it than some fanciful action of the medium. But there were other continued motions which could not be explained in that way at all; for example, that of a grindstone. This not only persisted in free rotation for a long time, but strongly resisted efforts to stop it, and in such cases no explanation in terms of a push from air rushing into any vacated space could apply. That in turn suggested that circular motions might be exceptions to Aristotle's dictum, an idea that fitted in rather well with his general scheme of things in which perfection and a variety of special physical properties were attributed to circles and circular motions of various kinds. This scheme had helped Aristotle to explain the motion of the heavenly bodies. For him, the earth was at absolute rest in the center of the universe, and the only motions appropriate to earthly bodies were straight motions up and down. Circular motion, being perfect, belonged naturally to the perfect heavenly bodies. To the fixed stars, Aristotle assigned a special source of motion called the Prime Mover. In later times some attempt was made to explain their regular rotation by analogy to the grindstone, since the stars had then come to be considered as embedded in a solid transparent crystalline sphere. Thus, without great violence to Aristotelian orthodoxy, the conservation of angular momentum could be conceived as a natural phenomenon in which Aristotle's outside mover might be replaced by an impressed force, which normally wasted away with time or through external resistance. In the special case of the stars, loss of motion could still be offset by action of the Prime Mover, or eliminated by postulating the absence of resistance to motion in the heavens, as was done by several philosophers.

While the strictest Aristotelians continued to oppose any postulation of impressed forces, more enterprising philosophers went on to extend this concept to the case of projectile motion under the general name of *impetus.* Loss of impetus by projectiles was likened to other familiar phenomena requiring no special explanation, such as the diminution of sound in a bell after it is struck, or of heat in a kettle after it is removed from the fire, and this accorded with a further important rule of Aristotle's that nothing violent can be perpetual. Hence to the extent that impetus theory paved the way for eventual acceptance of the idea that motion might be perpetually conserved, it rested on a philosophical basis that inhibited the taking of that ultimate step. Not only was an indefinitely enduring impressed force unnecessary to experience, but it was ruled out in theory by Aristotle; and to reconcile a theory with Aristotle's opinions was at that time just as important as to reconcile it with experience. Thus impetus theory, given its philosophical context, did not lead on to inertial physics; rather, it precluded the need for that so long as physical judgments remained qualitative and were not replaced by quantitative measurements. And that brings us to the time of Galileo.[3]

It should be remarked in passing that no fundamental revolution in science takes place until the way has been paved, usually by vague or incorrect notions, for the acceptance of a radically new idea. Hence the new idea, when it comes, is very likely to have the appearance of a natural extension of the old ones. That is why it was perfectly plausible for historians of science to suppose, when medieval impetus theory was first brought to their attention, that the inertial concept had originally arisen as a natural outgrowth of that theory. And indeed, it might have arisen so, in a different philosophical context. But the actual road to the first announcement of the inertial principle, through its first physical application, was not the same road at all as the one which had led people to a point at which they would be able to accept the new idea of indelibly impressed motion as the limiting case of lingering impressed motion.

The most objective summary of the relation of medieval impetus theory to inertia in the sense of modern physics has been given by Miss Annaliese Maier, as follows:

> Thus the situation around 1600 was that impetus theory was taken over by the official scholastic philosophy as such. That of course does not mean that there were not isolated supporters of the Aristotelian theory; nor, on the other hand, does it mean that orthodox philosophy on its part stood against impetus theory and forbade it. And here it was expressly contrasted with the Aristotelian view and exhibited in the earlier way. Among those who still clung thus to impetus theory in the sixteenth century, and who for the rest opposed those who bore the Aristotelian stamp, belong Telesio, . . . Bruno, . . . Benedetti, . . . and finally Galileo. For them, impetus theory was mainly a polemic point against Aristotelianism. It was this group alone that Duhem took into consideration, and those who follow his view. The result was naturally a not entirely accurate picture of the factual historical situation. Impetus theory at the end of the sixteenth century was seen as a resumption of the great revolution in thought of the fourteenth century, that was just having its full weight and full influence against Aristotelianism; and it was further thought that the new mechanics had developed in a straight line out of that impetus theory. But that is not how things happened.[4]

Miss Maier goes on to say that whereas in impetus theory each motion impresses an inhering moving force on the body moved, the inertial concept sees in uniform motion, as in rest, a state that is conserved so long as it is undisturbed; whereas according to impetus theory the moved body has an inclination to return to rest and thus opposes the inhering force, according to the inertial concept the inclination in the case of uniform motion is to continue with no resistance on the part of the body; and whereas impetus theory would allow for an acting force in circular as well as straight motion, the inertial concept postulates only a continuance of the latter. This last point, she says, is of secondary importance; the essential differences between the concepts of impetus and inertia are the two first named, for these are in direct conflict with the Aristotelian principle that everything moved requires some mover; "and this contradiction is so sharp that the new thought could develop not *out of* the old, but only *against* it."[5]

We may safely take Miss Maier's summary as definitive with respect to the historical aspect of the relation between impetus theory and the inertial concept. Grounded as it is on the most thorough analysis of the writings of the scholastics and of modern students of this problem, it comes as a welcome conclusion to a series of controversies that related rather to the theory of history than to the ostensible subject matter, and it is unlikely to be seriously modified by further investigations. But as to Miss Maier's comments on the conceptual or philosophical aspects of the matter, though I thoroughly agree with them, it must be admitted that they are by their nature less apt to receive universal acceptance. The late Professor Alexandre Koyré, for instance, took quite a different position; and though his main writings on the subject preceded those of Miss Maier, I do not believe that he was inclined to modify them after hers appeared. Here, for example, is an indication of his fundamental difference of opinion:

> Now, . . . if Galileo's dynamics is, at its deepest base, Archimedean and founded entirely on the notion of weight, it follows that Galileo could not formulate the principle of inertia. And he never did. In fact, in order to be able to do so—that is, in order to be able to affirm the eternal persistence not at all of movement in general, but of *rectilinear* movement; in order to be able to consider a body left to itself and *devoid of all support* as remaining at rest or continuing to move *in a straight line* and not *in a curved line*—he would have had to be able to conceive the motion of fall not as natural motion at all, but on the contrary . . . as caused by an external force.
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> Thus, throughout the *Dialogue,* impetus is found identified with *moment,* with movement, with speed . . . successive glides [of meaning] that insensibly lead the reader to conceive of the paradox of motion conserving itself all alone in the moving body; of a speed "indelibly" impressed on the body in motion.
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> In principle, the privileged situation of circular movement is destroyed; it is movement as such that is conserved, and not circular motion. In principle; but, in fact, the *Dialogue* never goes so far. And as was said, we never glide, nor ever will, to the principle of inertia. Never; no more in the *Two New Sciences* than in the *Dialogue,* does Galileo affirm the eternal conservation of rectilinear motion.[6]

Clearly, to Professor Koyré it would by no means be acceptable to say that the third of Miss Maier's distinctions between impetus theory and the inertial concept is of secondary importance; for him, the limitation of the inertial concept to uniform rectilinear motions was every bit as important as the recognition of continuance in a state of rest or motion by a body otherwise undisturbed. Now, opinions will always differ concerning the aspects of any concept which are to be considered essential and those which are secondary or subordinate. My opinion, like Miss Maier's, is that the essential aspect of the concept of inertia is that of motion and rest as states of a body which are indifferently conserved. If the disagreement ended there, all would be well. But Professor Koyré went on to argue at length that because Galileo asserted that truly straight motions are impossible in nature, *only* circular motions could for him be really perpetuated, and hence that the inertial concept was inextricably linked in Galileo's own mind with privileged circular motions. The prevalence of this view is illustrated by the following passage from a work by Professor E. J. Dijksterhuis:

> The situation is thus as follows: according to the Galilean law of inertia proper, a particle that is free from external influences (note that gravity is not included among them) perseveres in a circular motion having the centre of the earth for its centre. Over short distances this motion is considered rectilinear; subsequently the limitation to short distances is forgotten,

and it is said that the particle would continue its rectilinear motion indefinitely on a horizontal plane surface if no external factors interfered. Thus what might be called the circular view of inertia of Galileo gradually developed into the conception that was formulated in the first law of Newton.[7]

The imputation to Galileo of a view in which all inertial motions are essentially circular, rather than one in which some cases of inertial motion are illustrated by bodies supported on a smooth sphere concentric with the earth, seems to me wrong; this aspect of the question is discussed in the final essay. The present problem is that of reconstructing the steps taken by Galileo in arriving at the views he held concerning conservation of motion by an examination of all his writings. If my reconstruction is correct, it has very different implications for the history of science than the prevailing opinion as expressed by Professors Koyré and Dijksterhuis. For the first clue to conservation of motion in Galileo's thought had nothing to do with the question of projectile motion, and circularity entered into it because of angular momentum rather than translatory motion.

During his first years as professor at the University of Pisa, Galileo wrote a treatise on motion that he intended to publish. In that treatise, which survives in manuscript, he attacked Aristotle boldly, often in favor of ideas originated by medieval philosophers. He opposed various Aristotelian notions about the role of the medium, including that of antiperistasis, but at the outset he adopted the Aristotelian division of all motions into "natural" and "violent" motions. This concept was, however, somewhat modified from Aristotle's in the definition adopted by Galileo for the dichotomy; he said, "There is natural motion when bodies, as they move, approach their natural places, and forced or violent motion when they recede from their natural places."[8]

Without going into detail, Aristotle's physics may be described as a theory of natural places, high for light bodies (fire and air), low for heavy bodies (earth and water). Bodies were supposed to seek these natural places by an occult property inherent in them. Galileo dissented from this view; for him, all bodies were heavy bodies and differed only in density, so all tended to approach the center of the universe, which at that time was for him the center of the earth.

But as he pursued this idea in his treatise on motion, he came to question whether all motions *were* either natural or violent. He perceived that a body might be moving, and yet be neither approaching nor receding from the center of the earth; and he reasoned that any body rotating on that center itself would be moving neither naturally nor violently—in contradiction of Aristotle, who allowed no third possibility except "mixed" motion. Galileo went on to show that any rotating homogeneous sphere, wherever situated, would also have this un-Aristotelian kind of motion (assuming no friction of its axis with its support), since for every part of that sphere which was approaching the center of the earth at a given moment, an equal part would be receding from it; thus the sphere as a whole would be moving, but neither naturally nor violently. Others before Galileo had pursued similar reasoning. But Galileo arrived in this way at the idea that there was a third kind of motion which was not a mere mixture of the other kinds, as his predecessors had supposed.

This recognition of a special kind of motion was Galileo's first essential step toward the concept of inertia. For in the same treatise, analyzing the force required to maintain a body in equilibrium on an inclined plane, Galileo concluded that horizontal motion of a body on the earth's surface would similarly be neither natural nor violent, in the sense of Aristotle, and in a note added to this section he said that this should be called a *neutral* motion. He then went on to prove that, in theory at least, any heavy body could be moved on a horizontal plane by a force smaller than that required to move any body upward on any other plane, however gently inclined, but he was careful to add that:

> Our proofs, as we said before, must be understood of bodies freed from all external resistance. But since it is perhaps impossible to find such bodies in the realm of matter, one who performs an experiment on the subject should not be surprised if it fails; that is, if a massive sphere, even on a horizontal plane, cannot be moved by a minimum force. For in addition to the causes already mentioned, there is the fact that no plane can be actually parallel to the horizon, since the surface of the earth is spherical. . . . And since a plane touches a sphere in only one point, if we move away from that point, we shall have to be moving upward. So there is good reason why it will not be pos-

sible to move a (massive) sphere from that point with an arbitrarily small force.[9]

The manuscript treatise from which this is quoted was written about 1590, but Galileo did not publish it. He was dissatisfied with it for various reasons; principally, in my opinion, because he had tried in this treatise to account for the observed speeds of bodies along inclined planes by a formula for their equilibrium conditions, and in this he had not succeeded, by reason of a misconception of accelerated motion which is irrelevant to the subject of the present study. But in the process he had found a new approach to physics in his concept of a "neutral" motion. It is important to note that the origin of this concept was in no way related to impetus theory, with which Galileo was perfectly satisfied at the time, as shown in his chapter on projectiles.

In 1592 Galileo moved to the University of Padua, where he continued or resumed his observations of motion on inclined planes and in the pendulum. Here he wrote a little treatise on mechanics for his private pupils, which he also left unpublished. Dealing with the inclined plane in this new work, he wrote:

> On a perfectly horizontal surface, a ball would remain indifferent and questioning between motion and rest, so that any the least force would be sufficient to move it, just as any little resistance, even that of the surrounding air, would be capable of holding it still. From this we may take the following conclusion as an indubitable axiom: That heavy bodies, all external and accidental impediments being removed, can be moved in the horizontal plane by any minimal force.[10]

From these two propositions, written not later than 1600, Galileo can hardly have failed to deduce the corollary that horizontal motion would continue perpetually if unimpeded. This second essential step in his progress toward the principle of inertia was not explicitly stated until several years later. Yet Galileo must have been teaching it in his private classes, for one of his pupils, Benedetto Castelli, who had left Padua some time before, wrote to Galileo in 1607 mentioning "your doctrine that although to start motion a mover is necessary, yet to continue it the absence of opposition is sufficient."[11]

Galileo's ideas on these matters were probably not widely known until 1613. In that year he published a book on sunspots, in which he argued (among other things) for the sun's axial rotation, and as a preliminary to that argument he wrote:

> I have observed that physical bodies have an inclination toward some motion, as heavy bodies downward, which motion is exercised by them through an intrinsic property and without need of a special external mover, whenever they are not impeded by some obstacle. And to some other motion they have a repugnance, as the same heavy bodies to motion upward, wherefore they never move in that manner unless thrown violently upward by an external mover. Finally, to some movements they are indifferent, as are heavy bodies to horizontal motion, to which they have neither inclination . . . nor repugnance. And therefore, all external impediments being removed, a heavy body on a spherical surface concentric with the earth will be indifferent to rest or to movement toward any part of the horizon. And it will remain in that state in which it has once been placed; that is, if placed in a state of rest, it will conserve that; and if placed in movement toward the west, for example, it will maintain itself in that movement. Thus a ship . . . having once received some impetus through the tranquil sea, would move continually around our globe without ever stopping . . . if . . . all extrinsic impediments could be removed.[12]

In my opinion the essential core of the inertial concept lies in the ideas, explicitly stated above, of a body's indifference to motion or to rest and its continuance in the state it is once given. This idea is, to the best of my knowledge, original with Galileo. It is not derived from, or even compatible with, impetus theory, which assumed a natural tendency of every body to come to rest.[13] It is noteworthy that this first published statement of a true conservation principle was used by Galileo to support an argument for the conservation of angular momentum, in this case by the sun, as described previously. He seems always to have continued to associate the phenomena of inertia and of conservation of rotatory motion, which has led most historians to believe that Galileo's inertia had a circular quality. In my opinion, the association had quite another basis; namely, the linkage in Galileo's mind of these two phenomena by the unifying concept of a "neutral" motion which had first led him to an inertial principle.

Statements relating to inertia in Galileo's later books, the *Dialogue* of 1632 and the *Two New Sciences* of 1638, are more elaborate than the above but are essentially repetitions of it. His argument always proceeds from a consideration of motion on inclined planes to the limiting case of the horizontal plane, where motion once imparted would be perpetual, barring external obstacles or forces. The *Dialogue* is of particular interest for a long section dealing with the motion of a projectile, in which it is made clear that this motion would be rectilinear if it were not for the immediate commencement of the action of the body's weight, drawing it downward as soon as it is left without support. But Galileo never gave a statement of the law of inertia in the form and generality which we accept today. That was done by Descartes shortly after Galileo's death.

Because of the fundamental significance of the inertial concept to the later development of celestial mechanics, I should like to stress its importance in Galileo's arguments in favor of the Copernican theory and to mention some implications that have been drawn from that use of it. A strong objection to Copernicus in those days was that if the earth rotated at the rate of a thousand miles an hour, then any body separated from the earth, such as a bird or a cannonball or an object falling freely from a high place, would be rapidly displaced westward from an observer stationed on the earth. Copernicus had offered as a possible explanation for the absence of such effects some natural tendency of terrestrial bodies to share in the earth's motion wherever they were. But this was not widely accepted, and to Galileo it was no better than those "occult properties" invoked as explanations by the very philosophers against whom he contended. In the *Dialogue* he replaced this explanation by giving numerous examples of inertial motion, such as that of a ball dropped by a rider on horseback, and he refuted the idea that an object dropped from the mast of a moving ship would strike the deck farther astern than on a ship at rest. These arguments, based on observations that anyone could duplicate and supported by correct physical reasoning, did much to gain a fair hearing for the Copernican system from his contemporaries.

But here arises the problem in assigning Galileo's precise role in the discovery of the inertial principle. In the *Dialogue* Galileo sometimes spoke as though the inertial motions of bodies leaving

the earth's surface were itself circular, causing historians to question whether he himself fully understood the rectilinear character of translatory motion. Some have gone so far as to say that Galileo believed the planetary motions to be perpetuated by a sort of circular inertia. Leaving those views for the final essay, I wish here to point out the primarily strategic character of the passages from which they derive their only support.

Passages on projectile motion in the *Dialogue*[14] and the derivation of the parabolic trajectory in the *Two New Sciences*[15] show that Galileo as a physicist treated inertial motions as rectilinear. Nevertheless, Galileo as a propagandist, when writing the *Dialogue,* stated that rectilinear motion cannot be perpetual, though circular motion may be. In the same book he ascribed some special properties almost metaphysically to circles and circular motions.

The passages in question occur mainly in the opening section of the book entitled *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican,* and they should be construed in the light of the purpose for which that book was written. It was not written to teach physics or astronomy, but to weaken resistance to the Copernican theory, and it was very effective in doing so. For Galileo was not only an outstanding scientist, but also a first-rate polemicist and a writer of exceptional literary skill and psychological insight. He knew when he wrote the *Dialogue* that strong opposition could be expected from the professors of philosophy, most of them convinced Aristotelians. It was for that reason, I believe, that in the opening section of his book he deliberately conceded (or appeared to concede) to the philosophers everything he possibly could without compromising his one objective. This is still the best way to proceed if you wish to espouse an unpopular view. Accordingly, when I read the metaphysical praise of circles in the *Dialogue,* I do not conclude with most historians that its author was unable to break the spell of ancient traditions; rather, I strongly suspect an ulterior purpose in those passages. This suspicion is confirmed when I read his other books and his voluminous surviving correspondence and find nowhere else any trace of metaphysics about circles. On the contrary, Galileo often scoffs at such ideas; thus in his *Assayer,* published in 1623, he expressly denied that any geometrical form is prior or superior to any other, let alone that any shape is perfect, as Aristotle had claimed for the circle.[16] It is not likely that he had changed that opinion by 1632, as he was nearly sixty years of age before he published it in 1623.

All that Galileo wanted to accomplish in the *Dialogue* was to induce his readers to accept the ideas set forth by Copernicus, and Copernicus had placed the planets, including the earth, in circular orbits around the sun. Galileo probably knew better, having read at least the preface of Kepler's *Astronomia Nova,* from which he gained acquaintance with Kepler's tidal theory. But it was hard enough for him to get acceptance in Italy of any motion for the earth, and for his immediate purpose it would have been fatal to argue for an elliptical orbit. It was far better strategy for him to ennoble the circle, using arguments extracted from Aristotle himself, and to argue that circular motion was as suitable to the earth as to the heavens, if he wanted to win over or even neutralize any philosophers. And I can see in my mind's eye some of them starting to read the *Dialogue* for no other purpose than to find and answer hostile arguments against Aristotle, and then in the first forty or fifty pages finding themselves so much at home as to wonder whether there might not be some merit in the other ideas of so sound a writer.

That, in my opinion, is precisely what Galileo was up to when he composed those opening pages, balancing his adverse criticisms of Aristotle's physics with approving extracts from his metaphysics about circles. Galileo certainly did not state or believe that the celestial motions would perpetuate themselves merely by being circular in form, but this does not mean that he was averse from letting the philosophers believe that if they wished to. If Galileo had held such an opinion, he would not have hesitated to declare it; I see little point in looking for hidden beliefs behind the words of a man who spent his last years under arrest for disdaining to conceal his convictions. And the fact is that in the *Dialogue* itself, Galileo declared that he had no opinion about the cause of the planetary motions, but went on to say that if anyone could tell him the cause of gravitation, he could then give a cause for those motions.[17]

The amusing thing is that in saying that no rectilinear motion can be perpetual, Galileo was on sounder ground than are many of those who now criticize his having said it. Galileo's modern critics seem still blissfully unaware that in the last fifty years a question

has arisen about the meaning, the nature, and even the existence of straight lines in the physical universe. Perhaps the general law of inertia is tautological; perhaps it is our only definition of "straight motion." Certainly it has lost the absolute physical character with which those critics still invest it. Nor do Galileo's critics seem to realize that physicists now question the infinite extent of the universe, an attribute which would be a necessary condition of "perpetual straight motion" in the sense in which Galileo denied its possibility. For Galileo has been criticized both for his failure to declare the universe to be infinite, and for his denial of the possibility of perpetual straight motion—as if such views destroyed the modernity of his physics.

Galileo formulated at most a restricted law of inertia, applicable only to terrestrial bodies. Perhaps this too is a tribute to his modernity as a physicist, for there is an advantage in refusing to generalize beyond the reach of your available experimental evidence. That advantage is that four hundred years later, your restricted statement will still be true, while the speculations of your more daring colleagues may have gone out of date. Galileo's restricted law of inertia, applying only to heavy bodies near the surface of the earth, was in a sense all that was needed or justified in physics up to the time of Newton's discovery of the law of universal gravitation. Any speculation by Galileo about the behavior of bodies in interstellar space would at his time have been essentially meaningless metaphysics—the very sort of philosophizing that he had undertaken to replace with a science of physics. With the advent of Newtonian gravitation, inertia was freed from the terrestrial bonds imposed on it by Galileo and purged of the speculative character given to it by Descartes; for the first time, inertia became a universal law of physics.

Notes to Essay 12

1. Cf. Newton, *Principia,* ed. F. Cajori (Berkeley, 1947), p. 21.
2. *Dialogue,* p. 51.
3. Differences between Buridan's suggestion that conservation of angular momentum might persist forever in the absence of resistance,

and the more widespread conception that impetus always wasted away, may be neglected here for reasons that will presently appear.
4. A. Maier, *Zwei Grundprobleme der scholastischen Naturphilosophie* (Rome, 1951), pp. 304–5.
5. Ibid., p. 306.
6. A. Koyré, *Etudes Galiléennes* (Paris, 1939), pp. 246–47. Emphasis Koyré's.
7. E. J. Dijksterhuis, *The Mechanization of the World Picture,* trans. C. Dikshoorn (Oxford, 1961), p. 352.
8. I. E. Drabkin and S. Drake, *Galileo on Motion and on Mechanics* (Madison, 1960), p. 72.
9. Ibid., p. 68.
10. Ibid., p. 171.
11. Ibid., p. 171 n.
12. *Discoveries,* pp. 113–14. The passage is continued in essay 5 above.
13. Kepler, contemporarily with Galileo, in fact used the word *inertia* to denote the supposed tendency of all bodies to come to rest upon the removal of force.
14. *Dialogue,* pp. 174–95 passim.
15. *Two New Sciences,* pp. 244–57.
16. *Discoveries,* p. 263.
17. *Dialogue,* p. 234. The grounds for attributing to Galileo a belief in "circular inertia" are examined at length in essay 13.

13

The Case against "Circular Inertia"

Nearly all scholars presently hold that Galileo attributed to inertial motions an essentially circular form. This view was developed in a particularly interesting way by Alexandre Koyré.[1] It has wide implications both with regard to Galileo's physical conceptions and their supposed medieval roots.

Over the years, largely in the course of reconsidering the relevant passages in Galileo's *Dialogue* when revising the notes to successive editions of that work in English translation, I greatly modified my original support for the prevailing view. After arriving at a new view as to the origins of Galileo's inertial ideas, set forth in the previous study, I would have withdrawn my support entirely from the general belief, had it not been for the existence of one passage in which Galileo appeared to have unequivocally assumed the persistence of uniform circular motion in an unsupported body moving near the earth. Despite the fact that Galileo styled that passage a *bizzarria* and stated clearly that he did not believe it to hold precisely for actual bodies, its presence deterred me from going against the almost universal opinion of other students of Galileo.

Now, however, I believe that the passage represents a purely geometrical speculation, unconnected with projectile motions of any kind. It therefore seems to me time to reexamine the whole basis on which Galileo is believed to have seen circularity as an essential component in inertial motions. Before proceeding to that examination, I shall set forth briefly my interpretation of the *bizzarria* and explain why others have so long looked at it differently.[2]

In the *Dialogue* Galileo introduced a discussion concerning the probable shape of the path of a body falling from the top of a tower to its base as seen by an imaginary observer who did not share in the earth's diurnal rotation. He believed that the body would move nearly along a semicircle, of which the diameter would be the line from the top of the tower to the center of the earth.[3] This opinion was opposed by Marin Mersenne five years later in his *Harmonie Universelle.* Mersenne was struck by an absurdity that seemed to be implied. Assuming a diurnal rotation every twenty-four hours, the body would reach the center of the earth in exactly six hours. Yet Galileo had argued elsewhere in the *Dialogue* that a body would fall from the moon to the earth in less than four hours.[4] How, then, could he believe that a stone would require six hours to reach the center of the earth from a point close to its surface?

Mersenne's argument was based on a certain plausible interpretation of Galileo's diagram without close attention to the accompanying text. The diagram is simple and striking; a glance at it strongly tempts one to think of a heavy body descending from the top of the tower to the center of the earth, rather than just to its surface. Mersenne, and later critics, have accordingly been concerned with the question: "What would be the path of a body falling from a tower situated on a rotating and transparent earth, tunneled to its center from the base of the tower, as seen by a distant motionless observer who could watch the whole descent?" This was not Galileo's question, although the interpretation was not unreasonable, considering only Galileo's diagram, which was this:

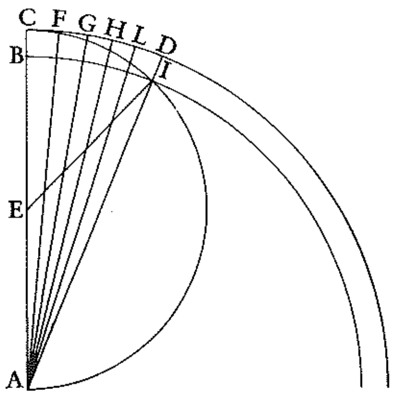

Fig. 11. Diagram shown in Galileo's *Dialogue.* The center of the earth is at A.

The question Galileo was discussing was stated thus: "What may one believe with regard to the line described by a heavy body falling from the top of a tower to its base?"[5] In his reply, Galileo first introduced the concept of uniform acceleration, but postponed discussion of the law of falling bodies to a later section of the *Dialogue.* Returning to the question, he again phrased it in the same way: "In the meantime, let us get back to the line described by the body falling from the top of the tower to its base."[6] The words, "to its base," show that no further generalization was intended.

Admitting that the diagram strongly suggests fall to the center, the manner of its construction as described in his text shows that Galileo considered the fall to be over when the body reached the base of the tower. The semicircle CIA was first constructed. Then successive positions of the tower were marked off and joined to the center of the earth. Finally, the positions of the falling body were identified with these intersections, which stop at the point I. It was only the arc CI that the text discussed. Using dotted lines for construction lines, the diagram would normally be drawn today in this manner:

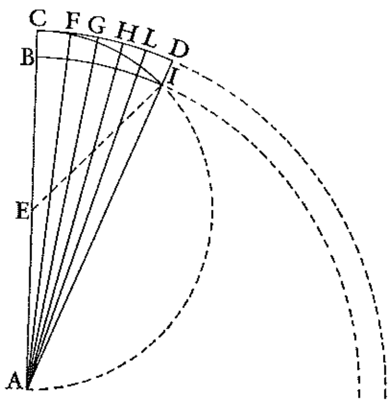

Fig. 12. Galileo's diagram redrawn to distinguish construction lines from lines referred to in his demonstration.

Albert Einstein pointed out to me many years ago another puzzle implied in the usual interpretation of this passage which seems to have escaped discussion. Why did Galileo make the falling body stop at the center of the earth, when by his own law of acceleration it should be going most swiftly? Elsewhere in the *Dialogue,* Galileo himself had discussed the continuation of motion past the center if a body were dropped through a tunneled earth.[7]

Clearly, if Galileo never intended to discuss any motion of the stone beyond that which it would have from the top of the tower to its base, the puzzles mentioned by both Mersenne and Einstein simply vanish. Neither the six-hour descent nor the sudden stoppage at the center have any basis in Galileo's text. This did say that the body "tends to end at the center," that its path "must tend to terminate at the center," and that "such motion tends eventually to terminate at the center," but there is no simple indicative, present or future, to suggest that the center is supposed to be reached.[8] Reference to the center was necessary in order to determine the path of the body above the earth's surface, but played no further role in the discussion.

The specific assignment of a uniform circular component in the short descent of this unsupported body (greatly exaggerated in any diagram) served only the necessities of a geometric speculation about an apparent path. It was based not on a physical assumption, but on the observed fact that a body falling from a tower grazes its side. It had to do not with any physical account of a projectile trajectory, but only with the analysis of an optical appearance from a point separated from the earth.

Let us now turn to the analysis of circularity in Galileo's idea of inertia, as set forth by Alexandre Koyré in the third of his *Etudes,* called *Galilée e la loi d'inertie.*[9] The first forty-four pages of that study are devoted to a statement of physical problems introduced by the Copernican theory, the proposed solutions to them offered by Copernicus, Bruno, Brahe, and Kepler, and the arguments that were brought against these solutions. Then, before examining Galileo's *Dialogue* and its anti-Aristotelian polemic, Koyré devotes four or five pages to a statement of his thesis. There follow some eighty pages of interpretation and comment on selected passages of the *Dialogue,* by which the thesis is ably supported. The balance of the monograph, except for two paradoxes presented immediately after the above sections, will not concern us here.

Now, in a work of the above structure, it is important to be clear about the basis on which the thesis is put forward, for the appearance of invincible solidity of the whole structure is largely

achieved by the interpretation of passages chosen to illustrate the thesis. Koyré's general position is stated in these words, which will be translated sentence by sentence as each point is examined in this study.

> *Le problème central qui préoccupe Galilée à Pise est celui de la persistance du mouvement. Or, il est clair que lorsqu'il étudie le cas du mouvement (de rotation) d'une sphère placée au centre du monde, ainsi que celui d'une sphère placée en dehors de ce centre, il a en vue la situation créée par la doctrine copernicienne; la sphère marmoréenne dont il analyse les mouvements représente, sans nul doute la terre; et ses mouvements sont ceux de la terre.*
>
> *Mais le résultat auquel il aboutit—en contradiction, d'ailleurs, avec les prémisses essentielles de la physique de l'*impetus*—nous révèle d'une manière éclatante les difficultés, et la source des difficultés, que recontraient sur leur chemin la physique et l'astronomie nouvelles.*
>
> *En effet, le résultat auquel aboutit l'analyse galiléenne, c'est la persistance naturelle, ou, plus exactment, la situation privilégiée du mouvement* circulaire.[10]

"The central problem with which Galileo was preoccupied at Pisa is that of the persistence of movement."

We can judge Galileo's interests at Pisa objectively only by his own writings while he was there—theorems on centers of gravity, the *Bilancetta,* and *De motu.* In the first two, his preoccupation was with the extension of the work of Archimedes beyond the two books written by the Greek mathematician that relate to mechanics. The first half of *De motu* concerns the application of the principle of Archimedes to a refutation of Aristotle's laws of falling bodies. That constitutes the most important part of *De motu.* Chapters 8, 10, 11, 12, and 13 all contain in their titles the phrase "in opposition to Aristotle," or words to that effect. Chapter 14 comprises the important section on motion on inclined planes; chapter 15 resumes the proofs in opposition to Aristotle. In none of these is the question of persistence of motion discussed; not even in chapter 14, where it is proved that motion on the horizontal plane can be produced by a minimal force. In chapter 16 the question is raised whether the rotation of a sphere situated at the center of the universe would be perpetual or not, but the answer is postponed and never taken up again. At the end of the same chapter it is intimated that a nonhomogeneous sphere situated outside the center of the universe cannot rotate perpetually, but again a promise of future explanation is not carried out. It is only in chapter 17, when Galileo comes to the subject of projectiles, that persistence of motion becomes a topic of discussion. Here Galileo adopts the accepted theory of a wasting impressed force in refutation of Aristotle. Circular motion is not a central question; in a single brief reference to rotatory motion, Galileo says only that it would last a long time, not that it is inherently perpetual. The rest of *De motu* has nothing to say about persistence of motion as such.

What preoccupied Galileo at Pisa was not the persistence of motion, as Koyré asserted. Even at Padua that is not a question on which Galileo left anything written. His *Mechanics,* composed there, contains a passage that implies persistence of motion on the horizontal plane, but only in an aside and only by implication. There is evidence that Galileo had developed that implication in lectures or conversations before 1607, but it was only in 1613, after he had returned to Florence, that he clearly stated an opinion on persistence of motion. What really preoccupied Galileo at Pisa was not the persistence of motion, but the refutation of Aristotle's physics. All this is evident from the structure of *De motu,* its chapter headings, its textual criticisms of Aristotle, and its general polemic tone.

"Now, it is clear that when he studies the case of movement (rotation) of a sphere placed at the center of the universe, as of that of a sphere placed outside that center, he has in view the situation created by the Copernican doctrine; the marble sphere whose movements he analyses represents, without any doubt, the earth; its movements are those of the earth."

Not only is this not clear; it is in a way self-contradictory. If Galileo had been a Copernican at the time he wrote *De motu,* he would not have placed the earth at the center of the universe, even metaphorically. What is clear is that in his anti-Aristotelian *De motu,* he wished to destroy even Aristotle's fundamental division of all motions into natural and violent motions. To do this, he showed that certain bodies might be in movement without their motion

being either natural or violent. It is possible that Galileo, while still at Pisa, was already interested in the Copernican implications, but it is by no means clear that he was. In *De motu* there is no reference, even in passing, to any astronomical topic except to the argument of some philosophers that the addition of a single star to the heavens would slow down or stop them. Answering this, Galileo treated the fixed stars as in daily circular rotation, without even a parenthetical remark to indicate that others (or he himself) believed that the fixed stars might not be in motion at all.

"But the result at which he arrived—in contradiction, moreover, with the essential premises of the physics of impetus—reveals to us in a striking way the difficulties, and the source of the difficulties, in the path of the new physics and the new astronomy."

The result to which Koyré here alludes is made clear in his next sentence, discussed below. It is, as will be shown, at least partly not Galileo's result at all. Whether it, or Galileo's own result, was in contradiction with the premises of impetus physics, Galileo certainly did not see any contradiction between the two at the time he wrote *De motu.* For in that work, Galileo not only adopted impetus physics for projectiles, but also gave a proof that impetus must waste away, and used this in his explanation of the phenomenon of an initial temporary acceleration in free fall. He saw no contradiction between a wasting impetus and his logical analysis of rotations, set forth in the same book, nor is there evidence that he saw any connection between that analysis and astronomy. For such a connection we have only Koyré's conjecture.

"In fact, the result to which the Galilean analysis led is the natural persistence, or more precisely the privileged character, of circular *movement."*

This is the thesis put forth by Koyré and adopted by most historians of science after him. Koyré appears to consider the result attributed to Galileo as one already reached at Pisa (though *aboutit* may refer to some later time). The passages of *De motu* cited by Koyré in his footnote here do not assert anything about the persistence of circular motion, nor do they deal with real physical spheres; still less do they assert that circular movements have any privileged character. In the same footnote, Koyré invites us to compare a passage in Galileo's *Mechanics,* a work composed at Padua after the Pisan period. It is the passage already mentioned above, which still did not assert, though it did imply, the persistence of motion on horizontal planes.

Koyré next reviews Galileo's analysis of the motion of a hypothetical sphere located at the center of the universe, as a motion neither natural nor violent, adding: "But the case of the sphere located at the center of the world is far from being unique; in truth, all circular movement (about the center) is such a movement that is neither natural nor violent."[11] This is where trouble truly begins. Galileo made no such assertion, which would include the case of circulation around the center of the universe, as well as the case of the rotation of a sphere located there. No circulations, and not even all rotations, are said by Galileo to be "neither natural or violent." Yet it is upon circulations (not mere rotations) that the whole question of "circular inertia" has been made to hinge. I shall return to this point shortly. First, however, it is important to note that Koyré's thesis attributes to Galileo a lifelong constancy to this principle: *"Circular movement occupies in physical reality an absolutely privileged position. . . . That is the Galilean situation. It is almost the same at Pisa, at Padua, and at Florence."*[12]

Now in fact, circular motion of any kind occupies only a small part of *De motu.* One chapter is dedicated to it, and that chapter deals hardly at all with physical reality; it is principally concerned with imaginary marble spheres, introduced for the purpose of showing that instances of motion can be adduced that will not fit into Aristotle's classifications of natural and violent. Part of one paragraph, discussing homogenous spheres rotating elsewhere than at the center of the universe, remarks that such spheres must be supported and will therefore be subjected to frictional resistance. That is the only case in which physical reality appears, and such circular motion is most certainly not given a privileged position there. Heterogenous spheres, moving just as circularly, would not continue to move forever according to Galileo. In another chapter, discussing projectiles, antiperistasis is refuted by a paragraph concerning grindstones. These are the sole discussions of circular motion, which is not given a privileged

position in *De motu* at all; it is merely seen to require separate discussion.

That Galileo held any single unifying view, virtually the same at Pisa, at Padua, and at Florence, is an inference for which no factual evidence exists. Growth and change characterized his thought; witness the evolution of his view of acceleration. As to circular motion, it is not a principal topic in any of Galileo's published books before 1632, nor in any of his letters. (The persistence of motion is not a principal topic either, though it was discussed in one paragraph of the *Letters on Sunspots.*) In *The Assayer,* Galileo denied circular orbits to comets and ridiculed the idea that any shape (particularly the circle) had any priority over any other; none, said Galileo, had patents of nobility, and none were absolutely perfect; perfection had to do with the intended use, and not the form.[13] So it is gratuitous, if not hazardous, to assume that when Galileo composed the *Dialogue* he had spent a lifetime ruminating over the persistence and absolute privilege of circular motion in the world of physical reality.

The underlying philosophical preoccupation attributed to Galileo by Koyré is equally unsupported by surviving correspondence. Galileo's letters are as devoid of metaphysical discussions as they are of encomiums on the marvels of the circle. Only after he was blind and had published his last book do we find him debating (with Fortunio Liceti) on any abstract principles. Since in that debate he sided with Aristotle, his final position can hardly be called a lifelong unifying philosophical view.

With the philosophical interpretation of passages from the *Dialogue,* conducted in the light of Koyré's thesis, we are not here concerned. Quite different interpretations are capable of being made on other assumptions, and Koyré's interpretation does not easily apply to certain other passages. Let us therefore pass on to the problem with which Koyré was left as he turned to discuss the successors of Galileo. He asks:

> If, as we believe has been shown, Galileo did not formulate the principle of inertia, how is it that his successors and pupils could think they found it in his works? And another [problem]: if, as we believe we have equally shown, Galileo not only did not conceive, but even could not conceive inertial movement

in a straight line, how did it come about—or better, how was it brought about—that this concept, before which the mind of a Galileo was halted, could appear easy, obvious, and self-evident to his pupils and successors?[14]

Let us try to resolve the paradoxes seen by Koyré. And first, let us remove from them the exaggeration inherent in the statement above; that is, that these things have been shown. If it were indeed shown, in the sense of being demonstrated, that Galileo did not conceive or formulate, and could not have conceived or formulated the idea of continued and undiminished rectilinear motion, then Koyré's paradoxes would have to be admitted. As things stand, however, we may for the present take Koyré's position not as something demonstrated, but only as one of several plausible hypotheses. In that case the paradoxes named give way to ordinary questions, questions that may serve as the basis for further fruitful research. These questions would be, for example:

"If Galileo could and did conceive of the possibility of continued and undiminished rectilinear motion, why did he not formulate that conception as a physical law in unequivocal terms? And since he did not so formulate it, whatever his reasons, how did it come about that his pupils and successors not only adopted it as a matter of course, but treated it as something derived from Galileo's own work?"

An answer to these questions should be sought in Galileo's own works, not in general philosophical and historical principles such as those to which Koyré appealed in support of his attempted demonstrations—demonstrations which led him only to paradoxes. Charming as paradoxes are to the philosopher, they are dead ends to the historian, who deals rather in real problems.

That Galileo could and did conceive of the *possibility* of continued uniform rectilinear motion is made evident at various places in the *Dialogue* and the *Two New Sciences.* The treatment of these in Koyré's monograph is unsatisfactory; he neglects to mention certain relevant passages in the *Dialogue,* and he explains away others in the *Two New Sciences* by an argument that proves only Galileo's denial of the *fact,* and not any denial of (or failure to recognize) the *possibility,* of rectilinear inertia. Thus Koyré correctly says, of the celebrated passage in the *Two New Sciences* deriving the parabolic trajectory, that continued uniform motion

in the horizontal plane is not, for Galileo, rectilinear motion at all; fundamentally it is circular motion, for the horizontal plane is not a mathematical plane, but the surface of a sphere concentric with the earth. This argument shows, not that Galileo overlooked the possibility of rectilinear inertia, but merely that he avoided any assertion of its existence in fact. Since it does not exist experimentally, that is quite correct. The *possibility* is here neither affirmed nor denied; for its recognition, we must look elsewhere.

A single instance will suffice to show that Galileo recognized the *possibility* of rectilinear inertia. The clearest and best example, as well as the most important, is the existence of the discussion in the Second Day of the *Dialogue* in which Galileo replies to the argument that rotation of the earth would cast off objects resting on its surface. In preparation for his argument, Galileo established this physical proposition:

"The circular motion of the projector impresses an impetus upon the projectile to move, when they separate, along the straight line tangent to the circle of motion at the point of separation . . . and the projectile would continue to move along that line if it were not inclined downward by its own weight, from which fact the line of motion derives its curvature."[15] Thus there would be no curving of the line in the absence of gravity. The ensuing discussion assumes that the rectilinear motion, if it could continue, would be uniform, and this conception is confirmed by Galileo's diagram, in which equal times are laid off along the tangent. The case of a projectile impelled by a cannon had been previously discussed and summed up in a postil: "Projectiles continue their motion along a straight line which follows the direction of the motion that they had together with the thing projecting them while they were connected with it."[16] Here, as elsewhere, Galileo does not assert that an actual cannon ever *is* at rest, or that the absolute path of the ball would ever actually be straight, but the *possibility* of a straight path is pointed out by the statement.

On the other hand, the real existence of "circular inertia," in the sense of an indelibly impressed impetus to follow the diurnal rotation of the earth, is excluded for Galileo by his argument against extrusion.[17] Serious consideration of such an impetus is in effect denied by Galileo's casual treatment of the Copernican suggestion that terrestrial rotation is natural for the earthly objects.[18] Had Galileo considered it a fact that every body resting on the earth is indelibly impressed with a circular motion identical with that rotation, his entire complicated mathematical argument could have been avoided, and in its place we should have had merely a reiteration of the Copernican position. The highly technical demonstration that Galileo attempted in explanation of the quiescence of detached bodies resting on a rotating earth could serve little purpose for himself, and still less for his readers, if he believed literally in "circular inertia." It is therefore significant that Galileo did not repeat and endorse the Copernican "circular inertia" argument, but put his analysis of tangential and central forces in place of it. There is simply no reason that Galileo should ever have worked out this demonstration in the first place if his own conviction was carried by "circular inertia."

Here, however, an interesting point arises. At several places in the *Dialogue,* particularly in connection with the fall of a weight from a tower or a ship's mast, the body is said by Galileo to follow in its descent the circle swept out by the tower in consequence of its diurnal motion, or of the ship in consequence of its own motion. This is indeed "circular inertia," and it is utilized in several places; for example: "Now as to that stone which is on top of the mast; does it not move, carried by the ship, both of them going along the circumference of a circle about its center? And consequently is there not in it an ineradicable motion, all external impediments being removed? And is this motion not as fast as that of the ship?"[19]

If we had to decide which of the two different conceptions Galileo himself held as an inner conviction—that is, whether the ineradicable motion impressed on a heavy body is the uniform rectilinear motion of which he spoke in connection with cannonballs and stones flung from whirling slings, or the circular motion about the earth shared with the ship's mast from which a stone is dropped—there is at least one strong hint to be gained from the discussions in which those two different ideas occur in the *Dialogue.* In the first-named case, whether the original motion is rectilinear (cannon shot) or circular (whirled object), Galileo always adds a comment to the effect that the object would continue along the straight line if it were not immediately drawn down by its own weight, as in the passage cited above. But in

the ship-mast or related examples, in which a circular motion is shared by the object with the whole earth, or is imparted to it solely because of the earth's spherical shape, Galileo never adds an analogous statement that the body would continue along that (circular) line if it were free of weight.

In short, Galileo does not say positively anywhere that a heavy body near the earth would continue in circular motion if deprived of support, in any circumstances, whether dropped from a ship's mast or rolled off the end of a level place; but he does say positively in several places that such a body would continue any rectilinear motion imparted to it were it not for the downward action of its weight.

Wherever it appears that Galileo asserts the continuance of a circular motion for terrestrial bodies, the motion is of a character that is indistinguishable from rectilinear motion by reason of the huge size of the earth. It has become customary to suppose him to have been thinking: "and yet, of course, the motion really *is* circular, though we can't see that it is." If that were the case, one would expect him to have made this specific qualification for the cannonball and for missiles flung from slings; but that is the exact reverse of what he did do. We may equally well suppose him to have been thinking, "but the circular character of the ship-mast motion or of the actual horizontal plane is merely accidental, resulting only from the fact that the earth happens to be round"; and then all his statements concerning terrestrial inertia would be consistent and rather easy to explain. The prevailing preference among historians of science for believing Galileo inconsistent and paradoxical on this matter is guided not so much by necessity as by a belief that the history of science receives more light thereby.

The discussion of semicircular fall in the *Dialogue* does not include any consideration of inertia in its physical aspect. The continuation of motion there is introduced as a mathematical assumption rather than an observed fact and is used for the purpose of a geometrical speculation about an apparent path rather than for a physical analysis of any projectile motion. Granting for the sake of argument that it involves inertia, this case corresponds to the ship-mast type of circular motion and hence to a path indistinguishable by observation from a straight line for the amount of arc under consideration. To justify the treatment of such arcs as straight lines, Galileo appealed throughout his life—from the unpublished *De motu* of 1590 to the final *Two New Sciences* of 1638—to the treatment by Archimedes of the pans of balance as hanging by parallel lines, though in fact they converge toward the center of the earth. That is why, in my opinion, it is wrong to suppose that Galileo considered the curvature of the earth as introducing an *essential* circular property into all imparted motions near that surface. Rather, his consistent lifelong statements show that he took Archimedes as his model in the treatment of physical problems, ignoring inconsequential discrepancies in the choice of physical postulates for his mathematical deductions.

There are also other grounds for believing that Galileo's references in the *Dialogue* to an ineradicable circular impetus or ineradicable rectilinear inertia give rise to merely apparent, and not real, inconsistencies. Considering the order in which the references occur, and their relative simplicity and complexity, there is some reason to suppose that Galileo used the less involved (Copernican) assumption of natural rotation earlier in the *Dialogue,* to lead his readers easily along, and the more refined assumption of tangential motion when the argument became more precise. That view is supported by the many instances in which he treated the earth's spherical surface as an approximation to the horizontal plane. It is also supported by considerations of style, since a precise statement of the tangential character of conserved motion would necessarily have been clumsy in his early illustrations as compared with the expressions used. On the whole, it is more plausible that Galileo personally considered rectilinear motion as essentially true for terrestrial bodies.

But we do not really have to decide for one view or the other; if anything, we should avoid a final decision. What we must recognize is that Galileo's discussion leaves open the *possibility* of either circular or rectilinear continuation of uniform motion in the case of free fall (a "natural" motion), whereas he unambiguously specifies *rectilinear* continuation for the "violent" component in the motion of cannonballs and terrestrial objects released from slings or flung from rapidly rotating wheels.

This point becomes still more significant when we turn from

the behavior of heavy bodies on or near the earth's surface (terrestrial physics) to Galileo's cosmological speculations (celestial physics). I paraphrase Galileo's thought thus: "For terrestrial physics (the physics of heavy bodies), the ineradicable *tendency* of terrestrial projectiles is to follow the line of the cannon, or the tangent to the circle of the sling, but the actual *motion* cannot be rectilinear because the body has weight. The *essential* motion of a terrestrial body is one thing; its accidental path is another. Where we cannot distinguish them, we do not have to decide between them as in the case of point-blank shots or of falling bodies sharing the earth's diurnal motion, where the straight tangent and the circular arc do not differ by an inch in a thousand yards." But where the distinction is clear, the *essential* rectilinearity is easily recognizable: "When the stone escapes from the stick, what is its motion? Does it continue to follow its previous circle, or does it go along some other line?—It certainly does not go on moving around. . . . It is necessarily along a straight line, so far as the adventitious impulse is concerned."[20]

These and similar passages answer the question whether Galileo could perceive the possibility of continued uniform rectilinear motion for terrestrial (heavy) bodies, and explain why his pupils and successors treated the principle of inertia as a part of his work, even though he did not formulate it clearly and unambiguously. For a possible answer to the question why he did not do so, we must look also at his cosmological speculations. It is evident that his failure to formulate the inertial law cannot have been, as Koyré supposed, an inherent incapability of perceiving its possibility. Something else made Galileo reluctant to generalize it for the entire universe. Our principal (if not our only) source of knowledge concerning Galileo's cosmological speculations is the First Day of the *Dialogue,* and particularly its opening section. Having begun with Aristotle's position, Galileo has his spokesman (Salviati) agree with Aristotle, supplying additional arguments in favor of the Aristotelian position up to a certain crucial point. But where Aristotle deduces the motions of the heavens from the perfection of the circle, Salviati introduces instead the postulate that the universe is perfectly *orderly.* From this he deduces that it is impossible for integral world-bodies (the planets and their satellites) to move in straight lines: for in that way the cosmic order would perpetually change. This postulate does not commit Galileo with respect to terrestrial objects. Cosmic order, on the other hand, can be preserved only if the heavenly bodies are at rest or in circular motion. Only that motion, he says, is capable of true uniformity; straight motion is either accelerated, decelerated, or infinite, the first two being nonuniform and the last being inadmissible in an *ordered* universe.

It is here that Galileo introduces his "Platonic concept" of cosmogony: the planets, having been created at a certain place, were moved with straight accelerated motion until each had received its assigned speed, at which point its motion was converted from a straight to a circular path by God, who willed that the planet keep that same velocity perpetually thereafter.[21] On the basis of this cosmogony and its attendant arguments, it is widely held today that Galileo attributed the motions of the planets in their orbits to "circular inertia." That, however, contradicts several clear statements by Galileo himself; moreover, it implies certain beliefs on his part that he cannot have held if he was even a competent, let alone a gifted physicist and astronomer.

First, we must note that in this passage Galileo attributes the continuance of the planets in their orbits to the will of God, and not to any physical principle whatever. Much later in the *Dialogue,* he expressly denies that he (or anyone else) knows by what principle the planets are moved.[22] Equally important is his refusal to grant that the universe has a center: "We do not know where that may be, or whether it exists at all. Even if it exists, it is but an imaginary point; a nothing without any quality."[23] Now, Galileo's entire understanding of perpetual uniform movement, wherever it is expressed in his writings, consists always in the body's path being such as not to approach or to recede from some center toward which it has a natural tendency to move. For ordinary heavy bodies, this is the center of the earth. Only by the most tenuous arguments, and in contradiction of Galileo's own words, can a case be made that he believed the planetary circulations to be analogous to such motions. For it cannot be seriously argued that Galileo believed the actual planetary motions to be literally perfectly circular around the sun as a common center. No competent astronomer since Aristotle had believed in homocentric orbits, with the possible exception of Fracastoro. Certainly no

Ptolemaic or Tychonian, let alone Copernican, believed in mathematically concentric paths for the planetary bodies themselves. Galileo was no great theoretical astronomer, but he certainly was aware of the classic problems of planetary orbits, variously solved by eccentrics, epicycles, ovals, and ultimately by ellipses.

If anyone wishes to contend that Galileo was actually so ill-informed as to believe that some set of perfect concentric circles—centered, moreover, about an occupied point—would fit the actual observed motions of planets, and that the "Platonic concept" of the *Dialogue* presents us with his mature astronomical convictions, then "circular inertia" demands further that Galileo must have believed those circular motions to be absolutely uniform. But in the Fourth Day of the *Dialogue,* Galileo argued that the circuits of the sun, the earth, and the moon are not uniform in speed.[24] Hence the attribution to Galileo of a belief in planetary motion by reason of "circular inertia," meaning by that absolutely uniform motion in perfect circles, can be maintained only at the price of rejecting his own words and his astronomical competence, or supposing him to have been so devout a Platonist as to hold to a cosmology refuted by his own perceptions.

We have now reviewed some of the factual weaknesses behind Koyré's thesis, and some of the needless difficulties to which it gives rise. If we abandon his interpretation of "circular inertia" as the unifying principle of Galileo's physics and his astronomy—as the unchanging core of his work at Pisa, Padua, and Florence—what interpretation shall we put in its place? To this question I wish to reply only tentatively, putting forth possibilities rather than conclusions.

The contradictions (or seeming contradictions) in the *Dialogue* are better resolved by paying very close attention to Galileo's exact words than by postulating some unifying conception behind them in Galileo's own mind and then saying in effect: "When he said this, he really meant thus-and-so." Perhaps he did not attempt to explain everything by one principle. Let me give an example.

When Galileo had occasion to speak of a supported body moving along a terrestrial horizontal plane, he usually went to the trouble of pointing out that since no true geometrical plane existed on a spherical surface, the so-called horizontal plane of our experience is really a portion of the sphere. Yet when he argued that a projectile released from a whirling sling tended to move along the straight line tangent at the point of separation, he did not add that the line was not really straight, but must share in the circularity of its previous path, or in that of the diurnal rotation, or anything of the sort. This hardly supports the view that he thought always of circularity and could not conceive of uniform rectilinear inertial movements. The fact that such movements are made nonuniform by air resistance and are curved by downward action of weight has nothing to do with their essential character, and Galileo was perfectly clear about that.

On the other hand, when he spoke of free fall along a tower or the mast of a ship, he plainly said on several occasions that the true path was a compounding of the straight motion toward the earth's center and the general circular terrestrial motion shared by the object before the commencement of its descent. This equally destroys any claim that Galileo really always had in mind a straight tangential motion. It may be that he had such an idea in mind, though he did not adduce it in every case for reasons of style; but there is no more evidence that he always thought of observed terrestrial inertial movements as essentially straight than that he always thought of them as essentially circular. If we pay strict attention to Galileo's own words, we shall have to say that he invoked no unifying principle for all cases of inertial movement.

It does not necessarily follow, however, that Galileo was inconsistent in the matter. If there is some element common to all the cases in which he specified an essentially straight inertial movement, and some other element common to all the cases in which a motion is spoken of as essentially circular, then the apparent inconsistency might vanish as thoroughly as did any supposed unifying principle.

Now it appears to me that Galileo is pretty consistent in applying the idea of essential circularity to instances in which the motion is a "natural" one in his sense; that is, a motion induced by an innate tendency of the body to move when it is set free. The idea of essential rectilinearity, on the other hand, he applied most specifically to instances of "violent" motion—cannonballs and projectiles thrown by slings. One may say that even in this he is not entirely consistent, for his long discussion of bodies that ought to be flung from the earth by its rotation is

an apparent exception. Such bodies are not subjected to an external force, yet they are treated as projectiles from slings would be treated; that is, as potentially moving along the tangent in a straight line. Whether the exception is real or apparent may be argued. Galileo's question was how such objects would move if they moved at all, and that question he treated on the analogy of the sling, or wheel, adding that the central tendency, lacking in the latter, prevented detachment from the earth.

In short, I doubt on the one hand that Galileo had a unifying principle in this matter, and on the other hand that he was vague and inconsistent about it. On one point he was quite definite and consistent, though we have made it hard for ourselves to see this. Tangible terrestrial objects subject to observation, to which an external impulse was imparted either by a straight push or by release from whirling, conserved the received impetus in the form of uniform rectilinear motion. Stated several times in the *Dialogue,* usually together with the idea of composition of independent motions, this conception was applied again in the *Two New Sciences,* and it was understood and adopted by Galileo's pupils and successors. He seems to me to be equally consistent in attributing essential circularity to terrestrial objects in "natural" motion only, where it happens that an observer could not actually distinguish the tangential from a very slightly arcal path.

It remains a question whether Galileo had an ulterior purpose if he made the distinction suggested above, and whether it had anything to do with a belief on his part about the planetary orbits. It seems to me not unlikely that he did have a reason, but one that had nothing to do with any cosmology. This reason was that for Galileo, heavy bodies on or near the earth strove by an innate tendency to reach its center (or rather, to reach the common center of gravity of all such bodies); and this they would never reach by the parabolic trajectories implied by rectilinear inertia, though they would reach it along a suitably chosen circular arc.[25] In other words, violent motion could be permitted to disturb natural order, while natural motion could not. If Galileo took such a view, his seeming vacillation between inertia and "circular inertia" in the *Dialogue* would be reasonably explicable without recourse to a unifying principle on the one hand, or the charge of inconsistency on the other.

Galileo's cosmological speculations, which occur almost entirely near the beginning of the *Dialogue*, are usually interpreted as evidence that Galileo believed in an extension of "circular inertia" to the planets. It has already been pointed out that, taken literally, this not only implies an extraordinarily poor knowledge on his part of the actual planetary orbits and the speeds of planets in them, but also contradicts various statements and denials of his own. In the preceding study I have set forth my view concerning the motivation for Galileo's cosmological speculations, giving attention to their polemic value in the *Dialogue* and remarking on the absence of similar passages in Galileo's voluminous correspondence. The fascination with circles that Koyré makes fundamental to the understanding of Galileo's physics is not evidenced by lifelong metaphysical speculations—as is, for example, Kepler's fascination with musical harmonies in the universe.

It is also worth pointing out that in the *Dialogue* itself, Galileo ridicules the Peripatetic insistence on a mathematical perfection of sphericity for the heavenly bodies,[26] and he cheerfully admits that perhaps no perfectly spherical body can be formed of actual matter.[27] This, for Galileo, does not invalidate mathematical reasoning about physics; it merely cautions the calculator to adjust his accounts as necessary.[28] Such things militate against the belief that Galileo was spellbound by circular perfection.

It is in that light, I think, that we should interpret such cosmological statements as: "I therefore conclude that only circular motion can naturally suit bodies which are integral parts of the universe as constituted in the best arrangement."[29] Koyré and his followers (indeed, some of his critics as well) want us to believe that this proves Galileo to have believed in his heart that absolutely perfect uniform circular motions carried the planets around the sun. Taken literally, this would require him to have been alone among all the astronomers after Aristotle to believe that all observed positions of the planets were compatible with a single set of uniform rotations about a fixed center. For in the passage just cited, Galileo does not say "circular motions," as if to allow epicyclic paths made up of combined circular motions; he requires "circular motion" to preserve order.

Against the prevailing view, which would make Galileo ignorant or scornful of actual observation, we may read his phrase "circular motion" as meaning no more than "circulation"; that is,

recurrent motion over a closed path. That is perfectly consistent with his manner of arriving at the proposition, which is deduced from the orderliness of the cosmos. It is also consistent with his customary good sense and with his obvious motive in the First Day, which was to prepare a basis for attributing to the earth a circulation about the sun.

The impropriety of Koyré's thesis as a basis for judging Galileo's beliefs about planetary motions seems to me to be conclusively shown by Galileo's mature rejection of the quest for causes in physics—the very attitude for which Descartes most criticized Galileo. To offer "sympathy," "antipathy," or any other occult quality in explanation of a physical effect was repugnant to Galileo. It was this kind of "causation" that he criticized in Kepler's theory of the tides. In the *Dialogue* he reproved Simplicio for offering "gravity" as a cause of the fall of bodies, in a passage that overtly rejected all purely verbal attempts to assign causes to planetary motions.[30] If the phrase "circular inertia" had existed at Galileo's time in the vague sense in which it is offered today as Galileo's own explanation of celestial motions, I cannot doubt that Galileo would have laughingly included it with the "informing spirits" and "guiding intelligences" that he ridiculed as explanations in this very passage.

It is fairly evident to me that Galileo did not offer a complete system of the universe. We need not construct one for him. It is a violation of Galileo's entire approach to physics to represent as his inner thought the completion of a system on the basis of a false principle of circular inertia—or any other unifying principle, true or false. None of his pupils or followers wrote a word to suggest that Galileo ever adhered to or taught such a doctrine; none of his critics noted and condemned "circular inertia" or praised it and condemned those passages in the *Dialogue* that clearly contradict it. If the term "circular inertia" had been presented to him and he had been asked whether that was his explanation of planetary motions, I think he would have replied:

> The introduction of such a phrase is in no way superior to the "influences" and other terms employed by philosophers as a cloak for the correct reply, which would be, "I do not know." That reply is as much more tolerable than the other, as candid honesty is more beautiful than deceitful duplicity.[31]

Notes to Essay 13

1. A. Koyré, *Etudes Galiléennes* (Paris, 1939; reprinted ed., 1966). Citations given below are to original edition; pages in reprint add ten to the number shown.
2. A detailed analysis is set forth in "Galileo Gleanings," XVI; see Bibliography.
3. *Dialogue,* pp. 164–67.
4. *Dialogue,* pp. 223–26.
5. *Dialogue,* p. 162.
6. *Dialogue,* p. 164.
7. *Dialogue,* pp. 22–23, 135–36, 227, 236.
8. Galileo's phrases are *va per terminare, vadia a terminare, andrebbe a terminar;* never *termina* or *terminerà.*
9. See note 1 above.
10. Koyré, *op. cit.,* pp. 195–96. Emphasis Koyré's.
11. Ibid., pp. 197–98.
12. Ibid., pp. 198–99.
13. *Controversy,* pp. 191–97, 279.
14. Koyré, *op. cit.,* p. 282.
15. *Dialogue,* p. 193.
16. *Dialogue,* p. 175.
17. *Dialogue,* pp. 196–203.
18. *Dialogue,* p. 142.
19. *Dialogue,* p. 148.
20. *Dialogue,* p. 191.
21. *Dialogue,* pp. 20–21, 29.
22. *Dialogue,* pp. 234–35.
23. *Dialogue,* pp. 33, 37, 319.
24. *Dialogue,* p. 453; cf. pp. 455–56 for Galileo's comments on the inadequacy of existing planetary theory.
25. *Opere,* XVII, 89–90; *Two New Sciences,* pp. 261–62.
26. *Dialogue,* pp. 80, 94.
27. *Dialogue,* pp. 208–9.
28. *Dialogue,* pp. 207–8.
29. *Dialogue,* p. 32.
30. *Dialogue,* pp. 234–35.
31. *Controversy,* p. 197.

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