11

Free Fall and Uniform Acceleration

Second only to the law of inertia in its importance to the origin of modern physics is the discovery of the law that governs the speed of bodies falling freely to the earth. This law, which relates the speed at any moment to the time elapsed from rest, and the spaces traversed from rest to the squares of the elapsed times, was first set forth in Galileo's *Dialogue* of 1632. Its consequences were developed in many ways in his *Two New Sciences* of 1638, particularly with respect to the motions of projectiles. The parabolic shape of ideal projectile trajectories, utilizing both ideas, was first disclosed by his disciple Bonaventura Cavalieri in 1632. Cavalieri had studied mathematics under Benedetto Castelli, Galileo's most able pupil at Padua.

The development of Galileo's law of falling bodies has been the subject of many studies and much debate. In modern times, those studies have been guided primarily by considerations of the history of ideas; that is to say, by reconstruction of Galileo's line of thought as continuing that of his predecessors. A highly plausible picture has thus been built up, and is widely accepted, in which Galileo is seen as the heir of a well-defined medieval tradition in the mathematical analysis of the problems of motion. Many of Galileo's writings, both published and manuscript, have been used to support that picture. At the same time, some lingering problems have kept this historical portrayal from being regarded as certain, however plausible it has been shown to be.

An approach to the problem from the other side—that is, by a study of all the surviving papers of Galileo and an attempt to put them into reasonable chronological order as a means of retracing his steps—has received less attention. The task is not a simple one, because many fragments were jotted on undated sheets

kept by Galileo, sometimes mixed with notes on quite different topics. Antonio Favaro first attempted to arrange these in tentative order of composition when he published them in Galileo's collected works. A close study of the manuscripts, with attention to the watermarks of the papers, the ink, and the handwriting, will be necessary before any final conclusions can be reached. Meanwhile, however, working from the published manuscripts and correlating them with stages of thought discernible in Galileo's published books and in his correspondence, I have arrived at a reconstruction that differs profoundly from the prevailing view. If it is substantially correct, as I believe it is, it will ultimately have some notable consequences for studies of Galileo's relations to his predecessors and for our conceptions of continuity in the history of physics. It is with this reconstruction that the present study is concerned; for corroborative details, the reader is referred to two papers on which it is based.[1]

Galileo's unpublished treatises on motion and on mechanics, composed before 1600, show that in those early years he believed that acceleration in free fall was not an essential and continuing phenomenon, but an evanescent event at the beginning of fall that was swiftly replaced by a constant speed, which in turn depended on the excess of weight of the body over the weight of a like volume of the medium through which it fell. Accordingly he linked the speed of motion with the effective weight of the falling body, just as Aristotle had taught, but according to a radically different rule. For fall along inclined planes, Galileo's rule departed widely from the observed events.

The year in which Galileo first began to make careful observations on the motions of pendulums is not known, but there is good evidence that he had done so by 1602, when he corresponded with Guido Ubaldo del Monte on the subject. His earlier derivation of the rule for equilibrium on inclined planes implicitly related motions thereon to pendulum motions and to descent along circular arcs.[2] Guido Ubaldo disputed a rule Galileo had given him concerning the latter, saying that experiments made along the rim of a sieve did not bear it out. Galileo defended the rule and attributed the discrepancy with experience to the crudeness of observations made.[3]

Careful observations of pendulums would suffice to call into question the assumption that acceleration is a temporary rather than a continuing effect, especially in a long pendulum swinging through a considerable arc. The same observations would suggest also the symmetry of descent and ascent, and Galileo had noted long before that a body thrown upward cannot remain for any time at a constant speed.[4] Now, by 1604 we find him in possession of a rule for the spaces traversed in successive equal times by a freely falling body. It is apparent that he had abandoned his previous assumption that acceleration was evanescent, and was seeking the rule that governed increase of speed in actual falling bodies.

What he had found was in fact the right rule for the relation of spaces traversed and times elapsed, but not the correct assumption for increase of speed. He appears to have been quite certain of the former, as if he had found some independent evidence for it, while he described his assumption about the latter as merely probable and not as itself demonstrated. The same assumption—that the increase of speed is proportional to the space traversed from rest—is to be found in nearly every author before Galileo who concerned himself with actual acceleration in free fall. Medieval authors who had dealt with the question of uniform acceleration as such, apart from falling bodies, made quite a different assumption; namely, that the increase of speed is proportional to the time elapsed. It took Galileo a long time to see that this was also the correct assumption for actual falling bodies, as had been suggested earlier by but one writer, Domingo de Soto. It took him still longer to see also that his former assumption was incorrect. To us it seems obvious that both could not be correct at the same time, but that was not obvious to Galileo or to any of his predecessors; at least, no evidence has yet been presented that anyone, even Soto, had so much as raised the question. Ultimately it became obvious to Galileo, but that fact and its consequences (some of which are quite curious) come much later in our story.

In the autumn of 1604, Galileo conversed with his Venetian friend, Fra Paolo Sarpi, concerning accelerated motion. From surviving letters it appears that Galileo had asserted that in free fall, the successive spaces passed over in equal times were as the odd numbers starting from unity; that a body thrown upward and one falling through the same height would move with the same degrees of speed in reverse order; and that the distances fallen from rest were in the ratio of the squares of the times of fall. Sarpi wished to see a proof of these relationships, which were in fact correct.

Thinking over the whole matter again at Padua, Galileo wrote that he had been unable to find any truly unquestionable principle to assume for his proofs, but had adopted one that he thought to be physically reasonable, and for which he had hoped eventually to find a demonstration; this was the common assumption that the speed increased with the distance fallen. In support of the assumption he adduced the observational evidence of pile-drivers, which show that force, and hence speed, increase with height of fall. These thoughts he sent to Sarpi in Venice, saying that on the above assumption he could demonstrate the conclusions in question. He asked Sarpi to think over this assumption prior to a visit to Venice that Galileo planned for the end of October 1604.[5]

An attempted demonstration was duly written out by Galileo and is preserved among his papers. Whether he discussed it with Sarpi is not known. At precisely this time the nova of 1604 was first observed, and Galileo's attention was diverted for a while from problems of physics. Galileo's demonstration was fallacious, and various attempts have been made to analyze the reasons that prevented him from perceiving its lack of rigor. Here it suffices to say that the concept of a mean speed did not appear in it, nor was the fundamental assumption of Galileo the same as that of medieval writers on uniform acceleration.[6] My own view is that the attempted demonstration was entirely ad hoc; that Galileo was perfectly certain of the truth of the conclusion, and was therefore less critical of the steps in the proof than he would have been had he not had independent reasons for believing in the result proved. One of those steps invoked a "contrary ratio" of speed and time, neither defined nor explained, that enabled him immediately to produce the desired conclusion.

But the real interest at this point is the source of Galileo's certainty as to the correctness of the conclusions; especially the correctness of the rather complicated notion that spaces traversed are as the squares of the times. Had he relied on the medieval

Merton School writers or on Soto, the correct assumption would have been explicitly in front of him and the conclusions would follow directly. But if he had relied on them, he would logically have shown greater faith in the assumption than in the conclusions. Hence we should look for other possible approaches. And there are two ways that he might have been led to his rule without having relied on the speculations of any of his predecessors.

One of these is by means of observation. I hasten to add that I do not mean by elaborate experiments such as those he later described as having performed in order to corroborate the rules. It would be grossly anachronistic, both with respect to the history of experimental physics and with respect to the known procedures of Galileo, to assume that he reached the mathematical law of fall by carefully controlled measurements of falling bodies. That he confirmed the law in that way is virtually certain.[7] But he could have arrived at it indirectly in a much simpler and more plausible way.

The times-squared law follows immediately from the odd-number rule for successive spaces in equal times. It has already been noted that in 1602 Galileo was making observations that led him to recognize the continuing character of acceleration. A crude and hence plausible way in which he could have confirmed that would be to allow a heavy ball to roll a considerable distance over any convenient smooth slope, such as a paved ramp. In order to find out whether the ball continued to accelerate, he would have only to mark its place after equal times—say pulse-beats—and compare the distances between marks. In that way the approximate 1–3–5–7 relationship between marks might have been noted, from which the square law would be evident to any mathematician of the time.[8]

A purely logical approach was also accessible to Galileo. If the spaces traversed in free fall grow uniformly, they form an arithmetic progression. It would, however, be a special kind of progression, in which the sum of the first two terms must be in the same ratio to the sum of the second two terms as the first term is to the second, and the sum of the first three must be in this same ratio to the second three, and so on, since we could have taken the double or triple (and so on) of whatever we took for the first arbitrary space. If one then asks whether such

a progression exists, one finds it almost immediately. It is not 1–2–3–4, because 3 plus 4 is not double 1 plus 2. But it is 1–3–5–7, the very next progression to be tried, because 5 plus 7 is three times 1 plus 3. Moreover, no other arithmetic progression will fulfill the condition.[9]

Thus we need not assume that Galileo found the law of falling bodies by reasoning from the conclusions of previous philosophical or physical writers; he might have found it by rough observation, or by mathematical reasoning of his own; perhaps by a combination of both, or in some other way that has not occurred to me. Whether or not he hit on the rule by the above reasoning, he certainly was in possession of that reasoning by 1615, when he conversed with one of his correspondents who later wrote of ". . . a proposition that Sig. Galileo told me as true but without adducing the demonstration for me; and it is that bodies in natural motion go increasing their [successive] velocities in the ratios of 1, 3, 5, 7 etc. and so on *ad infinitum;* but he did adduce a probable reason for this, [namely] that only in this proportion [do] more or fewer spaces preserve always the same ratio. . . ."[10] It seems to me very likely that Galileo would be willing to give out to his acquaintance only the initial reasoning that he himself had used to establish this law, keeping its correct derivation to himself for later publication. If so, it is an interesting clue to his procedure in 1604 and to the source of his faith in the conclusions he gave to Sarpi.

In 1604 Galileo was forty years old and had been a professor of mathematics for fifteen years. Much of that time he had devoted to studies of motion and mechanics. It is highly unlikely that after he was in possession of the correct law of falling bodies, in 1604, but before he modified his initial attempt to derive it from a false principle, about 1609, he turned back to the study of medieval writings for assistance. But did these writings assist in leading him to the correct law *before* 1604? The prevailing view is that long before that time, Galileo was already familiar with the Merton Rule (or mean-degree theorem) and that he used it repeatedly throughout his career.

This view has its basis in three grounds. First, there is the general continuity principle in the history of ideas: it is said that the doctrine of the latitude of forms, developed in the Middle

Ages, had become an integral part of university education before Galileo entered Pisa. Second, there exists a philosophical treatise in Galileo's own hand, dating from his student days (1584, to be precise), that mentions the doctrine and the names of several medieval authors who discussed it. Third, there is a formal resemblance between the diagram used in the 1604 fragment and diagrams employed previously in unpublished discussions of the Merton Rule. Taken together, these grounds seem very convincing in favor of the prevailing view, particularly when it is added that the Merton Rule seems to lie at the very basis of Galileo's ultimate presentation of his law of falling bodies in 1638. When these grounds are taken separately, however, it will be seen that each alone is highly provisional and may ultimately be rejected.

As to the first, there is no question that the doctrine of the latitude of forms, as developed in the fourteenth century, had a deep and continuing interest to philosophers up to the beginning of the sixteenth century. The mean-speed theorem was printed and reprinted several times between 1490 and 1515. After that time, however, it was not reprinted again during the sixteenth century in Italy. After 1530 there was in Italy a general drift of interest away from medieval writings toward those of classical antiquity on the one hand, and toward contemporary authors on the other. During the half-century that elapsed between the last printing of the Merton Rule in Italy and Galileo's matriculation at Pisa, mere lip service to great medieval writers may have replaced the serious study of their works.

As to the second point, there is good reason to believe that the 1584 treatise in Galileo's hand was not his own production, but was either a set of dictated lectures or his copy of a manuscript treatise composed by a professor. The virtual absence of changes and the nature of the relatively few corrections, coupled with the vast number of authorities cited, suggest such an origin. In any event, the treatise is by no means conclusive proof that Galileo himself had read the works of all the authorities mentioned in it, who number more than one hundred.

As to the formal resemblance in diagrams, this is not matched by any resemblance in the demonstrations based on them, nor does any representation of mean speed, essential to Merton Rule diagrams, appear in Galileo's works. Diagrams virtually identical

with those of the letter to Sarpi and the 1604 fragment will be found in Michael Varro's *De motu tractatus* of 1584, a work devoid of any connection with the Merton Rule. The use of a line to represent distances and of triangles to represent proportionality is not in itself sufficient evidence on which to base a case for a common source. Differences in aim and viewpoint militate against such a source.

Essential to the ideas of the Merton School writers was the concept that in uniform acceleration, "the motion as a whole will be as fast, categorematically, as some uniform motion according to some degree of velocity contained in the latitude being acquired, and likewise, it will be as slow."[11] The determination of such a degree—a single value by means of which an overall uniform change might be represented—constituted the Merton Rule, or mean-degree theorem, which stated that to the midpoint in time corresponded the mean degree in uniformly difform change. To represent a set of changing velocities, medieval writers took a single velocity, chosen from within that set, and the same in kind with every member of that set.

It is evident to us now that the comparison of two uniformly accelerated motions, or of two segments of a single such motion, could have been most simply carried out by utilizing the ratio of two means, each representing one of the motions and each being by definition the same kind of entity, capable thereby of forming a ratio. But that was not the procedure adopted by Galileo in 1604, after he had become convinced that acceleration was an essential and continuing phenomenon of actual falling bodies. The Merton Rule directly related instantaneous velocities, mean velocities, and times elapsed. Galileo related instantaneous velocities to spaces traversed, spaces traversed to sets of such velocities, elapsed times to such sets by their "contrary" relation to spaces traversed, and thereby, finally, times elapsed to spaces traversed. He did not assume the existence of a mean speed within the set, or attribute any property to a midpoint, temporal or spatial.

Even if we assume that as early as 1584 Galileo was familiar with the Merton Rule sufficiently to know that it applied to uniform acceleration, in which velocity increased proportionally to time, and that it made the mean speed correspond to the midpoint in time, we cannot reasonably maintain that he still remembered that rule in 1604. At that time he was convinced that the spaces traversed in equal times from rest were as the odd numbers commencing from unity, and that the successive distances from rest were as the squares of the times of fall. In attempting a mathematical proof of those convictions, he appears not to have tried the Merton Rule. Had he done so, the desired proof would have emerged at once. He told Sarpi that he had been unable to find an unquestionable principle on which to base a proof, and therefore had recourse to one that was merely physically probable; namely, that velocities increase in proportion to spaces traversed. This sounds as if he had tried to think of others, and makes it unlikely that among them he had remembered the Merton Rule and tried in vain to apply it, or to put the increase in velocity proportional to time.

The central role of the mean-speed concept for medieval mathematicians of motion and its total absence in the 1604 fragment are highly significant for the history of science. We merely miss the point when we attempt to make the Merton Rule the historical, as well as the logical, predecessor of the law of falling bodies. It is here as with impetus and inertia; medieval studies had prepared the way for recognition and acceptance of Galileo's physics, but they did not put it in his hands. It is a mistake to suppose that we can divine a man's ideas without paying attention to his precise words. I am not sure that Galileo ever used the expression "mean speed" (or "mean degree") in his life, though every medieval writer on the Merton Rule did. Nevertheless, English translators of the *Two New Sciences* put the expression in his mouth when he did not use it. For example, in Galileo's crucial first theorem on accelerated motion, they have him say ". . . a uniform speed whose value is the mean of the highest speed and the speed just before acceleration began," whereas Galileo said only, ". . . by a uniform motion whose degree of speed is as one-half to the highest and last degree of speed . . ." (*motu aequabili . . . cuius velocitatis gradus subduplus sit ad summum et ultimum gradum velocitatis*). The distinction may not be as trivial as it appears. What Galileo consistently presents only as a ratio, the medieval writers (and Galileo's translators) presented also as some kind of entity. There are reasons for the difference.

Generally, medieval writers were concerned with a specific

change from a definite *terminus a quo* to a definite *terminus ad quem,* in accordance with the Aristotelian concept of change. Philosophically, the determination of a single measure of overall change was the solution of their problem. The Merton Rule accomplished such a determination. It solved the problem not only for uniform acceleration from rest (change from its beginning), but also for change from any subsequent point to a definite *terminus ad quem.* It should be noted, however, that medieval writers solved this latter problem as a separate one, noting that such intermediate motions had their own means, but disclaiming the possibility of a general rule of proportionality. For like reasons they did not seek rules of proportionality for any change that had no *terminus ad quem.* Such a change was for Aristotelians a contradiction in terms; in the case of accelerated motion, it would lead to infinite speed. Proportionality was used by medieval writers to determine relations within a finite change; hence it tended to be confined by them to converging series.

Galileo cared little or nothing for the determination of a mean value as such, but he was deeply interested in proportionality in every form. It was his key to the discovery of physical relationships. (The mean proportional, not the arithmetic mean, became his specialty.) When he became convinced that the spaces in free fall progressed as 1, 3, 5, 7 . . . ad infinitum, he sought a general relationship of velocities, spaces, and times. It is perfectly true that the Merton Rule would have afforded a simple and direct path to the solution of that problem; it is also true that Galileo's ultimate solution at a later date coincided with that way and embraced that rule. Yet it is neither necessary nor even probable that he achieved either his first (incorrect) or his ultimate solution by means of applying the Merton Rule, in the sense of his having relied upon a past tradition for either solution. It is not necessary because there is more than one way to arrive at the same truth. It is not probable because of the chronological order in which his physical conclusions first appear, as well as because of fundamental differences in concepts and methods between mean-speed and velocity-ratio determinations as outlined above.

To sum up the stages of Galileo's thought on falling bodies to the end of 1604: He had begun, about 1590, with a causal explanation of speed of descent that related speed to the ratio of densities of the falling body and the medium. That ratio being constant, he supposed the speed to be essentially constant, and treated acceleration as a mere temporary event at the beginning of motion. Around 1602 he became aware of the fact that acceleration persisted throughout free fall and had to be taken into account. By 1604 he was convinced that the spaces fallen from rest were as the squares of the times, but assumed that the speeds increased in proportion to the distances fallen from rest, believing that he had proved the correct relationship from that assumption. He had not yet considered the growth of speed with time as a separate possibility since he was satisfied with his first attempted demonstration. He had just completed it when the nova of 1604 appeared, and for a time he was taken up with the controversies that arose over this celestial phenomenon. During the following summer he gave instruction to the young heir of the Medici family, later Cosimo II. He then composed his first book, dedicated to Cosimo, which dealt with the proportional compass. The plagiarism of this book led him into a legal action and the composition of another book against a pirating of his work, published in 1607. It is not surprising that nothing identifiable was done by Galileo concerning motion during this period.

It was probably during 1608 that Galileo turned his attention once again to problems of motion: at any rate, during the first half of 1609, he announced to the Roman mathematician, Luca Valerio, his intention of founding the science of motion on two propositions. From this it appears that he had been engaged in organizing his previous work, extending it, and preparing a systematic treatment of it for a book. A series of undated fragments regarding motion, preserved in Codex A of the Galilean manuscripts at Florence, may therefore be ascribed principally to the period beginning about 1608 and continuing, at broken intervals, up to 1635 or thereabouts. The problem of establishing a chronological order for these fragments, in order to retrace Galileo's steps, is simplified by the fact that the great bulk of them are fairly sophisticated and show a clear understanding of uniform acceleration, whereas we are concerned only with the smaller number that belong to the period before that understanding was completely achieved. Again, among the latter are some that are evidently associated with work done before 1602, and these do not bear on questions of acceleration at all. It is with the intermediate group that we are concerned.

First comes a proof, written in Latin, that is essentially identical with the fallacious demonstration composed in Italian in 1604.[12] This Latin version is not in Galileo's handwriting and is canceled by two crossed lines. The hand is that of Mario Guiducci, who did not meet Galileo until 1614, so the copy cannot be earlier than that year. The demonstration, however, is almost certainly one composed by Galileo in 1608–9 when he was preparing his book on the science of motion.

Another fragment of great importance begins with the words *Cum enim assumptum sit . . .*, and from its content it is evident that the words refer to the asumption contained in the demonstration copied by Guiducci.[13] This fragment is in Galileo's hand, and deals with a proof that the times of motion along the vertical and along an inclined plane of the same height are in the ratio of those lengths. This document is of considerable importance in reconstructing and dating the development of Galileo's ideas.

Galileo had believed at first, when his attack on problems of motion was still based on the idea of cause, that the speeds (not the times) of motion on vertical and inclined planes must be in the above proportion. This notion was set forth in his unpublished *De motu* of 1590, with the rueful remark that the ratios were not observed in actual bodies.[14] After his recognition that acceleration must be considered, he was in a position to obtain the correct result embodied in the fragment under discussion. This he did by an analysis in terms of one-to-one correspondence between points on lines of differing length, and between intercepted segments of such lines. On the verso of the same sheet he undertook to prove the corollary that the speeds from rest to two different points in the vertical were as the squares of the distances fallen. But here the diagram is only partly lettered, and the attempted proof is abruptly broken off. Over it, as the document is preserved today, are pasted two slips of paper bearing in Galileo's hand two propositions of an entirely different nature, referring to the "moments of gravity" (a phrase related to Galileo's earliest work on motion) of bodies along the vertical and along inclined planes of the same height.

Now, on 5 June 1609 Galileo wrote to Luca Valerio to secure his opinion on the validity of introducing two propositions relating effective weight to speed as a basis for a new science of motion. Galileo's letter is lost, but Valerio's reply, dated 18 July 1609, makes possible the identification of the two propositions with those pasted over the abandoned demonstration mentioned above. Thus it is safe to date the fragment with the pasted slips as belonging to the early months of 1609, and to identify that fragment (and the demonstration later copied by Guiducci that must originally have immediately preceded it) with the systematic treatment of accelerated motion that Galileo was preparing in 1609.

The same fragment further affords strong evidence that it was during the early months of 1609 that Galileo first detected something seriously wrong in his attempted demonstration of 1604, and still did not abandon the false assumption on which he had based it. What he did instead was to substitute, for the corollary he had attempted to prove, two propositions of a dynamic character as a basis for reasoning about speeds in fall. It was concerning the validity of this maneuver that he wrote to Valerio.

The manner in which Galileo discovered the inapplicability of his previous reasoning to the speeds over different vertical spaces, without seeing instantly that he must abandon the idea of having (instantaneous) speeds proportional to those spaces, is extremely interesting in its bearing on his use of diagrams to represent physical variables. I have reconstructed the process in some detail elsewhere.[15] It appears to me that by mid-1609 Galileo had grasped the essential nature of an acceleration proportional to space traversed; namely, that it cannot allow continuous growth from rest, but that if we grant the slightest discontinuity at the very beginning of motion, it is in no way self-contradictory. All that one must give up is the concept of such acceleration as *uniform;* and it is of interest that in all the fragments under consideration, there is no mention of uniform acceleration, but only of "natural acceleration" or of "accelerated motion." Quite possibly Galileo entertained the view for a time that natural acceleration is not quite mathematically uniform, in the above sense. Such a view would explain the fact of Guiducci's having copied, as late as 1614 (and his copy may be of even later date), a demonstration of Galileo's that still assumed increase of speed proportional to space traversed.

In any case, astronomy once more intervened at a crucial moment to interrupt Galileo's investigation of motion. Just as the

nova of 1604 had appeared on the very day that Galileo sent the earlier letter to Sarpi, so the news of the newly invented Dutch telescope reached Galileo's ears in the very same month that he was awaiting a reply from Valerio. From July 1609 to an undetermined later date Galileo was occupied with the duplication and improvement of the new instrument, with astronomical observations, with a change of position from Padua to Florence, and with controversies over floating bodies, sunspots, and other matters unconnected with the mathematics of motion. Hence the most that can be done with the ensuing fragments is to put them in a plausible order, without attempting to give them any precise dating.

The next development appears to have been the derivation of the double-distance rule for uniform motion following accelerated motion from rest.[16] The fragment containing this derivation proceeds also to apply it to the proposition that Galileo had already proved before he wrote to Valerio; namely, that the times of motion along inclined planes are as their lengths. Here it is of interest that Galileo makes no assumption concerning a mean speed, nor does he draw completely the triangular diagram associated with the medieval mean-speed theorem.

The long delay in any reference by Galileo to the double-distance rule is significant. That rule is not to be found in the 1604 letter or demonstration, nor in the Latin version of his similar demonstration of 1608–9. Yet it is a very simple rule to state, and it is even more comprehensible and impressive to the nonmathematician than the times-squared rule sent to Sarpi and embodied in those documents. I can only surmise that Galileo was not yet aware of the double-distance rule by 1609, and consequently that he was still oblivious to the medieval corpus of works in which that very rule was given for uniform change.

In the fragment just discussed, we find also a new proof of the proportionality of times along vertical and inclined planes (of equal height) to their lengths. This proof utilizes the mean-proportional relation for times of fall from rest to different points, embodied in the Latin version of the 1604 demonstration. The mean-proportional relation is of course a direct consequence of the times-squared relation; it has no direct and evident relation to the mean-speed concept.

Another fragment that seems to be of about the same date is the first to state, in flat contradiction with the demonstration of 1609, that the overall speeds of falling bodies are as the square roots, and not as the squares, of the distances fallen.[17] But no proof is offered, nor is there any notation to indicate either Galileo's joy at a new discovery or his recognition at last of the proportionality of increase of speed to elapsed time. The accompanying diagram suggests the manner in which the proposition first occurred to Galileo, because it represents accelerated motions separated from any consideration of instantaneous velocities, but associated with times of fall. It is probable that Galileo himself did not at once perceive the full import of this proposition, which, coupled with his long-standing knowledge of the times-squared rule, would at last put the speeds directly proportional to the times. At the same time it would clearly contradict any lingering idea that an acceleration could exist in which proportionality both to space and to time could be indifferently applied.

Finally there is a fragment containing a memorandum that I am inclined to date as belonging to 1615, though hesitantly. It reads as follows:

> The spaces [covered] in accelerated motion from rest and the spaces in uniform motion following accelerated motions, and made in the same times, maintain the same ratio between them; the latter spaces are doubles of the former. The times, however, and the velocities acquired, have the same ratio between them; this ratio is the square root of the ratio of the said spaces.[18]

This at last summarizes all the correct ideas. It embodies a concept that Galileo imparted orally to G. B. Baliani in 1615, and it has the appearance not of the record of some new development, but of a summary of earlier results, such as one often jots down upon resuming a previous inquiry that was left unfinished. Quite possibly it belongs to 1616, after Galileo had been silenced by the Church on astronomy; this would be a reasonable time for him to have returned to his studies of motion. It is also possible that the Guiducci copy mentioned earlier belongs to the years 1616–20; if so, the fragment cited above would be of still later date. But I doubt this, largely because of the Baliani communication.

In any event, this fragment ends the intermediate period and opens that of the more sophisticated notes, most of which appeared

in some form in Galileo's *Two New Sciences*. In the whole process up to this point I find no trace of any use of the concept of a mean speed, but only of ratios and of one-to-one correspondences. Galileo's ultimate discovery of the essential difference between putting speeds in acceleration proportional to space and relating them to time emerged as a result of conclusions that Galileo could not doubt and therefore felt obliged to reconcile; not as a result of his having explored first one alternative and then the other. It is my considered opinion that during all this time, they never appeared to him as truly alternatives at all; as we shall see, he ultimately admitted candidly that he long thought them to be equivalent and mutually compatible.

Galileo's ultimate published rejection of the proportionality of speed in actual fall to the spaces traversed from rest has been widely misunderstood for two reasons. First, it has generally been discussed apart from the context that precedes it; and second, the context in which it has been discussed in modern times is that of a knowledge of physics and a theory of history so deeply rooted as to induce us to alter Galileo's own words inadvertently. For we shall see translators into different languages agreeing in a mistranslation, from which it is clear that strong prepossessions are at work.

Alexandre Koyré, seeking the true meaning of Newton's *Hypotheses non fingo,* had occasion to remark of its English and French translators: "As the Italian proverb has it, *traduttore-traditore* (translators are traitors). . . . They did not limit themselves to translating; they made an 'interpretation,' and in so doing gave to Newton's assertion a sense that was not Newton's sense."[19]

Koyré was perhaps too severe with the offenders. Real difficulties exist in translation; not only the translator's prepossessions, but what he regards as common knowledge, will enter into the product. A striking illustration is provided by Koyré's own rendition of a passage from Galileo's *Two New Sciences*—a passage of considerable historical significance and one that has been the subject of dispute from the time of its original publication in 1638 down to the present. Koyré's translation in 1939 was even more defective than the English of 1914 or the German of 1891. It read:

> *Lorsque la vitesse à la même proportion que les espaces franchis ou à franchir, ces espaces seront franchis en temps*

égaux. Car si la vitesse avec laquelle le grave franchit l'espace de quatre coudées était double de la vitesse avec laquelle il a franchi les deux premiers, etc.[20]

The English translation of a quarter-century earlier at least preserved the initial plural noun, in accordance with Galileo's invariable treatment of all such measures as ratios:

> If the velocities are in proportion to the spaces traversed, or to be traversed, then those spaces are traversed in equal intervals of time; if, therefore, the velocity with which the falling body traverses a space of eight feet were double that with which it covered the first four feet (just as the one distance is double the other) then the time-intervals required for these passages would be equal. But for one and the same body to fall eight feet and four feet in the same time is possible only in the case of instantaneous [discontinuous] motion; but observation shows us that the motion of a falling body occupies time, and less of it in covering a distance of four feet than of eight feet; therefore it is not true that its velocity increases in proportion to the space.[21]

The German translation, two decades earlier still, was virtually identical with the English:

> *Wenn die Geschwindigkeiten proportional den Fallstrecken wären, die zurückgelegt worden sind oder zurückgelegt werden sollen, so werden solche Strecken in gleichen Zeiten zurückgelegt; wenn also die Geschwindigkeit, mit welcher der Körper vier Ellen überwand, das doppelte der Geschwindigkeit sein solle, mit welcher die zwei ersten Ellen. . . .*[22]

Thus all modern readers of this text, except perhaps some who have used the original Italian, are likely to have received the impression that pervades all recent discussions of it. Even those using the Italian may have had the same prepossessions as the three translators cited, and hence the same impression. That impression is that Galileo, in his published argument against proportionality of velocity to space traversed in uniform acceleration, relied on some concept of *average* speed in free fall, and made the naïve assumption that such *average* speed would obey the rule applying to uniform motion. Under that impression, many historians have advanced reconstructions of Galileo's thought in which he is supposed to have known and misapplied the Merton Rule.[23] This in turn has strengthened the widespread conviction that the historical inspiration behind Galileo's law of falling bodies was his study of certain medieval writings.

But the original wording shows that whatever the reasoning was that Galileo relied on in this argument, it had nothing to do with a mean-speed comparison, nor did it rely on the application of any theorem derived from the analysis of uniform motion. Galileo's own words were:

> *Quando le velocità hanno la medesima proporzione che gli spazii passati o da passarsi, tali spazii vengon passati in tempi eguali: se dunque le velocità con le quali il cadente passò lo spazio di quattro braccia, furon doppie delle velocità con le quali passò le due prime (sì come lo spazio è doppio dello spazio). . . .*[24]

That is to say:

> If the velocities have the same ratio as the spaces passed or to be passed, those spaces come to be passed in equal times: thus if the velocities with which the falling body passed the space of four *braccia* were doubles of the velocities with which it passed the first two (as the space is double the space). . . .

Although the word *velocità* remains the same in Italian whether singular or plural, the definite articles, verb forms, and relative pronouns here leave no doubt that Galileo meant the plural. The first use of "doubles" is also plural (*doppie*), unlike the second "double" (*doppio*).Very early English translations by Salusbury (1665) and Weston (1730) correctly gave the plural "velocities," but for *doppie* reverted to a singular. The anonymous Latin translator of 1699, however, preserved *all* the plurals of Galileo's Italian.

It is hardly a mere coincidence that all three early translators were almost perfectly faithful to Galileo's precise wording, while their three modern counterparts agreed almost completely in ignoring that wording. The modern translators were so well informed about the truths of physics that Galileo's strange syntax did not attract their attention. In the case of Koyré, scientific knowledge was reinforced by a philosophical prepossession that is of no small importance to the present state of opinion regarding Galileo's thought. What Galileo said was trivial to Koyré; the important thing was what he must have meant. But Koyré's treason to Galileo was loyalty to a higher cause; it was at worst a feat of legerdemain, of *traduttore-tragitore*. His fault was that of excessive knowledge, even greater than that of his German and English counterparts. The virtue of the early translators was that of scientific and historical ignorance, a state in which it was best to let Galileo speak for himself.

As previously remarked, the disputed passage has customarily been examined not only in mistranslation, but also out of context. It lent itself to the latter treatment because the rejection of space-proportionality was presented by Galileo as a "clear proof" in a single long sentence. It is easy to jump to a conclusion about what it is that was to be proved, something that may much better be determined by reading carefully the discussions that precede and follow the passage in question. Here I shall merely summarize that context, but the reader is urged to examine it carefully for himself.

Salviati has begun by reading, from a Latin treatise of Galileo's, the definition of uniform acceleration as "that in which equal increments of velocity are added in equal times." There follows a lengthy discussion of another matter, the relevance of which will be pointed out below. Returning to the definition, Sagredo suggests that its fundamental idea will remain unchanged, but will be made clearer, by substituting "equal spaces" for "equal times." To this he adds an assertion that in actual fall, velocity grows with space traversed. Salviati replies that he once held the same view, and that Galileo himself had formerly subscribed to it, but that he had found both propositions to be false and impossible.

Now, it is universally believed that Salviati was here asserting that the *definition* of uniform acceleration in terms of equal space-increments was false and impossible, implying in it an internal contradiction. His words, however, do not support such a view. Sagredo's two propositions are (*a*) that there is no fundamental difference between relating velocity in uniform acceleration to time and relating it to space, and (*b*) that in actual fall, speed is in fact proportional to space traversed from rest. Salviati is thus obliged to show Sagredo that those two propositions are false and impossible.

But at this point, Simplicio intervenes to assert *his* belief that (*c*) an actual falling body does acquire velocity in proportion to

space traversed, and that (*d*) double velocity is acquired by an actual body in fall from a doubled height. Nothing is said by Simplicio about the definition of uniform acceleration, nor does he overtly deny that doubled time of fall would equally produce doubled velocity. Both of Simplicio's assertions are restricted to material falling bodies; he asserts first that their speeds are proportional to distances traversed, and second that this is a simple geometric proportionality.

It is to Simplicio, not Sagredo, that Salviati replies with the disputed argument, which he prefaces with the words, "and yet [your two propositions] are as false and impossible as that motion should be completed instantaneously, and here is a very clear proof of it." Thus if we pay attention to the logical structure of Galileo's book, the proof in question relates only to actual falling bodies, and that is why it invokes observation as a step. Salviati's answer to Sagredo is by no means completed after that proof. In order to satisfy Sagredo, Salviati still must show that there is a difference in the consequences that flow from time-proportionality, and that those consequences *are* compatible with the observed phenomena of actual falling bodies. That Galileo is perfectly aware of all this is shown by the fact that the additional steps are carried out, in an orderly manner, in his ensuing pages. That part of the discussion, however, does not concern us here.

Let us instead examine Galileo's argument, correctly translated, in the hope of discovering his line of thought.

> If the velocities [passed through] have the same ratio as the spaces passed or to be passed, those spaces come to be passed in equal times: thus if [all] the [instantaneous] velocities with which the falling body passed the space of four *braccia* were [respectively the] doubles of those with which it passed the first two *braccia* (as one distance is double the other), then the times required for these passages [over the spaces named] would be equal.

No diagram accompanies this statement, and none has preceded it. Galileo expects his readers (indeed, his imaginary auditors) to grasp his meaning without a diagram. If he wanted them to conceive of and compare mean speeds, he would have had to introduce and to illustrate that concept. Instead, he called their attention to all the varying velocities with which the falling body moved, not to any uniform velocity that might represent this totality. If the plural "velocities" leaves any doubt on that score, the plural "doubles" removes it. Salviati did not slip inadvertently into the unusual and rather awkward plurals; they were essential to his argument, and he stressed them. If each conceivable velocity passed through in the whole descent is the double of some velocity passed through in the first half of the descent, then there is no way of accounting for any change in the time required for one descent as against the other. That is all there is to his argument. The first statement does not invoke a rule for comparing uniform motions, as is generally believed; the phrase "or to be passed" can only refer to continuing acceleration from rest. The opening words simply state in general terms what the balance of the passage applies to Simplicio's numerical exemplification.

But how many velocities are meant in each case by the deliberate plurals? The answer to that question had been given in the long discussion that intervenes between the definition of uniform acceleration and the argument with which we have been concerned. The relevance of that discussion becomes apparent only when the above argument is correctly understood; I, at any rate, regarded it as one of Galileo's habitual digressions, made to keep things interesting, as long as I accepted the general view that Galileo had erred in his "clear proof." Let us review that discussion.

Sagredo and Simplicio had at once objected to Galileo's definition on the grounds that it could not apply to real bodies, for it would require them to pass through an infinite number of speeds in a finite time. Salviati pointed out that this is in fact possible, because the body need not (indeed, cannot) remain at any one velocity for a finite time. He satisfied them that there could thus be infinitely many velocities in any uniformly accelerated motion, however small. That concept was fresh in the minds of Galileo's hearers when he spoke to them of the doubles of all the velocities in the whole motion as compared with those in its first half. No diagram was necessary for them, nor was any diagram appropriate to Galileo's meaning. The conception he desired was inescapable—or so Galileo thought. Removed from its context, it nevertheless seems to have escaped nearly everyone who analyzed the passage.

The single exception known to me is J. A. Tenneur, who in 1649 intervened in a dispute over this very point. His conception of Galileo's argument was this:

> If possible, let the heavy body fall [from rest] through two equal spaces AB and BC so that its speed at C has become double that which it had at B. Certainly, under the hypothesis, there is no point in the line AC at which its speed is not double that at the homologous point in the line AB. . . . Therefore the speed through all AC was double the speed through all AB, just as the space AC is double the space AB: therefore AC and AB are traversed in equal times.[25]

Tenneur had grasped the essential clue from Galileo's plurals, as shown in the second sentence above. If in the final sentence Tenneur reverted to the singular for each overall speed, instead of comparing the spaces and times for the infinite assemblages directly, as Galileo did, it was not a fault in this case, for he did not substitute any particular value, or assume any particular rule of "compounding," for it. That he understood Galileo's reasoning exactly is shown by his further argument, omitted here, based (like Galileo's) on the idea of one-to-one correspondence.

The absence of a diagram is also characteristic of the medieval writers, but their arguments began with the assumption of a representative single value within the set of speeds. Galileo's argument in the *Two New Sciences* needed only the notion of one-to-one correspondence between two infinite aggregates. That concept had already been developed earlier in the same book.[26] Thus Salviati's reply to Simplicio refers to no single speed and may be properly paraphrased thus:

> If all the infinite instantaneous velocities occurring in actual accelerated fall from rest over any finite space, say one of four *braccia,* were the respective doubles of all the infinite velocities occurring over the first half, or two *braccia,* of the same fall, then no difference in the times of fall could be accounted for. But we observe a difference in the times; hence proportionality of speed to space traversed from rest cannot govern the fall of actual bodies.

In this there is no appeal to the correct definition of uniform acceleration, or to any of its consequences; neither is any contradiction asserted to exist within the correct law for falling bodies, which is merely shown to be in conflict with experience. Nor is there any illicit use of a rule restricted to uniform motion, as suggested by writers from the time of Tenneur's opponent to our own day.

Sagredo's opinion that it is a matter of indifference whether time or space be taken as the measure of velocity-increments in uniform acceleration has important historical implications. One might suppose it to have been introduced merely to enliven the dialogue. In that case, however, it would scarcely have been necessary to have Salviati admit that he had once accepted the notion, and still less so for Galileo to acknowledge publicly his own former misapprehension. The purpose of diversion would be as well served by having Simplicio offer the incorrect definition as a rival to the correct one, and state that one must be false if the other was true. Instead we have Sagredo, who is never the spokesman for foolish positions, asserting that he believes the two to be equivalent. Salviati's admission that he had once believed this, to say nothing of Galileo's similar admission in print, show that the view was widely held and needed refutation. What is important about Galileo's admission is that it concerns not just the idea of space-proportionality, but also the idea of its equivalence to time-proportionality.

Inability to believe that the two could ever have been considered equivalent is so natural to us that we tend to impute one view or the other to Galileo's predecessors. We note that the Merton School writers (and Oresme) put velocity in uniform acceleration proportional to time, while many Peripatetics (and Tartaglia) put speed in free fall proportional to space. Soto gave free fall as a case of uniform acceleration. All these things are true, but they do not imply (as we are likely to think) that any one of these views was regarded as contradicting another.

Sagredo's assumption seems to have been universally adopted up to the time of Galileo. It reflects this kind of reasoning: "Space and time are both measures of velocity, so they must be proportional to each other with respect to velocity. Acceleration is merely increase of velocity, and if uniform acceleration is proportional either to spaces traversed or times elapsed, it must thereby be proportional also to the other. Since it is easier to think of equal spaces than equal times, it is more sensible to say that in uniform acceleration increases in proportion to spaces traversed. But if anyone prefers to relate it to times, there is no reason he should not do so." No one worked out the "proportion" for space, and only the "average" had been worked out for time.

The Merton Rule writers, who were quite specific in using time as the measure of uniform difform motion, were in fact discussing change in general, of which local motion was only a special case. The Merton Rule is really a general mean-degree theorem, not just a mean-speed theorem. Looked at in that way, it is clear that the existence of that rule did not automatically call attention to a problem of changing speed any more than to a problem of changing heat, or redness, or any other variable quality. Thus Albert of Saxony could remark that in free fall the speed grew with the growing spaces traversed, and at the same time he could know that the Merton Rule was valid for all changes with respect to time, without his perceiving any contradiction.

Had the possibility of a contradiction been perceived, one would expect it to have been raised as a question in at least some of the many commentaries on Aristotle's *Physics* written in *quaestio-dubitatio-responsio* form. At least some writers of such commentaries may be presumed to have been familiar with both views and to have been fond of disputation; yet they did not pose the question "Whether velocity increases in a falling body in proportion to distance or to time." The right answer to that question first appeared in 1545, when Domingo de Soto linked free fall to the Merton Rule, but in producing the answer he did not ask the question. In using free fall to exemplify the Merton Rule, it is likely that he also considered this linkage compatible with space-proportionality in free fall, since he did not contradict that popular opinion.

The first person known to have both asked and answered the question was Galileo. To clarify the previous confusion was precisely his purpose when he introduced the question in the *Two New Sciences,* where he also candidly confessed that it had long escaped his attention.

Notes to Essay 11

1. "Galileo's 1604 Fragment on Falling Bodies," *British Journal for the History of Science,* IV, 4 (December, 1969), 340–58; "Uniform Acceleration, Space, and Time," *BJHS,* V, 1 (June, 1970), 21–43.
2. See essay 5.
3. *Opere,* X, 97–100.
4. I. E. Drabkin and S. Drake, *Galileo on Motion and on Mechanics* (Madison, 1960), p. 85; *Opere,* I, 315.
5. *Opere,* X, 115. Full translations of the letters mentioned and of Galileo's attempted demonstration are contained in the first paper cited in note 1 above.
6. Galileo assumed increase of speed proportional to distance; they assumed increase proportional to time.
7. Contrary to the opinion of Alexandre Koyré, the experiments Galileo described in his *Two New Sciences* were quite adequate to verify the law; see Thomas Settle, "An Experiment in the History of Science," *Science,* 133 (1961), 19–23.
8. It is natural to object that such an observation, even if carefully prepared and carried out, would never accurately give 1–3–5–7. This is true, but beside the point. The question here being one of discovery, and not of verification, it is sufficient to describe some means by which the 1–3–5–7 law might have been suggested. A casual, even a careless measurement, using a string to represent the first interval as a unit, would here be an advantage to discovery. If careful measurement in some conventional unit gave the values 1.6, 5.4, 8.2, 11.3 for distances moved by a cannonball down a marble slab, no numerical relation would automatically suggest itself to a person interested only in the continuance of increase; but a string of length 1.7 would promptly answer 1–3–5–7 as a probable rule. With any rule once in mind, refinements for its verification would quickly suggest themselves.
9. Many progressions can be found that will fill the condition, such as 1–7–19–37–61 . . . , but not other uniform (arithmetic) progressions. Christian Huygens, as a young man, used similar reasoning to derive the odd-number rule for uniform acceleration and to exclude all geometrical progressions.
10. G. B. Baliani to B. Castelli, *Opere,* XIII, 348. The letter was written in 1627, but Baliani's only conversations with Galileo had occurred in 1615.

11. M. Clagett, *Science of Mechanics in the Middle Ages* (Madison, 1959), p. 271. The passage cited is from Heytesbury.
12. *Opere,* VIII, 383.
13. *Opere,* VIII, 388.
14. Drabkin and Drake, *op. cit.,* pp. 68–69; *Opere,* I, 301–2.
15. In the second paper cited in note 1 above.
16. *Opere,* VIII, 383–84.
17. *Opere,* VIII, 380.
18. *Opere,* VIII, 387.
19. A. Koyré, *Newtonian Studies* (Cambridge, Mass., 1965), p. 36.
20. A. Koyré, *Etudes Galiléennes* (Paris, 1939), II, 98, note 2. A new French translation of Galileo's book, published in 1970, repeats Koyré's mistake in this passage.
21. *Two New Sciences,* trans. H. Crew and A. De Salvio (New York, 1914, and later eds.), p. 168.
22. *Unterredungen und mathematische Demonstrationen,* trans. A. von Oettingen (Leipzig, 1891, and later eds.), II, 16–17.
23. See, for example, A. Koyré, *Etudes Galiléennes,* II, 95–99; I. B. Cohen, "Galileo's Rejection of the Possibility of Velocity Changing Uniformly with Respect to Distance," *Isis,* 47 (1956), 231–35; A. R. Hall, "Galileo's Fallacy," *Isis,* 49 (1958), 342–46; *Discorsi,* ed. A. Carugo and L. Geymonat (Torino, 1958), pp. 776–78.
24. *Opere,* VIII, 203.
25. J. A. Tenneur, *De motu accelerato* (Paris, 1649), p. 8. Tenneur employed a diagram designed to refute his opponent, but for the purpose of this argument a single vertical line AC, bisected at B, suffices. The deduction of Huygens (note 9 above), was first published in Tenneur's book.
26. *Two New Sciences,* pp. 31–33.