HISTORIA MATHEMATICA 1 (1974), 129-150

MATHEMATICS AND DISCOVERY IN GALILEO'S PHYSICS

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SUMMARIES

Galileo's steps in the discovery of the law of free fall and its application to inclined planes are retraced from one of his letters and some manuscript notes. Proofs of two preliminary theorems are reconstructed, and his methods of calculation are analyzed. Eudoxian proportion theory, and not mean-speed analysis, was the foundation of Galileo's work on motion.

Le chemin suivi par Galilée au cours de la découverte de la loi de chute libre, et ses applications aux plans inclinés, est reparcouru à partir d'une de ses lettres et des manuscrits. Les démonstrations de deux théorèmes préliminaires sont reconstituées, et ses méthodes de calcul sont analysées. La théorie eudoxienne des proportions, au lieu d'une analyse fondée sur la vitesse moyenne, est présentée comme le vrai fondement de l'oeuvre de Galilée sur le movement.

I passi compiuti da Galileo durante la scoperta della legge di caduta libera, e della sua applicazione ai piani inclinati, sono ripercorsi partendo da una lettera e alcuni manoscritti. Le dimostrazioni di due teoremi preliminari sono ricostruite, e i suoi metodi di calculo sono analizzati. La teoria delle proporzioni di Eudossio, e non quella della velocità media, fu la base dell' opera di Galileo sul moto.

I.

> *Truly I begin to understand that although logic is a most excellent instrument to govern our reasoning, it does not compare with the sharpness of geometry in awakening the mind to discovery.* [Galileo 1973, 133]

So said Simplicio, normally a spokesman for Aristotle, in the *Two New Sciences*, the book in which Galileo presented the mathematical theory of freely falling bodies which he had worked out some thirty years earlier. That theory was published in deductive form, starting from a single definition (of uniform acceleration) and a single postulate (that the same speed is attained in fall from rest through the same vertical height along any inclined plane). This orderly unfolding of results affords no clue to the procedures by which Galileo had in fact been led to them in the first place. When we reconstruct his steps from his own rough notes, we find that mathematics was indeed his most fertile source of discovery; hence it was natural for him to have one of his interlocutors express the above view. And since Galileo had begun his investigations of motion along conventional logical lines which had led him into many fallacies and errors, it was suitable to place the remark in the mouth of an Aristotelian philosopher.

Past attempts to reconstruct Galileo's procedures in discovering his new science of motion have made little use of his manuscript notes. These are bound in haphazard order in volume 72 of the Galilean manuscripts preserved at the National Library in Florence, and many of them consist only of diagrams and calculations with little to identify their nature and purpose. Indeed, as might be expected, the individual sheets that most likely record significant discoveries are characteristically chaotic in appearance. The orderly development of implications of each basic discovery is less difficult to trace among these notes, but it is also much less interesting than the identification of probable discovery documents.

Once a discovery has been made, the use of mathematics to develop its implications is almost routine, at least to us, and it was hardly less so to Galileo, though his methods were very elementary compared to ours in this regard. There are in fact two sorts of mathematical discovery in physics. One sort consists in the following out, systematically, of implications, and some of these may be so surprising as to be entitled to be called "discoveries," in the sense that they were unforeseen by the investigator. They were, however, implicit in what had gone before, and any mathematician would have been perfectly capable of finding them. The other sort of discovery is not a rigorous consequence of what has gone before, though it may have been suggested by that; it consists in the perception that a certain mathematical relationship holds for physical phenomena considered in a certain way. These two types of discovery in mathematical physics probably do much to account for the historical fact that progress seems to be jerky; the consequences of a discovery of the second type are usually exhausted in a generation or two, with innumerable discoveries of the first type, whereas centuries may elapse between bona fide discoveries of the second type. It is mainly with the latter that I shall be concerned in this paper.

The popularly-offered reconstructions of Galileo's procedures in establishing his new science of motion are certainly mistaken with respect to the role of mathematics in them. It is quite true that if Galileo had started out with a correct definition of uniform acceleration, as he did in his final published book, he would have been led ineluctably to his conclusions; and it is also true that such a definition had been given in the Middle Ages. All he would have needed to do would be to have applied this definition to the case of free fall, and of course to have added the postulate concerning speeds at the ends of inclined planes, which seems really to have been rather trivial and easy. And it is thus that Galileo's work is presented in textbooks, as a rather humdrum extension of medieval analyses of motion.

One trouble with that account is that Galileo's first treatise on motion, far from including a correct definition of uniform acceleration, does not mention that concept at all; and indeed, it treats acceleration as essentially irrelevant to the mathematics of free fall and as entirely irrelevant to motion along inclined planes. Another trouble is that in 1604, nearly a quarter-century later, Galileo seems to have adopted a quite erroneous rule for speeds in free fall as a basis for deriving the times-squared law, which in fact follows directly from the correct definition. A third difficulty is that the fundamental concept employed by all medieval analysts, that of the mean speed, appears nowhere in Galileo's published works nor even in any of his private notes on motion, in which his final theorems were worked out. Hence the simplistic historical theory that Galileo's science of motion was worked out as an extension of medieval results is quite false, though it remains true that any competent mathematician could have so extended those results, given the idea that free fall is in fact a case of uniform acceleration, and given the postulate about inclined planes.

There is a very good reason that no mathematical physicist had done this. Or rather, there are two such reasons, one of them mathematical and the other physical. The mathematical reason is that it was only shortly before the time of Galileo that the Eudoxian theory of proportion, embodied in the fifth book of Euclid's *Elements*, became available again to European mathematicians. It was that theory which Galileo applied to free fall, treating the growth of speed as continuous in the modern mathematical sense, and all his results depended on that treatment. The physical reason is that the causal approach to acceleration in free fall, demanded by Aristotelian principles, could not allow this to be rigorously continuous. No one before Galileo had been willing to abandon the idea of cause in physics, and in fact many of his younger contemporaries, including Descartes, rejected his assumption that in order to reach any speed from rest, a body must first have passed through every possible lesser speed. The opponents of Galileo preferred hypothetical causes to the principle of sufficient reason. But these topics must not detain us; our purpose is to reconstruct Galileo's procedures, in which mathematics replaced philosophy and led on to his discoveries concerning free fall.

II.

In his first treatise on motion, composed at Pisa about 1590, Galileo reduced the conditions of equilibrium on inclined planes to the law of the lever [Drabkin and Drake 1960, 63-69]. Since the topic of his treatise was motion, he tried to find from this a rule for the ratio of speeds along inclined planes. He reasoned that since weight was the cause of downward motion, it was also the cause of speed; and since speed varies with the slope of the plane, he assumed that speeds along planes of equal height should be inversely proportional to the lengths of those planes. He noted that such ratios were not borne out by actual trial, but among the reasons he listed for this he did not mention acceleration, which at the time he considered to be only a negligible effect at the very beginning of fall.

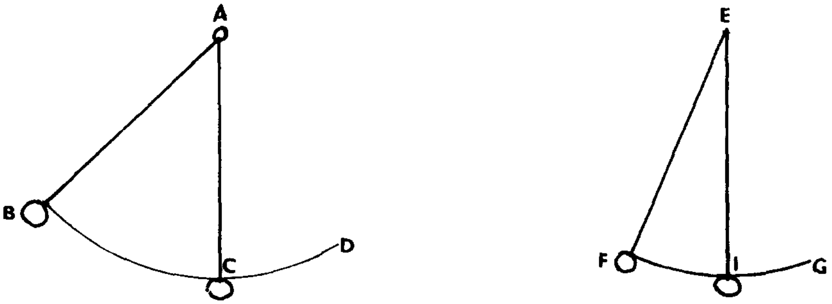
Shortly after moving to Padua in 1592, Galileo composed a treatise on mechanics. In an expanded version of this, probably about 1600, he refined his derivation of equilibrium conditions on inclined planes. This time he related the tendency to motion along an inclined plane to that of fall along a vertical circle tangent to the plane, whether the body was supported by an arc or was suspended from the center of the circle. [Drabkin and Drake 1960, 173] Though he was careful to note that this applied to the tendency at the initial point of fall, he still did not allude to acceleration.

By the year 1602, Galileo had concluded that descent along any chord to the lowest point of a vertical circle was made in equal time, and also that descent was swifter along two conjugate chords than along the single chord determined by them. He also conjectured that descents along all arcs of the lower quadrant were isochronous. Guidobaldo del Monte, to whom Galileo communicated these ideas, replied that they were implausible and were not borne out by experiments in which a ball was dropped along the inner surface of a large hoop. The earlier letters are lost, but Galileo's reply (dated 29 November 1602) reads as follows [Favaro 1934, Vol. 10, 97-100]:

> *You must excuse my importunity if I persist in trying to persuade you of the truth of the proposition that motions within the same quarter-circle are made in equal times. For this having always appeared to me remarkable, it now seems even more so that you have come to regard it as impossible. Hence I should deem it a great error and fault in myself if I should permit this to be repudiated by your theory as something false; for it does not deserve that censure, nor yet to be banished from your*

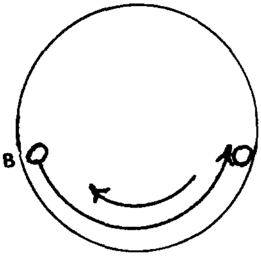
mind -- which better that any other will be able to keep it the more readily from exile by the minds of others. And since the experience by which the truth has been made clear to me is so certain -- however confusedly it may have been explained in my other [letter] -- I shall repeat this more clearly so that you, too, by making this [experiment], may be assured of this truth.

Therefore take two slender threads of equal length, each being two or three braccia long; let these be AB and EF . Hang A and E from two nails, and at the other ends tie two equal lead balls (though it makes no difference if they are unequal). Then, removing both threads from the vertical, one of them very much, as by the arc CB , and the other but little, as by the arc IF , let them go free at the same moment of time. One will begin to describe large arcs like BCD , while the other describes small ones like FIG . Yet in this way the moveable B will not consume more time in passing the whole arc BCD than that which is used by the other moveable F in passing the arc FIG . Of this I am rendered quite certain, as follows.

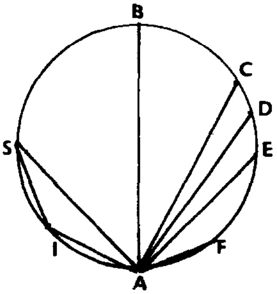


The moveable B passes through the large arc BCD and returns by the same DCB and then goes back toward D , and it goes 500 or 1000 times repeating its oscillations. The other goes likewise from F to G and then returns to F , and similarly will make many oscillations; and in the time that I count, say, the first 100 large oscillations BCD , DCB , and so on, another observer counts 100 of the other oscillations through FIG , very small, and he does not count even one more -- a most evident sign that one of these large arcs BCD consumes as much time as each one of the small ones FIG . Now if all BCD is passed in as much time [as that] in which FIG [is passed], though [the latter is] but one-half thereof, these being descents through unequal arcs of the same quadrant, they will be made in equal times. But even without troubling to count many, you will see that moveable F will not make its small oscillations more frequently than moveable B makes its large ones; they will always go together.

The experiment you tell me you made in the [rim of a vertical] *sieve may be very inconclusive, perhaps by reason of the surface being not perfectly circular, and again because in a single passage one cannot well observe the precise beginning of motion. But if you will take the same concave surface, and let ball B go freely from a great distance, as at the point B , it will*



go through a large distance at the beginning of its oscillations, and a small one at the end of these; yet it will not on that account make the latter more frequently than the former. Then as to its appearing unreasonable that given a quadrant 100 miles long, one of two equal moveables might traverse the whole, and the other but a single span [in the same time], I say that it is true that this contains something of the wonderful. But our wonder will cease if we consider that there could exist a plane as little tilted as that of the surface of a very slowly running river, so that in this [plane] a moveable will not have moved naturally more than one span in the time that on another plane, steeply tilted (or coupled with a great [initial] impetus even on a small incline), it will have passed 100 miles. Perhaps the proposition has inherently no greater improbability than that triangles between the same parallels and on equal bases are always equal, though one may be quite short, and the other a thousand miles long. But sticking to our subject, I believe I have demonstrated that the one conclusion is no less thinkable than the other.



Let BA be the diameter of circle BDA erect to the horizontal, and from point A out to the circumference draw any lines AF, AE, AD, and AC . I show that equal moveables fall in equal times, whether through the vertical BA or through the inclined planes along lines CA, DA, EA, and FA . Thus, leaving at *the same moment from points B, C, D, E, and F , they arrive at the same moment at terminus A ; and line FA may be as small as you wish.*

And perhaps even more surprising will this appear, [which is] also demonstrated by me: that line SA being not greater than the chord of a quadrant, and lines SI and IA being any [two chords conjugate to SA] whatever, the same moveable leaving from S will make its journey SIA more swiftly than just the trip IA starting at I .

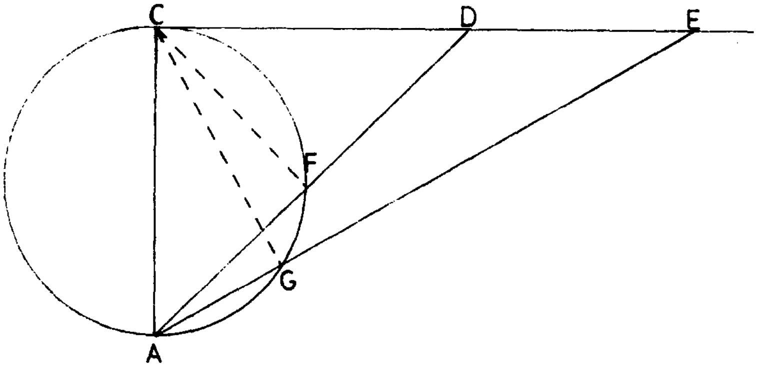
I have demonstrated this much without transgressing the bounds of mechanics. But I cannot manage to demonstrate that the arcs SIA and IA are passed in equal times, which is what I seek.

Do me the favor of conveying my greetings to Sig. Francesco [del Monte?], and tell him that when I have a little leisure I shall write to him of an experiment that has come to my mind for measuring the force of percussion. And as to his question, I think that what you say about it is well put, and that when we commence to deal with matter, [then] by reason of its accidental properties the propositions abstractly considered in geometry commence to be altered -- from which, thus perturbed, no certain science can be assigned, though the mathematician is so absolute about them in theory. I have been too long and tedious with you; please pardon me, and love me as your most devoted servitor.

III.

Of all the interesting points raised by this letter of 1602, we are concerned only with the nature of Galileo's proofs of the two propositions about descent along chords of a vertical circle. He considered those proofs to belong to mechanics, and he did not mention acceleration. Those clues suffice for reconstruction of his probable proofs in 1602, which do not survive in their original form among the surviving notes. [1] Previous speculations about them have assumed that Galileo's proofs followed from correct premises. I shall take nothing for granted except Galileo's knowledge of geometry and what he is known to have assumed earlier.

One of the problems posed by Galileo in his *De motu* of 1590 was that of finding two planes of equal height along which the speeds would be those of two bodies having different "natural" speeds in free fall. (The weights of bodies in air were supposed to affect their speeds.) His idea was thus to equalize the times by dropping the faster body along the longer plane. A logical variant of this problem would be to seek two distances along different planes which would be traversed in the same time. Under the mistaken assumption of uniform speeds determined by slopes in accordance with equilibrium conditions, Galileo's theorem of 1602 is evident from the following diagram:

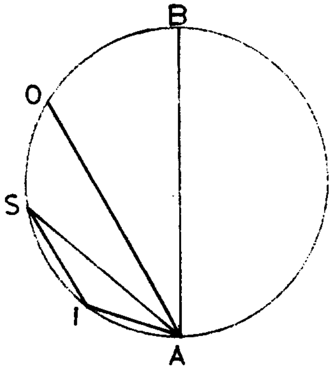


By Galileo's mistaken premise, the speed along DA is to that along EA as EA is to DA . Since CA is the mean proportional between AF and AD , and also between AG and AE , it is evident that EA is to DA as FA is to GA . (EA is to DA as sin CDA is to to sin CEA ; angles CDA and ACF are equal, as are angles CEA and ACG ; hence AF and AG are inversely proportional to AD and EA .)

Hence that the overall speeds along the chords are as the chords follows as a consequence of the mistaken assumptions. But when overall speeds are as the distances traversed, the times consumed are equal; this rule is valid for all motions, whether uniform or not. It can be proved from Aristotle's definitions of equal and greater speed by using the Eudoxian definition of "same ratio," as shown later by Galileo in the *Two New Sciences*. [cf. Drake 1973b] Galileo's reasoning at the time of writing to Guidobaldo was doubtless of this form.

Thus this striking result, that the times of descent are equal through all chords to the bottom of a vertical circle, was discovered mathematically from a false premise. The result would be easy to test by releasing two balls simultaneously from points properly marked on facing planes of different slope, or by propping two boards against the sides and bottom of a large circular frame. If Galileo made such a test, however, it would have further confirmed him in his mistaken belief that "speeds" along planes of the same height are inverse to the lengths of planes. At the same time, it would make the experimental failure of his earlier (and illusory) ratios even more puzzling to account for.

The second proposition likewise followed from the same false premise. Chords SA and IA are traversed in equal times, by the foregoing theorem, and the motion along SA is the swifter, by Aristotle's definition (more distance covered in the same time). Then motion along SI is still swifter, since at this speed it is OA , parallel to SI , that would be traversed in the same time as SA , again by the foregoing theorem. But the



motion in IA ensuing after motion through SI cannot be slower than the motion in SI alone, since in this case the body starts with that speed from I ; and even if IA were level, motion through it would not be less swift than that with which it started. Hence the motion in every part of descent SI-IA is swifter than that in SA , and is at least swift enough to traverse OA in the time of SA or IA . It follows that descent along SI-IA is completed in less time than descent through SA , and hence than descent through IA alone. [Favaro 1934, 213]

All these things are in fact true, and Galileo was later to prove them correctly, after he had discovered the law of acceleration in free fall and extended it to inclined planes. But here it will be good to note that these correct results are much easier to reach from Galileo's mistaken assumption of constant speed depending only on slope of plane than they were for him to deduce from the law of acceleration. Thus, if it is assumed that the appropriate speed is gained immediately, without acceleration, on the steeper plane, no problem arises when SI is very short; but is is not equally obvious, under the assumption of acceleration, that an overall gain in speed will more than make up for the increased length of descent along conjugate chords. The analysis gave Galileo a good deal of trouble later, when he tried to prove his earlier theorems by means of the law of free fall; indeed, his various attacks on propositions 6 (equal times along equal chords) and 36 (time is shorter along conjugate chords) are among the hardest of the fragments to arrange in chronological order.

Both these theorems, discovered mathematically, involved the comparison of "speeds," though there had been no prior attempt to define that concept. The existence of a *ratio* of speeds had been assumed from a well-defined concept in mechanics, that of a ratio of weights. Galileo later compared speeds by confining himself to time-ratios and distance-ratios, virtually eliminating the need for our speed concept. Under Euclidean rules no ratio

4 miles with 10 of speed in $\overset{6}{4}$ hours

9 miles with 15 of speed [in] $5^{?}$ hours

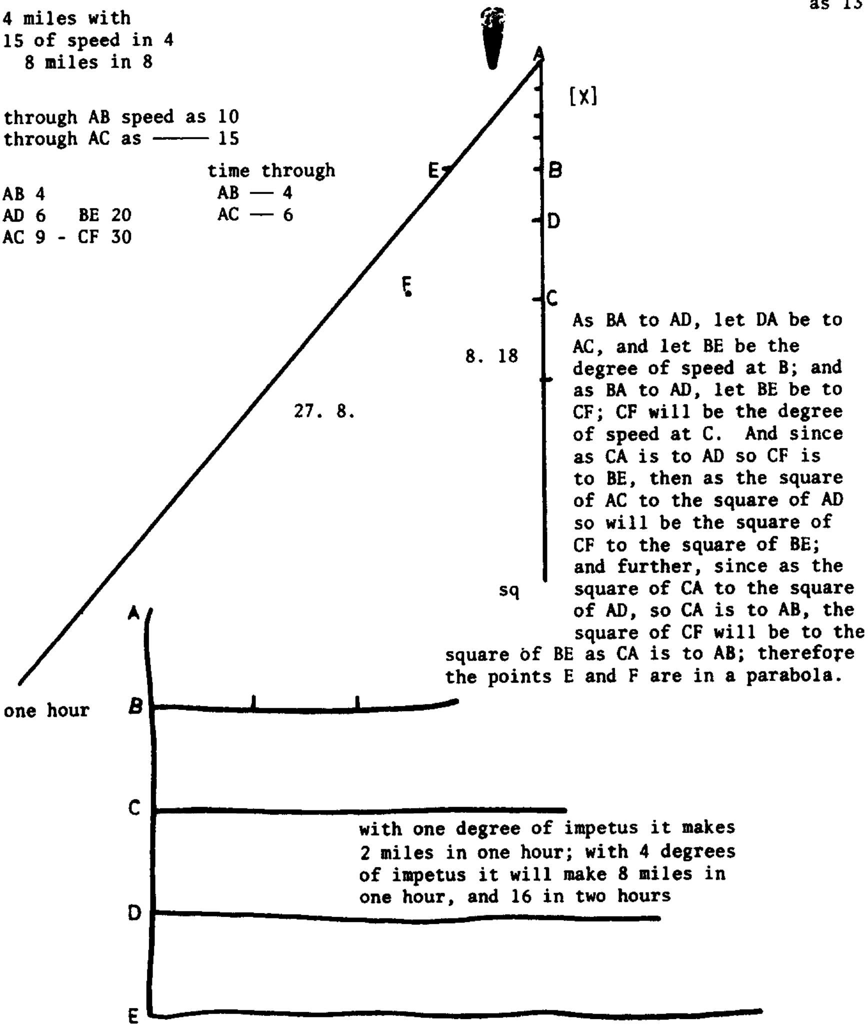
through AB
speed as 4
through AC
as 13 1/2

4 miles with
15 of speed in 4
8 miles in 8

through AB speed as 10
through AC as —— 15

time through
AB — 4
AC — 6

AB 4
AD 6 BE 20
AC 9 - CF 30



As BA to AD, let DA be to AC, and let BE be the degree of speed at B; and as BA to AD, let BE be to CF; CF will be the degree of speed at C. And since as CA is to AD so CF is to BE, then as the square of AC to the square of AD so will be the square of CF to the square of BE; and further, since as the square of CA to the square of AD, so CA is to AB, the square of CF will be to the square of BE as CA is to AB; therefore the points E and F are in a parabola.

with one degree of impetus it makes 2 miles in one hour; with 4 degrees of impetus it will make 8 miles in one hour, and 16 in two hours

Figure 1: English transcription of Mss. Gal., vol. 72, f. 152r

could be formed of magnitudes unlike in kind, such as distance and time; hence speed could be rigorously treated only by proportionalities.

IV.

The importance of acceleration in free fall probably did not become apparent to Galileo until Guidobaldo's objection to his theorems in 1602 stimulated him to suggest the use of pendulums in place of circular surfaces. Observations of long pendulums call attention to acceleration. The bob of a Foucault pendulum is visibly accelerated on each downswing, as one of three braccia (about six feet) long would be. Having in mind the association in his *Mechanics* between initial speeds on an inclined plane and along the tangent circle, Galileo may well have speculated that a similar, but even faster, increase of speed ought to occur along a plane of fixed slope, since the successive tangent planes become less and less tilted as the pendulum approaches the lowest point. At any rate, Galileo did turn his attention to the question of acceleration as such during 1603-04, and successfully searched for a rule linking distances, speeds, and times in free fall.

The discovery of this rule in turn was mathematical in character, and it also started from an initial false premise, though in this instance the error was immediately corrected in the process of discovery. The document recording this event (f. 152r) has recently been reproduced and analyzed. [Drake 1973a] Here I shall include only an English transcription of it (Figure 1) and a brief comment. It is a typical discovery document, written partly in Italian and partly in Latin; it starts with a common but false hypothesis of medieval origin, and ends with the correct result. A mistaken attempt to apply compound ratios of time and distance, at the upper right, is related to still another fragment, probably of 1601. To obviate conflicts of ratios, Galileo adopted the mean proportional, and this put the times-squared law into his hands.

Galileo's first move after obtaining the law of free fall was to return to his investigations of motion along inclined planes and to test its applicability to them. The principal document here is f. 189r (Plate 1), linked to f. 152r by the parabola roughly sketched at the left. This sheet is also linked to Galileo's letter to Paolo Sarpi in October, 1604, and to the important demonstration written out for him at the same time, by another figure, probably drawn with the sheet turned clockwise through 90°, which shows a parabola in a right triangle. This diagram marks Galileo's reduction of the inconvenient parabola of speeds to a simple triangle by the simple device of squaring the abscissae. [2] In the proof written out for Sarpi, he dealt with "speed" as the square of the value we now use, which

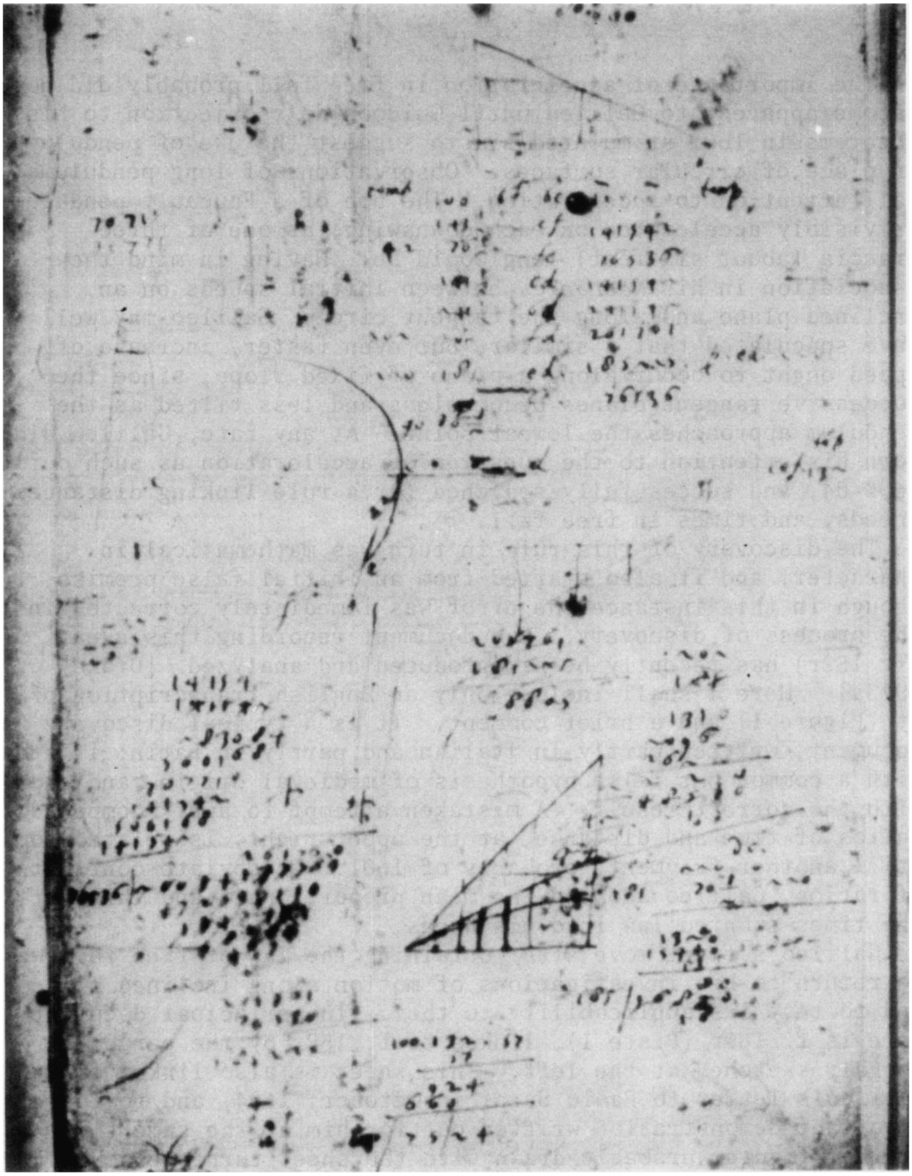
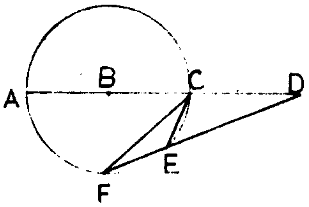


Plate I: *f. 189r, vol. 72, Mss. Galileiani (reproduced with permission of the Biblioteca Nazionale Centrale di Firenze, Florence, Italy)*

time				time
	100	BF, BC	100000	
	141 1/2	CF	141542	
42	76 1/2	FE	76536	
	141 1/2	CD	141572	
	241 1/2	BD	241572	
262	262	FD	261761	
220	185	ED	185225	ED 220
90	76 1/2	CE	76536	

7071
70771



141542
141542
283084
566168
707710
141542
566168
141542
20034137764

76536

261760

261761
185225
8805

[abandoned for]:

262
185
1310
2096
262
$\sqrt{48570}$ 220

ED DC

220
76 1/2
1320
1540
11
185) 16830 (90

Figure 2: A partial transcription of f. 189r, vol. 72, Mss. Galileiani.

value Galileo himself adopted in 1609. That f. 189r comes very early among Galileo's developments of the law of free fall is also shown by the erroneous value adopted for $\sqrt{2}$, corrected in calculations belonging to 1605 that will be shown later. Here Galileo wrote 7071 as a first approximation for $\sqrt{2}/2$ and then mistakenly wrote 70771 (for 70711) as the next approximation.

The calculations made on f. 189r confirmed that the new law of free fall could be applied to inclined planes consistently, and that the theorems previously sent to Guidobaldo del Monte were consistent with this treatment. Since the reproduction of the manuscript is hard to read, a partial transcription (Figure 2) is provided for use in following the reconstruction of Galileo's procedure.

Galileo's procedure was the following:

1. He drew the circle with quadrant chord CF and its two equal conjugate chords CE and EF , extending the latter out to intersect the horizontal ABC , produced. The radius BC was taken as 100000, later reduced to 100 in order to simplify the calculations.

2. The calculated total length of FD (using the erroneous value for $\sqrt{2}/2$ previously mentioned) was 261761 (or 262); the length of ED was obtained by subtracting from this the length of FE , 76536, giving 185225. Calculation of the time through ED by the mean-proportional rule relating times to distances from rest was then begun just below and to the right of the diagram, but was broken off in favor of using only three-digit figures.

3. Taking the length FD as a measure also of the time from rest through that distance, T_{DE} was computed from the proportionality $T_{DE} : T_{DF} :: \sqrt{185\ x\ 262}$; this gave $T_{DE} = 220$.

4. The next step was of capital importance, since it marks Galileo's abandonment of his old mistaken assumption that speeds should be inverse to lengths of plane and his adoption of the direct proportionality of times to lengths of planes of the same height. Here Galileo assumes that $T_{DE}:T_{CE}$ :: DE:CE; this enables him to calculate $T_{CE} = 90$ (immediately below the previous work).

5. Since the correct new assumption makes the times to F from rest at any points along the horizontal BCD proportional to the lengths of the corresponding planes, the time of descent along EF after fall from D along DE can be obtained by simple subtraction, and thus $T_{DF} - T_{DE} = {}_{D}T_{EF} = 42$. (The time through a distance in motion from rest at any point other than the initial point of that distance will be designated by showing the point of rest as a preceding subscript; thus ${}_{D}T_{EF}$ means the time through EF in motion starting from rest at D .)

6. Galileo's next assumption is in effect that of the single postulate adopted in the *Two New Sciences*. He supposes that the time along EF after fall from rest at a given height remains

the same, whether fall from that height to E is vertical, or is supported on the incline. This amounts to assuming that the same speed is acquired in a given vertical fall from rest, regardless of the path of fall. This assumption, at the time of writing f. 189r, probably had no other basis than that it made possible a very simple calculation. Here it gave the total time along the conjugate chords CE-EF as 90 + 42, or 132. This is a shorter time than T_{CF} = 141, confirming Galileo's earlier theorem that the time along conjugate chords is shorter than that along their corresponding single chord. And for the first time it indicated a quantitative measure of these times and their difference.

In all the above, the new assumptions required had their basis in mathematical simplicity alone, though they were in a way not devoid of experimental confirmation, since the results conformed to two theorems that Galileo already knew to be borne out by test. A rough direct test would also have been possible for the assumption that speeds are the same at the ends of two planes of equal height; Galileo would only have had to watch balls rolling along a level surface after leaving the planes. But whether or not Galileo made such a test, it is evident from the work on f. 189r that he did not reach his postulate on that basis.

Two other investigations are found on f. 189v, one of which relates to the general relation of motions on planes differing in both height and slope, but these probably belong to a somewhat later date. [3]

V.

The next important fragments in point of time are ff. 166 and 183, which show a very elaborate diagram and a related tabulation of distances and times. Across the face of the diagram, after making the tabulation, Galileo wrote a number of notes. The diagram is accordingly reproduced [Plates II, III, and IV] from Favaro's printed edition, where the notes were placed separately and do not obscure the diagram. Many of the calculations relating to these sheets are preserved on ff. 184 and 192. All these sheets bear watermarks related to one another and to dated letters of Galileo's, placing them in the year 1605.

The purpose of the diagram and tabulation is evident. Having found the law of free fall and related it to motion on inclined planes and broken lines, Galileo was in a position to resume his earlier inquiry into the question of fall along circular arcs. He now approached this through an analysis of the time of fall along the chord of a quadrant as compared with the time along two equal conjugate chords; that is, two sides of the inscribed octagon, then along four sides of the 16-gon, eight sides of the 32-gon, and so on. A relation discovered among the ratios

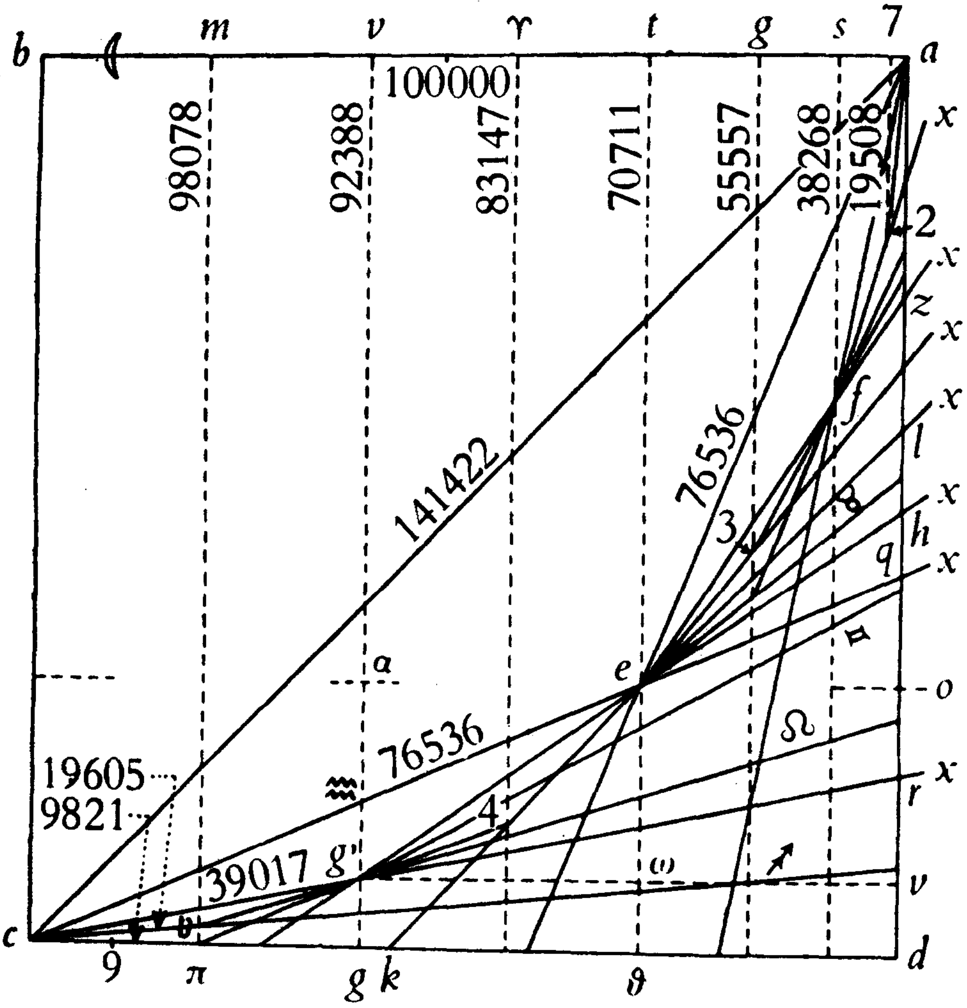


Plate II: *Mss. Galileiani, vol. 72, f. 166r* (Reproduced from A. Carugo and L. Geymonat, ed., *Galileo Galilei: Discorsi e Dimostrazioni Matematiche Intorno a Due Nuove Scienze* (Torino, Paolo Boringhieri, 1958), p. 544. *The notes Galileo wrote across the diagram or elsewhere on the same leaf are given separately in Plates III and IV.*

Cum semidiameter sit 100000, quadrantis circumferentia est $\left\{\begin{matrix}157143\\157042\end{matrix}\right.$; seu si semidiameter sit 1000, circumferentia quadrantis $\left\{\begin{matrix}\text{plus} & 1570\\ \text{minus} & 1571\end{matrix}\right.$; si □ sit 1000000, ◜ erit 785250.

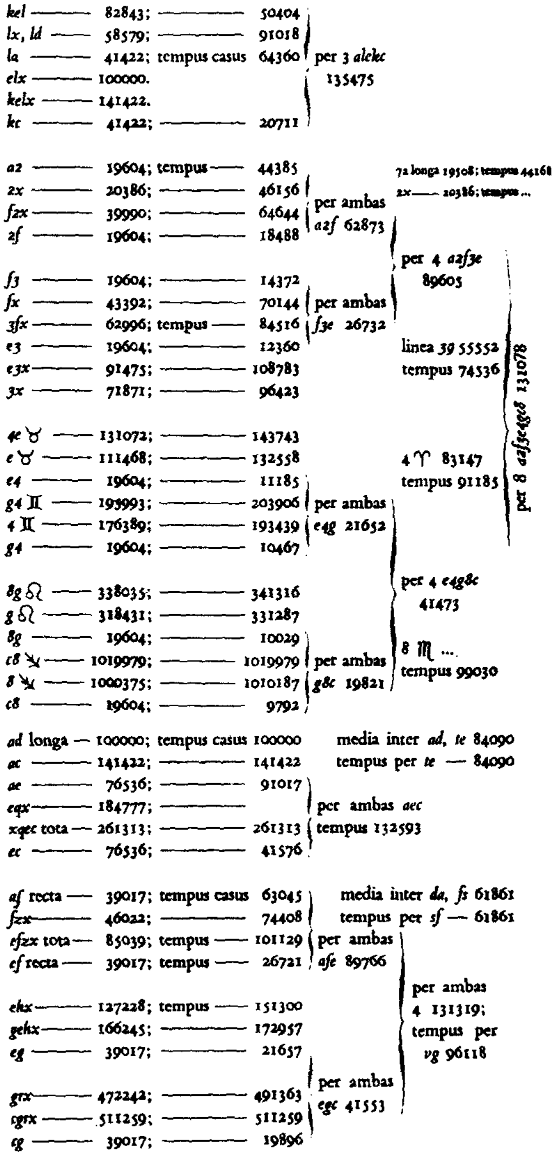
Tempus quo conficeretur circumferentia quadrantis, si esset recta et ad perpendiculum, 125331.

Tempus per *ac* ad tempus per 2 *aec* est ut 1000 ad $937\,^1/_2$ fere; tempus per *ec* ad tempus per 2 *egc*, ut 1000 ad $866\,^3/_5$; tempus per 8 *c* ad duas suas, ut 1000 ad $733\,^2/_3$.

ad longa puncta 180; sit tempus casus per ipsam m^1 180, et per ambas *adc* m^1 270.
ac ———— $254\,^3/_5$; ———————— m^1 $254\,^3/_5$.
ae ———— 138; tempus casus per illam m^1 164.|
ec ———— 138; tempus casus per eam post *ac* m^1 75, et per ambas *aec* m^1 239. 420
af recta — $70\,^1/_2$; tempus etc. $113\,^1/_2$ } et per ambas *afe* m^1 $161\,^1/_2$
fe ——— $70\,^1/_2$; tempus casus post *af* 48 }
eg ——— $70\,^1/_2$; tempus ———— 39 } et per ambas *egc* 75
gc ——— $70\,^1/_2$; ———————— 36 }
et per 4 *afegc* $236\,^1/_2$

Considera num tempus per *ac* ad tempus per duas *aec* sit ut radix radicis lineæ quæ a centro *b* super *ac* cadit perpendiculariter, ad radicem radicis perpendicularis ex eodem centro super *ae*. Tempus per 2 *egc* ex quiete in *e* est 66326; deberet autem esse 71757,|si casus per *egc* ad tempus per *ec* haberet eandem rationem quam casus per *aec* ad casum per *ac*: movetur ergo citius per *egc* quam per *aec*. Et ex quiete in 8 tempus per duas 8 *c* ad tempus per solam 8 *c* est ut 14378 ad 19598: longe igitur adhuc citius movetur quam per 2 *egc*.

Plate III: *Mss. Galileiani, vol. 72, f. 166r* (Reproduced from A. Carugo and L. Geymonat, ed., *Galileo Galilei: Discorsi e Dimostrazioni Matematiche Intorno a Due Nuove Scienze* (Torino, Paolo Boringhieri, 1958), p. 542-544. Transcription by A. Favaro, p. 419-421.)



kel —— 82843; —— 50404
lx, ld —— 58579; —— 91018
la —— 41422; tempus casus 64360 per 3 *alekc*
elx —— 100000. 135475
kelx —— 141422.
kc —— 41422; —— 20711

a2 —— 19604; tempus —— 44385 72 longa 19508; tempus 44161
2x —— 20386; —— 46156 2x —— 20386; tempus ...
f2x —— 39990; —— 64644 per ambas
2f —— 19604; —— 18488 *a2f* 62873
per 4 *a2f3e*
89605
f3 —— 19604; —— 14372
fx —— 43392; —— 70144 per ambas
3fx —— 62996; tempus —— 84516 *f3e* 26732
e3 —— 19604; —— 12360 linea *39* 55552
e3x —— 91475; —— 108783 tempus 74536
3x —— 71871; —— 96423

per 8 *a2f3e4g8c* 131078

4e ♉ —— 131072; —— 143743
e ♉ —— 111468; —— 132558 4 ♈ 83147
e4 —— 19604; —— 11185 tempus 91185
g4 ♊ —— 193993; —— 203906 per ambas
4 ♊ —— 176389; —— 193439 *e4g* 21652
g4 —— 19604; —— 10467

per 4 *e4g8c*
41473
8g ♌ —— 338035; —— 341316
g ♌ —— 318431; —— 331287
8g —— 19604; —— 10029
c8 ♐ —— 1019979; —— 1019979 per ambas 8 ♏ ...
8 ♐ —— 1000375; —— 1010187 *g8c* 19821 tempus 99030
c8 —— 19604; —— 9792

ad longa — 100000; tempus casus 100000 media inter *ad*, *te* 84090
ac —— 141422; —— 141422 tempus per *te* — 84090
ae —— 76536; —— 91017
eqx —— 184777; —— per ambas *aec*
xqec tota — 261313; —— 261313 tempus 132593
ec —— 76536; —— 41576

af recta — 39017; tempus casus 63045 media inter *da*, *fs* 61861
fzx —— 46022; —— 74408 tempus per *sf* — 61861
efzx tota — 85039; tempus —— 101129 per ambas
ef recta — 39017; tempus —— 26721 *afe* 89766
per ambas
ehx —— 127228; tempus —— 151300 4 131319;
gehx —— 166245; —— 172957 tempus per
eg —— 39017; —— 21657 *vg* 96118
per ambas
grx —— 472242; —— 491363 *egc* 41553
cgrx —— 511259; —— 511259
cg —— 39017; —— 19896

Plate IV: *Mss. Galileiani, vol. 72, f. 183r* (Reproduced from A. Carugo and L. Geymonat, ed., *Galileo Galilei: Discorsi e Dimostrazioni Matematiche Intorno a Due Nuove Scienze* (Torino, Paolo Boringhieri, 1958), p. 545-546. Transcription by A. Favaro, p. 421-422.)

of those times would lead to the arc as a limit. Some notations written over the diagram embody such conjectures; but it appears that Galileo then abandoned this project in favor of further explorations of fall along straight lines in various combinations. Of special interest is his concept of *casus*, discussed below.

Galileo's diagram (Plate II) does not show the circle of which *ac* is the chord of a lower quadrant; this circle would pass through points *a-f-e-g-8-c*. [4] To compute the figures in the tabulation requires only the assumptions previously made on f. 189r -- that the times along inclined planes of equal height are proportional to the lengths of plane, and that the times from rest to two points along a given plane are as the shorter distance is to the mean proportional between the two distances.

The letter *x*, seen along the vertical *ad* in Plate II, indicates the point of intersection of the designated line with the horizontal *ba*, extended. Zodiacal symbols near *ad* likewise refer to to points of intersection with the upper horizontal line. Galileo's procedure in the calculations was to apply the mean-proportional rule to vertical distances, and then to adjust them to the incline by recourse to similarity of triangles. Thus the mean proportional between *ad* and *te* is 84090, which is multiplied by 76536/70711 to get 91707, the time through *ae*.

A notation across the diagram to which I alluded earlier reads as follows: "The time through the two [planes] *eg-gc*, from rest at *e*, is 66236; it should be 71757 if [that] fall (*casus*) had the same ratio to fall through *ec* as fall through *ae-ec* has to fall through *ac*." Here the word *casus* means "time of descent in units in which the time through *ad* is 100000." This meaning can be ascertained by deriving Galileo's figure of 66236 (or rather, 66326, since it appears that he made a transposition in writing this). The derivation, outlined below, shows that he was able not only to calculate the time along a broken line of any number of sections, but that he could do this in terms of a single standard unit of measure. Since T_{ad} and *ad* had both been taken as 100000, Galileo had in effect a unit speed, giving him a means of comparing speeds without violating the Euclidean rule against forming ratios between magnitudes unlike in kind. The modern reader may object that the concept of "unit speed" already implies such a ratio. I shall not argue this point here, but am content to point out that the calculations outlined below foreshadow Galileo's later device of taking a selected line to represent both a distance and the time of fall from rest through that distance, which device was used frequently in the theorems of the *Two New Sciences*, and in his notes. [5]

The table on f. 183 gave the time of fall through *eg-gc*, but only after initial fall from rest through *ae*; that is, it gave not T_{eg-gc}, but rather ${}_aT_{eg-gc} = 21657 + 19896 = 41553$. The new problem was to find T_{eg-gc}, and to find this in units such that

T_{ad} = 100000. Galileo's procedure was a direct extension of what had been done on 189r.

Since ad = 100000, ec = 76536. Let $T_{ec} = ec$; then $T_{e\theta} = e\theta$ = 29289. Now, $e\omega = vg - te$ = 21677, the difference between verticals dropped from ab to g and to e. Hence $T_{e\omega}$ is 25197, mean proportional of $e\omega$, $e\theta$. It follows that $T_{eg} = 25197(eg/e\omega)$ = 25197(39017/21677) = 45353. Likewise ${}_{e}T_{gc} = {}_{e}T_{\omega\theta}(gc/\omega\theta)$ = 29289 - 25197)(39017/7612) = 20974. And $T_{eg-gc} = T_{eg} + {}_{e}T_{gc}$ = 45353 + 20974 = 66327.

Thus Galileo had developed a systematic procedure for the calculation of times of fall along any broken lines in terms of the time of fall through a standard vertical distance. (It should be noted, however, that these comparisons would not be borne out by experiment because of the factor of 5/7 for inertial moment in rolling as against the rate of acceleration in free fall.)

One other notation on f.166 also deserves comment: "Let ad be 100 *punti* long; let the time of fall through this be 180 minutes, and through both ad-dc, 270 minutes." This implies the the double-distance rule, to which Galileo may have been led by noting the numerical relations as the inclined planes on f. 166 were approaching the horizontal. The rule was proved by Galileo later, probably in 1607, in a memorandum establishing one-to-one correspondence between speeds in uniform and in uniformly-accelerated motions.

VI.

The foregoing documents show the manner in which purely mathematical considerations entered into Galileo's basic discoveries concerning free fall. The procedures seem to me rather different from Plato's demonstration that the triangle and the number 3 are the cause of fire, and from Kepler's proof that the number of planets must be six in order to accommodate the five Platonic solids each once and only once. Nor do they particularly resemble the calculations of Swineshead, Heytesbury's proof that uniform acceleration from rest is possible, or the famed triangular proof of Oresme that such acceleration is represented by its middle speed.

Galileo's first correct theorem concerning fall was that of the equality of times of descent along chords of a vertical circle to its lowest point. He reached that theorem by valid mathematical reasoning from a false assumption about ratios of speeds and without considering the role of acceleration at all. His attempt to persuade a friend that the conclusion was correct appears to have called his own attention to the importance of acceleration. An attempt to discover consistent ratios for accelerated motion, starting from an erroneous legacy of the Middle Ages, was successful through the adoption of an arbitrary mathematical device. That success gave him the law of free fall, but in a form that tempted him to define "velocity" in a bizarre way and then to abandon that concept for several years in favor of time and distance ratios. By means of these, he was able to discover many theorems concerning accelerated motion without formalizing their conceptual basis separately from the mathematics employed in them. It was only toward the end of his life that he turned to that task.

The profound difference between medieval and Galilean physics involves many things. Not least among these was the restoration of Eudoxian proportion theory in the sixteenth century, an event of great importance to the history of mathematics quite apart from the work of Galileo. An excessive concern with the history of philosophy on the part of historians of science has tended to conceal this fact and to create an illusion of greater continuity between the fourteenth century and the seventeenth than is justified by the events. It is probably true that given the correct text of Euclid and the authentic works of Archimedes, Galileo needed nothing from the Middle Ages for his work on motion. It is probably also true that given only the medieval Euclid, all the works of Jordanus Nemorarius, Thomas Bradwardine, William Heytesbury, Jean Buridan, and Albert of Saxony, this would not have enabled Galileo to go even as far as Nicole Oresme in the approach to a valid mathematical physics in the modern sense of that phrase.

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NOTES

1. A proof on mechanical assumptions is found on f. 151, but this is probably of later date. A similar proof was given as the second of three under Theorem 6 on accelerated motion in the *Two New Sciences*; this resembles the proof on f. 160.
2. Explanation of the proof for Sarpi was given in Drake 1973a, 91-2, on the basis that Galileo meant by *velocità* what we call v^2, before this diagram was noted.

3. The page was folded and the other side used with the paper turned at right angles to the position of f. 189r at one time, and then reversed at another. The two portions are unrelated, and the writing is smaller.
4. Galileo ran out of letters for this diagram and continued with numbers and conventional symbols; see below.
5. Galileo did not explicitly justify the procedure when he introduced it in the Third Day of the *Two New Sciences.* It was also used in the Fourth Day for "impetus," and explained in the discussion there. He would not have considered time-distance fractions legitimate, as we do, though his contemporary Marin Mersenne moved in that direction.

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A QUOTE ON HISTORY OF MATHEMATICS COURSES

"... A year or two ago, we were arguing about the syllabus for students studying mathematics at the University. It was widely agreed that the existing syllabus was too difficult. Two possibilties for change presented themselves, the obvious one of making the existing syllabus easier, and the much less obvious one of introducing certain new courses which it was claimed would be easier and more suitable for the weaker half of the students. A suggestion for such a course, later adopted, was that instruction be given in the history of mathematics. It was taken as axiomatic by ninety percent of the committee which had to deal with the problem that a course on the history of mathematics would be easier than mathematics itself, a proposition which escaped me. The whole affair was buried and forgotten in my mind until I happened to be acting as examiner. To my astonishment I found that the questions in the examination dealing with the history of mathematics were answered by only one candidate. When the final list was drawn up in order of merit it turned out that the solitary man stood at the head of the list. What struck me as particularly curious was that my colleagues seemed in no way interested in this complete reversal of what had been claimed before. So far from the history of mathematics being suitable for the weaker students, it was suitable only for the best."

-- from *Encounter with the Future* by Fred Hoyle (Simon and Schuster, 1965)