The Pendulum Swings again: A Mathematical Reassessment of Galileo's Experiments with Inclined Planes

Alexander Hahn[1] , Notre Dame

Abstract. After over 300 years of scrutiny, the subject of Galileo continues to be pursued with unabating intensity. Dava Sobel's *Galileo's Daughter* points to the popular interest in the man and his legacy. The Catholic Church, understandably interested in dispelling the notion that its censure of Galileo centuries ago is proof positive that religious faith and science as well as ecclesiastical authority and free pursuit of scholarship are irreconcilable, continues to offer explanations. New books, articles and conferences probe both in breadth and in depth the magnetic field charged by Galileo, science, and the Church.

Galileo's analysis of the physics of motion has also received considerable attention. In particular, a great deal has been written during the past thirty years about the structure and objectives of three experiments with inclined planes. Galileo had carried them out in Padua and recorded them in his working papers. The assessments of the three experiments differ widely in points of detail, but all regard them as sophisticated, ingenious, and remarkable. This article presents a new critical study of these experiments. Its conclusion is that one of the experiments is indeed a success, but that the other two fail and are abandoned because Galileo did not have a firm enough grip on the underlying physical principles and mathematical relationships.

1. Galileo on Motion. Galileo's journey of discovery of the laws of motion is lengthy, twisted, and anything but smooth. We recall it only briefly. It is described in [10], [11] and, in compelling detail, in [38].

Galileo's early efforts in Pisa (1589-1592) to understand the phenomenon of motion are the subject of the *De motu* manuscripts. Central to Galileo's explanations are two underlying concepts, a basic one of *uniform specific speed* of fall and the auxiliary one of an *impressed force*, that account for the nonuniform motions actually observed. The uniform specific speed of the object depends on the medium in which the object moves and is determined - via Archimedean hydrostatics - by the difference in the densities between object and medium. The impressed force is something that is put into an object by an external mover. Once imparted, this impressed power decays

[1] I wish to thank Ernan McMullin for much information about Galileo and his scientific pursuits, and in particular for drawing my attention a number of years ago to the research of Stillman Drake on Galileo's working papers. Thanks also go to Noel Swerdlow for very useful insights, and to Neil Delaney, David Kirkner, and Timothy O'Meara for many stimulating discussions about Galileo and his work.

gradually and the speed of the body changes steadily until the impressed power is completely dissipated and the object assumes its natural constant speed specific to the medium in which the motion takes place. Galileo's thinking during this time is still heavily influenced by Aristotelian and medieval elements. Only towards the end of the *De motu* does Galileo begin to accept the possibility that acceleration may be a fundamental feature of fall.

During the Padua period (1597-1610) Galileo's understanding of motion undergoes transition. His 1604 letter to Fra Paolo Sarpi provides important testimony: "the spaces passed in natural motion are in proportion to the squares of the times taken, and consequently that the spaces traversed in equal successive time intervals are to the odd numbers ..." Diverted by his interests in astronomy and the well documented conflict with the Church, Galileo does not present the final synthesis of his theory of motion until 1638. The *Discorsi* [2] is the exposition of a learned, four day long conversation among Salviati who represents Galileo himself, Sagredo an open minded supporter of the new science, and Simplicio, an adherent of the old Aristotelian point of view. Days three and four of these discussions focus in vernacular Italian on the treatise *De motu locali* - a book within a book in scholarly Latin - that is Galileo's own systematic treatment of motion. It is intended to be a clear and rigorous presentation in final form. Errors and missteps from earlier stages of Galileo's thinking are visible in the probing comments of the protagonists. The discussion analyzes the motion of objects undergoing constant acceleration, both in the situation of free fall and along inclined planes. Galileo derives his conclusions deductively, often with geometric constructions. He expresses quantitative relationships between time, distance, velocity, and acceleration, in terms of proportions; indeed, he uses only proportions of magnitudes of the same kind, for example, distance to distance, velocity to velocity, but not distance to time. In particular, he does not have the real number system and decimal notation at his disposal and does not formulate his conclusions in terms of equations involving variables and constants. (The systematic development of all this was only in its infancy at this time.) Within his context, Galileo arrives at the following basic insights.

1. All bodies falling in a vacuum do so with the same constant acceleration. For a body falling from rest, the speed is proportional to the elapsed time. This is so both in the situation of free fall and for balls rolling on an inclined plane.

2. The law of fall, namely, that the distance covered by a body moving from rest (again, either in free fall or rolling on an inclined plane) is proportional to the square of the time of the motion.

3. The trajectory of a projectile has parabolic shape.

Galileo regards two underlying principles as fundamental. One is a principle of inertia, namely that a body moving with a certain constant velocity (speed and direction) will continue to move with that same velocity unless an outside force acts on it [2] . The second is a principle of superposition of motions, namely that projectile motion can be conceptualized as an independent composite of a motion with constant velocity and a vertical motion with constant acceleration. It must be emphasized that Galileo's basic concepts (for example, velocity and acceleration) lack the precision that calculus would later give them and his principles of inertia and superposition never reach a definitive and general state. See [5], [10] and [38] for instance.

2. Galileo's Experiments. There is general agreement that Galileo arrived at his conclusions by combining the thinking of predecessors, geometric deductions, thought or "arm chair" experiments, and actual "hands on" experiments. However, the assessments of the relative importance of these elements have changed over the years. In particular, the question - central to an understanding of Galileo the physicist - as to the quality and purpose of Galileo's hands on experimentation has received a diversity of answers.

Galileo's contemporaries Descartes and Mersenne exercised skepticism about his experiments. See [5, p. 107] and [8, p. 20]. For the most part, however, historians have taken Galileo's word for it (as expressed on the third day of the *Discorsi*) that he used carefully constructed experiments as an important tool of both discovery and verification of his fundamental insights into motion. This includes Kant [3, preface] and Mach [4]. This point of view prevailed until the late 1930's when the influential historian Koyré proclaimed that Galileo's real experiments were most certainly inadequate and that he relied on thought experimentation. See [5, pp. 106-107] and [6, p. 224]. The pendulum begins to swing back in 1961 when Settle [8] reconstructed Galileo's basic apparatus and demonstrated that Galileo *could* have carried out with satisfactory precision the experiments with inclined planes he described. While emphasizing the point that the progressive evolution of Galileo's understanding of motion must surely have had an experimental aspect, essays written as recently as 1967 were still circumspect and tentative about the precise nature and impact of these experiments. See [10, pp. 11-12] and [11, pp. 328-329]. The backward swing of the pendulum gained speed instantly in the early 1970s when Drake [12] perused Galileo's unpublished working papers on motion in the Biblioteca Nazionale Centrale

[2] It is important to note that in this article "inertia" will be used to refer simply to a principle of uniform motion and not to Newton's definitive version of the concept. For Newton, inertia is an inherent property of the mass of a body that causes it to resist any change in its state of rest or motion along a straight line. This article parallels Galileo understanding of inertia as only a kinematic principle or an assertion about the motion of an object under ideal conditions.

in Florence and found written records of Galileo's experiments with inclined planes. They are the "smoking gun" that refutes the position of Koyré. These working papers - there are 160 sheets or *folios* - are now bound as Volume 72, or Codex 72, of the Galileo manuscripts. The websites

http://www.imss.fi.it/ms72/INDEX.HTM

or

http://www.mpiwg-berlin.mpg.de/Galileo_Prototype/index.htm

provide wonderful and very useful electronic renditions of the folios of the codex. Many of the folios are drafts of theoretical discussions that would later appear in the *Discorsi*. (The electronic version provides precise connections with cross-references.) Many folios are filled with computations. A number of folios, 80r, 81r, 86a r, 87, 90r, 91v, 102, 107, 111, 113r, 114v, 115r, 116v, 117, 152r, and 175v among them, contain diagrams and data that suggest studies of motion. The abbreviations r and v stand for "recto" and "verso", the "front" and "back" of the sheet in question. (In the listing just given, if neither r nor v appears, then both sides of the sheet are relevant.) Some of these folios are geometric explorations of the parabola and some are records of experiments. The historians who have studied them consider it "well substantiated by the evidence" (watermarks, for example) that they stem from the later Paduan period 1604-1610. For example, see Naylor [20, p. 366].

The present article will focus on 81r, 114v and 116v. Each of these folios gives evidence of an experiment in which Galileo has placed an inclined plane on a table, lets a ball roll down the plane, and records quantitative data about the ball's flight from the table's edge to the ground. Salviati informs us on the third day of the *Discorsi* that Galieo repeated some of his experiments "a full hundred times." Thus it would seem that each recorded measurement represents a cluster of trials. The general conclusions of Drake [12, 27, 32, 36, 37], Drake-MacLachlan [16], Naylor [13, 18, 19, 20, 25, 26, 28], and Hill [33, 35] - these are the historians who have studied them most thoroughly - are in agreement:

Drake [32, p. 4] uses folios 81r and 114v to conclude that Galileo is a "skilled experimentalist capable of holding his results within a variance of four units ..." The unit referred to here is Galileo's *punto*, or "point", a unit of length slightly less than one millimeter.

Naylor [18, pp. 168-169], reflecting about 81r, speaks of "indications that Galileo carried out meticulous, thorough-going studies of the form of projectile motion" and suggests that "Galileo had a striking talent for combining a mathematical approach to nature with a considerable mathematical technique. The simplicity and power of this particular form of experiment is quite remarkable."

Hill [35, p. 666] comments that "worksheets 81r, 114v, and 116v reveal an impressive experimental program, ingenious in structure, ambitious in concept, eminently successful in execution. This series of procedures enabled Galileo to provide powerful, perhaps empirically decisive, evidence for both the new speed law and the parabolic trajectory."

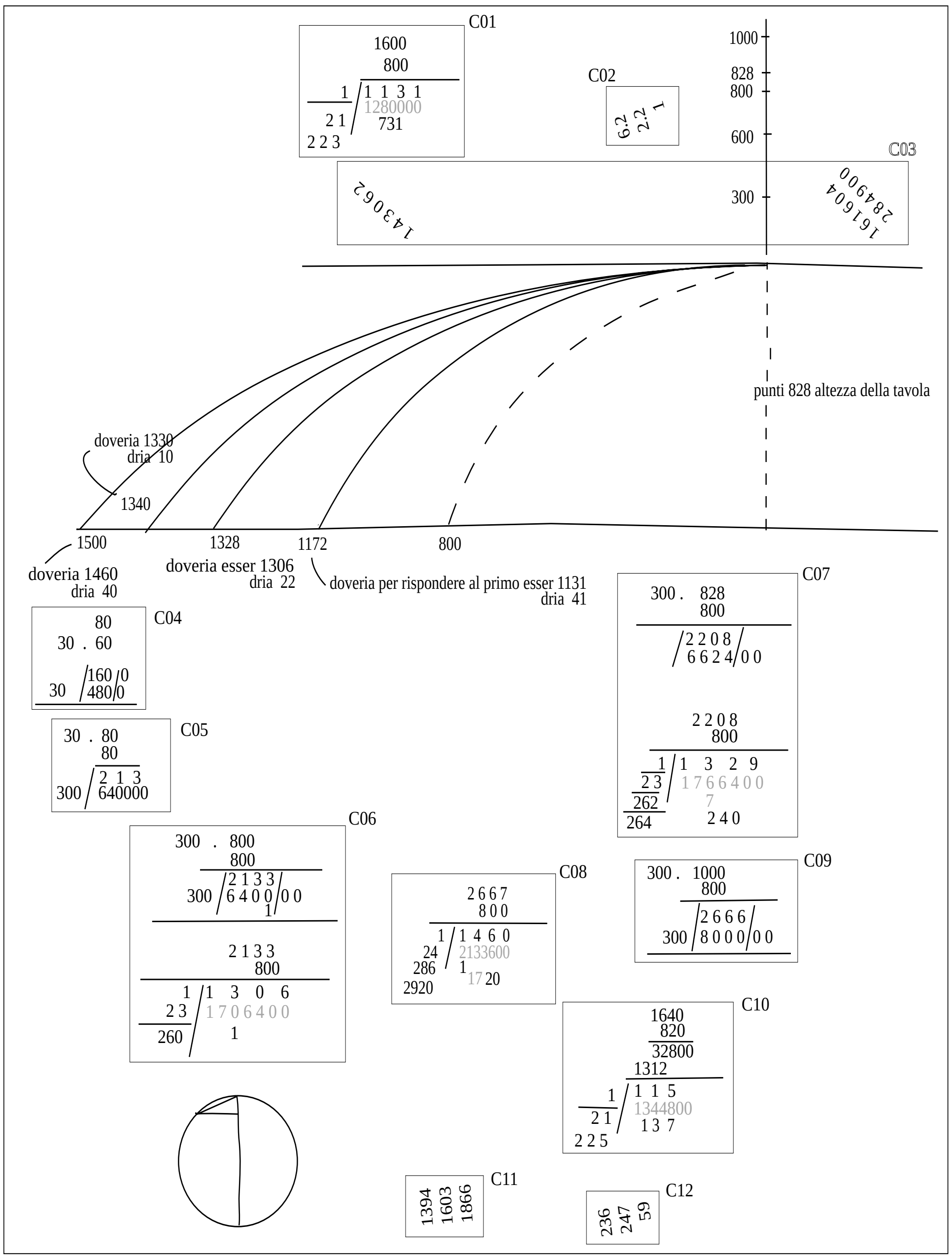
It is a fact that Galileo's record of the experiments on these folios omits important details, in reference to both the descriptive and numerical elements. Thus, an important ingredient in the studies of these folios has been the careful reconstruction of the experiments from the information that Galileo does supply. These reconstructions - both actual and mathematical - become an important part of the evidence. The numerical data that they generate is carefully compared with the analogous data from Galileo's record. These comparisons are used to inform the authors' comments about the plausibility of their reconstructions and the validity of their analyses of the experiments. Unfortunately, in terms of particulars (for example, the inclination of the inclined plane and release heights of the balls), these reconstructions as well as the conclusions drawn from them - specifically the purpose and precision of the experiments - differ widely.

This state of affairs calls for a sober re-examination of these folios. What aspects of his insights about motion did Galileo put to the test? How precise were his experiments? What conclusions can legitimately and compellingly be drawn from Galileo's record of them? Is there indeed convincing evidence that they were successful? The answer to these questions is the purpose of the discussion that follows. The focus will be on the folios themselves (rather than the reconstruction of the experiments) and on related aspects of the *Discorsi*. The folios 116v, 81r, and 114v and all the information on them are reproduced below. The originals can be studied at either of the websites listed above. The organization of the calculations on 116v and 114v into rectangular "frames" follows the practice of the websites.

3. The Experiment of Folio 116v. The statement *punti 828 altezza della tavola* tells us that Galileo recorded distances in units he calls *punti* (that is to say "points") and that he had a table 828 punti high. There is agreement among the historians already mentioned (based on evidence from folio 166r) that one *punto* is equal to approximately 0.94 millimeters. The diagram together with the computations on the folio confirm that he placed an inclined plane on the table, fixed an angle of inclination, and released a ball (likely of bronze) from the respective heights h of

$$300, \ 600, \ 800, \ 828, \ \text{and} \ 1000$$

punti above the horizontal table top. Galileo might have made use of a curved deflector to provide a smooth transition for the ball from the inclined plane to the horizontal table. His sketch on folio 175v shows that he considered such deflectors. After a short run on the table, the ball flew off to land on a horizontal floor. Galileo measured the respective distances from the point of impact of the ball to the base of



Folio 116v (size of original: 306 by 207 mm) the table (the point directly below the start of the ball's flight) and recorded these on the folio as

(a) 800, 1172, 1328, 1340, and 1500

punti. These are the experimental values that correspond to the various heights of release listed above.

3A. Understanding the Folio. We now turn to the analysis of the experiment as well as the computations that Galileo carried out. Consider the ball in its initial position on the inclined plane. Let

h = the height of the ball above the table, and

d = the distance from the ball to the bottom of the inclined plane.

Now release the ball and let

t = the time it takes for the ball to descend to the bottom of the plane,

v = the speed of the ball at time t. This is also the speed of the ball at the beginning of its fall from the table. Finally, let

R = the distance from the point of impact of the ball to the point on the ground precisely below the starting point of the ball's flight.

At the time of the experiment - before the end of the Paduan period in 1610 - Galileo had discovered, or at least wrestled with, all essential aspects of his program on motion as outlined in Section 1 above. In particular, he was in a position to put to the test the proportion

$$\text{(i)} \quad v \propto t$$

as well as the square law

$$\text{(ii)} \quad d \propto t^2$$

(deduced from (i) in Proposition II. Theorem II of the *Discorsi*). From his principle of inertia he could assume that the horizontal component of the velocity is constant throughout the ball's flight and hence equal to v. (Given the relatively small velocities, distances and times, Galileo could safely assume that air resistance would not play a significant role. See [19, p. 408].) In reference to the vertical component of the ball's flight, Galileo knew that the time of fall of the horizontally projected ball from the table to the ground is independent of its starting velocity v. So this time is equal to the time t_0 that it takes for a ball to fall vertically from rest through the height of the table. Notice that these observations rely on the principle of superposition. Galileo can conclude that

$$\text{(iii)} \quad R \propto v$$

with t_0 the constant of proportionality. By similar triangles (the angle of inclination of the inclined plane is fixed) $h \propto d$. After putting the above proportions together, Galileo has

$$\text{(iv)} \quad h \propto d \propto t^2 \propto v^2 \propto R^2 \ .$$

Therefore, $R^2 \propto h$. So, if releases of the ball at the heights of h_0 and h above the table result in points of impact at the respective distances of R_0 and R from the foot of the table, then

$$\text{(v)} \quad \tfrac{R^2}{R_0^2} = \tfrac{h}{h_0} \ .$$

It is this relationship that the experiment recorded on folio 116v is designed to confirm. Galileo's next step is to insert the values $h_0 = 300$ and $R_0 = 800$ from the experiment. By doing so, he in effect determines, or at least approximates, the constants of proportionality that link R^2 and h, or equivalently, R and $\sqrt{h}$. The equation

$$\text{(vi)} \quad R = \tfrac{800}{\sqrt{300}} \sqrt{h} \ .$$

captures what he does. It remains for Galileo to compute R for h successively equal to 600, 800, 828, and 1000, and to compare the resulting values with the measurements for R that were provided - see **(a)** - by the experiment. The successive values for R that Galileo computes are (in punti)

(b) — , 1131, 1306, 1330, and 1460.

The — refers to the value $R = 800$ that was used along with the corresponding $h = 300$ to obtain (vi).

Galileo records these numbers on the folio with the phrase *doveria esser* (or simply *doveria*) meaning "ought to be." He also includes his calculations. For example, the calculation for $h = 600$ is carried out in frame C01. Galileo first computes $R^2 = \frac{800 \cdot 800 \cdot 600}{300} = 1600 \cdot 800 = 1280000$ and calculates $R = \sqrt{1280000} = 1131$. For $h = 800$, this is done in C06. For $h = 1000$, the computation can be seen in frames C09 and C08. In C09, Galileo computes $1000 \times 800 = 800000$ and divides this result by 300 to get 2666. In C08, he multiplies the more accurate value 2667 of this computation (the actual value is $2666\frac{2}{3}$) by 800 to get 2133600. This is R^2. To get R, he calculates $\sqrt{2133600} = 1460$. The computation in frame C10 is analogous to that of C01 and suggests that Galileo also considered a table height of 820 punti. Note that some of the computations are only approximations and that the computation $\sqrt{1344800} = 115$ in frame C10 is incomplete. In the course of computing the square root of a number, Galileo crosses the digits of the number out. In the rendition of the folio above these numbers are entered in a lighter shade.

Galileo compares his experimental values **(a)** to his theoretical values **(b)** and records the respective differences of $41, 22, 10$, and 40 punti using the abbreviation *dria* for *differentia*. The fact that the theoretical values fall short of the experimental values (from about 1 to 4 centimeters) seems contrary to expectation. After all, the experimental values are subject to the retarding effects of the imperfections in Galileo's experimental setup, whereas the theoretical values are not. The explanation is provided by the fact that Galileo's theory, as captured by equation (vi), depends on one data point from the experiment. We will see, in particular, that the measured distance of 800 punti (corresponding to the height of 300 punti) falls short of the predicted mark. So the constant $\frac{800}{\sqrt{300}}$ is too small, and thus all of Galileo's computed values are too small as well.

We turn next to the question of the precision of the experiment of folio 116v. We will test the accuracy of the experimental values **(a)** against the predictions of elementary mechanics. (Galileo's theory can't be used because it depends on his experiment.) We will only outline these matters here. The details are available in many texts, for example, in Chapter 9.3 of the basic calculus text [42][3] . Note that the analysis that follows goes far beyond what Galileo was familiar with.

3B. The Underlying Mathematics. Return to the ball on the inclined plane and assume that the ball is homogeneous. Let $t = 0$ be the instant at which it is released. For any time $t \geq 0$, let $f(t)$ be the frictional force on the rolling ball (*a priori* it depends on t). This is the force that rotates the ball. Assume that there is neither slippage (as the ball would experience on a frictionless surface) nor any additional retardation of the motion down the plane (as would be the case if the surface were "bumpy" or "sticky"). The connection between the torque produced by the frictional force, the resulting angular acceleration of the ball, and the ball's index of inertia (this connection is provided by the rotational analogue of force = mass $\times$ acceleration),

[3] My interest in the experiments of Galileo had its beginning in my efforts to develop applications of calculus with interesting historical connections for this book.

leads to the equation

$$f(t) = \frac{2}{5} ma(t)$$

where m is the mass of the ball and $a(t)$ is its linear acceleration down the plane. By Newton's second law and the fact that the component of gravity down the plane is $F = mg \sin \beta$, where β is the angle of inclination of the plane, we get

$$ma(t) = F - f(t) = mg \sin \beta \; - \frac{2}{5} ma(t)\,,$$

and therefore,

$$a(t) = \frac{5g}{7} \sin\, \beta\,.$$

This informs us in turn that the velocity of the ball at the bottom of the plane is $v = \sqrt{\frac{10}{7} gh}$. (Alternatively, this equation can be established by using the law of conservation of energy. See [17, pp. 398-399].) Combining this with one of the basic equations of projectile motion and letting y_0 be the height of the table, provides the connection

$$R = 2\sqrt{\frac{5}{7}\; y_0}\; \sqrt{h}$$

between the starting height h and the distance R from the point of impact of the ball to the foot of the table. With the substitution $y_0 = 828$ this equation becomes

$$\text{(vii)} \qquad R = 2\sqrt{\frac{5}{7}\; 828}\; \sqrt{h}\;.$$

Plugging Galileo's starting heights of 300, 600, 800, 828, and 1000 into equation (vii) for h, we get the values (again in punti)

(c) 842, 1191, 1376, 1400, and 1538

for the corresponding distances R.

This model applies to the ideal situation: a perfectly round and homogeneous ball; a path that is perfectly smooth and flat with no tilts other than the inclination of the plane; a force of friction that rotates the ball without slippage but provides no additional impedance; and a deflector that provides a perfectly smooth transition from the plane to the table. In addition, to conform to the situation of the model, the table as well as the floor on which the ball impacts need to be perfectly horizontal. There is, of course, no such perfection in the context of Galileo's experimental setup. In sum, the expectation is that the ball will land short of its theoretical target. A comparison of the lists of numbers **(a)** and **(c)** confirms this. We know, of course, from the discussion on the third day of the *Discorsi*, that Galileo is fully aware that his fundamental laws of motion apply only in idealized situations and that any experiment or "real" situation will encounter "impediments." Notice that the "bottom lines" of the analyses of Sections 3A and 3B, namely the equations (vi) and (vii), differ only in the value of the constant, and that $\frac{800}{\sqrt{300}} \approx 46.19$ falls short of the correct value $2\sqrt{\frac{5}{7} \cdot 828} \approx 48.64$.

So far we have said nothing about the groove that guides the ball down the plane. The description of an inclined plane experiment in the *Discorsi* [2, Crew-Salvio p. 171, compare Drake p. 169] informs us that there was a channel "a little more than one finger in breadth" cut into the inclined plane, and that "having made this groove very straight, smooth, and polished, and having lined it with parchment, also as smooth and polished as possible, we rolled along it a hard, smooth, bronze ball ..." The fact that Galileo says nothing specific about the groove presents a problem, because different configurations of the cross-section require different theoretical explanations. We now let d be the diameter of the ball and consider the most likely possibilities. If the cross-section of the groove is a circular arc of radius greater than the radius $\frac{d}{2}$ of the ball, then in the ideal situation, the ball will roll on the bottom of the groove throughout its descent. This is a situation to which the mathematical model already described applies. Asssume next that the groove has rectangular cross-section and let $w > 0$ be its width. If $d \leq w$, then the ball is supported by the bottom of the groove and rolls entirely within the groove. Again, the model already described applies. But if $d > w$ and the groove is deep enough, then the rolling ball does not touch the bottom of the groove and is instead supported by its two edges. In this case, the dynamics are different. The mathematical model of this situation (obtained by an analysis similar to that above) provides the relationship

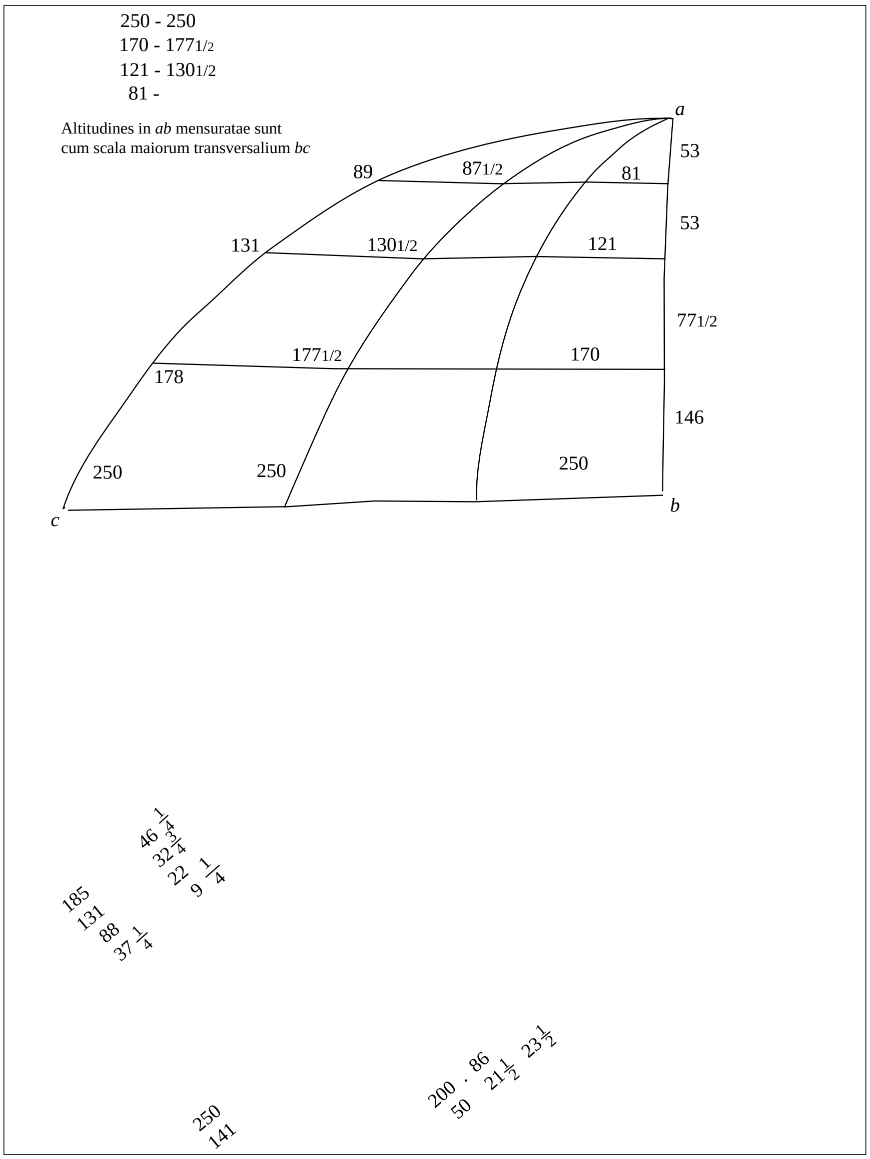
$$\text{(viii)} \qquad R = 2\sqrt{\frac{y_0}{1 + \frac{2}{5} \cdot \frac{d^2}{d^2 - w^2}}}\,\sqrt{h}\ .$$

This equation also applies to a groove with a cross-section in the shape of an isosceles triangle, if w is taken to be the distance between the two points of contact of the ball with the groove. Let $y_0 = 828$ punti be the height of the table. Because $\frac{d^2}{d^2 - w^2} > 1$, the value of equation (viii) is less than the value of equation (vii) for any $h > 0$. In particular, the values for R that equation (viii) supplies for the respective starting heights h equal to 300, 600, 800, 828, and 1000 are less than the values **(c)** supplied by equation (vii). Hence the values provided by equation (viii) will be closer to Galileo's experimental values **(a)**.

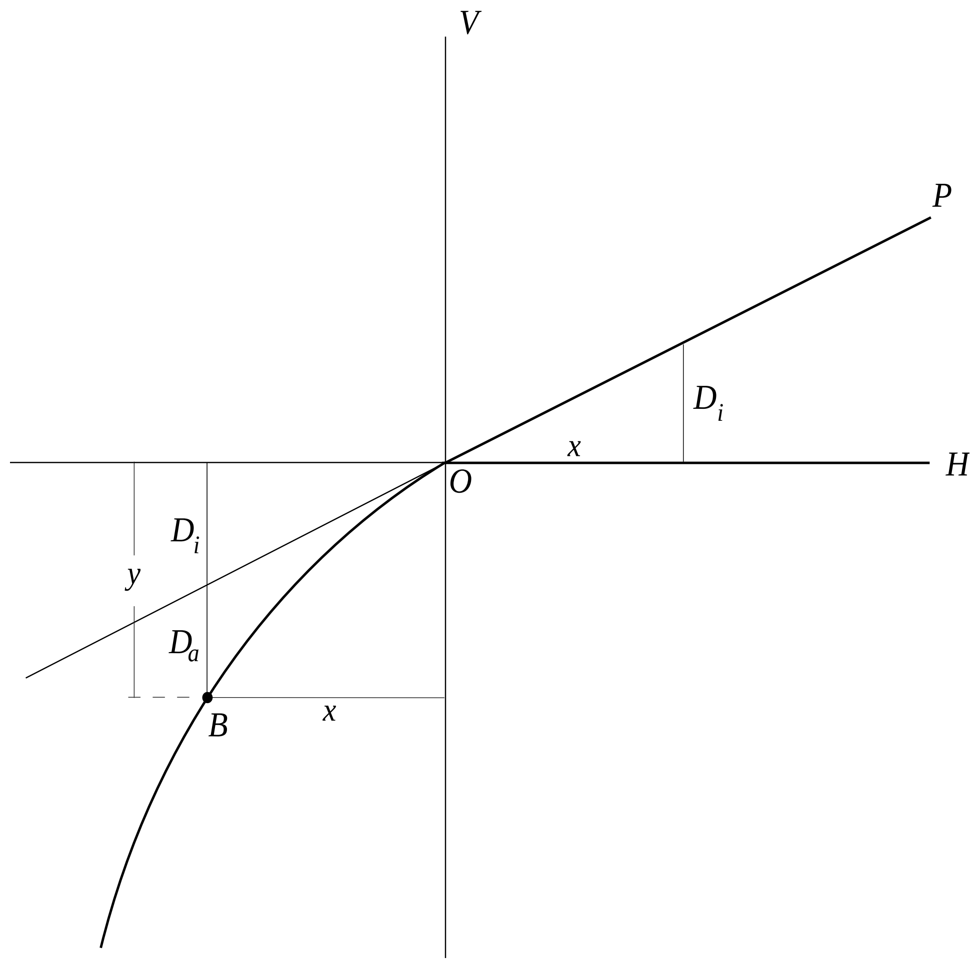
Now to the comparison of Galileo's experimental data against the predictions of the theory. It follows from the analysis of the cross-section of the groove that the respective differences between the experimental data **(a)** and the predictions **(c)** are the largest possible. Therefore, in assessing the accuracy of the folio 116v experiment, these differences provide the worse case scenario. The differences are $-42 = 800 - 842$, $-19 = 1172 - 1191$, $-48 = 1328 - 1376$, $-60 = 1340 - 1400$ and $-38 = 1500 - 1538$ punti. In terms of percentages, this amounts to -5.0%, -1.6%, -3.5%, -4.3%, and -2.5%, respectively. What can be said about this discrepancy? While the inclined planes used by Galileo seem no longer to exist, we do know - see [34] for example - that the apparatus that Galileo used in other investigations was well crafted. The physicists Shea and Wolf [17], considering the many sources of possible experimental error in the folio 116v experiment, regard the data generated by Galileo to fall "within acceptable limits of experimental error." All indications are that this assessment is correct. For example, Naylor [13, pp. 109-111] reconstructed the folio 116v experiment with considerable care (the cross-section of the groove was a circular arc of radius greater than $\frac{d}{2}$) and obtained distance data very close to Galileo's.

4. The Experiment of Folio 81r. There is a consensus among historians - see [18], [35], and [37, Chapter 8] - that folio 81r focusses its attention on the trajectories of balls that are propelled obliquely into space after having descended down an inclined plane placed on a table. In important contrast to folio 116v, the balls drop directly from the inclined plane and there is no horizontal deflection. Each of the three curves on the folio corresponds to a certain fixed angle of inclination of the plane and fixed starting height of the ball. In repeated trials Galileo intercepts the flight of the ball with horizontal planes placed at different heights and marks the points of impact. Evidently, he starts by placing the intercepting plane at a distance of $53+53+77\frac{1}{2}+146 = 329\frac{1}{2}$ punti below the plane of the table and "calibrates" the three trajectories so that the the points of impact are at the respective horizontal distances of 250, $250 + 250 = 500$, and $250 + 250 + 250 = 750$ punti from the table. He then successively raises the intercepting plane as indicated and measures the horizontal distances of the points of impact of the ball for each of the three trajectories. The Latin phrase on the folio informs us that these vertical and horizontal distances are given in the same scale. Galileo, interested in the shape of the three curves, has in effect provided five sets of coordinates for each of them.

Observe that other than the diagram, the folio contains only three small clusters of numbers (some of which are related to those of the diagram). One of these considers a group of four numbers and divides each by 4. However, there is nothing to suggest a successfully completed experiment. Nor does Codex 72 appear to contain folios with computations that inform 81r. Before jumping to conclusions, however, it is necessary to look at 81r in the light of the relevant context. We know that Galileo regarded the verification of the parabolic law as a primary goal of his studies on motion. During



Folio 81r (size of original: 304 by 205 mm) the Paduan period he invested considerable effort towards this end. See [14, p. 336] and [38, pp. 205-213] for instance. Given this, one would have to believe that Galileo designed the experiment of folio 81r - at least in significant part - to test the parabolic hypothesis, and that he would have regarded the experiment a success only if it provided solid evidence for or against it. Let's consider what Galileo was up against. The diagram below abstracts the essence of what is happening. It shows the path of the ball down the inclined plane from right to left to the edge of the inclined plane at the point O. Thereafter, the ball is in flight and is shown in typical position B. The plane determined by the incline and the horizontal plane of the table top are denoted (both in cross section) by P and H. The line V is the vertical through O. The distance from B to the line V is denoted by x and that from B to H by y. The



question facing Galileo for each of the three curves is this. Are the pairs x and y that he singles out in the diagram of the folio in a relationship that is parabolic, or at least close to parabolic?

Let's analyze the matter in Galileo's terms. The principle of superposition of motion tells us that the motion of the ball from O to its typical position B is the composite of an inertial component along P and a uniformly accelerated component down to B. These components result in the vertical displacements D_i and D_a respectively. The distance D_i can be obtained by measuring up to the inclined plane from the table top at a distance of x units from O. Notice that $D_i = c_i x$ where c_i is the slope of the inclined plane. What about D_a? Let t be the elapsed time of the motion of the ball from O to B. The proportion $x \propto t$ is a consequence of the inertial motion. So $t^2 \propto x^2$. By considering the downward accelerated motion, $D_a \propto x^2$. Galileo could have approximated the corresponding constant of proportionality c_a à la 116v by using $D_a = y - D_i$ and the first data point ($x = 81, y = 53$ for the inner curve). Since he knew the basics about parabolas, it would remain for him to test whether the pairs of distances x and y that he has recorded satisfy the equation

$$y = D_a + D_i = c_a x^2 + c_i x$$

in an approximate way.

Why doesn't Galileo carry this out on folio 81r? Unlike Fermat and his famous margin, he had more than room enough (on the bottom half of the page and the empty reverse side). Or on a separate folio? Or elsewhere? The short answer is that he was not able to do so. Why not?

We have just seen that a meaningful assessment of the data of 81r relies on the principles of superposition and inertia. The principle of superposition isolates from the ball's flight a component of motion along the non-horizontal line P and it is this non-horizontal motion to which the principle of inertia is applied. The fact is, however, that Galileo neither formulated nor used the principles of inertia and superposition at this level of generality. The studies [5, pp. 186-187], [10, pp. 27-31], and [38, pp. 225-226] support this point. For example, on the first day of the *Dialogo* [1] - its publication in 1632 occurs more than twenty years after the experiment of folio 81r - Sagredo tells us:

> "In the horizontal plane no velocity whatever would ever be naturally acquired, sice the body in this position will never move. But motion in a horizontal line which is tilted neither up nor down is circular motion about the center; therefore circular motion is never acquired naturally without straight motion to precede it; but being once acquired, it will continue perpetually with uniform velocity."

Early on the fourth day of the *Discorsi* (published in 1638), the principles of inertia and superposition are discussed only in the context of a motion that is the composite of a *horizontal* motion of constant speed and a vertical motion that is accelerated. Galileo then uses them to provide a proof of the parabolic law in the case of *horizontal* projection. But he presents no such proof in the oblique case. There is convincing evidence that Galileo's protracted efforts to come to grips with the obliquely projected trajectory - the gunner's problem - were unsuccessful. The analysis in [38, pp. 205-213, 225-226, 241-243, 251-258, 262-264] draws this conclusion from a careful study of both the *Discorsi* and the relevant theoretical studies in the folios of Codex 72. Indeed, when the proof in the oblique case is supplied by Cavalieri in 1632, Galileo reacts with dismay in a letter (see [7, p. 75] or [10, p. 23]) written in the same year to his frequent correspondent Cesare Marsili:

> ”I cannot hide from your most excellent Lordship that the information scarcely pleased me, seeing that the first fruits of more that forty years' study, of which I had revealed a large part in close confidence to the said Father, were to be taken away from me, and I was to be deprived of that glory that I desired so ardently and that I was promising myself after such long efforts; for really my first intention, that which incited me to meditate upon motion, had been to find that line, and though I succeeded in demonstrating it, I know how much trouble I had in arriving at that conclusion.”

The proof of the parabolic ”line” that ”said Father” Cavalieri and later Torricelli in 1644 provide are based on the more general principles of inertia and superposition. See [5, pp. 236-244] and [38, pp. 274-276]. They are the same as those used in the analysis of 81r presented above. In spite of Galileo's assertion that he succeeded, the demonstration he refers to does not appear in any of his published works, letters, or manuscripts. The empty bottom half of folio 81r (as well as the empty reverse side) is a concrete example of this gap. Galileo was unable to exploit the promising diagram and abandoned this early effort to analyze the trajectory of an obliquely projected ball.

However, the diagram does provide information about the accuracy with which Galileo observed and measured the trajectories. Return to the previous figure and take the lines H and V to be the x and y axes of a standard coordinate system with the punto as the unit of length. Because the horizontal and vertical distances travelled by the balls were small, their speeds were small as well. So we will assume that air resistance was negligible, and that the trajectories that Galileo observed were precise parabolas. In terms of the coordinate system, each parabola is given by an equation of the form $y = -ax^2 + bx$, where a is a positive constant, b is non-negative, and $x \leq 0$. The line of the sloping plane P has equation $y = bx$ and ax^2 measures the vertical deviation of the motion of the ball from this line. (This parallels the earlier analysis of folio 81r.) Let's turn to the inner trajectory. In reference to the coordinate system, Galileo's data points are

$$(-81, -53),\ (-121, -106),\ (-170, -183.5),\ (-250, -329.5)\,.$$

The parabola of the form $y = -ax^2 + bx$ that fits these data points best - in the sense of least squares - is

$$(*) \qquad y = -0.00351906\, x^2 + 0.446797\, x.$$

(Any of several software packages - in the present case *Mathematica* - can be used to supply this equation.) If Galileo's cluster of data points were to fall on a parabola precisely, then $(*)$ would be that parabola. Therefore, an assessment of the extent to which they do not gives insight into the accuracy of Galileo's measurements. The x-coordinates of the points on parabola $(*)$ whose y-coordinates correspond to

$$-53, -106, -183.5, \text{ and } -329.5$$

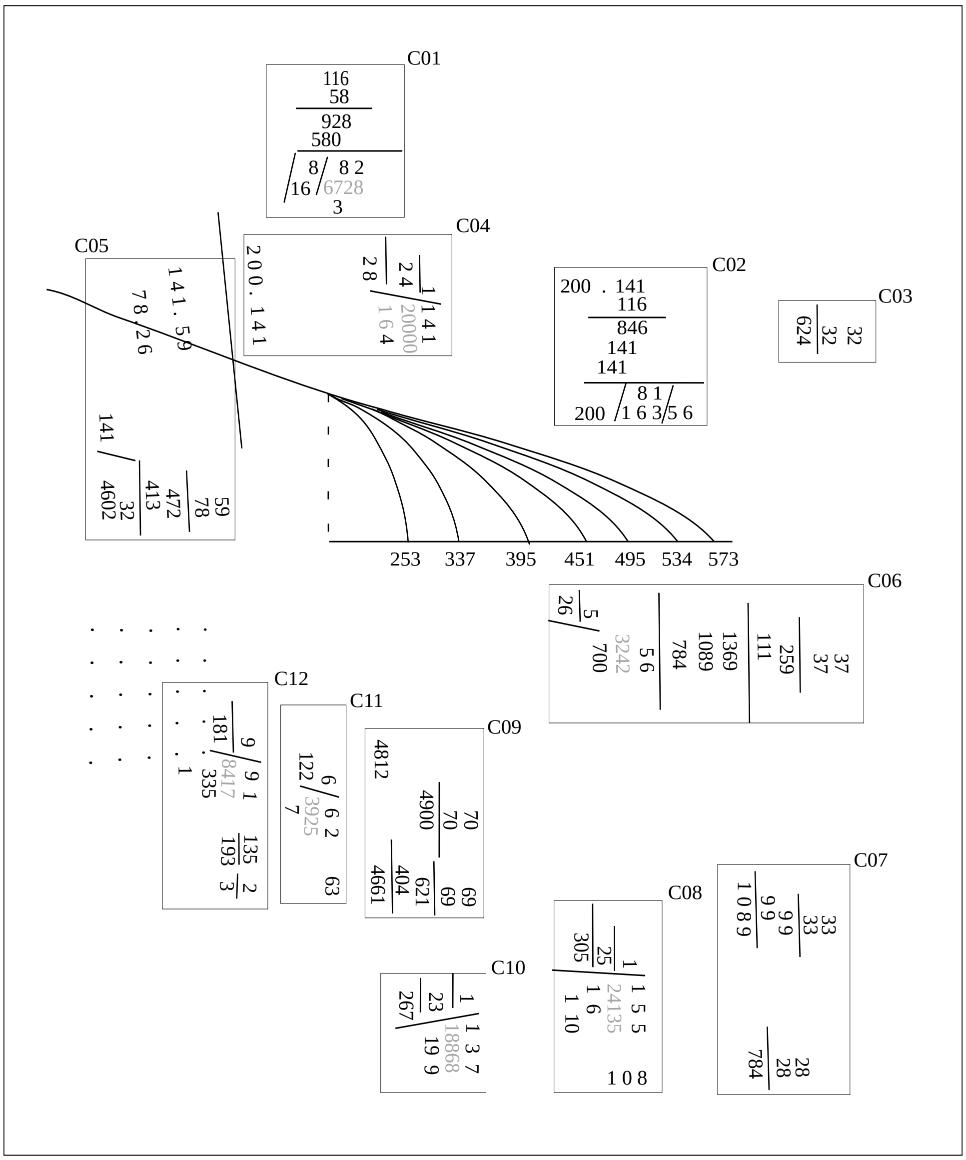
are $-74.7, -121.4, -173.5$, and -249.0, respectively. When comparing these numbers with the horizontal distances that the folio records for the inner curve, we see that the difference $81 - 74.7 = 6.3$ is the largest. The indication is that at least some of the measurements deviate from the reality that Galileo observes by more than 6 punti. Similar analyses of the middle and outer curves provide inaccuracies in excess of 9 and 12 punti. Inaccuracies of this size are hardly surprising considering what is involved. Galileo has to set the intercepting planes perfectly horizontally at the vertical distances of $53, 106 = 53 + 53, 183.5 = 53 + 53 + 77.5$ and $329.5 = 53 + 53 + 77.5 + 146$ punti below the plane of the table, locate with precision the points of impact of the ball, and measure the distances from these points to the table.

This analysis applies only to distance measurements involving the trajectory. In the context of folio 116v, there will be additional inaccuracies having to do with the inclined plane, the groove, and the ball. In particular, we can safely conclude that the experimental inaccuracies will be much greater than 6 punti in each of the trials of the folio 116v experiment. The claim by Drake that Galileo experimented "within a variance of four units" is therefore not sustainable.

5. The Experiment of Folio 114v. There is general agreement that the sketch on folio 114v also depicts the trajectories of a ball that had rolled down an inclined plane placed on a table. The angle of inclination is fixed, the ball rolls down from various heights, and the numbers

253, 337, 395, 451, 495, 534, and 573

are the recorded distances (in punti) of the corresponding points of impact of the ball from the foot of the table. In contrast to 116v, there is no information about the



Folio 114v (size of original: 285 by 202 mm) starting heights of the ball. Nor is there any information about the initial direction of the motion (because the angle of inclination of the plane is not supplied).

What information do Galileo's calculations provide about the experiment? Frame C01 computes $116 \cdot 58 = 6728$ and then $\sqrt{6728} = 82$. Frame C02 calculates $\frac{141 \cdot 116}{200} = 81$. Frame C03 shows only a fragment of a multiplication. (The top right corner of the folio is missing). Frame C04 carries out $\sqrt{20000} = 141$. Frame C05 computes $\frac{59 \cdot 78}{141} = 32$ and lists two pairs of numbers. Frame C07 shows that the squares of 33 and 28 are 1089 and 784 respectively. Frame C06 shows the square of 37 to be 1369, shows that the sum of 1369, 1089 and 784 is 3242, and calculates the square root of this number as 56. Frames C08 and C10 compute $\sqrt{24135} = 155$ and $\sqrt{18868} = 137$. Frame C09 computes the squares of 70 and 69 to be 4900 and 4661 respectively and lists the number 4812. Frame C11 calculates $\sqrt{3925}$ as 62 and lists the more accurate value 63. Frame C12, finally, computes $\sqrt{8417} = 91$ and seems to point out that $\frac{135}{193}$ is approximately $\frac{2}{3}$. Note that some of these computations provide only approximations and that $69^2 = 4761$ (and not 4661). The study of these computations does not reveal any apparent connections with the diagram on the folio and the sequence of distances that are recorded on it. (Nor are such connections provided in previous studies of folio 114v.) In particular, there appears to be no evidence on the folio that suggests a successful experiment.

What might the purpose of 114v have been? The diagram on the folio shows that the trajectories are oblique. Because the three folios under study were crafted at about the same time, it seems reasonable to suppose that Galileo intended to pursue for oblique projection what he had accomplished on 116v for horizontal projection. So he might have attempted (perhaps among other things) to establish a relationship between the distances $R = 253, 337, 395, 451, 495, 534,$ and 573 of the points of impact of the ball from the foot of the table and the other relevant parameters: the height y_0 of the table, the angle of inclination ϕ_0 of the plane, and the heights h from which the ball was released.

Let's have a look at the relationship between these parameters that a current mathematical model provides. Inserting the speed $v = \sqrt{\frac{10}{7}gh}$ that the ball has at the bottom of the plane (see Section 3B) into the standard equation for the range of a projectile, shows that

$$R \;=\; -\frac{5}{7}h\,\sin(2\phi_0) \;+\; \sqrt{\frac{5^2}{7^2}h^2\sin^2(2\phi_0) + \frac{20}{7}hy_0\cos^2\phi_0}\;\;.$$

The most simple special case of this equation (other than the case $\phi_0 = 0$ that 116v considers) is $\phi_0 = \sin^{-1}(\frac{1}{2}) = 30°$ which Galileo could have achieved simply by raising the end of the plane a distance of one half its length above the table top. In this case,

$\sin 2\phi_0 = \cos \phi_0 = \frac{\sqrt{3}}{2}$ and the equation simplifies to

$$R = -\frac{5}{14}\sqrt{3}\ h\ + \frac{\sqrt{3}}{2}\ \sqrt{\frac{5^2}{7^2}h^2\ + \frac{20}{7}hy_0}\ =\ -\frac{5}{14}\sqrt{3}\ h\ + \frac{\sqrt{3}}{2}\sqrt{\frac{5}{7}\ h}\ \sqrt{\frac{5}{7}h + 4y_0}\ .$$

Finally, taking $y_0 = 53 + 53 + 77.5 + 146 = 329.5$ from folio 81r (as [35] plausibly suggests), we get

$$R = \frac{5}{14}\sqrt{3}\ h\ + \frac{\sqrt{3}}{2}\sqrt{\frac{5}{7}h}\ \sqrt{\frac{5}{7}h\ + 1318}\ .$$

It is most unlikely that Galileo - working with proportions - could have made much headway in the direction of understanding the relationship between h and R even in this simple situation. Only algebraic equations arising from analytic geometry could have provided the necessary insights.

Therefore, as the examination of the folio itself already suggests, Galileo's diagram is the residue of an inconclusive and abandoned experiment and his calculations served a later and different purpose. Naylor [19, p. 403] observed long ago that the diagram on the folio is unrelated to the calculations, but concluded instead that the latter must be lost.

6. Previous Assessments of the Folios. It was argued in Sections 4 and 5 that the experiments whose footprints are visible on folios 81r and 114v failed and that Galileo abandoned them. The experiment of 81r failed because Galileo did not have a broad enough grasp on the principles of inertia and superposition, and that of 114v was not successful because the relationships between the numerical parameters of the experiment were beyond his understanding. But how is it that accomplished historians are able to conclude that these experiments were significant achievements? And what are their arguments?

Drake agrees that the experiments recorded on the two folios did not succeed, but does not leave it at that and says ([37, p. 124]):

> "Without analytic geometry, he lacked the mathematical tools for success, which is why he never mentioned the investigations. It is precisely because they did not succeed that they are of great interest, for they show how Galileo went about attacking a physical problem when it lay beyond his powers of solution."

He then goes on to provide very specific details about Galileo's experimental setup about which Galileo himself was completely silent. See [32, p. 4] and [37, pp. 124-127]:

"Analysis of Galileo's measurements of downward oblique projections, on ff. 114v and 81r, made it possible to describe in fair detail the apparatus and procedures he had used. His inclined plane was grooved to guide the ball, as decribed in *Two New Sciences* [i.e., the *Discorsi*], width of groove $8\frac{1}{2}$ *punti* being such as to reduce the linear acceleration by 5.75%, the ball used being of radius 10 *punti*. For f. 114v the angle of the plane was set so that horizontal advance was double the vertical

descent; that is, at $\tan^{-1}\frac{1}{2}$ or 26°.565. For f. 81r, the angles where $\sin^{-1}\frac{1}{3}$, $\frac{1}{6}$, and $\frac{1}{12}$. Galileo's measurements of projections on both documents were accurate within 4 *punti*."

How does Drake arrive at this? He reconstructs the experiments of the folios from the fragments that Galileo provides. When a reconstruction provides data that are in tight agreement with the recorded measurements of the folio, Drake regards both the reconstruction and the conclusions drawn from it as the likely explanations of Galileo's procedures. In light of his analysis of folios 114v and 81r, Drake revises his earlier assessment of folio 116v. See [32, pp. 4-5] and [37, pp. 110-113]:

"This information, gleaned over a period of years, made it no longer tenable to suppose that on f. 116v Galileo had measured incorrectly by as much as 40 *punti*, as seemed to be implied by calculations of his own on that page. A skilled experimentalist capable of holding his results within a variance of four units would not err by ten times that amount in two out of five recorded measurements. Accordingly, I have re-examined f. 116v ... "

Whereas his earlier conclusions rely on actual reconstructions of the experiments, his re-examination of 116v turns to theory. In particular, Drake selects the mathematical model of equation (viii) of Section 3B. His choices $d = 20$ punti for the diameter of the ball and $w = 8.5$ punti for the width of the rectangular groove "slow down" the theory, but not enough to bring the predictions to within 4 punti of Galileo's experiment. To accomplish this, Drake argues that the height of Galileo's table must have been $y_0 = 800$ for some trials of the experiment and $y_0 = 820$ for the others, instead of the 828 punti that the folio specifies. Refer to [32, pp. 5-13] and [37, Chapter 7].

That Drake's strategy of reconstruction is fundamentally flawed is demonstrated by the other analyses of folios 81r and 114v. Their authors produce different reconstructions that also generate data that is in close agreement with Galileo's fragmentary record. However, the insights about the experiments that are derived from them deviate substantially from those of Drake.

Even though a comparison of the diagrams of 114v and 116v suggests the contrary, Naylor infers from his reconstruction that folio 114v is an experiment about horizontal projection rather than oblique projection. He thinks that the experiment covers the same territory as 116v and considers it a success. In reference to Drake's analysis he says ([13, p. 115]):

> "The view that Galileo would roll spheres down an incline, compile a list of observations, and then realize his total inability to interpret the information, certainly seems a little out of character."

Naylor regards the experiment of 81r to have been a successful test of the parabolic trajectory. In [18, p. 167] he argues that

> "... there are good grounds for believing that Galileo suspected that the curve of the projectile trajectory could well be a parabola. If this was the case, then his knowledge of the parabola would enable him to design his experiment in such a way as to test this hypothesis. His experiment suggests that it was designed on the basis of knowledge of certain relatively simple yet unique properties of parabolas having a common axis."

In Naylor's version of Galileo's experiment the three parabolic trajectories are compared not only under the assumption that all three parabolas have the same axis but that they also have the same apex. But is this assumption consistent with the data on the folio? The same least squares approximation that produced the equation $y = 0.446797\,x - 0.00351906\,x^2$ for the inner trajectory (see Section 4) provides the equations $y = 0.177481\,x - 0.00097007\,x^2$ for the middle trajectory and the equation $y = 0.10604\,x - 0.000447278\,x^2$ for the outer trajectory. Elementary calculus tells us that the axes of these parabolas cross the x-axis near $x = 63, x = 91$, and $x = 119$, respectively. Therefore, the axis of the parabola that fits the inner data best is 56 punti from the axis of the parabola that fits the outer data best. In the context of the distances recorded on folio 81r, this is excessive. In other words, there is no indication that the experiment of folio 81r deals with parabolas that have a common axis.

Hill [35] also makes use of reconstructions. He considers 114v and 81r to be connected and argues in [35, p. 661] that 114v provisionally, but effectively, tests whether the speed of the ball in its descent (from rest) down the inclined plane is proportional to the square root of the distance that it covers. He finds his data to "broadly approximate" this interpretation of the folio. Would broad approximation have been good enough for Galileo? Note that the 116v experiment tests the speed law in a more effective way. (See the proportion (iv) of Section 3A.)

As to 81r, Hill does not think that the middle and outer curves of the diagram represent actual trajectories. Instead, he argues that the second set of measurements $87\frac{1}{2}, 130\frac{1}{2}, 177\frac{1}{2}, 250$ as well as the third $89, 131, 178, 250$ are each the horizontal distances (from the table) of a downward projected ball. In this view, the three trajectories that Galileo considers differ only slightly, with the ball landing at a distance of 250 punti from the table in each case. Hill summarizes his study of 81r with the statement ([35, p. 656]) that Galileo

> "can conclude that for projections with a fixed baseline value, trajectories approach semiparabolic status as (or very nearly as) their projective angles approach the horizontal, just what one would expect if the parabolic trajectory hypothesis were correct."

Refer again to the coordinate system discussed in Section 4. The semiparabola that Hill refers to is the parabolic trajectory through the points $(0, 0)$ and $(-250, -329.5)$ obtained by horizontal projection. Check that its equation is $y = -0.005272x^2$. It follows that this semiparabolic trajectory is $100.3, 141.8, 186.6$ and (as we already know) 250 punti from the table at the respective distances of $53, 106, 183.5$, and 329.5 punti below the table top. In terms of the respective horizontal distances, the step from the first trajectory (this is the inner trajectory corresponding to the data, 81, 121, 170, 250) to the second trajectory (corresponding to the data $87\frac{1}{2}, 130\frac{1}{2}, 177\frac{1}{2}, 250$) is indeed - as Hill asserts - an advance towards the semiparabola just described. Observe, however, that the second trajectory and the third trajectory (corresponding to the data $89, 131, 178, 250$) are virtually identical. The differences between the respective horizontal distances - the largest difference is less that 1.5 millimeters - fall well within the error range already discussed in Section 4. Therefore, the step from the second trajectory to the third is not an advance towards the semiparabola. So contrary to Hill's interpretation, there is no approach to semiparabolic status. Even if Galileo had carried out the experiment as described, he would only have had very indirect evidence for the parabolic hypothesis for an oblique trajectory.

In summary, each of the analyses of the experiments of folios 81r and 114v discussed in this section encounters difficulties. In particular, neither the scenarios of success that they describe nor the specific information about the experiments that they provide find support in the evidence supplied by the folios.

7. Conclusions. The role that experiment and measurement have historically played in the validation of a scientific theory, especially a developing theory, is a subtle one in general. See Kuhn [21, Chapter 8]. But in the case of folio 116v, we have seen compelling evidence in Section 3 that this experiment is - in terms of both the validity of the concept and the precision of the execution - a compelling confirmation of Galileo's account of motion. This folio refutes the contention of Koyré to the effect that Galileo's experiments are woefully inadequate. It also confirms that Galileo's analyses on day three of the *Discorsi* as well as the proof on day four of the parabolic trajectory in the case of a horizontal projection are, at least in a substantial way, the achievements of Galileo's work in Padua. Full credit goes to Stillman Drake for uncovering the meaning of folio 116v and for recognizing its relevance.

While there is consensus in the literature that folio 116v describes a successful experiment, there is disagreement as to its purpose. Drake and MacLachlan [16] believe that it is Galileo's aim to test his principle of horizontal inertia. Most historians think that Galileo is in pursuit of the time-squared law of fall. See Hill [35, p. 662]. The discussion of Section 3A shows that the experiment tests none of Galileo's insights independently, but that it in fact tests Galileo's account of motion as a whole. Therefore, it tests neither his law of fall nor his principle of inertia directly. Whereas the test of either the law of fall or the principle of inertia necessarily involves the measurement of time, the experiment of 116v bypasses any need to measure this elusive variable. This point is of historical significance. The inclined plane experiment that Galileo describes in the *Discorsi* is designed to shore up the law of fall and some of its consequences. (See Proposition II. Theorem II and its Corollaries in [2, pp. 166-171]). It therefore focusses on the connection between distance and time. Settle [8, p. 21] points out in the context of his reconstruction of this experiment that "the measurement of time is the most controversial and the most difficult." Indeed, it is the measurement of time that is an explicit target of the skepticism expressed by Koyré (and also Descartes and Mersenne) about Galileo's experiments with inclined planes. See [5, pp. 106-107 and 126].

From our modern perspective, 116v is a successful illustration of experiment testing theory. Was Galileo's perspective the same? What did his experiments mean to him? Why is there no reference to the folio 116v experiment in the *Discorsi*? There have been efforts to answer these questions. In both the *Dialogo* and the *Discorsi* experiments quite clearly serve a didactic even rhetorical function (at least in part) and Galileo stretches his descriptions of them beyond the limits of his observations. Naylor [19] discusses the contrast between these descriptions and Galileo's actual experimental procedures, and suggests plausibly that "the problems to be met in making the folio 116v experiment intelligible for his readers were ... [an] obstacle to its inclusion in the *Discorsi*." Machamer [44] develops the point that for Galileo to have an intelligible explanation, he needs to have an accompanying experience - often provided by a mechanical device - that illustrates the phenomenon. In particular, Machamer regards the idea that Galileo's insistence on making his thinking about motion intelligible by mechanical models "fits well" with the practice in geometry (the mathematical environment in which Galileo worked) of proof by geometrical construction.

The evident failure of the experiments of folios 81r and 114v does not detract from the success of 116v. But it does call for a reassesment of Galileo the experimental scientist. Recall Koyré's assertion ([5, p. 106]) "Galileo's experiment is beautifully conceived; the idea of substituting a body rolling down an inclined plane for a body in free fall is truly a mark of genius. But, we are obliged to note, its execution is not of the same order as its conception." It is indeed the case that Galileo's experiments with inclined planes are ingenious in their concept, but rather than their execution, it is Galileo's understanding of the underlying physical principles and mathematical relationships that is not always up to the task.

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Department of Mathematics, University of Notre Dame, Notre Dame, IN 46556, USA.
e-mail: hahn.1@nd.edu