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WITH AN APPENDIX BY DOMENICO GIULINI

Hunting the White Elephant: When and How did Galileo Discover the Law of Fall?

In *The Stolen White Elephant* Mark Twain tells the story of a white elephant, a gift of the King of Siam to Queen Victoria of England, which somehow got lost in New York on its way to England. An impressive army of highly qualified detectives swarmed out over the whole country in search of the lost treasure. And after a short time an abundance of optimistic reports with precise observations were returned by every detective giving evidence that the elephant must shortly before have been at the very place he had chosen for his investigation. Although one elephant could never have been strolling around at the same time at such different places over a vast area, and in spite of the fact that the elephant, wounded by a bullet, lay dead the whole time in the cellar of the police headquarters, the detectives were highly praised by the public for their professional and effective execution of their task.

The Argument

We present a number of findings concerning Galileo's major discoveries which question both the methods and the results of dating his achievements by common historiographic criteria. The dating of Galileo's discoveries is, however, not our primary concern. This paper is intended to contribute to a critical reexamination of the notion of discovery from the point of view of historical epistemology. We claim that the puzzling course of Galileo's discoveries is not an exceptional comedy of errors but rather illustrates the normal way in which scientific progress is achieved. We argue that scientific knowledge generally develops not as a sequence of independent discoveries accumulating to a new body of knowledge but rather as a network of interdependent activities which only as a whole makes the individual steps understandable as meaningful "discoveries."

Introduction

The present paper deals with sources documenting Galileo's work and that of his contemporaries on issues that have always been a focus of studies in the history of science, the discovery of the law of fall and of the parabolic shape of the projectile trajectory, discoveries which mark, according to common understanding, the origins of classical mechanics. In spite of being the subject of more than a century of historical research, the question of when and how Galileo made his major discoveries is, however, still only insufficiently answered. It is generally

assumed that he must have found the law of fall around the year 1604 and that he
discovered the parabolic shape of the trajectory of projectile motion only several
years later. There is, however, no such agreement concerning the question of how
he arrived at these achievements. In particular, it is still controversial whether he
found these laws primarily by empirical observations or by theoretical specula-
tion.

As far as the date of the discovery of the parabolic trajectory is concerned, we
shall show that as early as 1592 Galileo and Guidobaldo del Monte jointly per-
formed an experiment on an inclined plane, which Galileo later in the *Discorsi*
described as producing a parabolic trajectory. The law of fall, according to our
reconstruction, was merely a trivial consequence of the recognition of the para-
bolic shape of the trajectory.

We argue further that at first this experiment was not an exciting or dramatic
event at all for Galileo. In particular, that the experiment suggested the parabolic
shape of the trajectory, implying the law of fall, seems initially not to have been
significant to him. However, Galileo did immediately consider it remarkable that
the experiment suggested a symmetry of natural and violent motion which had no
foundation in the prevailing Aristotelian theory of nature that he largely shared.
Even after he had grasped the parabolic shape of the trajectory and its most plau-
sible explanation by the law of fall, he did not realize for a considerable time the
further theoretical implications of the experiment. Only much later did he pas-
sionately maintain that the discovery of the parabolic shape of the projectile tra-
jectory was his most important breakthrough. And finally in the *Discorsi* he
claimed priority for himself in performing this experiment and explaining its out-
come, the parabolic shape of the curve.

Galileo's conviction that the shape of the projectile trajectory is parabolic was
based on dynamical arguments. These arguments allowed him to infer that the
trajectory is curved in the same way as the catenary, although from a modern
point of view the arguments must be regarded as fallacious. Furthermore, we
shall show that Galileo, when he checked by means of a hanging chain the valid-
ity of his claim that the trajectory and the catenary are both parabolic, he arrived
at the correct result that the curve of the hanging chain deviates considerably
from a parabola. Nevertheless, he stuck to his invalid argument and tried to find
an indubitable proof of the parabolic shape of both curves. Finally, he became
convinced that he had found such a proof and intended to make it a core topic of
the *Fifth Day* of the *Discorsi*. It was only his declining health that prevented him
from realizing this plan, and the *Fifth Day* remained unwritten.

The interpretation of clues which seem to indicate a discovery or a new idea
presupposes an indisputable answer to two questions. Firstly, what does it mean
to say that someone has discovered something? Secondly, how can such a discov-
ery be identified using clues provided by the available sources? We claim that the
answer to these questions concerning the nature of discoveries is not at all obvi-

ous. On the contrary, as will become clear, premature answers to these questions have led historians of science astray.

As a consequence, it has, first of all, been widely neglected that Galileo saw a close connection between the parabolic trajectory and the catenary, the curve of a hanging chain. Secondly, explicit hints given by Galileo in his publications and his correspondence concerning when and how he became convinced of the parabolic shape of both curves have been disregarded or have been considered as unreliable because they seemingly did not fit into an alleged scheme of his work.

In the following we address the canonical issue of the discovery of the law of fall and the origins of classical mechanics from a different perspective, in which the notion of discovery is not taken for granted but rather itself becomes a subject of reflection. Consequently, sections dealing with details of Galileo's scholarly activities will be complemented by sections which either investigate these activities in the broader social and historical context, such as the challenges of technology to engineer-scientists, or pursue questions of historical epistemology, such as the question of whether experiments or theoretical reflections were the foundation of Galileo's judgments, beliefs, and research strategies.

The paper begins with a brief account of the canonical viewpoint of Galileo's major discoveries (*The Standard Dating of the Discovery of the Law of Fall and of the Parabolic Trajectory*) and then turns to an issue neglected in the standard accounts, Galileo's preoccupation with the catenary (*The Neglected Issue: Trajectory and Hanging Chain*). Two sections are devoted to the evidence that testifies to the significance of this neglected issue throughout Galileo's life (*Evidence I: Galileo Using Hanging Chains* and *Evidence II: Viviani's Addition to the Discorsi and Guidobaldo's Protocol of an Experiment*). The role of a particular theoretical context for Galileo's interpretation of his findings is analyzed in the following two sections (*How Can the Aristotelian View of Projectile Motion Account for a Symmetrical Trajectory?* and *Decomposing the Trajectory—Neutral Motion and the Law of Fall*). The next section returns to the historiographical aspect of dating Galileo's discoveries and questions the standard dating without, however, reducing it to the alternative attribution of a different date to the discovery of the parabolic shape of the projectile trajectory and of the law of fall (*Dating Guidobaldo's Protocol*). In order to illuminate Galileo's own perspective on his discoveries, the next three sections discuss the impact of the changing contexts of Galileo's work, from the scholarly environment of Pisan philosophy and mathematics via the intellectual challenges of early modern engineering technology, to the open horizons of the intellectual elites of Venice, one of the industrial and commercial centers of the early modern world (*Guidobaldo del Monte as an Engineer-Scientist*, *Galileo in the Footsteps of Guidobaldo del Monte*, and *Galileo and Paolo Sarpi—Towards a New Science of Motion*). Having developed a complex picture of the contexts of Galileo's discoveries and his understanding of them, the paper turns to the question of the mutual influence of theory and experiment which led to his final research project on the shape of the trajectory and the

law of fall (*Did Galileo Trust the Dynamical Argument?—Galileo the Experimenter* and *Did Galileo Trust his Experiments?—Galileo's First Attempt of a Proof*). The next section elaborates on the observation of how unsatisfactorily open an individual scientific biography usually ends, in the case of Galileo's biography in an unpublished proof of an erroneous theorem representing what he considered to be the keystone of his new theory of motion (*Returning to the Dynamical Argument—The Final Proof*). The final section turns again from the historiographic perspective to the broader issue of the paper (*When and How Did Galileo Discover the Law of Fall?*). In a first appendix four letters documenting the changing contexts of Galileo's work are presented in English translation. In a second appendix Galileo's claims concerning his discoveries are reexamined from the perspective of modern physics.

The Standard Dating of the Discovery of the Law of Fall and of the Parabolic Trajectory

The question of when and how Galileo made his celebrated discoveries of the law of fall and of the parabolic shape of the projectile trajectory has been extensively discussed in the last one hundred years by historians of science. Contrary to the testimony of Galileo's disciple Viviani,[1] who ascribed such discoveries already to the young Galileo, it is widely accepted today that these discoveries date into the late Paduan period. Most writers date the discovery of the law of fall to the year 1604 and assume that Galileo discovered the parabolic shape of the projectile trajectory even some years later. In his influential *Galileo Studies*, Alexandre Koyré lapidarily affirmed:

> The law of falling bodies—the first law of classical physics—was formulated by Galileo in 1604. (Koyré 1966, 83)

This dating is primarily based on the few contemporary documents by Galileo himself which provide clues to his knowledge of the law of fall and the form of the trajectory. In particular, two letters by Galileo stand out because of the testimony they offer to his knowledge at certain precise dates. The first letter is directed to Paolo Sarpi and dated 16 October 1604 (Galilei 1890-1909, X: 116); it provides clear evidence that, at this point in time, Galileo knew the law of fall. The second letter is directed to Antonio dei Medici and dated 11 February 1609 (Galilei 1890-1909, X: 228). It shows that Galileo, by that time, knew that projectiles reaching the same height take the same time to fall down, a property that fol-

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[1]   According to Viviani, Galileo discovered the isochronism of the pendulum already as a student in Pisa around 1583; see Viviani's letter to Leopoldo dei Medici, Galilei 1890-1909, XIX: 648; see also his biography, Galilei 1890-1909, XIX: 603. He claims furthermore that Galileo performed experiments on free fall already between 1589 and 1592 when he was professor in Pisa; see Viviani's biography of Galileo, Galilei 1890-1909, XIX: 606.

lows, from a modern point of view, from the decomposition of the parabolic trajectory into its horizontally uniform and vertically accelerated components.

Of course, these documents provide at most a *terminus ante quem* for Galileo's discoveries. The hesitation of modern Galileo scholars to follow Viviani's testimony and to accept an earlier date is, partly at least, due to the study of Galileo's early manuscript, presumably kept in a folder labelled *de motu antiquiora scripta mea* in the following simply referred to as *De Motu*, which in spite of its anti-Aristotelian tendency shows him as deeply influenced by the categories and assumptions of medieval Aristotelian physics.

The cornerstones 1604 and 1609 define a scaffolding for more or less speculative stories about what really happened at those times. It is, indeed, customary to find these dates in reconstructions of the sequence of Galileo's discoveries, even though the claims of what made up these discoveries widely diverge among different authors – some even maintain that the law of fall was only discovered in 1609 and the parabolic trajectory perhaps even later. As an example we quote the succinct reconstruction of the sequence of Galileo's discoveries by the influential German historian of science Friedrich Klemm:

> Attempts showing him that the difference in the speed of fall of bodies of the same size with different specific weights becomes smaller the more the medium is diluted, suggested to Galileo around 1600 to assume that the speed of fall in vacuum is going to be of equal magnitude for all bodies. (...)
>
> The next step is now to give up all considerations about the cause of the growing speed of fall and to limit himself to treating mathematically the motion of fall taking place for all bodies in a vacuum in the same way. In 1604 Galileo makes the assumption, convinced that in nature everything is constituted as simply as possible: the speed of fall in vacuum increases with the distance of fall traversed. This approach leads him into contradictions. Finally in 1609 he comes to recognize the process of fall as a *uniformly* accelerated motion, that is, he comes to the insight that the velocity grows with time. Starting from here he obtains by using developments of the late middle ages: the distances are to each other as the squares of the times. This again gives him the possibility to verify the hypothetical approach $v = a \cdot t$ by the experiment (the fall trench experiment). (Klemm 1964, 79-80)

This reconstruction is obviously guided by the supposition that Galileo proceeded methodically and step-by-step from one discovery to the next. Naturally, such a view of Galileo's progress must remain speculative as long as it is not supported by a detailed analysis of contemporary sources.

Fortunately, an extensive collection of Galileo's notes on his research on mechanics has survived and is now kept in the Galilean Collection of the National Library in Florence, the so-called *Codex Ms. Gal. 72*. However, these notes are neither dated nor preserved in their original order of composition. The most influ-

ential interpretations of these notes are those going back to the publications of Stillman Drake who devoted a great deal of his research to the study of these notes. Drake derived his interpretations in particular from his attempts at a chronological ordering of the pages of that manuscript. He writes:

> Arranged in their order of composition and considered together with theorems found on other pages or in the text of *Two New Sciences*, those notes tell a story of the origin of modern physical science. It is not the story on which historians of science were generally agreed in 1974, nor did I then foresee the extent to which that would in time be modified by Galileo's working papers. (Galilei 1989, vii, part from the introduction to the second edition of Drake's translation of the *Two New Sciences*)

Drake's claim that his story differs from the story on which historians of science were generally agreed may be true for his reconstructions of the process of Galileo's discoveries which have changed over time. Contrary to such claims, however, the dates which he derived for the major steps do not differ very much from what has been commonly accepted. Elsewhere he writes:

> From the beginning of his professional career, Galileo's main interest was in problems of motion and of mechanics. His first treatise on motion (preserved in manuscript but left unpublished by him) belongs probably to the year 1590, when he held the chair of mathematics at the University of Pisa. It was followed by a treatise on mechanics begun at the University of Padua in 1593 and probably put into its posthumously published form around 1600. Galileo made some interesting discoveries concerning fall along arcs and chords of vertical circles in 1602, and two years later he hit upon the law of acceleration in free fall. With the aid of this law he developed a large number of theorems concerning motion along inclined planes, mainly in 1607-8. Toward the end of 1608 he confirmed by ingenious and precise experiments an idea he had long held: that horizontal motion would continue uniformly in the absence of external resistance. These experiments led him at once to the parabolic trajectory of projectiles. Meanwhile he had been at work on a theory of the breaking strength of beams, which seems also to have been virtually completed in 1608. (Galilei 1974, ix)[1]

But even after Drake had extensively studied Galileo's working papers, and after repeatedly changing his views on Galileo's discoveries, he accommodated the dating of Galileo's manuscripts to the standard dating rather than the other

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[1] Drake explains how he himself came to three different versions of the process of Galileo's discovery of the law of fall. The third version he finally adopted in this article was not the last one. His very last one is contained in the preface to his edition of the Discorsi. This version is essentially based on a fanciful interpretation of folio 189 of Ms. Gal. 72 (Drake 1973). We shall publish elsewhere a reconstruction of this folio which will show that Drake's interpretation of this folio cannot be maintained.

way around. In his latest book, Drake gives a detailed time-table of Galileo's activities and achievements (Drake 1990, XIIIf); according to this time-table, in 1602 Galileo "begins studies of long pendulums and motion on inclines", in 1604 he "discovers the law of pendulums from careful timings; finds the law of fall," and only in 1608 he "discovers the parabolic trajectory by measurements."

It thus emerges as a peculiarity of recent Galileo historiography that the dates at which Galileo supposedly made his major discoveries have remained largely unchallenged, in spite of the relatively weak direct evidence available for this dating. This peculiarity is all the more surprising as the dating and the sequence of Galileo's discoveries have been extensively and controversially discussed in the older Galileo literature around the turn of the century. The possibility that Galileo first discovered the parabolic form of the trajectory and only then the law of fall was, for instance, seriously considered by Emil Wohlwill, together with the possibility that the law of fall was discovered much earlier than is now commonly assumed (Wohlwill 1993, I: 144-162 and Wohlwill 1899).[1] But Emil Wohlwill's substantial contributions had as little impact on the Galileo studies of the last fifty years as those of his eminent Italian contemporaries, Antonio Favaro and Raffaello Caverni. As we will see in the following, there are good reasons to take up the debate where it was left a century ago.

The Neglected Issue: Trajectory and Hanging Chain

Concerning the sources from which Galileo derived his major discoveries there is much less agreement among recent historians of science than concerning the dating. The assumptions about his sources range in fact from pure empirical evidence achieved exclusively by means of careful experimentation and precise measurements, on the one hand, to predominantly theoretical speculation in direct continuation of scholastic traditions only scarcely supported by empirical demonstrations, on the other hand.[2]

In spite of the wide range of different reconstructions of the discovery process,[3] however, a simple fact has nearly been completely neglected both by the older

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[1]   In the latter article Wohlwill still argues that the law of fall was discovered shortly before he wrote his letter to Paolo Sarpi in 1604 in which the law is explicitly mentioned. In the first volume of his final work on Galileo published shortly before his death, however, he developed an ingenious argument for a quite different dating. Based on the sources available at that time and, in particular, based on the first publication of excerpts from Galileo's notes by Caverni and Favaro, he developed a reconstruction of Galileo's discovery which he qualified as "only the most probable" way of how Galileo might have found the law of fall; this reconstruction is essentially coherent with what is presented in the following. The new evidence provided here shows that Wohlwill's conjecture was much more sound than the interpretations which are currently *en vogue*.

[2]   In discussions, these positions are still represented prototypically by Drake, on the one side, and Koyré, on the other side.

[3]   For the literature on this subject, see the references in section 3.3.1 of Damerow, Freudenthal, McLaughlin, and Renn 1992; see in particular, the ingenious reconstruction of Galileo's inclined plane experiment, described in Settle 1961.

and the more recent literature: for Galileo, there exists a close connection between the parabolic trajectory and the catenary, that is, the curve of hanging chains. This neglect is all the more astonishing as the connection is explicitly made a subject of discussion in his final word on the matter, the *Discorsi*. In the course of the discussions of the *Second Day*, Galileo's spokesman Salviati describes two methods of drawing a parabola, one of them involving the trajectory of a projected body, the other one using a hanging chain:

> There are many ways of drawing such lines, of which two are speedier than the rest; I shall tell these to you. One is really marvelous, for by this method, in less time than someone else can draw finely with a compass on paper four or six circles of different sizes, I can draw thirty or forty parabolic lines no less fine, exact, and neat than the circumferences of those circles. I use an exquisitely round bronze ball, no larger than a nut; this is rolled [*tirata*] on a metal mirror held not vertically but somewhat tilted, so that the ball in motion runs over it and presses it lightly. In moving, it leaves a parabolic line, very thin, and smoothly traced. This [parabola] will be wider or narrower, according as the ball is rolled higher or lower. From this, we have clear and sensible experience that the motion of projectiles is made along parabolic lines, an effect first observed by our friend, who also gives a demonstration of it. We shall all see this in his book on motion at the next [*primo*] meeting. To describe parabolas in this way, the ball must be somewhat warmed and moistened by manipulating it in the hand, so that the traces it will leave shall be more apparent on the mirror.

> The other way to draw on the prism the line we seek is to fix two nails in a wall in a horizontal line, separated by double the width of the rectangle in which we wish to draw the semiparabola. From these two nails hang a fine chain, of such length that its curve [*sacca*] will extend over the length of the prism. This chain curves in a parabolic shape, so that if we mark points on the wall along the path of the chain, we shall have drawn a full parabola. By means of a perpendicular hung from the center between the two nails, this will be divided into equal parts. (Galilei 1974, 142f)

At a prominent place, the end of the *Fourth Day* of the *Discorsi*, Galileo returns to what he considered a surprising fact, the parabolic shape of the catenary:

> *Salviati*. (...) But I wish to cause you wonder and delight together by telling you that the cord thus hung, whether much or little stretched, bends in a line that is very close to parabolic. The similarity is so great that if you draw a parabolic line in a vertical plane surface but upside down—that is, with the vertex down and the base parallel to the horizontal—and then hang a little chain from the extremities of the base of the parabola thus drawn, you will see by slackening the little chain now more and now less, that it curves and

adapts itself to the parabola; and the agreement will be the closer, the less curved and the more extended the parabola drawn shall be. In parabolas described with an elevation of less than 45°, the chain will go almost exactly along the parabola.

Sagredo. Then with a chain wrought very fine, one might speedily mark out many parabolic lines on a plane surface.

Salviati. That can be done, and with no little utility, as I am about to tell you. (Galilei 1974, 256)

However, before Salviati can fulfill his promise of further explanations, he is interrupted by Simplicio who asked for a demonstration of the impossibility to stretch a rope or chain into a perfectly straight line before he continues. Unfortunately, we do not get the end of the story. About three pages later the *Discorsi* end abruptly because, as we know and will further discuss later, Galileo was not able to finish his book as planned. We are asked to wait for the unwritten part when Simplicio, satisfied with the answer, tried to bring up the issue again:

Simplicio. I am fully satisfied. And now Salviati, in agreement with his promise, shall explain to us the utility that may be drawn from the little chain, and afterward give us those speculations made by our Author about the force of impact.

Salviati. Sufficient to this day is our having occupied ourselves in the contemplations now finished. The time is rather late, and will not, by a large margin, allow us to explain the matters you mention; so let us defer that meeting to another and more suitable time. (Galilei 1974, 259)

The question of what Galileo through his spokesman Salviati intended to say about the hanging chain deserves attention not only for the sake of the curiosity of the issue but also for a systematic reason. Galileo's claim that the curve of a hanging rope or chain comes arbitrarily close to a parabola is obviously wrong. A correct mathematical representation of the catenary presupposes, as we know today, the knowledge of hyperbolic functions.[1] Galileo and his contemporaries had no chance to derive, and not even to mathematically describe the shape that a hanging chain assumes. In fact, the actual catenary even deviates considerably from a parabola as long as the distance between the two suspension points of the chain is not much greater than the vertical distance between the suspension points

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[1] The catenary is represented by the equation

$$y = h \cdot \cosh\frac{x}{h}$$

with a parameter h depending on the horizontal tension (which is constant for a specific catenary) and on the derivative of the volume of the chain or rope considered as a function of its length (a ratio which is also assumed to be constant over the length of the chain or rope). For an extensive discussion of the catenary see Appendix B.

and the lowest point of the hanging chain. But if the distance between the two suspension points substantially exceeds the vertical distance between the suspension points and the lowest point of the chain, the parabola is a reasonable approximation of the catenary.

Was it this approximation that Galileo had in mind when he identified the catenary with the parabola? There is overwhelming evidence that this is not the case. In the finished parts of the *Discorsi* he clearly pointed out that he assumed not a fortuitous but a substantial relation between the parabolic trajectory and the catenary.

> *Salviati.* Well, Sagredo, in this matter of the rope, you may cease to marvel at the strangeness of the effect, since you have a proof of it; and if we consider well, perhaps we shall find some relation between this event of the rope and that of the projectile [fired horizontally].
>
> The curvature of the line of the horizontal projectile seems to derive from two forces, of which one (that of the projector) drives it horizontally, while the other (that of its own heaviness) draws it straight down. In drawing the rope, there is [likewise] the force of that which pulls it horizontally, and also that of the weight of the rope itself, which naturally inclines it downward. So these two kinds of events are very similar. (Galilei 1974, 256)

This supposed theoretical relationship between the catenary and the trajectory will be extensively discussed in the following. Previously, however, another question has to be answered: did Galileo really produce, as he claimed, parabolic curves by means of projected bronze balls and hanging chains or did he merely invent these stories in order to give his idea of a theoretical connection between catenary and trajectory a lively illustration?
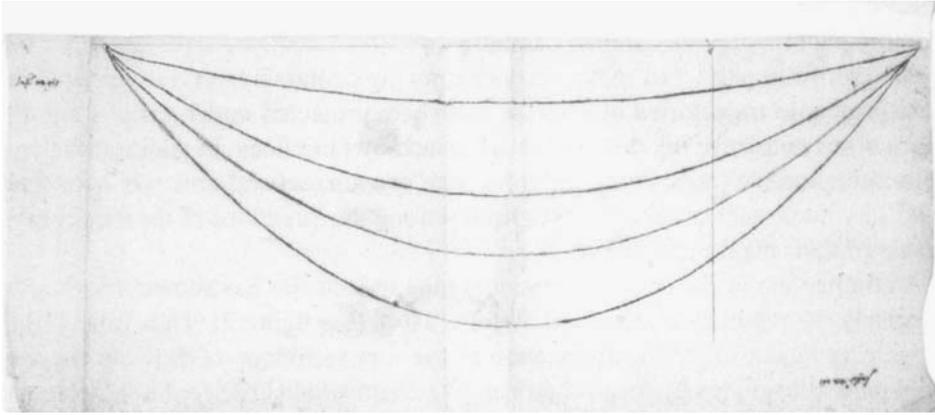
Evidence I: Galileo Using Hanging Chains

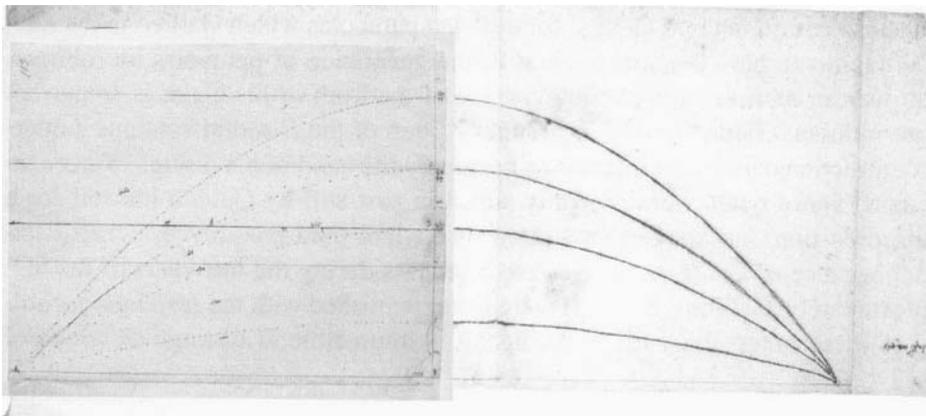**Figure 1.** Ms. Gal. 72, folio 41/42 with curves produced by means of a hanging chain

Figure 2. Ms. Gal. 72, folio 41/42 (folio in the back) used as a template for drawing projectile trajectories on folio 113 (folio in front)

Fortunately, this question can easily be answered in the case of the first method of drawing parabolic lines he describes. Among Galileo's notes on motion, Ms. Gal. 72, there is a folded sheet of rough paper designated as folio 41/42 (see figure 1) that has obviously been used for drawing catenaries as Galileo described it in the *Discorsi*. The sheet was fixed to the wall by means of two nails; the holes in

the sheet of paper, through which the nails were driven, are still visible.[1] Chains of different length where fixed at these two nails and their shape was copied to the paper by means of some needles. Finally, using this perforated sheet, the resulting curves were copied by letting ink seep through the little holes pierced into the paper along the hanging chains.

Another folio page, 113 recto, shows a drawing containing curves which represent projectile trajectories of oblique gun shots projected under various angles. The curves consist of ink dots which are joined by faint lines. In addition to these representations of trajectories, the folio page contains several drawn or scratched auxiliary lines, such as straight lines representing the directions of the shots or the levels of their maximum heights.[2]

A comparison of the curves representing the trajectories has shown that they fit precisely the template represented by folio 41/42 (see figure 2). Thus folio 113 is a preserved example of the application of the very technique of drawing supposedly parabolic curves by means of a hanging chain which Galileo describes in the *Discorsi*.

Evidence II: Viviani's Addition to the *Discorsi* and Guidobaldo's Protocol of an Experiment

In the case of the second method for drawing parabolas which Galileo in the *Discorsi* claims to have been using, that is, the generation of parabolas by rolling a ball over an inclined mirror, the evidence of the truth of his claim is somewhat more indirect. Galileo's copy of the first edition of the *Discorsi* contains numerous corrections, notes, and additions mostly by the hand of his disciple Vincenzio Viviani. These notes were probably added in part still by Galileo himself for a revised edition, but apparently written by Viviani (and possibly by other disciples) because of Galileo's progressive blindness during the last years of his life. Unfortunately, Galileo's *Discorsi* were never published with the revisions according to these notes, most likely because it is impossible to distinguish which of

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[1] The distance between the two suspension points is 443 mm. According to two notes, one on the left and the other on the right side of the curves, "total amplitude 465," this distance was measured by Galileo as 465, probably indicating 465 "points." Thus, the size of the unit used by Galileo is 0.95 mm.

[2] Several uninked construction lines can be found on the folio page 113r. An analysis of these lines has provided evidence that a basic unit of exactly the same size was used on this folio as on folio 41/42. A set of parallel lines can be identified which are drawn vertically to the baseline of the parabolic trajectory in equal distances of precisely 15 "points" measured in the basic unit of folio 41/42. The total distance measured along the baseline from the origin of the shots to the vertical representing the target is divided by the parallel lines precisely into 16 parts of 15 "points" each. Furthermore, an arc of a circle with a radius of 100 "points" can also be identified. At the vertical target line a number "100" is written exactly 100 "points" above the base line. But not only the small unit is common to both folios. Assuming that the distance between the parallel lines defines a higher unit of 15 "points," the "total amplitude 465" on the template folio 41/42 measures precisely 31 of these units.

these notes were authorized by Galileo and which of them were inserted by Viviani on his own account only after Galileo's death. In any case, these notes provide striking insights into consequences of Galileo's *Discorsi* which were either implicit but insufficiently expressed in the printed text of the first edition or could be achieved immediately by an elaboration of his practical and theoretical achievements.

With regard to the present problem of whether or not Galileo really used the second method of drawing parabolas, the inspection of his copy of the first edition of the *Discorsi* provided a surprise. On a sheet of paper[1] inserted close to Galileo's description of the second method for drawing parabolas one finds two curves which show the typical characteristics of such a method: the indications of the bouncing of the ball at the beginning and the slight deformation of the parabola at the end due to friction (see figure 3).[2] If one could be certain that it was Galileo himself who produced these parabolas, it would be thus clear that the description in the *Discorsi* refers to an experiment he had actually performed himself.

The claim that Galileo used this method receives strong confirmation by the analysis of another manuscript, although this manuscript is definitely not written by Galileo himself but by Guidobaldo del Monte, a correspondent, benefactor, and a close associate of Galileo in his early research on mechanics. This document nevertheless provides additional evidence and even allows the conclusion, as we will show, that Galileo was familiar with this method of drawing parabolas already long before he wrote the *Discorsi*.

At the end of a notebook of Guidobaldo there are two drawings which possibly depict an inclined plane used for such an experiment, together with a protocol which perfectly resembles by the description of Galileo's second method mentioned in the *Discorsi* (see figures 4 and 5). A closer inspection of Guidobaldo's drawings shows that they actually represent a roof which may have offered a convenient setting ready-at-hand for originally trying out a method similar to that described by Galileo on a scale comparable to that of ballistics, the usual context in which projectile motion was considered at that time.[3]

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[1] Folio page 90v of Galileo's copy of the *Discorsi*, Galilei after 1638.

[2] This judgment is based on a careful repetition of the experiment under controlled conditions with the help of equipment designed by Henrik Haak and realized by the workshop of the Fritz Haber Institute of the Max Planck Society. Henrik Haak has furthermore assisted us in the reproduction of the historical experiment; the results will be the subject of a forthcoming publication.

[3] Henrik Haak, who constructed the apparatus for our reproduction of the historical experiment, has directed our attention to the fact that the inclined planes depicted by Guidobaldo immediately before and almost immediately behind the protocol represent a roof construction.
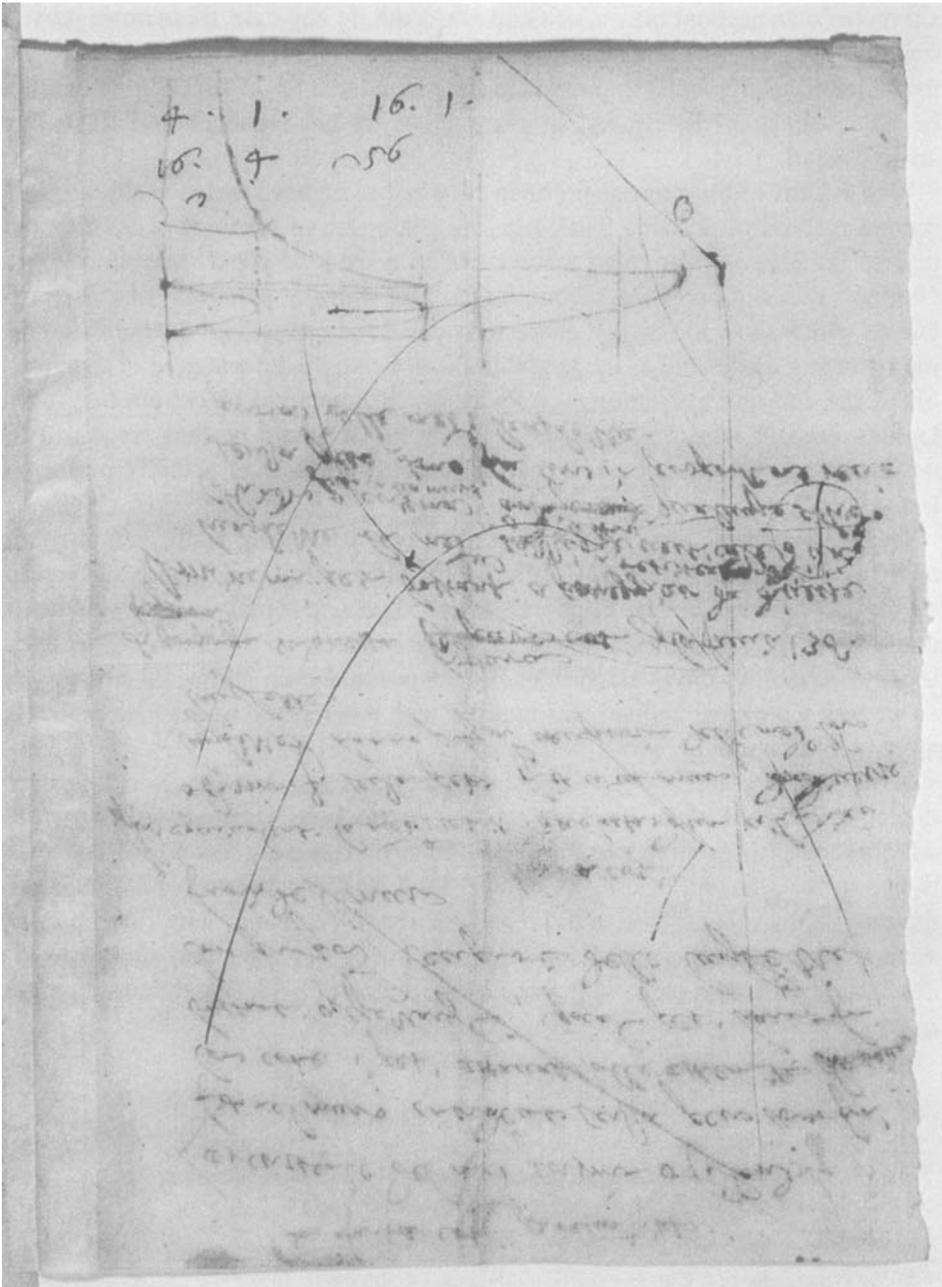
Figure 3. Sheet of paper found in Galileo's copy of the first edition of the Discorsi (Ms. Gal. 79, folio page 90 verso) containing two parabolic curves generated by an inked ball thrown along an inclined plane, inserted near to the place where this method is described
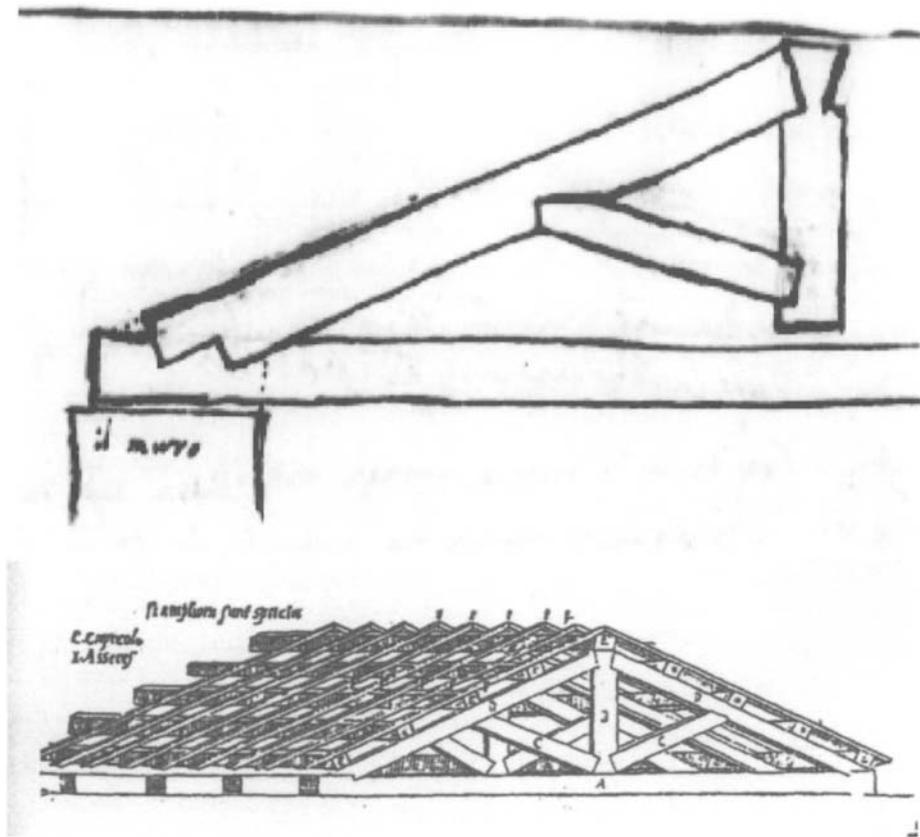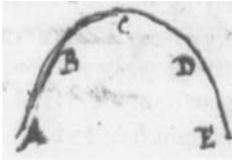
Figure 4. Guidobaldo's protocol of the projectile trajectory experiment

Figure 5.   Guidobaldo's sketch (del Monte ca. 1587-1592, 237) and a contemporary representation
of a roof with "capriata" (illustration by Palladio, reproduced from Tampone 1996, 71)

Guidobaldo's protocol not only describes precisely the experimental setting but
also reports results, such as the symmetry of the generated curve and the close
relation to the curve of a hanging chain, that can be deduced from the observation
that the curves in both cases result from the same configuration of forces. The
protocol begins with a summary of consequences that can be drawn from the out-
come of the experiment, followed by a description of the method applied. It ends
with a theoretical interpretation of the symmetry of the trajectory:

> If one throws a ball with a catapult or with artillery or by hand or by some
> other instrument above the horizontal line, it will take the same path in fall-
> ing as in rising, and the shape is that which, when inverted under the horizon, a
> rope makes which is not pulled, both being composed of the natural and the
> forced, and it is a line which in appearance is similar to a parabola and
>
> hyperbola ⁀ . And this can be seen better with a chain than with a

rope, since [in the case of] the rope *abc* , when *ac* are close to each other, the part *b* does not approach as it should because the rope remains hard in itself, while a chain or a little chain does not behave in this way. The experiment of this movement can be made by taking a ball colored with ink, and throwing it over a plane of a table which is almost perpendicular to the horizontal.

 Although the ball bounces along, yet it makes points as it goes, from which one can clearly see that as it rises so it descends, and it is reasonable this way, since the violence it has acquired in its ascent operates so that in falling it overcomes, in the same way, the natural movement in coming down so that the violence that overcame [the path] from *b* to *c*, conserving itself, operates so that from *c* to *d* [the path] is equal to *cb*, and the violence which is gradually lessening when descending operates so that from *d* to *e* [the path] is equal to *ba*, since there is no reason from *c* towards *de* that shows that the violence is lost at all, which, although it lessens continually towards *e*, yet there remains a sufficient amount of it, which is the cause that the weight never travels in a straight line towards *e*.

The similarity of this protocol of Guidobaldo's experiment and Galileo's description of his second method to draw parabolas raises, of course, the question of what relation exists between these two reports. Did Guidobaldo and Galileo independently make the same observation? If not, who of them did the experiment and who only heard of or reproduced it? We will show in the following that not only are both referring to the same experiment, but that, moreover, Galileo was even present when this experiment was performed. First, however, the issue has to be analyzed in some more detail. (del Monte ca. 1587-1592, 236)[1]

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[1] A transcription of the text has been first published by Libri 1838, IV: 397f. Its significance for dating Galileo's early work on motion was first recognized by Fredette 1969. The experiment described by Guidobaldo has been extensively discussed in Naylor 1974. Naylor concludes that Galileo could not have been convinced by the outcome of this experiment alone of the parabolic shape of the trajectory and that it was only in 1607 that he arrived at this conviction. Naylor thus agrees with the standard dating of this discovery, a conclusion that we will attempt to refute in the following. The crucial significance of the experiment for Galileo's discovery of the law of fall was first suggested by Damerow, Freudenthal, McLaughlin, and Renn 1992, 336f.

How Can the Aristotelian View of Projectile Motion Account for a Symmetrical Trajectory?

When has the technique of tracing the trajectory of a ball, which both Galileo and Guidobaldo described, been developed? A first clue to the answer to this question is provided by the outcome of Guidobaldo's experiment itself. This outcome, as it is reported in Guidobaldo's note, was in one respect incompatible with the view of projectile motion prevailing at the time of the young Galileo. In the Aristotelian tradition, projectile motion was conceived of as resulting from the contrariety of natural and violent motion, the latter according to medieval tradition acting through an impetus impressed by the mover into the moving body. According to this understanding of projectile motion, the trajectory cannot be symmetrical because the motion of the projectile is determined at the beginning and at the end by quite different causes. At the beginning it is dominated by the impetus impressed into the projectile, at the end by its natural motion towards the center of the earth.

At the time of Galileo, this tradition was primarily represented by Tartaglia's systematic treatise on artillery, his *Nova Scientia* published in 1537 (Tartaglia 1984). In this treatise, he struggled with the problem that Aristotelian dynamics could not be satisfactorily brought into accordance with the knowledge of the practitioners on projectile motion at that time. The Aristotelian distinction between natural and violent motion seemed to be promising as part of an axiomatic foundation of a theory of projectile motion, perfectly represented by the axiomatic exposition in the first book of Tartaglia's *Nova Scientia*. On the other hand, this foundation did not even provide a definite answer to such a simple question as whether or not the projectile trajectory has any straight part. Tartaglia knew very well that any shot systematically deviated from the target in the line of vision and he was perfectly able to explain this phenomenon:

> Truly no violent trajectory or motion of uniformly heavy bodies outside the perpendicular of the horizon can have any part that is perfectly straight, because of the weight residing in that body, which continually acts on it and draws it towards the center of the world. (Drake and Drabkin 1969, 84)[1]

But in contradiction to this assumption he also assumed:

> Every violent trajectory or motion of uniformly heavy bodies outside the perpendicular of the horizon will always be partly straight and partly curved,

---

[1] Tartaglia is already in his *Nova Scientia* very explicit in this point (see the footnote by the editors, see also Damerow, Freudenthal, McLaughlin, and Renn 1992, 144f). Nevertheless, this aspect of Tartaglia's theory is still widely neglected. It seemingly does not fit into the simple scheme of historical explanation which associates Aristotelian dynamics with a preclassical conception of the projectile trajectory and classical physics with its parabolic shape. For a recent example see, for instance, the monograph on Tartaglia by Arend 1998, chapter 4, in particular 174f.

and the curved part will form part of the circumference of a circle. (Drake and Drabkin 1969, 84)

According to Tartaglia's theory, the trajectory of a projectile consists of three parts. It begins with a straight part that is followed by a section of a circle and then ending in a straight vertical line (see figure 6). This form of the trajectory also corresponds to Tartaglia's adaptation of the Aristotelian dynamics to projectile motion in the case of artillery, a case that was, of course, much more complicated than what was traditionally treated in Aristotelian physics.
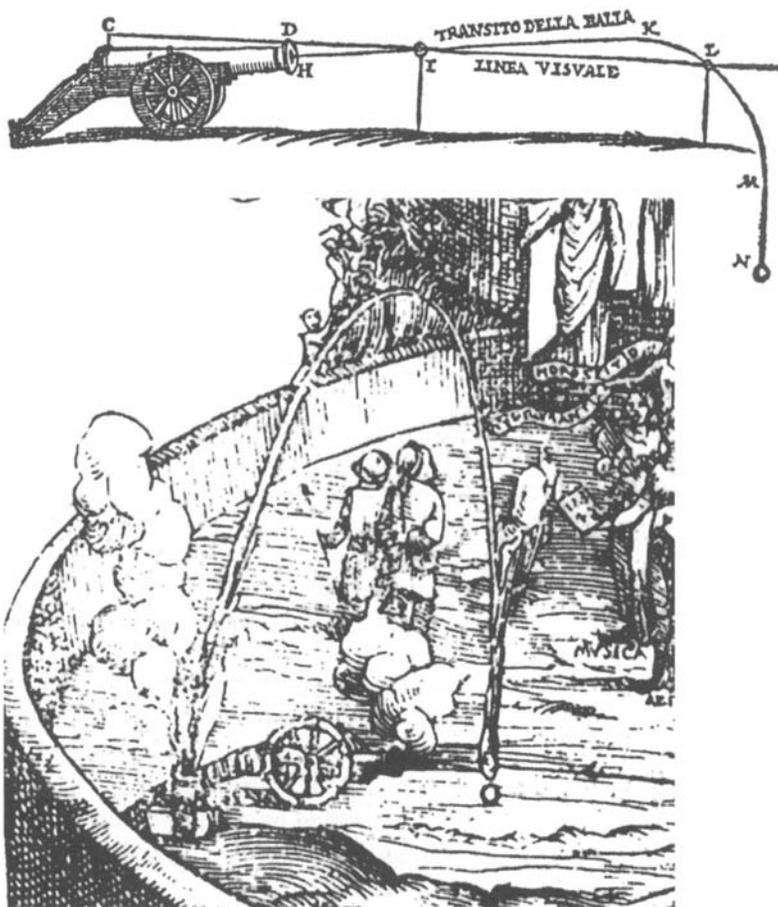

Figure 6. Tartaglia's projectile trajectories according to his theory and according to his practical experience[1]

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[1] The first figure is taken from N. Tartaglia 1959, the second one from S. Drake and I. E. Drabkin 1969.
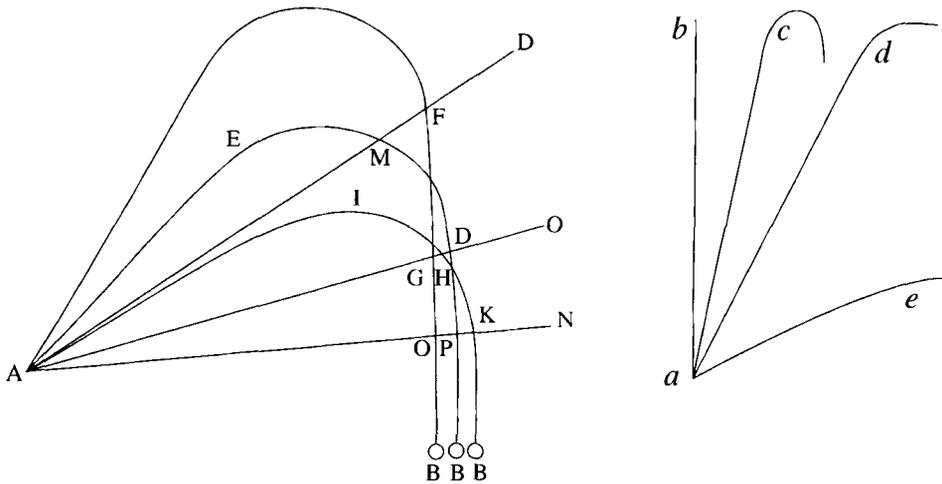
Figure 7. Comparison of Tartaglia's construction of projectile trajectories (left) with Galileo's figure in *De Motu* (right)[1]

The first part of the trajectory was conceived by Tartaglia as reflecting the initially dominant role of the violent motion, whereas the last straight part is in accord with the eventual dominance of the projectile's weight over the violent motion and the tendency to reach the center of the earth. The curved middle part might have been conceived of as a mixed motion compounded of both violent motion in the original direction and natural motion vertical downward. But, since violent and natural motion were supposed to be contrary to each other, this conclusion appeared to be impossible to Tartaglia. He claimed instead the curved part to be exclusively due to violent motion as is the first straight part of the trajectory. He proved the proposition:

> No uniformly heavy body can go through any interval of time or of space with mixed natural and violent motion. (Drake and Drabkin 1969, 80)

Tartaglia's construction of the trajectory was influential throughout the sixteenth century, although it could not be brought into agreement with the simple explanation for the obvious fact that non-vertical projection is never perfectly straight, and, in addition, corresponds only roughly to the visual impression of the motion of projectiles, and certainly could not be justified by precise observations of their trajectory. The simple shape of Tartaglia's trajectory, however, immediately allows one to draw a number of conclusions about projectile motion by geometrical reasoning that seemed to be theoretically convincing and practically useful.

It is well known that Galileo originally also adhered to this theory. In his early manuscript *De Motu*, written about 1590, he contributed to this theory by proving

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[1] The first figure is taken from Tartaglia 1984, 11. The second figure has been redrawn on the basis of a microfilm reproduction of the original manuscript.

at the end of his treatise the proposition that objects projected by the same force move farther on a straight line the less acute are the angles they make with the plane of the horizon. At that time, he obviously had no doubt that the traditional view of a straight beginning of the trajectory was correct, adding his own contribution to further developing this theory (see figure 7). He tried to explain the different lengths of the straight parts of the trajectories of bodies projected under different angles by arguing that different amounts of force are impressed into the body according to the different resistances if the angle of projection is varied. In the course of the proof of this proposition, however, he developed Tartaglia's theory further in the direction already taken by Tartaglia himself. In Tartaglia's later publication, the *Quesiti*, he again, possibly under the pressure of Cardano's criticism of his claim that natural and forced motion cannot act simultaneously,[1] stated even more clearly than in his *Nova Scientia* that the trajectory is in no part perfectly straight. Galileo elaborated the theoretical explanation for the curvature of the trajectory given by Tartaglia even further. This theoretical explanation brought him into conflict with an argument he had developed earlier in order to explain acceleration in free fall. Galileo argued that only in the case of vertical projection violent and natural motion due to their contrariety cannot act together at the same time, whereas in the case of oblique projection the trajectory may well be explained by the simultaneous effects of both.

> When a ball is sent up perpendicularly to the horizon, it cannot turn from that course and make its way back over the same straight line, as it must, unless the quality that impels it upward has first disappeared entirely. But this does not happen when the ball is sent up on a line inclined to the horizon. For in that case it is not necessary for the [impressed] projecting force to be entirely used up when the ball begins to be deflected from the straight line. For it is enough that the impetus that impels the body by force keeps it from [returning to] its original point of departure. And this it can accomplish so long as the body moves on a line inclined to the horizon, even though it may be only a little inclined [from the perpendicular] in its motion. For at the time when the ball begins to turn down [from the straight line], its motion is not contrary to the [original] motion in a straight line; and, therefore, the body can change over to the [new] motion without the complete disappearance of the impelling force. But this cannot happen while the body is moving perpendicularly upward, because the line of the downward path is the same as the line of the forced motion. Therefore, whenever in its downward course, the body does not move toward the place from which it was projected by the impressed force, that force permits it to turn downward. For it is sufficient for that force that it keeps the body from returning to the point from which it departed. (Galilei 1960, 113)

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[1] See Drake and Drabkin 1969, 100-104. On Cardano's criticism see Arend 1998.

The assumption that the curved part of the trajectory of a body projected obliquely may be compounded of violent and natural motion at the same time immediately raises the question of what ratio they might have. Galileo obviously felt it necessary to assume in the case of vertical projection that the impressed force had to disappear entirely before the projected body can turn downward. This, however, contradicts the theory of vertical projection which he developed in the context of his discussion of acceleration of free fall and according to which, at the turning point of a projection directed upwards, impressed force and weight of the body equilibrate each other:

> For a heavy body to be able to be moved upward by force, an impelling force greater than the resisting weight is required; otherwise the resisting weight could not be overcome, and, consequently, the body could not move upward. That is, the body moves upward, provided the impressed motive force is greater than the resisting weight. But since that force, as has been shown, is continuously weakened, it will finally become so diminished that it will no longer overcome the weight of the body and will not impel the body beyond that point. And yet, this does not mean that at the end of the forced motion this impressed force will have been completely destroyed, but merely that it will have been so diminished that it no longer exceeds the weight of the body but is just equal to it. To put it in a word, the force that impels the body upward, which is lightness, will no longer be dominant in the body, but it will have been reduced to parity with the weight of the body. And at that time, in the final moment of the forced motion, the body will be neither heavy nor light. (Galilei 1960, 89)

The uncertainty of Galileo about the problem of how to account for the shape of the trajectory of a projectile in terms of the interaction of violent and natural motion indicates the implicit difficulty of the medieval Aristotelian view of projectile motion mentioned above. This difficulty must have become even more demanding when a symmetrical shape of the trajectory had to be taken into account. Galileo or anybody who performed the experiment recorded in Guidobaldo's protocol or heard about its outcome must have realized immediately that it sheds new light upon the prevailing view of projectile motion. The trajectory cannot be symmetrical unless the impetus determining the first part acts exactly the same way as the natural motion acts in the second part. Hence, the symmetry of the trajectory must have been remarkable to everybody who was familiar with the Aristotelian view of projectile motion. This is probably the reason why Guidobaldo, before he described the experimental setting, started his note by stating this puzzling fact, and then drew attention to an observation that might make it plausible. He compared the constellation of violent force and natural tendency in the case of the trajectory with another case, showing a similar constellation, but exhibiting a perfect and intelligible symmetry: the catenary.

The explanation for the unexpected symmetry suggested by this comparison implies certain assumptions which challenged the medieval Aristotelian theoretical framework even more. Whereas in this tradition violent motion and natural motion were contraries which could not contribute together to one and the same motion the comparison with the catenary requires that they act jointly and in the same way, mutually exchanging their roles when ascending turns into descending. This conclusion is, in fact, drawn in Guidobaldo's protocol.

For the same reason, the prevailing belief that the beginning part and the ending part of the projectile trajectory are straight lines had definitely to be dismissed. From the first moment on the trajectory has to be curved by the weight of the projectile, even though it looks perfectly straight. Accordingly, as Guidobaldo writes, the violence is gradually lessened but is never lost completely.

Summing up: The immediate outcome of the experiment described in Guidobaldo's protocol is the observation that projectile motion has a *symmetrical trajectory*. Although this observation could be explained within the conceptual framework of the medieval Aristotelian tradition, such an explanation indirectly implied characteristics of projectile motion that were specific and unusual at the time of the young Galileo:

- The symmetry of the dynamic situation suggested that the *projectile trajectory and the catenary have the same form*.

- It further implied that the projectile never moves in a perfectly straight line but in a curve determined by two components, the violent one and the natural one. Both have to *act equally while mutually changing their roles* in ascending or descending, respectively.

Decomposing the Trajectory—Neutral Motion and the Law of Fall

There is still another implication of the experiment recorded in Guidobaldo's note which is somewhat more hidden but leads to much more dramatic consequences. For someone mathematically educated like Galileo or Guidobaldo the symmetrical curves of projectile trajectories and hanging chains must have raised immediately the question of whether these curves coincide with any of the well-known curves of ancient or contemporary mathematics. And indeed, Guidobaldo mentioned already in his protocol the hyperbola and the parabola as curves which look similar to the curves generated by the experiment. He must also have been well aware of the fact that in order to ascertain that such a curve fits the trajectory exactly the curve had to be derived from assumptions regarding the forces that determine in an equal way the curves of the trajectory and the catenary.

This, however, was by no means a simple task. It is difficult already to decide which of the two curves, the hyperbola or the parabola, is a more promising can-

didate, although the curves generated by the same procedure documented by the folio sheet in Galileo's hand copy of the Discorsi mentioned above clearly exclude the hyperbola option. Moreover, the dynamical explanation given in the protocol implies, on closer inspection, that the constantly decreasing ratio between violent and natural motion in descent assumed in the protocol is incompatible with the asymptotic behavior of the hyperbola. It follows, in fact, from this asymptotic behavior that this ratio should approach a constant different from zero. But once the parabola has been chosen, its geometrical properties, well-known since ancient times, suggest certain assumptions about the forces and how they act together. Mathematically trained scientists like Galileo or Guidobaldo will have been able to see that in every point of the trajectory or of the hanging chain the square of the horizontal distance from the highest or lowest point respectively is proportional to the vertical distance. Any assumed relation of the two dimensions of the parabola with the two types of forces, the violent and the natural, leads therefore automatically to statements about how precisely these forces generate the curves of the catenary and the trajectory. This suggests in particular to conceive of the motion of a projectile as being composed of two motions, a uniform horizontal motion and a vertical motion that is first an upwards decelerated and, after having reached the highest point, downwards accelerated motion.

This consideration within the conceptual framework of contemporary Aristotelian thinking does, of course, neither lead to the mechanical explanation of the catenary in classical physics, nor to that of projectile motion. It is not even sufficient to construct a precise analogy between the way in which the forces act together in the case of the catenary and in the case of the trajectory as it is so strongly pointed out both in Guidobaldo's protocol and even more vividly later in Galileo's Discorsi. But if Galileo had ever followed this line of intuitive thought, he would have realized that the assumption of a uniform motion along the horizontal is closely related to an amazing insight he achieved already much earlier when he studied the motion on inclined planes with different inclinations, and that the second motion leads to an extraordinary discovery, that is, what later was called the law of fall.

Galileo hit on the phenomenon of a uniform horizontal motion when he investigated the force of a body moving down differently inclined planes. This force decreases with decreasing inclination of the plane. Thus, for the case of a horizontal plane he proved in the De Motu manuscript:

> A body subject to no external resistance on a plane sloping no matter how little below the horizon will move down [the plane] in natural motion, without the application of any external force. This can be seen in the case of water. And the same body on a plane sloping upward, no matter how little, above the horizon, does not move up [the plane] except by force. And so the conclusion remains that on the horizontal plane itself the motion of the body

is neither natural nor forced. But if its motion is not forced motion, then it can be made to move by the smallest of all possible forces. (Galilei 1960, 66)

This result is obviously incompatible with the Aristotelian dynamic law according to which the velocity of a motion is proportional to the moving force. Later it became a cornerstone of Galileo's theory of projectile motion, but when Galileo first hit on this result he seems to have had trouble reconciling it with the traditional understanding of natural and forced motion. He added into the margin a note indicating that he intended to interpret the unusual horizontal motion as mixed motion in the Aristotelian sense and added a justification of this statement:

> From this it follows that mixed motion does not exist *except circular* [deleted]. For since the forced motion of heavy bodies is away from the center, and their natural motion toward the center, a motion which is partly upward and partly downward cannot be compounded from these two; unless perhaps we should say that such a mixed motion is that which takes place on the circumference of a circle around the center of the universe. But such a motion will be better described as 'neutral' than as 'mixed.' For 'mixed' partakes of both [natural and forced], 'neutral' of neither. (Galilei 1960, 67)[1]

In accordance with this last remark, he then deleted the words "except circular," obviously because he preferred to return to the traditional view held by Tartaglia that motions mixed of natural and forced motions are impossible. From the viewpoint of later classical physics this reluctance to accept the motion of projectiles to be compounded of natural and forced motions appears to have been a major obstacle against generalizing the concept of "neutral" motion to a general concept of inertial motion.

But even without such a generalization the concept of neutral, horizontal motion that is neither natural nor forced paves the way to the law of fall. If the parabolic trajectory is decomposed into a neutral, horizontal motion and a natural, vertical motion, then in order to ensure the symmetry of the trajectory the easiest way is to consider the neutral motion as uniform. Then, however, the geometrical theorem which is the basis of such a decomposition, stating that the vertical distances are proportional to the squares of the horizontal distances from the vertex, implies the proportionality of the vertical distances to the squares of the times represented by the horizontal distances, that is, the law of fall.
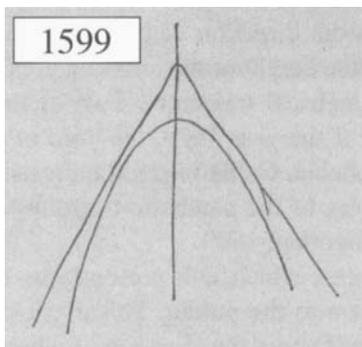
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[1] See also the reference to the whole problem in the *Discorsi*, Galilei 1974, 157-159.

Dating Guidobaldo's Protocol

In view of such implications of Guidobaldo's experiment concerning Galileo's major discoveries, the question of when it was first performed becomes significant. The experiment was, of course, made before Guidobaldo's death in 1607. The previous analysis of possible consequences of the observations reported by Guidobaldo and Galileo show furthermore, on the one hand, that Galileo when he worked at his *De Motu* manuscript around 1590, that is about two years after first contacts between Galileo and Guidobaldo are documented by the correspondence between them, cannot yet have been aware of the outcome of the experiment. On the other hand, it is also evident that Guidobaldo, when he wrote the protocol, was not yet familiar with Galileo's discovery of the parabolic shape of the trajectory. It is also unlikely that he knew at that time already Galileo's law of fall, because otherwise he would have immediately recognized the close relationship between this law and a parabolic shape of the trajectory and, consequently, would not have considered a hyperbolic shape as an alternative. Thus, the time window of possible dates of the protocol ranges from 1590 to 1607 with the qualification that it must have been written before Guidobaldo had any knowledge of Galileo's discoveries of the law of free fall or the parabolic trajectory, in case they had been made already earlier. This latter clue would not help to date Guidobaldo's protocol if the standard dating of Galileo's discoveries to 1604 and 1609 would be correct. However, as we mentioned already, there is strong evidence that Galileo achieved these major results of his work much earlier and that, in fact, these discoveries are closely connected with the experiment described by Guidobaldo.

It seems that Galileo concealed as long as possible the discovery of the parabolic shape of the trajectory, in contrast to the discovery of the law of fall. In the publication of his *Dialogo* in the year 1632 in which he made known the law of fall for the first time, he also included a misleading discussion of the trajectory of a projected body which seems to indicate that even at that time he still had no idea of the true shape of this trajectory (Galilei 1967, 165ff).
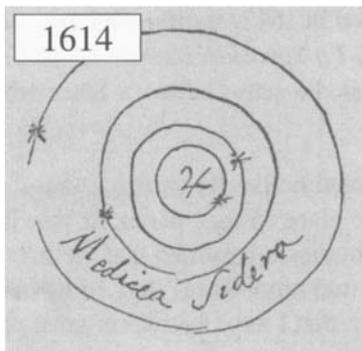
There are indications, that he, indeed, consciously kept his discovery a secret. Following a tradition of the time, he occasionally showed his admiration to a close friend by signing in an "Album Amicorum" with an allegorical drawing containing an allusion to an important achievement which could take the form of a riddle (Galilei 1890-1909, XIX: 204).
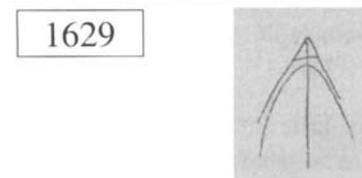
1599

Hoc, Thoma Segete, observantiae et amicitiae
in te meae signum ita perenne servabis,
ut indelebili nota pectori meo virtus infixit tua.
    Galileus Galilei N. Flor.^{us}, Mat.^{rum}
in Academia Pat.^{na} professo (sic),
m. pp.^{a} scripsi Murani, Idib. Augusti 1599.

> This, Thomas Segget, will serve you as a
> sign of my esteem and friendship towards you
> – so durably as your virtue has sticked it to
> my heart by an undestructible mark.

1614

An. 1614. D. 19 Novembris.
Ut nobili ac generoso studio D. Ernesti Brinctii
rem gratam facerem, Galileus Galileius
Florentinus manu propria scripsi Florentiae.

1629

1638

Accedens non conveniam
Galileus Galileius m. p.^{a} scripsi,
die 8^{a} Martii 1629, Florentiae.

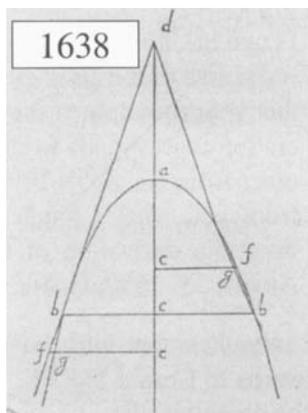> Approaching, I rather not join.

> Figure at the beginning of the
> *Fourth Day* of the *Discorsi*
> used in the proof of the first
> theorem which states that a body
> which is projected horizontally
> describes a semi-parabola

Figure 8. Allegorical drawings of Galileo representing discoveries and a corresponding diagram
in the Discorsi

Four such allegorical drawings used by Galileo are known (see figures 8).[1] One of them expresses his discovery of the satellites of Jupiter and is dated November 19, 1614; the three others each depict a parabola together with its middle axis and its tangents at two symmetrical points, symbolizing a geometrical relation between these tangents and the height of a parabola. The latter figures are similar to the diagrams at the beginning of the *Fourth Day* of his *Discorsi*, representing the parabolic trajectory of a body projected in the direction of the tangent and reaching a maximal height which is one half of the height of the crossing point of the tangents with the middle axis of the symmetrical trajectory. Two of these three figures are dated March 8 and March 20 of the year 1629, the third one is dated as early as August 1599. If, as is most probable, Galileo indeed represented by this allegorical use of a parabola his discovery of the parabolic trajectory, he thus must have made this discovery already earlier than 1599.

It was not until 1632 that certain circumstances, which will presently be discussed, forced him to make his discovery known to the public. This event provides us with a direct statement of Galileo himself about the question of when he made this discovery. When Bonaventura Cavalieri in 1632, shortly after the publication of Galileo's *Dialogo*, published his book *Lo Specchio Ustorio overo Trattato delle Settioni Coniche* on parabolic mirrors, he sent Galileo a letter which contains the following information:

> I have briefly touched the motion of projected bodies by showing that if the resistance of the air is excluded it must take place along a parabola, provided that your principle of the motion of heavy bodies is assumed that their acceleration corresponds to the increase of the odd numbers as they follow each other from one onwards. I declare, however, that I have learned in great parts from you what I touch upon in this matter, at the same time advancing myself a derivation of that principle. (Bonaventura Cavalieri to Galileo, August 31, 1632, Galilei 1890-1909, XIV: 378)

This announcement must have shocked Galileo. In a letter written immediately afterwards to Cesare Marsili, a common friend who lived like Cavalieri in Bologna, he complained:

> I have letters from Father Fra Buonaventura with the news that he had recently given to print a treatise on the burning mirror in which, as he says, he has introduced on an appropriate occasion the theorem and the proof concerning the trajectory of projected bodies in which he explains that it is a parabolic curve. I cannot hide from you, my dear Sir, that this news was any-

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[1]   Another drawing which symbolizes Galileo's discovery of the parabolic shape of the projectile trajectory, and a text, including the date March 20, 1629, which both belong to a hitherto unknown entry in an "Album Amicorum" were recently uncovered as part of a telescope, where they were placed in order to pretend that the telescope was built by Galileo himself and therefore represents a unique and valuable instrument; see Miniati, Greco, Molesini, and Quercioli 1994.

thing but pleasant to me because I see how the first fruits of a study of mine of more than forty years, imparted largely in confidence to the said Father, should now be wrenched from me and how the flower shall now be broken from the glory which I hoped to gain from such long-lasting efforts, since truly what first moved me to speculate about motion was my intention of finding this path which, although once found it is not very hard to demonstrate, still I, who discovered it, know how much labor I spent in finding that conclusion. (Galileo to Cesare Marsili, September 11, 1632, Galilei 1890-1909, XIV: 386)

Galileo received immediate answers both from Marsili and from Cavalieri, written on the same day (Bonaventura Cavalieri to Galileo, September 21, 1632, Galilei 1890-1909, XIV: 395, and Cesare Marsili to Galileo, September 21, 1632, Galilei 1890-1909, XIV: 396). Marsili assured Galileo of Cavalieri's full loyalty. Cavalieri himself expressed his deep concern about Galileo's anger and tried to convince him by a number of different reasons that he did not intend to offend Galileo by his publication. He first claimed that he was uncertain whether the thesis that the trajectory has a parabolic shape entirely corresponds to Galileo's intentions. Then he adduced as an excuse that he was convinced that the thesis had been already widely spread. He furthermore added that he had been uncertain whether the thesis was of any value to Galileo. Finally, he claimed that he had reason to assume that Galileo at that time had published his result already long ago:

I add that I truly thought that you had already somewhere written about it, as I have not been in the lucky situation to have seen all your works, and it has encouraged my belief that I realized how much and how long this doctrine has been circulated already, because Oddi has told me already ten years ago that you have performed experiments about that matter together with Sig.r Guidobaldo del Monte, and that also has made me imprudent so that I have not written you earlier about it, since I believed, in fact, that you do in no way bother about it but would rather be content that one of your disciples would show himself on such a favorable occasion as an adept of your doctrine of which he confesses to have learned it from you. (Bonaventura Cavalieri to Galileo, September 21, 1632, Galilei 1890-1909, XIV: 395)[1]

The conflict about Cavalieri's intended publication of the parabolic shape of the trajectory provides two pieces of information which are highly significant for the question of when and how Galileo really made his discovery.

First, Galileo's claim in his letter to Cesare Marsili makes it conceivable that he had discovered the parabolic shape already around forty years before he wrote this letter, that is, as early as or even earlier than 1592. If this should be true, this

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[1] See also the discussion of this correspondence in Wohlwill 1899.

discovery must have been one of the earliest discoveries of Galileo that chal-
lenged his *De Motu* theory.[1]

Second, Cavalieri brings Galileo's claim in connection with experiments on
projectile motion that Galileo had performed together with Guidobaldo del
Monte. The only experiment that is known and fits the account of Muzio Oddi is
the one reported in Guidobaldo's protocol. It follows that either Galileo himself
must have been present during this experiment or at least have known about it in
the case that there were further experiments on projectile motion jointly per-
formed by them.

Cavalieri claims that he had heard about these experiments already ten years
earlier, that is around 1622, from Muzio Oddi. Indeed, there is independent evi-
dence confirming Cavalieri's report. It turns out that Cavalieri and Muzio Oddi
happened to be both living in the same place, Milan, between 1620 and 1622, that
is, just around the time mentioned in the above passage.[2] But Muzio Oddi himself
must have recalled these experiments as having been made much earlier, because
Guidobaldo del Monte died already in 1607. Muzio Oddi was born and lived –
with interruptions – in Urbino. He mentioned that he had been for a short time a
disciple of Guidobaldo del Monte. Between 1595 and 1598 he left Urbino to
become a military architect in the Bourgogne, with the exception of a period
between 1596 and 1597 when he served as an architect in Pesaro. In 1599 he
began to get in trouble with the authorities, first in 1599 for illegal fishing and
bathing naked in a river, then in 1601 for allegedly stealing from the closet of the
Grand Duke; as a consequence he had to flee in the same year from Urbino into
the territories of the Venetian Republic. He returned only in 1605 after an
amnesty, but soon got again in trouble with the rulers of the city – because of cer-
tain favors received from the Grand Duchesse – so that he was arrested again, in
the "Rocca di Pesaro." He stayed in prison until he was released in 1610 and
finally left Urbino for Milan.[3]

What are the possible dates for occasions on which Muzio Oddi could have
heard from Guidobaldo del Monte about the experiments performed together
with Galileo? Since, according to his own testimony, he was acquainted with
Guidobaldo del Monte already as a disciple, it cannot be excluded that he heard
about the experiments even before he left in 1595. The best opportunity must, of
course, have been the time between 1596 and 1597, when he worked as an archi-
tect in Pesaro itself. A further possibility is the time period between 1598 and

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[1]   It has earlier been assumed that Galileo's claim of such an early discovery of the parabolic
shape of the trajectory was exaggerated and that in his letter to Marsili Galileo was actually refer-
ring to his treatment of projectile motion in *De Motu*, see Camerota 1992, 79. In the light of the
explicit mentioning of the joint experiment with Guidobaldo des Monte in Cavalieri's letter of Sep-
tember 21, 1632, and the precise coincidence of the date of the experiment and Galileo's claim, dis-
cussed here, there can be no doubt that he must have said the truth and that the alternative
assumption has turned out to be untenable.
[2]   For Cavalieri's biography, see Gillispie 1981, for Oddi's biography, see Gamba and Monte-
belli 1988, chap. IV.
[3]   See the short biographical sketch in Gamba and Montebelli 1988, 111-113.

1601 when he stayed again in Urbino, that is, not far away from Pesaro. If we exclude the possibility that Guidobaldo del Monte could have contacted him while he was in prison, the latest date for an encounter is the short stay in Urbino in the year 1605, but given the circumstances of this stay, it is quite unlikely that he should have discussed such experiments with Guidobaldo del Monte at that time. Summing up, the experiments will probably have taken place before 1601, most likely even before 1597. This dating is compatible also with the *terminus ante quem* 1599 suggested by the allegorical drawings mentioned above.

In view of this evidence in favor of an early dating for Galileo's discoveries of the parabolic shape of the trajectory and of the law of fall, it can no longer be excluded that Galileo's own reference to a "study of mine of more than forty years" in his letter of 1632 must actually be taken literally, even though it points at a date for these discoveries as early as 1592.

This circumstantial evidence suggests a close reexamination of the events around the time of 1592 in order to find out whether something special might have happened in this period. It is well known that indeed the year 1592 represents a turning point in Galileo's career. As a result of strong support from Baccio Valori, Consul of the Accademia Fiorentina in 1588 and later representative of Ferdinando I de Medici in the Accademia del Disegno, from Giovanni Vincenzo Pinelli, the head of a group of literary and culturally interested people in Padua, and, in particular from Guidobaldo del Monte, Galileo received, in late 1592, an appointment at the University of Padua (Drake 1987, 32). Earlier in the same year, when Galileo was still desperate about his future and planned a trip to Venice in order to explore his chances of obtaining a position, he received a consoling letter from Guidobaldo who invited him to travel through Monte Baroccio on his way to Venice:

> It also saddens me to see that your Lordship is not treated according to your worth, and even more it saddens me that you are lacking good hope. And if you intend to go to Venice in this summer, I invite you to pass by here so that for my part I will not fail to make any effort I can in order to help and to serve you; because I certainly cannot see you in this state. (Guidobaldo del Monte to Galileo, February 21, 1592, Galilei 1890-1909, X: 47)[1]

Galileo actually travelled twice during this year from Florence to the Venetian republic, the first time probably towards the end of August or in early September in order to receive the appointment (Giovanni Ugoccini to Belisario Vinta, September 21, 1592, Galilei 1890-1909, X: 49), the second time sometime between October and early December when he finally moved to Padua.[2] In the meantime, he had to go back to Florence in order to get permission from the Grand Duke to

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[1]   On the basis of this letter a visit of Galileo with Guidobaldo had been conjectured also by other authors, see e.g. Gamba and Montebelli 1989, 14.

[2]   Galileo was finally in Padua by the middle of December as attested to by his correspondence; see Galilei 1890-1909, X: 50ff.

leave Tuscany and take the chair in Padua.[1] There is no reason to doubt that Galileo followed Guidobaldo's invitation on one of these two occasions.

When Galileo arrived at Padua he immediately visited Giovanni Vincenzo Pinelli (Giovanni Vincenzo Pinelli to Galileo, September 3, 1592, Galilei 1890-1909, X: 47) in whose house he also lived for some time in the beginning of his Paduan stay.[2] At some point during Galileo's early stay in the Venetian Republic he must have encountered, possibly in the house of Pinelli, Paolo Sarpi with whom he long afterwards, stayed in close scientific contact. In the notebook of Sarpi under the header "1592" the following entry can be found:

> The projectile not [moving] along the perpendicular to the horizon never moves along a straight line, but along a curve, composed of two straight motions, one natural, and the other one along where the force is directed. The impressed [force] at the beginning is always greater, and, for this reason, the beginning comes very close to the straight line; but the impressed force continues decreasingly and it returns [in a] similar [way] to [that of] the beginning if it [i.e. the impressed force] has the proportion to the natural [force], as the natural one had to it [i.e. the impressed force], and in everything and all the time the descending is similar to the ascending. If, however, the one [i.e. the impressed force] of the projectile expires, the motion finally comes rectilinearly downward; but if (as has been assumed before) it is infinitely divisible and diminishes according to proportional parts, the motion never comes to be a straight line. Hence, the motion of the projectile is compounded by two forces: one of which always remains the same, and the other always decreases.

> A similar line is caused by a suspended rope, because its suspension would like to pull each part laterally towards it [i.e. the fixed ends], while it [i.e. the rope] would like to move downwards; therefore, the parts closer to the beginning share more of the lateral [force], and the parts closer to the middle share more of the natural [force], the middle has equal shares of both of them and is the vertex of the figure. (Notes number 537 and 538 in Sarpi 1996, 398f)[3]

In view of the in no way uncertain terms with which Sarpi gives a description of the trajectory of projectile motion that is in flat contradiction with the accepted view, it seems inconceivable that this description should be unrelated to Guidobaldo del Monte's interpretation of the experiment described in his proto-

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[1]   Galileo left Venice to Florence on September 27, 1592; see Giovanni Ugoccini to the Grand Duke of Tuscany, September 26, 1592, Galilei 1890-1909, X: 50.
[2]   Benedetto Zorzi to Galileo, December 12, 1592, Galilei 1890-1909, X: 51; see also Favaro 1966, I: 50.
[3]   Due to the local calendar in the Venetian republic, the entries of 1592 may include notes up to February 28, 1593 which makes it even more likely that these entries were made at a time when Galileo and Sarpi were already in close contact.

col. Although it is obvious that this note has been written independently of the actual wording of Guidobaldo del Monte's protocol, it corresponds point by point to the at that time unorthodox theoretical assumptions which in this protocol are used to explain the symmetry of the trajectory which was the unexpected outcome of the experiment.

The symmetry is attributed to a symmetry of the dynamical constellation. This dynamical constellation is conceived as paradigmatically represented by a hanging chain. As an implication the trajectory is everywhere conceived as determined by two components, a violent one and a natural one which explains that the trajectory is nowhere perfectly straight. Finally, according to the dynamical interpretation given for the symmetry of the trajectory, violent and natural motion have to mutually exchange their roles in the ascending and the descending part of the trajectory respectively.
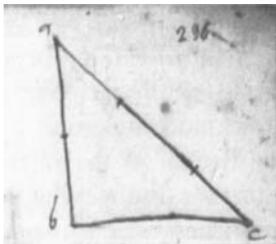
Given the fact that direct contact between Guidobaldo del Monte and Paolo Sarpi in 1592 is extremely improbable and that, as has been discussed already above, Galileo claims in his *Discorsi* that he himself had invented the method to trace the trajectory by means of an inclined plane as it is described in Guidobaldo del Monte's protocol, the remarkable correspondence of this protocol and Paolo Sarpi's note strongly suggests that it was nobody else but Galileo himself who communicated the information about the outcome of the experiment and its interpretation to Paolo Sarpi. The striking similarities between Guidobaldo del Monte's protocol and Paolo Sarpi's note find indeed a simple explanation if Galileo on his first trip[1] from Florence to the Venetian republic in 1592 followed the invitation of Guidobaldo del Monte, performed together with him the experiments on the projectile trajectory and afterwards discussed the puzzling results with his new friend Paolo Sarpi, who became one of his most important intellectual companions in the coming years of his work at Padua. At the same time, Galileo's claim in his conflict with Cavalieri in 1632 that his discovery of the parabolic shape of the trajectory of projectile motion reaches back to work done more than 40 years ago as well as Cavalieri's report that he had heard about Galileo's discovery of the parabolic trajectory by means of experiments performed together with Guidobaldo del Monte, turn out to be perfectly justified, whoever of the two had the idea for designing these experiments. Consequently, according to common historiographic criteria the discovery of the parabolic shape of the trajectory of projectile motion has to be dated to the year 1592. It must, in fact, have been one of Galileo's earliest discoveries reported in his latest publication, the *Discorsi*.

In addition to the reoccurrence of the experiment reported in Guidobaldo's notebook in Galileo's *Discorsi* and of its interpretation in the notebook of Paolo

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[1] It must have been the first trip because as late as January 1593 Guidobaldo was still not informed about the outcome of Galileo's negotiations with the authorities of the Venetian republic on his remuneration; see Guidobaldo del Monte to Galileo, January 10, 1593, Galilei 1890-1909, X: 53f.

Sarpi, further evidence of Galileo's participation in performing the experiment is provided by entries immediately before and after Guidobaldo's report. The first entry in Guidobaldo's notebook that appears to be related to Galileo's visit at Guidobaldo's house in Monte Baroccio refers to Galileo's invention of a hydro-static balance, the "Bilancetta." The problem of determining the specific weight of a substance following a procedure traditionally ascribed to Archimedes has been treated earlier in Guidobaldo's notebook, but without referring to Galileo's instrument (del Monte ca. 1587-1592, 119f). The fact, that such a reference to this instrument, and even a full treatment of it including a demonstration, is found in the last part of Guidobaldo's notebook together with other notes on Galileo's topics, is a strong indication that Guidobaldo received an explanation of this instrument from Galileo himself (del Monte ca. 1587-1592, 232-234).

On the facing page before Guidobaldo's report on the projectile trajectory experiment, another experiment concerning the resonance of strings is described which is also discussed in the *Discorsi*; we shall return to this experiment further on. The same coincidence of an entry in Guidobaldo's notebook and Galileo's writings is also found for the other entries on the same page, written immediately above and below Guidobaldo's report on the experiment concerning the projectile trajectory. In a short note above the description of the experiment Guidobaldo considers the flow of water along an inclined channel, serving to drive a mill. He writes:[1]

When a descent [caduta] will be of a height of ten, in order to give water to a mill the channel should be 15 [sic!], as the descent *ab* and the channel *ac*. But due to the general rule, *ac* should usually be elevated by ca. 45 degrees, according to the consideration of the quantity of water which one has. (del Monte ca. 1587-1592, 236)

The theme of water moving along an inclined plane is common in Galileo's writings, from his early treatise *De Motu*, via a letter to Guidobaldo del Monte in 1602 (Galileo to Guidobaldo del Monte, November 29, 1602, Galilei 1890-1909, X: 97-100), to his criticism in 1630 of a plan for straightening the river Bisenzio.[2] In the latter, Galileo explains more in detail that the motion of water along an inclined plane differs from that of a solid body since here one has to take into account not only the tilt of the plane but also the quantity of water flowing along it:

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[1]   The entry has been discussed by Gamba 1995, 101. In this article the figure for the length of the channel has been transcribed as 25 as suggested by the appearance of the number in the text. Gamba has later (personal communication) convincingly argued that the figure has to be read as 15 consistent with the designation of length units in the figure.

[2]   See the discussion in Drake 1987, 320-329.

> Now because in the acceleration of the course of the highest waters little part is played by greater slope and much by the great quantity of supervening water, consider that in the short channel although there is greater tilt than in the longer, the lower waters of the long [channel] are more charged by the great abundance of higher waters pushing and driving, by which impulse is more than compensated the benefit that could be derived from greater slope. (Galileo to Raffaello Staccoli, January 16, 1630, Galilei 1890-1909, VI: 639; translation quoted from Drake 1987, 328)

Both passages deal with the same physical effect, water flowing along a tilted channel or river. Guidobaldo apparently refers to a practitioners' rule according to which that tilt should usually be about 45 degrees in order to drive a mill and considers a situation in which a greater distance has to be bridged from the water source to the mill, resulting in a less steeply inclined channel (41.8 degrees). The question that must have motivated this consideration was surely that of the effect of this changed tilt on the flow of water; it may have well been triggered, as Gamba suggests in his paper, by a practical problem. Does the lesser inclination yield a smaller flow of water and is hence incapable of driving the mill? Guidobaldo's final remark refers to a consideration of the quantity of water that is required for the response to such a question and is hence in complete agreement with the essence of Galileo's argument in the passage quoted above. It is therefore plausible to assume that Galileo and Guidobaldo discussed how the laws of motion along inclined planes derived by Galileo in his treatise *De Motu* are changed if they are applied to channels or rivers, in which case, according to Galileo, the quantity of the liquid somehow compensates the effect of the tilt of the plane.

The short text written by Guidobaldo below his description of the experiment also summarizes an argument that is found in Galileo's writings. This text reads:
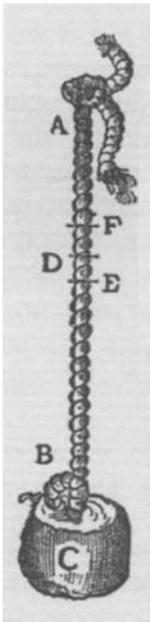
> A cord which sustains a weight, sustains as much if it is short as it does when it is long; it is indeed true that in the long one [it breaks more easily], first, because of its gravity, second, because in the long one there can be many weak parts. Assume [può esser] that it [i.e. the long one] breaks more easily and by less weight. But if the cord would be sustained a little above from where it breaks because of its cracking and the weight would be a little underneath, without doubt it would break in the same way because it would have cracked in the same way. (del Monte ca. 1587-1592, 236)

Galileo inserted exactly the same argument between the propositions and proofs concerning the rigidity of bodies at the *Second Day* of the *Discorsi*. There one finds the following dialogue between his spokesman Salviati and the Aristotelian philosopher Simplicio:

Simplicio. (...) we see a very long rope to be much less able to hold a great weight than if shorter; and I believe that a short wooden or iron rod can support much more weight than a very long one when loaded lengthwise (not [just] crosswise), and also taking into account its own weight, which is greater in the longer.

Salviati. I think that you, together with many other people, are mistaken on this point, Simplicio, at least if I have correctly grasped your idea. You mean that a rope, say forty braccia in length, cannot sustain as much weight as one or two braccia of the same rope.

Simplicio. That is what I meant, and at present it appears to me a highly probable statement.

Salviati. And I take it to be not just improbable, but false; and I believe that I can easily remove the error. So let us assume this rope *AB*, fastened above at one end, *A*, and at the other end let there be the weight *C*, by the force of which this rope is to break. Now assign for me, Simplicio, the exact place at which the break occurs.

Simplicio. Let it break at point *D*.

Salviati. I ask you the cause of breaking at *D*.

Simplicio. The cause of this is that the rope at that point has not the strength to bear, for instance, one hundred pounds of weight, which is the weight of the part *DB* together with [that of] the stone *C*.

Salviati. Then whenever the rope is strained at point D by the same 100 pounds of weight, it will break there.

Simplicio. So I believe.

Salviati. But now tell me: if the same weight is attached not to the end of the rope, *B*, but close to Point *D*, say at *E*; or the rope being fastened not at *A*, but closer to and above the same point *D*, say at *F*; then tell me whether the point *D* will not feel the same weight of 100 pounds.

Simplicio. It will indeed, provided that the length of rope *EB* accompanies the stone *C*.

Salviati. If, then, the rope, pulled by the same hundred pounds of weight, will break at the point *D* by your own admission, and if *FE* is but a small part of the length *AB*, how can you say that the long rope is weaker than the

short? Be pleased therefore to have been delivered from an error, in which you had plenty of company, even among men who are otherwise very well informed, and let us proceed. (Galilei 1974, 119-120)[1]

It is likely that by mentioning "men who are otherwise very well informed" who made the same error as Simplicio, Galileo had nobody else in mind but Guidobaldo himself. We shall see below that this is not the only case of Guidobaldo having entered an argument of somebody else into his notebook although he himself believed or had believed the contrary to be true. In any case, even if there were no independent evidence for Galileo having visited Guidobaldo at the very time when he performed the projectile experiment, the fact alone that the entries immediately before and after his protocol of the experiment reappear in Galileo's *Discorsi*, just as it is the case for the experiment itself, makes it difficult to imagine anything else than that Galileo was present when Guidobaldo entered these notes into his notebook.

But also the other parts of the notebook provide evidence for the dating of Guidobaldo del Monte's protocol of the projectile trajectory experiment into the year 1592.[2] A comparison of the entries in the notebook with the correspondence of Guidobaldo del Monte shows that most of the entries at the beginning of the notebook must have been written between the years 1588 and 1590. An unquestionable *terminus a quo* is given by entries related to publications of Fabricius Mordente (1585), Giovanni Battista Benedetti (1585), and Francesco Barozzi (1586); an equally unquestionable *terminus ad quem* by the fact that the last third of the notebook is mainly devoted to problems of perspective related to his work on a book which in great parts has been completed around 1593 and has finally been published in 1600. As far as a direct relation between entries of the notebook and issues mentioned in the correspondence can be established, they can all be dated into the years between 1588 and 1590; that is, work on a geometrical problem of Pappus (mentioned 1588)[3], the correspondence with Galileo on the center of gravity of paraboloids (beginning 1588)[4], and work on the cochlea (mentioned 1589 and 1590)[5]. Furthermore, a loose sheet of paper is inserted in

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[1]   It may be worth noting that in Sarpi's notebook shortly after the note on the outcome of the trajectory experiment one finds an entry about the resistance of bodies involving an argument similar to the one treated in Guidobaldo's notebook and Galileo's *Discorsi*, although no concrete experimental setting is described (see note number 543 in Sarpi 1996, 405). Sarpi merely limits himself to an application of Galileo's indirect proof to a general consideration of the resistance of continuous bodies against breakage. This note by Sarpi may represent a third case, in addition to those of the projectile trajectory and the catenary, in which an argument by Galileo is reported in both Guidobaldo's and Sarpi's notebooks.
[2]   Guidobaldo's notebook has been widely neglected by historians of science. Certain passages of this notebook, in particular those on the hydrostatic balance and on motion in media (see below) have, however, been intensively discussed during a visit of Pierdaniele Napolitani and Pierre Souffrin in Berlin and during a workshop in Pisa organized by Pierdaniele Napolitani. The results of these discussions will appear in future publications.
[3]   It is mentioned in Guidobaldo del Monte to Galileo, September 16, 1588, Galilei 1890-1909, X: 37 as a problem that Guidobaldo had already earlier communicated to Galileo.
[4]   See Guidobaldo del Monte to Galileo, January 16, 1588, Galilei 1890-1909, X: 25f and the subsequent letters.

the notebook with astronomical data for a horoscope for a date in the year 1587.[1] Thus, the fact that the protocol of the projectile trajectory experiment performed in 1592 is written on one of the last pages of the notebook is in any respect in accordance with the dating of the other entries in the notebook.

Galileo was able to think and thus, as a competent mathematician, capable of drawing the obvious conclusion from the experiment that, as a consequence of the dynamical assumptions laid down in Guidobaldo's protocol, the trajectory cannot be hyperbolic but must be parabolic. In the following, we shall therefore simply assume that he must have been indeed, as he claimed, aware as early as 1592 of the parabolic shape of the projectile trajectory, possibly sharing this knowledge with his patron Guidobaldo. While this insight would qualify, by ordinary historiographical standards, as a "discovery," such a qualification leaves, however, completely open the question of what this "discovery" actually meant for both Galileo and Guidobaldo at that time. In order to understand the impact of this discovery one has to study the contexts in which it occurred – which may well be different for Guidobaldo and for Galileo.

Guidobaldo del Monte as an Engineer-Scientist

Guidobaldo del Monte represented a new type of engineer-scientists which emerged in the sixteenth and seventeenth centuries, in distinction from traditional academics.[2] The emergence of this new social group and its epistemological motives cannot be adequately understood without taking into account the technological development that had taken place at least since the Renaissance in certain European urban centers. The essence of this technological development is visible in the remarkable difference between large-scale projects in the early modern period and in ancient urban civilizations. Ancient large-scale projects, such as the construction of the Babylonian zikkurats and Egyptian pyramids, involved enormous challenges for labor organization, mastered by a class of high-rank officials with appropriate administrative knowledge about the acquisition, allocation, and maintenance of labor force. There is, however, no historical record of a comparative body of engineering knowledge adequately corresponding to the implicit technological complexity of these large-scale projects, nor of any social group representing technical in contrast to administrative knowledge. Even in the case of the most advanced construction projects of the Roman empire, there is hardly any trace of a technical intelligentsia which, beyond the organization of labor, developed a specific canon of technical knowledge—other than the type of com-

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[5] See Guidobaldo del Monte to Galileo, August 3, 1589, Galilei 1890-1909, X: 41 and Guidobaldo del Monte to Galileo, April 10, 1590, Galilei 1890-1909, X: 42f.

[1] del Monte ca. 1587-1592, 212.

[2] For historical discussions from which our account has benefited, see Bertoloni Meli 1992; Biagioli 1989; Micheli 1992; Gamba and Montebelli 1988; and Lefèvre 1978.

pilation of rules and standard models exemplarily represented by the work of Vitruv—that challenged ancient theories. The large-scale projects of the early modern period, such as the construction of the Florentine dome, on the other hand, are inconceivable without a group of specialized artisans, technicians, and engineers that combined administrative with technological competence. Due also to the limited availability of labor-force and other resources, these artisan-engineers were continuously confronted with technical and not only logistic challenges. In reaction to these challenges they were forced to explore the inherent potential of traditional technical knowledge in order to create new technical means, as for example the set of machines developed by Filippo Brunelleschi in order to build the Florentine cupola without an inaffordably more expensive scaffolding (see di Pasquale 1996). The engineers in early modern times were thus not only carriers of a traditional canon of knowledge, as it had been largely the case of the ancient administrators of large-scale projects, but were involved in a cumulative, self-accelerating process of innovation.

The technical knowledge of these engineers developed independently of the academic traditions and had itself, at first, little impact on the dominant scholastic Aristotelian interpretation of nature. While this knowledge was still largely transmitted in traditional social forms, that is by learning on the job within guild structures, it occasionally became the subject also of literary productions, as is illustrated by the writings of Leon Battista Alberti, Piero della Francesca, Leonardo da Vinci, and others. This knowledge thus became part of a new interpretation of nature and of man's place in it, entering an intellectual discourse in which alternatives were searched to the dominant scholastic interpretation of nature and society. Consequently the new technical knowledge, or rather its reflection in the new kind of technological literature, was brought, at least potentially, into conflict with Aristotelian interpretations of natural processes and technical devices. In the course of this entry of technical knowledge into an intellectual world it was brought into contact also with the heritage of antiquity, comprising not only alternatives to the Aristotelian theory of nature (e. g. Platonism or atomism) but also an unexploited richness of antique mechanical knowledge as represented by the *Mechanical Questions* of Pseudo-Aristotle and the writings of Archimedes.

In the sixteenth century this development led to the formation of a new category of intellectual (practical mathematicians and engineer-scientists such as Cardano, Tartaglia, Commandino, Oddi, Benedetti, Brahe, Kepler, Ricci, Stevin) who were no longer necessarily and, in any case, not completely involved in technical practice in the same way as the engineers themselves, but who rather specialized in the reflection of the new type of knowledge produced by this practice and, of course, in the attempt to make that reflection useful again for practical purposes. While their emergence as a technical elite gradually gained support from a new kind of institutionalized learning, exemplified by the Florentine Accademia del Disegno, their social status remained precarious throughout early modern times,

making them dependent on the unreliable patronage of the courts and necessitating an equally unreliable overstatement of the practical relevance of their theoretical projects. It is exactly this group which formulated projects typical also for Galileo's research, for example Tartaglia's new science of ballistics or Benedetti's new science of motion in media. From what has been pointed out above concerning the inherent complexity of the new objects of knowledge it follows that in fact all of the engineer-scientists shared the problem of a considerable disproportion between their pretentious claims and their actual chances to attain success in their projects. Since they all searched for a new theoretical foundation of the practical knowledge in whose reflection they were engaged, they also necessarily shared an anti-Aristotelian attitude. Both their social status and their occupation make it understandable that they were, in addition, usually involved in competition and sometimes bitter controversies among themselves. Indeed, given the heterogeneous field of knowledge they were exploring, they could and did find reasons to search for alternative ways to create a "new science" of this or that subject. Nevertheless, for practically all of them the ancient works of mechanics, in particular the *Mechanical Questions* attributed at that time to Aristotle and the recently revived works of Archimedes, as well as the writings of Jordanus Nemorarius, provided a common core of mechanical knowledge which set the standard for any "new science" to be developed.

Guidobaldo del Monte precisely fits the characteristics of the engineer-scientist, apart from the fact that his social status as a feudal aristocrat made him independent of the unreliable patronage of the courts and of the search for a way to gain income from his passion.[1] His qualities as a practical man could not be better demonstrated than by the fact that, in 1588, he became inspector of Tuscan fortifications, but his competence was by no means restricted to the qualifications of a practitioner. At the beginning of his carreer he studied mathematics and philosophy, first at the university of Padua, later as a private disciple of Commandino at Urbino. Already shortly after he had finished his studies, he wrote one of the most influential books on mechanics of the century (published in Latin 1577, translated into Italian by Filippo Pigafetta and published 1581). His theoretical orientation may be represented by his commentary on Archimedes' work on the centers of gravity (published 1588). That he did, however, not conceive of theory as being separated from practical applicability is made clear by his credo as expressed in the preface to his mechanics:

> For mechanics, if it is abstracted and separated from the machines, cannot even be called mechanics.[2]

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[1] For a succinct biography of Guidobaldo del Monte, see Drake 1987, 459, concerning his social status, see the discussions in Biagioli 1989 and in Allegretti 1992.
[2] Translation quoted from Drake and Drabkin 1969, 245.

This insistence of the relation between mechanics and machines was consequential also for the way in which Guidobaldo del Monte approached the problems of theoretical mechanics. In a letter from 1580 he wrote:

> Briefly speaking about these things you have to know that before I have written anything about mechanics I have never (in order to avoid errors) wanted to determine anything, be it as little as it may, if I have not first seen by an effect that the experience confronts itself precisely with the demonstration, and of any little thing I have made its experiment. (Guidobaldo del Monte to Giacomo Contarini, October 9, 1580; translated from Micheli 1992, 98)

Where this coincidence between theoretical conclusions and practical verification did not take place, such as it is, according to Guidobaldo, the case of mechanical processes involving motion, he tended to avoid the subject.[1]

Apart from his publications on mechanics, Guidobaldo del Monte wrote books on further topics which fit into the social pattern of an engineer-scientist with such an orientation. He published on geometry and perspective (*Planispheriorum universalium theorica* 1579; *Perspectiva* 1600). Further books have been published posthumously (*Problemata astronomica* 1609; *Cochlea* 1615). From a letter by his son Orazio written to Galileo after the death of his father[2] it is furthermore known that Guidobaldo had left several minor works unpublished (*In Quintum*; *De motu terrae*; *De horologiis*; *De Radiis in aqua refractis*; *In nono opere Scoti*; *De proportione composita*, and another booklet on instruments invented by him). In a letter by Muzio Oddi further minor works by Guidobaldo del Monte are listed, whose thematic range, as far as it is known, seems to fall within that represented by his other writings.[3]

Guidobaldo's intellectual profile which is represented by these publications and manuscripts is perfectly reflected by the contents of the notebook which contains also his protocol on the projectile trajectory experiment.[4] The notebook comprises 245 mostly numbered pages (together with some inserted sheets) and begins with extensive notes on sundials, continues with notes on a set of problems of plane geometry, which are followed by entries on mechanical problems, notes on spherical astronomy, and notes on geometrical problems of stereometry, and then, after a mixture of various entries comprising critiques of contemporary authors as well as further notes on sundials, the notebook contains extensive notes on perspective; at the end of the notebook one finds again a mixture of various entries, among them the protocol of the experiment on projectile motion. The notebook thus testifies both to Guidobaldo's practical and to his theoretical interests. It shows not only his familiarity with the tradition of antique mechanics

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[1] See Gamba and Montebelli 1988, 76.
[2] Orazio del Monte to Galileo, June 16, 1610, Galilei 1890-1909, X: 371f.
[3] See Gamba and Montebelli 1988, 54.
[4] del Monte ca. 1587-1592, 236f.

but also his awareness of the works of his contemporaries, in particular of Commandino, Clavius, and Benedetti. Most remarkably, as mentioned already above, in his scattered entries on mechanical problems, Guidobaldo occasionally goes beyond his own book on mechanics, treating, e.g., the problem of the bent lever in close connection with the inclined plane. This improved treatment was apparently stimulated by a close reading and critique of Benedetti's book and most probably by a suggestion of Galileo. In addition to the more extensive sets of notes, one also finds scattered entries (in part discussed above) on such diverse topics as two methods of describing a hyperbola, problems of artillery, the motion of heavy bodies in media, the reflection of light by a mirror, the motion of the center of gravity of the earth, astrology, the sound of chords, and water supply for mills.

While the notes on the projectile trajectory experiment fit, as we have seen, perfectly into the chronological order of the entries in Guidobaldo's notebook, their content shows a certain contrast to the bulk of the other notes. In fact, with a few further exceptions, these notes correspond to Guidobaldo's intellectual profile as it is also known from his other writings and his correspondence. The protocol of the experiment, on the other hand, belongs to the small group of entries which have no counterpart in Guidobaldo del Monte's publications and seem not to belong to the areas of his main interests. The notes on projectile motion as well as other entries, some of which belong to this exceptional type, correspond, however, to topics known as having been subject of the work of Galileo during his time in Pisa. Besides the projectile trajectory, these subjects are the motion in media, Heron's crown problem, the inclined plane, and the sound of chords. From the correspondence between Guidobaldo and Galileo we know in fact that they exchanged since early 1588 not only letters but also copies of their work which unfortunately have not survived. It is therefore no surprise to find among Guidobaldo's notes some that appear to be related to Galileo's contemporary interests as they are represented by his early works *La Bilancetta* and his treatise *De Motu*. Therefore, whereas in general the young Galileo learned from his patron Guidobaldo del Monte, it may well be that, in this case, Galileo challenged his older colleague with subjects that did not belong to his familiar areas of competence. It may have been precisely because the study of motion was not among Guidobaldo del Monte's main concerns that he did, for all we know, not pursue the line of research suggested by the unexpected outcome of the experiment which would have led him immediately to discover the law of fall implicit in the result of this experiment. To take this last step remained, however, the privilege of Galileo, as far as we know.

Galileo in the Footsteps of Guidobaldo del Monte

Galileo's approach to the knowledge of his time followed along a path that brought him into closer contact with academic traditions than it was apparently the case for Guidobaldo del Monte. His intellectual development involved in fact two strands, a more technical one and a more philosophical one. Galileo's family background and education placed him among the engineer-scientists, while his early academic career introduced him into the scholastic philosophy of his time. In the following, we will see that it was primarily the contact with Guidobaldo del Monte which, in a decisive moment of Galileo's intellectual development, encouraged him to take up the life-perspective of the risky but rewarding career of an engineer-scientist.

Galileo's father Vincenzio was a musician and theoretician of music from which the young Galileo could learn much not only about the wide and curious field of acoustic phenomena produced by instruments but also about the possibility of applying mathematics to such phenomena. Thus it became an ongoing theme of interest to him, to which he dedicated a number of ingenious observations which are like gems interspersed with the wealth of his writings on diverse subjects, ranging from comets to mechanics. The experimental acuity of such observations may be illustrated by an episode of the *Discorsi* in which Galileo shows how to transform an artisanal operation, the scraping of a brass plate with a sharp iron chisel in order to remove spots from it, into an operation performed for the purpose of generating knowledge about the frequencies of sounds. In other observations of this type, he studied the dependence of the height of a tone from the size, material, and tension of a string, varying the latter by attaching different weights to it. Galileo also related the vibrations of the strings of an instrument to the swinging of a bell and to oscillations of a pendulum and derives from this comparison an explanation for consonance phenomena:

> The cord struck begins and continues its vibrations during the whole time that its sound is heard; these vibrations make the air near it vibrate and shake; the tremors and waves extend through a wide space and strike on all the strings of the same instrument as well as on those of any others nearby. A string tuned in unison with the one struck, being disposed to make its vibrations in the same times, commences at the first impulse to be moved a little; (...) it finally receives the same tremor as that originally struck, and its vibrations are seen to go widening until they are as spacious as those of the mover. This wave action that expands through the air moves and sets in vibration not only other strings, but any other body disposed to tremble and vibrate in the same time as the vibrating string. If you attach to the base of the instrument various bits of bristle or other flexible material, it will be seen that when the harpsichord is played, this little body or that one trembles according as that string shall be struck whose vibrations are made in time

> with it. The others are not moved at the sound of the string, nor does the one
> in question tremble to the sound of a different string. (Galilei 1974, 99f)

Such observations and interpretations go probably back to experiments which
Galileo's father may have performed together with him around 1588-89.[1] At least,
when Galileo visited Guidobaldo del Monte in 1592 on his way to Padua, he must
have been already so familiar with this explanation that it was made a topic in
their discussions. In fact, as has been mentioned already, immediately before the
protocol of the projectile trajectory experiment Guidobaldo del Monte made a
note on experiments with different strings put in defined tensions by attaching
weights to them. He describes the dependence of the height of a tone on the size,
material, and tension of the strings, emphasizing the relation between the tone of
a string and a characteristic motion ascribed to it. From this relation, an explana-
tion of consonance is developed that is essentially identical with that given by
Galileo in the *Discorsi*:

> From this one can also give the reason by which cause, if two instruments
> are close to each other which have many strings and if a straw is placed on
> the strings of one of them and if on the other one a string is touched, one
> then hears that that string of the other instrument which will be in unison
> with the one that is touched also sounds, and the others do not sound. And
> this could be produced by that [reason] that the air of the string that is struck
> because of its agitation moves all the other strings, but because those that are
> not in unison cannot receive the same motion of that which is struck, while
> that which is in unison can receive it, only this one sounds, and the others do
> not sound.[2]

The similarity between these passages confirms once more that some of the
entries in Guidobaldo del Monte's notebook must have indeed been written under
the influence of his discussions with Galileo. The dating to the year 1592 of this
entry furthermore shows that it must have been indeed Galileo's family back-
ground that had brought him first into contact with the ways in which new knowl-
edge was acquired on the basis of practical experience by the engineer-scientists.[3]

It seems, however, that Galileo's father was not only familiar with the fascina-
tion of searching for a theoretical formulation of practical knowledge but was
also aware of the precarious social status of those who gave in to this fascination
and made it the preoccupation of their professional careers. He decided, in any
case, to save the young Galileo from the uncertain fate of an engineer-scientist
and to rather secure him a more ordinary career by pressing him to enter a field
with a low level of certain knowledge but a high level of guaranteed income,
medicine.

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[1]   See Drake 1987, 17 and Settle 1996.
[2]   del Monte ca. 1587-1592, 235, see Gamba and Montebelli 1988, 182 for a transcription.
[3]   The role of Galileo's family background has been emphasized in particular in Settle 1996.

But even though Galileo initially ceded to his father's wish and began the study of medicine, he did not give up his interests and pursued, under the guidance of the engineer-scientist Ostilio Ricci, studies of mathematics and mechanics, comprising the works of Euclid and Archimedes.[1] Not long after these initial studies, Galileo's striking mastery of Euclidean geometry and Archimedian proof techniques testify to the success of Ricci's teaching. Both the fascination by and the competence in these matters eventually became so strong that the young Galileo even succeeded in convincing his father that mathematics and mechanics had to become his professional occupation. His first independent writing, the short treatise *La Bilancetta probably* written around 1586, accordingly dealt with the construction of an instrument based on Archimedian theory but designed for a practical purpose, that of determining specific gravities. Later he worked out highly specialized theoretical problems in the Archimedian tradition, such as the problem of the center of gravity of parabolas.

In the course of his university studies Galileo also encountered another tradition of antique knowledge, the Aristotelian philosophy dominating the intellectual world of his time. Whether he took up its intellectual challenge or simply because he wanted to increase his chances for gaining an income by teaching, he began to thoroughly familiarize himself with this philosophy, and in particular with Aristotelian physics. At that time he started to compose his treatise on motion which has already been mentioned above. It was originally written in dialogue form but later changed into a more systematic elaboration, treating the fundamental assertions of the Aristotelian theory of motion. Using Archimedian concepts, such as extrusion, in order to analyze traditional Aristotelian problems, such as motion in a medium, Galileo succeeded in giving his treatise an anti-Aristotelian twist that made it possible for him to pose as a natural philosopher developing a theoretical foundation of his own.

Galileo's technical elaborations of Archimedian problems brought him into contact with some of the leading mathematicians and engineer-scientists of his time, such as Clavius. Clearly, by far the most consequential contact was that to Guidobaldo del Monte. When Galileo began an exchange with him at the beginning of 1588, he happened to work on topics closely related to Guidobaldo del Monte's main occupation at that time. While initially Galileo and Guidobaldo just exchanged letters concerning the technicalities of a proof developed by Galileo, they soon came into close scientific cooperation. From the few surviving letters we can in fact conclude that they not only must have kept each other informed about their scientific interests by, at least over some periods, an almost day-by-day correspondence but that they also regularly exchanged their works for mutual criticism.

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[1]   For the contents of Ricci's teachings, see Settle 1971.

From the letters between Galileo and Guidobaldo del Monte we know, for instance, that the latter sent Galileo a copy of his commentary on Archimedes as soon as it was printed, asking for Galileo's criticism:

> I believe that in your modesty you say that you like my book which I sent you, but I pray you as much as I can, please warn me if there is anything with it, because I still have all the books at hand and it would be an easy thing to correct it where necessary. I would be very grateful if you would do me this favor. (Guidobaldo del Monte to Galileo, May 28, 1588, Galilei 1890-1909, X: 33)

It is therefore also plausible that an entry found in Guidobaldo del Monte's notebook containing a treatment of the problem of the inclined plane which contradicts the way he treated it in his *Mechanics* derives from an exchange between Guidobaldo and Galileo. In his entry, Guidobaldo treats the inclined plane in fact no longer following Pappus as it was done in his *Mechanics* and in earlier parts of the notebook, but rather in precisely the way found in Galileo's contemporary treatise *De Motu*, where it is derived from the bent lever. In his *De Motu*, Galileo actually criticizes the treatment of Pappus, thus implicitly criticizing also his patron Guidobaldo. In view of the close relationship to Guidobaldo precisely in these years, it is indeed unlikely that he should have hidden this criticism from his older colleague.

In spite of this intellectual closeness, there was, however, also a remarkable difference in their interests. It is, in particular, hardly imaginable that Guidobaldo with his emphasis on rigorous Archimedian proofs, on the one hand, and on practical applicability of mechanical knowledge, on the other, had much sympathy for the subtle problems of natural philosophy addressed by Galileo in his treatise *De Motu*. There are, in fact, a few hints pointing at this diversity of intellectual orientations. Guidobaldo was familiar with Benedetti's work which includes a theory of motion very similar to that of Galileo, that is, a theory also involving the Archimedian extrusion principle. Benedetti's major book, *Diversarum Speculationum*, is mentioned in Guidobaldo's notebook where its mechanical foundations are heavily criticized. In the notebook one also finds a short passage where a theory of motion in media is sketched which is similar to both Benedetti's and Galileo's approach in that it also makes use of extrusion but which diverges in the precise formulation of the basic laws of motion. This passage may well be a reaction by Guidobaldo to Benedetti's or Galileo's speculations but apparently remained without any consequence for his work.

This reluctance to accept theoretical considerations concentrated on problems of Aristotelian natural philosophy may also explain a short remark in a letter by Guidobaldo written at the time when it is generally assumed that Galileo had completed at least a first version of his *De Motu*. The remark expresses Guidobaldo's satisfaction with Galileo's return to problems of the center of grav-

ity, the initial point of the common interest between them and the area in which he especially appreciated Galileo's competence:

> Because I did not have any letters from you for many days, your [letter] pleased me greatly (...)
>
> Moreover, I was very pleased to see that you have returned to the center of gravity; and you have done enough, having found what you wrote to me [et ha fatto assai haver trovato quanto mi ha scritto]; and I also have found some things but I cannot conclude my search for a certain tangent which drives me to despair, because it seems to me that I have found it by following a certain path, but I cannot demonstrate it and clarify it in my own mind with the demonstration: but your letter consoled me greatly, because I see that you search and do not conclude your search so quickly, whereas I am [usually] not surprised when I do not find [something]. But do not be surprised if I still do not send you what I promised to show you, due to the fact that I need to copy a lot of things; but as soon as I can, I will send them to you, because what I really appreciate above all else is having your opinion. (Guidobaldo del Monte to Galileo, December 8, 1590, Galilei 1890-1909, X: 45)

Considering the dramatic changes in Galileo's scientific activities after his move to Padua, the initial difference between the scientific interests of Guidobaldo del Monte and Galileo become even more evident. We do not know when Galileo first met Guidobaldo del Monte personally, but there is no evidence that this happened before he visited him on his way to Padua in 1592. It is true that Galileo already at that time was a multi-talented intellectual with a broad spectrum of interests and considerable competence also in the field of technology. However, compared with the scope of activities of an engineer-scientist like Guidobaldo del Monte as a supervisor of fortifications involved in large-scale projects, and running his own workshop which offered facilities for experimentation and production of instruments, Galileo's activities in such areas must have looked very modest. This, at least, would explain why Galileo in the following year drastically changed his fields of interests and in many respects copied the types of activities which were characteristic of engineer-scientists in general at that time, and in particular, the activities of Guidobaldo del Monte of running his own workshop, of inventing and producing instruments, and of reflecting and taking notes on a specific set of topics such as mechanics, military technology and architecture, practical geometry, and surveying. He thus returned to those activities which must have impressed him as a young man when he took private lessons from Ostilio Ricci.

In fact, one of the first works which Galileo produced after this "practical turn" of 1592 was his treatise on mechanics, very much in the style of Guidobaldo's book[1] in its combination of rigor and concentration on the simple machines. Galileo's mechanics was circulated only in manuscript, also because it was used for teaching purposes.[2] This was also the case for his introductions into military architecture and fortification dating from approximately the same time, which also have survived in manuscript copies. In 1596 he composed a treatise on measuring heights and distances by sighting and triangulation, probably also used for teaching purposes. Galileo's private disciples, among them young noblemen coming from various European countries, were in fact an important source of income for him, in addition to the modest salary he received from the university, and perhaps a remuneration he received for the horoscopes he prepared. His first real publication appeared only in 1606 and was dedicated to a military compass he had designed about 1597, following the example of Guidobaldo del Monte. This compass, as well as other instruments designed by Galileo, such as an instrument for gunners developed in 1595 or 1596 also following the example of Guidobaldo,[3] were produced by a Paduan artisan and eventually in a workshop of his own. The treatise on the military compass had also at first been used for private lessons and was finally published only because Galileo intended to protect his invention. His technological concerns are, in fact, perhaps more characteristic for the beginning of his Paduan period than his writings. As early as 1593, he received a Venetian privilege for a machine to raise water and was consulted by a Venetian official on matters of naval architecture. Around that time Galileo must have begun to frequent the recently expanded arsenal of Venice which made a lasting impression on him, as is well known from the opening to the *Discorsi*.
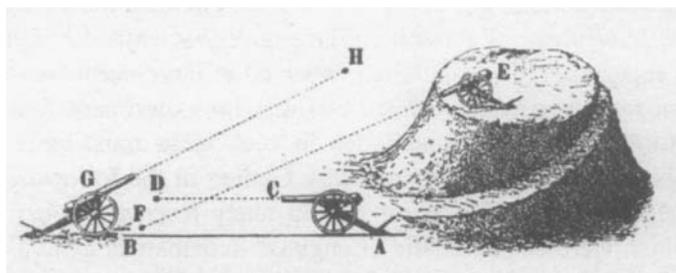

Figure 9.   Artillery shots in Galileo's treatise on fortification[4]

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[1]   Historians of science usually try to point out the differences of Galileo's work on mechanics to all the numerous treatments of this subject by other authors, often following the *Mechanical Questions* of Pseudo-Aristotle. Even if one admits the alleged superiority of Galileo's treatment of the subject, it has to be emphasized that Galileo's work with regard to its canonical contents and the intention to improve the program of Pseudo-Aristotle's *Mechanical Questions* of explaining all mechanical devices by reducing them to the lever perfectly fits into this tradition.

[2]   For this and the following, see Drake 1987, chap. III, 33- 49. See also Wohlwill 1993, 1: 141.

[3]   See Schneider 1970.

[4]   Figure reproduced from Galilei 1890-1909, II: 93

As a result of this practical turn, Galileo handled in his early Paduan period topics in a way quite different from how he dealt with them in his unpublished *De Motu* manuscript. Not the structure of the object but the purpose of the knowledge to be gained about it determined predominantly how an object was treated. A typical example is the treatment of different types of shots in his treatise on fortification. In contrast to the way he dealt with problems of artillery in the *De Motu* manuscript, here the trajectories are presented as if they were straight lines (see figure 9), because in the context of teaching the nomenclature of artillery any further differentiation would contribute nothing to the purpose of making his disciples learn the military vocabulary.
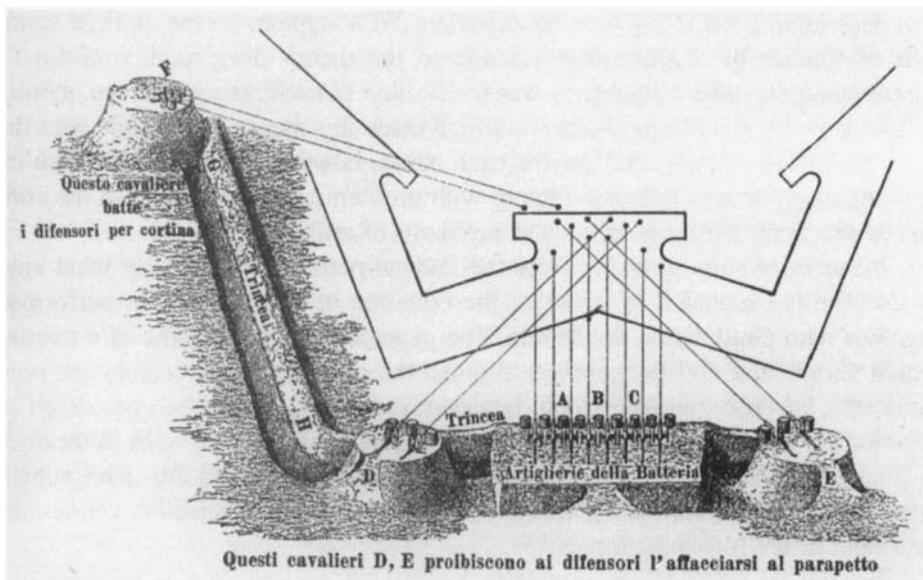

Figure 10. Artillery shots in Galileo's treatise on military architecture [1]

Also in his treatise on military architecture, Galileo depicted trajectories simply as straight lines (see figure 10), in this case, however, for a different reason. By the time of Galileo, the constructions of military architecture had achieved considerable improvements with regard to the resistance of this architecture against destruction by artillery. These improvements were based on a sophisticated application of geometry to the shapes of fortification buildings. Complex shapes of such buildings prevented the possible aggressor in the case of a military attack to bring his weapons into a position favorable for the intended destruction of these buildings. On the other hand, these improvements required also enhanced geometrical knowledge on the side of the aggressor in an attack. In the case of the example from Galileo's treatise on military architecture given here, Galileo argued for a suitable design and placement of platforms for the aggressor so that

[1]   Figure reproduced from Galilei 1890-1909, II: 51

the defender is unable to use the geometry of the fortification building for an effective defense. In this case, the abstract representation of possible directions of the artillery shots by straight lines is even more adequate to the problem than a more realistic representation including the shapes of possible trajectories.

On the background of such a practical turn, it therefore comes as no surprise that Galileo was similar to Guidobaldo del Monte also in that one respect which is our central concern here: For a long time to come Galileo did not show any theoretical interest, just as it was the case for Guidobaldo del Monte, in the puzzling outcome of the projectile trajectory experiment and its implications, that is, the symmetry of the trajectory, the simultaneous effects of a violent and a natural component of the motion in every point of the trajectory, and the quadratic function determining the downward acceleration. What appears to the modern historian of science as a blunt contradiction to the theory developed in Galileo's unpublished *De Motu* manuscript was for Galileo himself, as we will see, merely an incentive for a slight modification which made this theory compatible with the outcome of the experiment. For the time being, however, he left his treatise on motion incomplete in favor of dealing with problems of motion only in the context of practical applications, such as problems of artillery.

A manuscript page probably from the Paduan period shows clearly what kind of use Galileo intended to make of the outcome of the experiment performed together with Guidobaldo del Monte. The page contains the outline of a treatise which shows that Galileo planned to make the curve of the trajectory the core topic of a specific treatise, which, however, can by no means be considered as substituting his discarded *De Motu*, but was rather designed as a work in the style of the treatises on artillery of the time. Galileo's treatise would thus have substituted Tartaglia's influential *Nova Scientia*. The treatise was planned to contain the treatment of the following topics:[1]

> Particular privileges of the artillery with respect to the other mechanical instruments.
> Of its force and from where it proceeds.
> If one operates with a greater force in a certain distance or from nearby.
> If the ball goes along a straight line if it is not [projected] along the vertical.
> Which line the ball describes in its [course].
> On the course and the time of charging the canon
> Impediments which render the canon defective and the shot uncertain
> On mounting [the canon] and dismounting it
> On the production of the caliber
> On the examination of the quality and the precision of the canon

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[1]   Galilei ca. 1602-1637, folio page 193r. In the dating we follow Drake, although no direct evidence for this dating is available. This dating is, however, strongly suggested by the text. The dominance of practical interests in the intended publication fits perfectly into the work of Galileo in Padua after his practical turn. The practical interests which the outline represents were pursued by him up to the composition of the *Discorsi*, but never appeared so pure again as here.

> If, when the canon is longer, it shoots farther and why
> In which elevation you shoot farthest and why
> That the ball in turning downwards in the vertical returns with the same
> forces and velocities as those with which it went up
> Various balls [specially] prepared and lanterns and their use

This table of contents of Galileo's planned and never written treatise clearly shows that he was well aware of the practical importance of the outcome of the experiment recorded in Guidobaldo del Monte's notebook. In particular, he refers to the symmetry of the trajectory, its continuously curved shape, and its dynamical composition exclusively in terms of improving the precision of artillery. Possibly, however, Galileo's planned treatise was intended only for purposes of private teaching. Such a usage of the knowledge acquired by the experiment on projectile motion certainly fits well with Galileo's efforts – extended over a period of 40 years – not to make this knowledge publicly available.

In summary, after his move to Padua, Galileo not only came in close contact but shared all essential characteristics with the engineer-scientists of his time. This development indicates a radical reorientation of his life. Galileo had been earlier engaged primarily in Aristotelian physics, which was not only the official dogma of the church but also the main basis for interpreting nature at that time. He was also familiar with and even fascinated by other ancient traditions, in particular the mathematical methods represented by Euclid's Elements and even more by the application of these methods by Archimedes.

Through Guidobaldo del Monte Galileo came into close contact with the experimental techniques and the research interests of a leading engineer-scientist. The theoretical basis of these techniques was not so much Aristotelian physics but rather the ancient tradition of mechanics as it was represented by the Pseudo-Aristotelian *Mechanical Questions* and numerous contemporary treatises in this tradition. As a consequence of this reorientation, Galileo's move to Padua not only marks an advance in his academic career but also a practical turn, that seemed to leave the theoretical interests of his Pisan years far behind. The discoveries of the parabolic shape of the trajectory and of the law of fall in 1592 therefore did simply not come at the right moment in his life for having any dramatic consequences on his theory of motion.

Galileo and Paolo Sarpi—Towards a New Science of Motion

The fact that Galileo changed his activities so drastically towards contemporary technology and its seemingly adequate theoretical foundation in mechanics does not imply, however, that his previous activities were forgotten without any trace. On the contrary, life in Padua had a dimension which was quite different from what his patron Guidobaldo del Monte represented to him. Both in the house of

Giovanni Vincenzo Pinelli and at the university, Galileo encountered numerous intellectuals who were engaged in the great debates of the time, in theology, in Aristotelian philosophy, in astronomy, as well as in other fields. In these encounters Galileo had occasion to continue to pursue and even to develop the interests he had cultivated in the academic environment of Pisa.

Nobody within this group seems to have had a greater affinity with Galileo's own interests than Paolo Sarpi whom he met, for all we know, in 1592 in the house of Pinelli. A comparison of how discussions with Galileo were reflected in the notebooks of Guidobaldo and Sarpi shows that surely Paolo Sarpi appreciated the competence of the young Galileo for quite different reasons than Guidobaldo del Monte. In addition to the note on the result of the projectile trajectory experiment in the notebook of Sarpi, there are a number of topics which indubitably reflect discussions with Galileo Galilei, such as the latter's tidal theory summarized in notes that can be dated to 1595.

The affinity between Sarpi and Galileo and its distinction from the intellectual interests that had joined Guidobaldo and Galileo become clear from the range of topics covered by Sarpi's notes as well as from the theoretical focus in the treatment of these topics. An outstanding example is provided by their joint fascination with a theory of the type of Benedetti's theory of motion in media, a fascination that was, it seems, not shared by Guidobaldo. It is well known that Galileo's *De Motu* shows many similarities with Benedetti's writings published between 1583 and 1585. But even without these similarities, it is hardly believable that Galileo could have been unaware of these writings when he was composing his treatise, although Benedetti's name is not mentioned. It does appear, however, in the notebook of Guidobaldo del Monte, who heavily criticized Benedetti's mechanics on the very same pages which also contain his note on Galileo's derivation of the law of the inclined plane from the law of the bent lever. It is therefore plausible to assume that also the theories of motion advanced by Benedetti and Galileo were a subject of discussion between Guidobaldo and Galileo. In fact, the motion of bodies in media is treated in Guidobaldo del Monte's notebook, as we have mentioned. In his note on that subject Guidobaldo attempted, however, to formulate an alternative approach which remains closer to Aristotelian dynamics than that of Benedetti and Galileo.

This topic remained, in any case, only a passing interest of Guidobaldo's, whereas it was central to Sarpi's thinking on motion. In fact, the problem of natural motion and its explanation in contrast to Aristotelian physics was, as Sarpi's notes show, of central concern to him. In the 1580s, that is, long before he first met Galileo, he had intensively studied Benedetti's theory of motion in media, attempted to further develop it, performed experiments related to it, and dedicated extensive notes to this theory which show him as an ardent adept of Benedetti. In view of the close similarity between Benedetti's theory and Galileo's *De Motu*, it therefore comes as no surprise that Sarpi and Galileo must have immediately after their first encounter entered intense discussions on problems of motion.

Their common interests were, in fact, not limited to the laws of motion in media, but also comprised the problem of the motion of the earth, the relation between violent and natural motion, the explanation of violent motion by an impressed force, the relation between the effects of motion and of weight, the explanation of accelerated motion, and last but not least the shape of a projectile trajectory.[1] In spite of the many differences in detail between Sarpi's notes and Galileo's ideas as they are known from *De Motu*, their writings represent strikingly many common interests and questions, although they had not been acquainted with each other at the time these writings were produced. In particular, they both had views on projectile motion which suggested that, in general, the projectile trajectory cannot be symmetrical. In the tradition of Tartaglia's influential *Nova Scienza* Paolo Sarpi reconstructs, for instance, as Galileo did in his *De Motu*, projectile motion as resulting from the interplay of natural and impressed violent motion:

> The distinction between the violent and the natural is deduced from the principle that it is either outside or inside, the natural being the force that is inside and has formed the body and has provided it with its means [of motion]. The violent one which has been introduced by an external force soon diminishes, because it has no means to continue its action and finds a contrary and more powerful force from the inside so that the first [the natural] has the subject disposed in its way and fights and expulses the second [violent] one; the second is more powerful in the beginning and dominates but then it is forced to recede and loses. (Note number 100 in Sarpi 1996, 123f)

This reconstruction of projectile motion, according to the few dates which Sarpi inserted between the notes written at some time between 1578 and 1584, is in sharp contrast to the note corresponding to Guidobaldo del Monte's protocol on the projectile trajectory experiment, which Sarpi wrote in 1592 and, according to our interpretation, after the first encounter with Galileo. But Sarpi's note on the outcome of this experiment does not simply contradict his earlier reflection on that matter. At the same time, it shows how the puzzling symmetry of the trajectory can be made compatible with the conception of the interaction of violent and natural motion which was followed by both Sarpi and Galileo: In order to reconstruct the symmetrical trajectory it has to be merely assumed that the relation between violent and natural motion in ascending and descending is exactly the same, but with exchanged roles. This consequence has, indeed, explicitly been drawn in both Guidobaldo del Monte's protocol and Paolo Sarpi's note, and it is surely also the way Galileo brought the puzzling symmetry of the projectile tra-

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[1]   The editors of Sarpi's notebook conclude from these similarities that many of the ideas of Galileo from that time are taken from Paolo Sarpi; see Sarpi 1996, 400. This conclusion is unconvincing because it neither takes into account the common sources used by both of them, nor does it take into consideration the diverging developments of Galileo's and Sarpi's thought during the time of their close interaction.

jectory in accordance with his theory of motion as it was developed in his treatise *De Motu*. Thus, the discovery must have been conceived by all of them at the beginning as being much less dramatic than it must have looked from a later perspective. The reason for Galileo's negligence of the consequences of the discovery of the parabolic shape of the projectile trajectory was, therefore, not only the shift of his interests towards applicable technological knowledge which was associated with his move from Pisa to Padua but also the simplicity with which the new insight could be made compatible with his former beliefs.

That Sarpi's notes after 1592 indeed also reflect, albeit in an indirect way, discussions about views held by Galileo becomes clear from the context of Paolo Sarpi's entry on the symmetry of the projectile trajectory (comprising the notes with the numbers 537 and 538) among his other entries. He must have had extensive discussions on Galileo's theory developed in *De Motu* which are reflected in a series of entries immediately surrounding those on the projectile trajectory. These are essentially the notes from number 532 which is the first note on motion for the year 1592, to note number 542, which according to Sarpi's dates was written still in the same year. Furthermore a number of later notes show that Paolo Sarpi and Galileo kept in contact still discussing problems which are essentially based on Galileo's theory in *De Motu*.

This is not the place to discuss these reflections on Galileo's theory in detail. Nevertheless, a short overview has to be given in order to gain indications on Galileo's thoughts in this intermediate period of latent development of his natural philosophy, on which otherwise the historical documentation is so scarce. The notes start with several arguments focussing on the impression of force into a projectile and the force of percussion which a projected body exerts. The last entry preceding the notes 537 and 538, which deal with the projectile trajectory, essentially represents a summary of Galileo's explanation of acceleration in *De Motu*. The entries immediately following these notes concern the refutation of a physical counter-argument against Copernicanism involving the dissipation of objects from the rotating earth, an argument that is well known from Galileo's later *Dialogo*.[1] The subsequent two notes of the year 1592 are about bodies and their weights and motions in media (notes 541 and 542), the central topic of *De Motu*.

A note somewhat separated from the bulk of the "Galilean" notes described so far, but still dating from 1592, deals with the motion of a pendulum and the motion of a spinning top as examples of ongoing motions (note 547), a topic also discussed in Galileo's *De Motu* (Galilei 1890-1909 I: 335). The first entry made in 1593 (note 558) concerns the refutation of Aristotle's proportional relation between the weight and the speed of a falling body, again an argument exten-

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[1]   One of the oldest indications of Galileo's adherence to Copernicanism, overlooked by the editor. In a later year, 1595, there are entries on Galileo's tidal theory; notes 569, 570 and 571 in Sarpi 1996, 424-427. Only concerning these latter entries the editor discusses their possible relation to Galileo's Copernicanism. See also Drake 1987, 37.

sively discussed in *De Motu*. When Galileo visited Venice once more in 1595, he apparently returned to his intensive discussions with Paolo Sarpi. Sarpi's notes of 1595 in fact contain a discussion of Galileo's tidal theory based on his Copernican views (notes 569-571).

This short overview of the notes which probably reflect discussions of Paolo Sarpi with Galileo shows that the results of the experiment on the projectile trajectory was not *the* central issue that bothered them really. The emendation of the Aristotelian explanation of the trajectory by the interplay of violent and natural motion required for explaining its symmetry had seemingly settled any dispute on this matter. On the other hand, Sarpi's notes nevertheless show that serious difficulties remained, and it even seems that Paolo Sarpi himself was somewhat hesitant to fully accept the symmetry of the trajectory. Note number 535 at least contains what was probably his strongest argument against the explanation of the symmetry of the trajectory by assuming a corresponding symmetry of violent and natural motion in the ascending and descending part of the trajectory and nothing indicates that Galileo had a sufficient answer to the problem. Paolo Sarpi compares in this note the force impressed into the projected body (for the sake of a drastic demonstration, his argument involves the arrow of an arquebus[1] shot vertically into the air) with the force which it is able to expend when it comes down, coming to the conclusion that these forces cannot be equal. It even cannot be excluded that he checked this conclusion by an experiment. In any case, the outcome of such an experiment would have been clear. Sarpi wrote:

> Reason would have it that when a heavy body has all the force to go up that it can receive, it weighs as much in going down as in going up in the same places distant from the starting points. But it is clear that an arquebus strikes through a table by way of the bullet that passes it, whereas, who would charge it even more, so that [the bullet] would go up much higher, [the bullet] in coming down would nevertheless hardly leave any mark on the table. (Note number 535 in Sarpi 1996, 391-393)

Even more than a decade later Paolo Sarpi confronted Galileo with this counterargument against the symmetry of upward and downward motion. They were still in touch around that time, had meanwhile exchanged their views on many other subjects, such as magnetism and its treatment by Gilbert,[2] but had apparently left their discussions of motion essentially at the point they had reached in the 1590s. In 1604 Paolo Sarpi returned to the subject, bringing up in a letter he wrote to Galileo once again questions on which he had made notes at the time when they first met, twelve years before.[3] On this occasion, he reminded Galileo of what they had agreed upon and on what the problems were that their discussions had left open. In his letter Sarpi thus confirms that Galileo was actually the source of

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[1] An arquebus is an early type of a portable gun supported on a tripod or on a forked rest.
[2] See Paolo Sarpi to Galileo, September 2, 1602, Galilei 1890-1909, X: 91f.
[3] Paolo Sarpi to Galileo, October 9, 1604, Galilei 1890-1909, X: 114.

some of the notes he made in 1592 by unambiguously ascribing the views recorded in these notes to him. But the letter also shows that Sarpi himself continued to be puzzled by questions earlier recorded in these notes, in spite of his discussions with Galileo at the time. These questions concern, firstly, the quantities of impressed force which bodies of different kinds can receive and, secondly, the very counter-argument that Sarpi had earlier raised against Galileo's claim of the symmetry of the trajectory of projectile motion:

> We have already concluded that a body cannot be thrown up to the same point [termine] if not by a force, and, accordingly, by a velocity. We have recapitulated – so Your Lordship lately argued and originally found out [inventò ella] – that [the body] will return downwards through the same points through which it went up. There was, I do not remember precisely [non so che], an objection concerning the ball of the arquebus; in this case, the presence of the fire troubles the strength of the argument. Yet, we say: a strong arm which shoots an arrow with a Turkish bow completely pierces through a table; and when the arrow descends from that height to which the arm with the bow can take it, it will pierce [the table] only slightly. I think that the argument is maybe slight, but I do not know what to say about it. (Paolo Sarpi to Galileo, October 9, 1604, Galilei 1890-1909, X: 114)

Galileo responded to this question in his famous letter written a week later, on October 16, 1604:

> Concerning the experiment with the arrow, I believe that it does acquire during its fall a force that is equal to that with which it was thrown up, as we will discuss together with other examples orally, since I have to be there in any case before All Saints. Meanwhile, I ask you to think a little bit about the above mentioned principle. (Galileo to Paolo Sarpi, October 16, 1604, Galilei 1890-1909, X: 116)

Just as Sarpi had done in his letter, so also Galileo alluded to what both men had discussed before, in particular the law of fall and the symmetry of projectile motion. However, as is well known (and is also evident from Galileo's brief rebuttal in the above passage), the emphasis of this letter is not on the experimental aspect of the problem raised by Sarpi,[1] but a problem of a quite different kind, condensed in the invitation to think about a new "principle."

> Thinking again about the matters of motion, in which, to demonstrate the phenomena [accidenti] observed by me, I lacked a completely indubitable

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[1]   The counter-argument of Sarpi was a serious problem for Galileo which he probably solved only late and eventually mentioned in the *Discorsi*, see Galilei 1974, 95f and 227-229 . That he solved this problem only late is indicated by a manuscript note in the hand of his son Vincenzio (born in 1606), where this problem is still mentioned as an open one, see Galilei 1890-1909, VIII: 446.

principle which I could pose as an axiom, I am reduced to a proposition which has much of the natural and the evident: and with this assumed, I then demonstrate the rest; i.e., that the spaces passed by natural motion are in double proportion to the times, and consequently the spaces passed in equal times are as the odd numbers from one, and the other things. And the principle is this: that the natural moveable goes increasing in velocity with that proportion with which it departs from the beginning of its motion; (...)

Without doubt, Galileo's letter to Sarpi documents a fundamental change in his thinking on motion. Whereas Galileo had earlier used his knowledge about the parabolic shape of the trajectory only in the context of practical mechanics, he now evidently aimed at constructing a deductive theory of motion based on a principle which seemingly follows directly from his experiences with the force of percussion and is apparently confirmed by the symmetry between vertical projection and free fall.[1] This principle postulates the increase of the degrees of velocity in proportion to the distance traversed. What is really new in his letter to Sarpi (and the reason why historians of science have paid the utmost attention to it) is the fact that Galileo at that time obviously had discovered a set of properties of accelerated motion, the most prominent one of them being the law of fall, which he now tried to arrange into a deductive system. Unfortunately, the letter is not very explicit in the enumeration of these discoveries. In his response to Sarpi he furthermore mentioned a proof for the law of fall but did not even give this proof but mainly served himself of the new principle in order to argue for the symmetry between vertical projection and free fall, that is, in order to treat precisely the issue that was controversial between him and Sarpi.

Is there any information about the developments that led Galileo from his occupation with practical mechanics to this challenging new theoretical program? Indeed, there has been preserved a letter to his patron Guidobaldo del Monte written two years earlier which makes perfect sense when interpreted in the context of a reorientation from practical mechanics to the goal of creating a new deductive theory of motion.

Galileo's letter to Guidobaldo del Monte from the 29th of November, 1602, is only one of a series of letters exchanged between Galileo and Guidobaldo del Monte at that time which are all lost with the exception of this one. From its content it can be concluded that Galileo must have informed Guidobaldo del Monte about a set of new discoveries, among them the isochronism of the pendulum and the isochronisms of motion on inclined planes which can be represented as chords of a circle. He furthermore had informed Guidobaldo del Monte about experiments to verify the discoveries.

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[1] See Galilei ca. 1602-1637, folio 128 for Galileo's mention of percussion; for the argument concerning the symmetry between vertical projection and free fall, see the discussion in Damerow, Freudenthal, McLaughlin, and Renn 1992, 169f.

Galileo's earlier and now lost letter must have been interpreted by Guidobaldo del Monte as a deviation from what he thought to be standards for working on mechanics. Guidobaldo del Monte had meanwhile checked with his methods Galileo's discoveries and must have come to the result that they cannot be maintained. The letter by Galileo which has been preserved is a reaction to Guidobaldo del Monte's critique. It is friendly and humble in its tone, as was customary between them, but shows clearly that Galileo had, in Guidobaldo's eyes, departed from their common ground in mechanics as Galileo had learned it from his patron.

In particular, Guidobaldo del Monte had probably taken up one proposal of Galileo, that is, to check the isochronism of the pendulum by what supposedly was an identical arrangement, a ball rolling within a semi-sphere. The result, however, turned out to be negative; Guidobaldo del Monte tried directly to compare the times of two different balls descending from different heights to the bottom of a spherical "box" and did not find that these times of descent were the same. He possibly tried the same experiment also with inclined planes in a spherical container with an equally negative result. Guidobaldo del Monte must have informed Galileo about his negative result and must have added a critique stating that Galileo violated the principles of mechanics with the result that his discoveries were obviously absurd.

Galileo's answer, which fortunately has been preserved, starts with a humble, but insistent reiteration of his claims:

> Your Lordship, please excuse my importunity if I persist in wanting to persuade you of the truth of the proposition that motions within the same quarter-circle are made in equal times, because having always seemed to me to be admirable, it seems to me [to be] all the more so, now that your Most Illustrious Lordship considers it to be impossible. Hence I would consider it a great error and a lack on my part if I should allow it to be rejected by your speculation as being false, for it does not deserve this mark, and neither [does it deserve] being banished from your Lordship's understanding who, better than anybody else, will quickly be able to retract it [the proposition] from the exile of our minds. And because the experiment, through which this truth principally became clear to me, is so much more certain, as it was explicated by me in a confused way in my other [letter], I will repeat it here more clearly, so that you, by performing it, would also be able to ascertain this truth. (Galileo to Guidobaldo del Monte, November 29, 1602, Galilei 1890-1909, X: 97-100)

Then follows a detailed technical description of an experimental setting for comparing the oscillations of two equal pendulums, each consisting of a string of "two or three braccia"[1] length and a ball made of lead, together with a report of

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[1]   The braccia measured in Padua at that time about 0.6 m, see Zupko 1981, 47.

Galileo's own results. This report is remarkable because, as often when Galileo speaks about his experiments, he obviously exaggerated the outcome; it seems technically impossible that he might have reproduced with two different pendulums 1000 oscillations in precisely the same time.[1] In any case, Galileo's claim suggests that he used quite heavy lead balls which, with all likelihood, where the ones he had ordered and received after July 1599 from a foundry.[2]

Next, Galileo criticizes Guidobaldo del Monte's own attempts to check the isochronism by means of a ball descending along a spherical path, attributing the negative results as being due to insufficient precision of the time measurement as well as to the deviations of the path and the smoothness of the surface from ideal conditions.

The second half of the letter deals with Guidobaldo del Monte's theoretical arguments against Galileo's discoveries. It starts with a critique of arguments against the two theorems of Galileo concerning the isochronism of descent along spherical paths and paths along inclined planes which Guidobaldo obviously must have based on plausibility instead of on a mathematical proof:

> With regard now to the unreasonable opinion that, given a quadrant 100 miles in length, two equal mobiles might pass along it, one the whole length, and the other only a span, in equal times, I say it is true that there is something wondrous about it; but [less so] if we consider that a plane can be at a very slight incline, like that of the surface of a slow-moving river, so that a mobile will not have traversed naturally on it more than a span in the time that another [mobile] will have moved one hundred miles over a steeply inclined plane (namely being equipped with a very great received impetus, even if over a small inclination). And this proposition does not involve by any adventure more unlikeliness than that in which triangles within the same parallels, and with the same bases, are always equal [in area], while one can make one of them very short and the other a thousand miles long. But staying with the subject, I believe I have demonstrated this conclusion to be no less unthinkable than the other.

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[1] A closely related description is later given in the *Discorsi*, Galilei 1974, 226f. For a discussion of Galileo's exaggeration see Appendix B.

[2] See his bookkeeping accounts, Galilei 1890-1909, XIX: 132. The price for the two balls was 4 Lire. According to Galileo's notes in the years 1599 and 1606 on different kinds of brass, its price in these years varied between $\frac{1}{2}$ and 3 Lire for one Libra of brass, that is at that time in Padua about 0.4 kg; see Zupko 1981, 47, 134. Similarly a lot of $3\frac{1}{2}$ Libre of spoons, probably purchased for using the metal, was bought at a price of little less than $\frac{1}{2}$ Lira per Libra. This seems to be about the price for "cheap" metals. Assuming this price for the balls he bought and assuming also that they were indeed the ones made of lead he used in his experiment, they must have had a weight of about 1.6 kg and a diameter of about 6.5 cm. This is in accordance with the assumption that Galileo used quite heavy balls for his experiment. If the price would be considerably cheaper as one might expect in view of the fact that lead was used for artillery ammunition, this weight may have been correspondingly higher. A doubling of the diameter of the balls corresponds to an eight times higher weight or an eight times cheaper price.

Galileo continues his letter with a summary of his findings which he considered to be related to the isochronism of the pendulum. He gives a precise formulation of the isochronism of inclined planes inscribed into a circle which he claims to have been able to prove. He mentions the theorem that the descent along a broken chord (that is the sequence of two subsequent chords SI and IA) needs less time than the descent along the lower of the two chords (IA) alone, again maintaining to have found a proof. We do not have any indication that this claim was incorrect, all the more as there are several closely related folio pages in the manuscript Ms. Gal. 72 containing notes related to this theorem and even a complete proof.[1] But the only way that can be imagined for a proof under the conditions of Galileo's knowledge and which is attested to by manuscripts and the later publication in the *Discorsi* is based on the law of fall. In other words, in 1602 the law of fall as implication of the symmetrical parabolic shape of the projectile trajectory must have been so familiar to Galileo that he not even made a point of mentioning it.

Finally, Galileo explains in his letter the strategy of how he was trying by means of the two propositions on motion along inclined planes to provide a proof for the isochronism of the pendulum which he obviously considered at that time as an important discovery he had made:

> Until now I have demonstrated without transgressing the terms of mechanics; but I cannot manage to demonstrate how the arcs *SIA* and *IA* have been passed through in equal times and it is this that I am looking for.

Clearly, Galileo is trying here to reassure Guidobaldo del Monte that, in spite of the novelty of the subject for traditional mechanics, he is still adhering to the principles of this mechanics which had been the starting point of their exchange. For Guidobaldo these principles did not only comprise the theory as it is exposed in the ancient texts but also a strict correspondence between theory and practical experience. This attitude led him to be skeptical with regard to studies involving motion and is exemplified, in particular, also by his attempt to check Galileo's claims by experiments. In view of the failure of these experiments, Galileo was thus moving here on slippery territory. In the concluding passage of his letter, he attempted to appease Guidobaldo del Monte, in spite of his insistence that he had remained within the limits of mechanics, by explicitly agreeing with the latter's view that the practical verification of theorems involving matter in motion is indeed more problematic than in other parts of mechanics and in pure geometry:

> Regarding your question, I consider that what your Most Illustrious Lordship said about it was very well put, and that when we begin to deal with matter, because of its contingency the propositions abstractly considered by the geometrician begin to change: since one cannot assign certain science to

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[1]   See for instance Galilei ca. 1602-1637, folio 186v.

the [propositions] thus perturbed, the mathematician is hence freed from speculating about them.

I have been too long and tedious for your Most Illustrious Lordship: please excuse me, with grace, and love me as your most devoted servant. And I most reverently kiss your hands.

In spite of Galileo's friendly attempt to come to an agreement with his patron, it cannot be overlooked that he was on the way towards a radical break with the principles of Guidobaldo del Monte's mechanics, in particular with his strict adherence to the outcome of experiments. The achievements accumulated after his practical turn brought him finally back to questions of a theory of motion from which he had once started. The tension with Guidobaldo del Monte which becomes visible in his letter shows that his intellectual exchange with Paolo Sarpi begins to become influential for his Paduan activities.

Summing up, we have argued that the outcome of the projectile trajectory experiment performed in 1592 implied the law of free fall but that, as a result of Galileo's "practical turn," for a time period of about ten years there was no reason for him to elaborate the theoretical consequences of this experiment. Moreover, his exchange with Paolo Sarpi, which took up the issues of his treatise *De Motu*, led to a modification of his former theory in order to explain the symmetry of the trajectory; this modification seemingly made any drastic revision unnecessary. However, as early as in the year 1602, Galileo had derived already a set of propositions on motion that transgress not only the theory of *De Motu* and the reflections about this theory as they are represented by Paolo Sarpi's notes, but also the range of mechanics as it was conceived by Guidobaldo del Monte whom Galileo with his practical turn after his move to Padua tried to emulate so faithfully. Galileo's discoveries in the sequel of the projectile trajectory experiment comprise, as far as can be inferred from the few sources preserved, at least the law of fall, the isochronism of inclined planes in a circle, the broken chord theorem, and finally the isochronism of the pendulum. It comes therefore not as a surprise that only two years later we see Galileo on a new track, attempting no longer to integrate his discoveries into mechanics but trying to construct a completely new deductive theory which could substitute for the theory developed in *De Motu*.
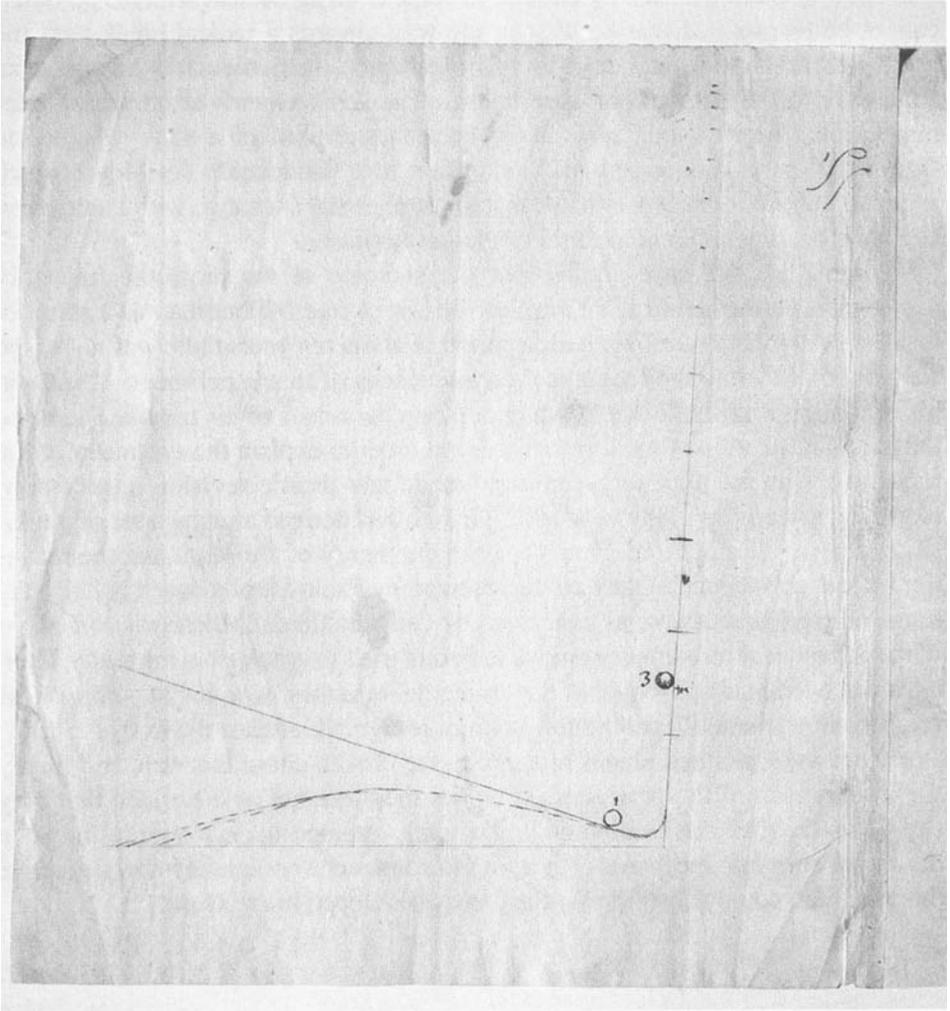
Figure 11. Ms. Gal. 72, folio page 175 verso, containing a drawing with an erroneous construction of the projectile trajectory
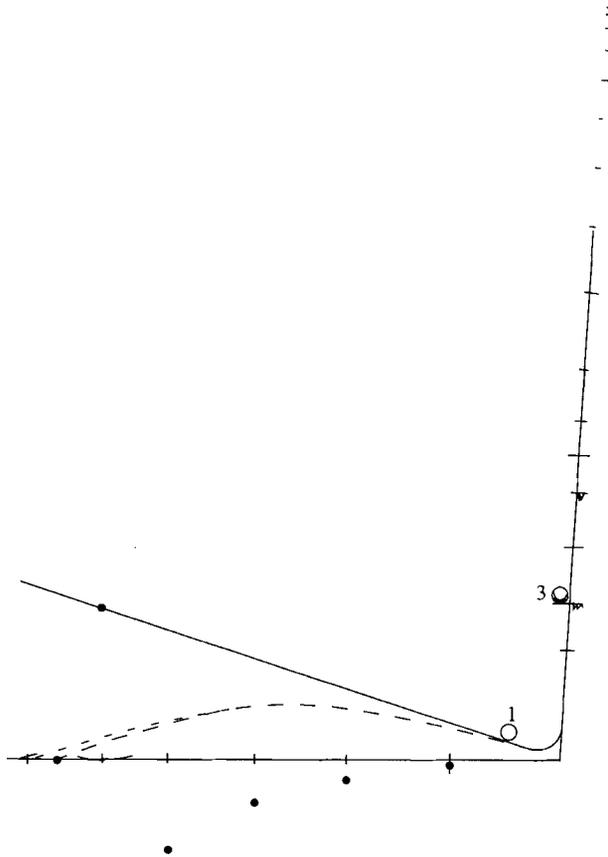
Figure 12. Based on construction lines and marks that are not drawn with ink Galileo's erroneous argument can be reconstructed

Did Galileo Trust the Dynamical Argument?—Galileo the Experimenter

Here is not the place to discuss the intricate development from the discovery of the law of fall to the final theory of motion as it was published in the *Discorsi*.[1] It is well known that it was still a long way to go for Galileo from the proof of the law of fall from the erroneous principle mentioned in his letter to Paolo Sarpi in 1604 to the comprehensive deductive theory of accelerated motion in his final publication

Moreover, the detailed analysis of this final achievement shows that he neither proved the law of fall in a way which could survive the restructuring of medieval natural philosophy into classical physics, nor did he find a complete proof of the parabolic shape of the trajectory which could at least stand up to his own criteria of theoretical consistency and rigor. Although coming close, he never in his life achieved an insight into the two principles that formed the basis of the scientific productivity of classical mechanics, which are, the principle of inertial motion and the principle of superposition of motions. His conceptual background remained that of *preclassical mechanics* and only his disciples were the first to reformulate his achievements in these categories of classical mechanics.

Here we shall concentrate instead on the question of how the Guidobaldo experiment which was at the outset of the discovery of the law of fall became finally incorporated into the new deductive theory of motion based on the law of fall. The way in which the law of fall was developed from the parabolic shape of the projectile trajectory immediately suggested how, vice versa, the parabolic shape of the projectile trajectory can be derived from the law of fall. Could that really work? Well, it worked at least for one particular case. The discovery of the law of fall resulted from a decomposition of the parabolic trajectory into two components which from a modern point of view have to be conceived of as a horizontal, uniform inertial motion and the vertically accelerated motion of fall. However, for Galileo there was no universal inertia. Only in the case of horizontal motion had he found already on the basis of his early treatise *De Motu* that this motion would persist without acceleration or deceleration as long as no forces or resistances intervene, a phenomenon which he designated as "neutral motion."

In the case of oblique projection, however, this interpretation seemed not to be applicable. Instead, in view of his discovery of the law of fall and the use he made of it as a basis for a new science exclusively constructed with the help of this powerful means of deduction, it must have appeared unavoidable to solve also

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[1]   This has been treated elsewhere, see Damerow, Freudenthal, McLaughlin, and Renn 1992, chap. 3. This book also includes virtually exhaustive references to the literature up to 1991 on the development of Galileo's science of motion and its context, including works by Caverni, Dijkster-huis, Koyré, Wohlwill, as well as other essential contributions of the older literature in Italian, Dutch, French, and German which have been neglected by recent Galileo scholarship. According to our opinion, among the most significant contributions on this topic published after Damerow, Freudenthal, McLaughlin, and Renn 1992, chap. 3 are Hooper 1992, Takahashi 1993a, Takahashi 1993b, Porz 1994, Abattouy 1996, and Remmert 1998.

this question by a straightforward application of this law: Why on earth should the projectile trajectory be anything else than the composition of a decelerated motion in the direction of the projection, such as the motion on an inclined plane, and of the vertically accelerated motion of fall? In fact, a folio page dating also into Galileo's Paduan period[1] has been preserved which displays a construction aimed at determining the shape of the trajectory of oblique projection by applying exactly this idea (see figures 11 and 12).

Unfortunately, this transfer of Galileo's interpretation of horizontal projection to the case of oblique projection does not yield a vertically oriented parabola. Galileo must have realized that the curve resulting from this construction was not symmetrical, and he definitely gave up this attempt to compose oblique projection from two non-uniform motions, both governed by the law of fall.[2]
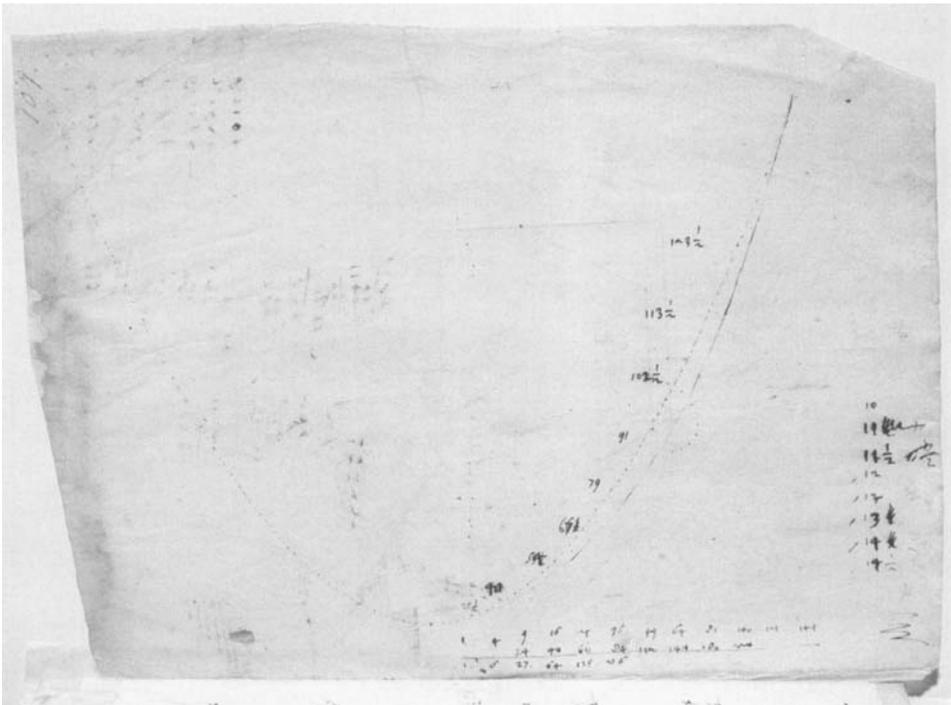

Figure 13. Ms. Gal. 72, folio page 107 recto

[1] Galilei ca. 1602-1637, folio 175v, see the discussion of this folio page in Damerow, Freudenthal, McLaughlin, and Renn 1992, 205-209.
[2] The correct superposition of an oblique inertial motion and free fall is found as a correction of a figure and an addition to the text in Galileo's annotated copy of the *Discorsi*, see folio pages 157 and 158 with annotations to the pages 262 and 263 of Galilei after 1638.
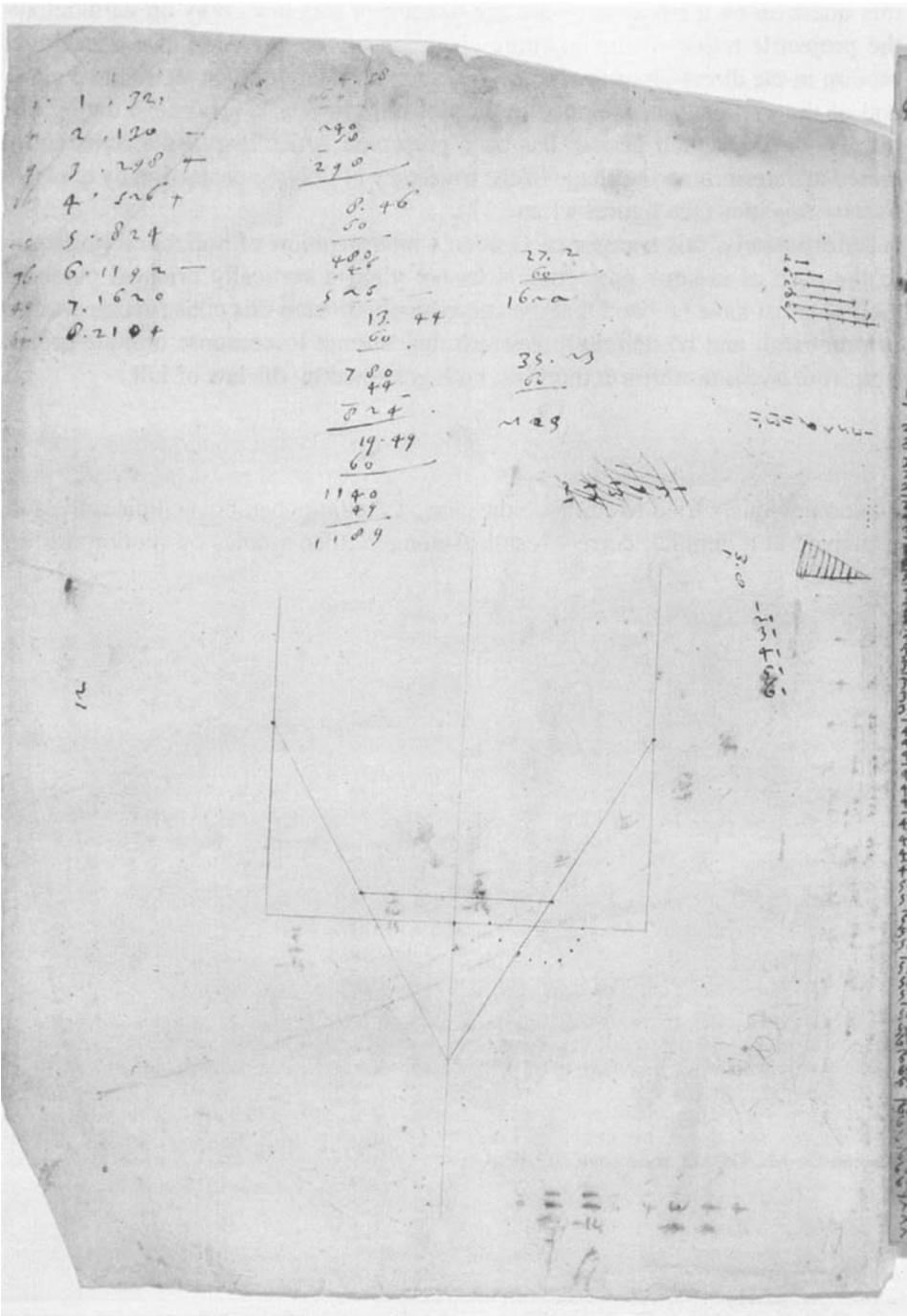
Figure 14. Ms. Gal. 72, folio page 107 verso

What he tried instead was to obtain a better understanding of projectile motion from experiments, supplementing the Guidobaldo experiment which had originally initiated his departure from traditional mechanics. According to the surviving documentation of these experiments, they seem to have failed altogether in providing unambiguous evidence of the symmetrical, let alone parabolic shape of the trajectory.[1] Thus, Galileo was forced to return to the source of his original insight, that is the common dynamical interpretation of the projectile trajectory and the curve of a hanging chain.

Given this return under the conditions of his richer theoretical knowledge as well as the facilities he had now for systematically performing sophisticated experiments,[2] did he really never realize that the catenary is not a parabola? In view of the evidence concerning his treatment of the subject in his last publication, the *Discorsi*, the answer seems clear: He seems either never to have realized this error or he must have consciously kept such an insight disguised.

Nevertheless, there is striking evidence that neither of these alternatives is true. Galileo obviously tried to compare the catenary and the parabola empirically, and he arrived at a definite, correct result. Among Galileo's notes on motion a folio has been preserved, folio 107, documenting this experiment (see figures 13 and 14).[3]

The folio is datable by its watermark to belong to the Paduan period.[4] It contains on the obverse two curves with a common upper endpoint and a common

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[1]   The few manuscripts bearing evidence to Galileo's experimental study of projectile motion are the folio pages 116v, 114v and 81r of his manuscripts on motion, Galilei ca. 1602-1637. They have been the subject of controversial discussions in the literature. Drake expressed his final account of the success of the experiments on oblique projection in the following statement with which we agree: "It is precisely because they [the experiments] 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." Drake 1990, 124

[2]   For new findings on Galileo's experimental practise see Settle 1996.

[3]   The reverse side of this folio has been interpreted as such a comparison in various papers by Naylor; for his views on Galileo's theory of projectile motion see Naylor 1974; 1975; 1976a; 1976b; 1977; 1980a; 1980b; Naylor and Drake 1983.

[4]   Identification of watermarks has been introduced by Stillman Drake as a means of ordering and dating Galileo's folios, see Drake 1979. In the context of preparing an electronic representation of the manuscript, we have begun to systematically check Drake's assignments. Drake's identification of watermarks could be made only when the manuscript was still bound. Identifications of watermarks used here depend on a preliminary inspection of the unbound manuscript and differ therefore partly from Drake's identification.

The special form of the watermark of folio 107, a thin crossbow (Drake's type 6), occurs usually together with another watermark, a crown (Drake's "Mountains", type C12) on a double sheet of paper. Galileo mostly cut these sheets into two pieces so that only one of these watermarks can be found on the page. This makes it difficult to decide whether these watermarks occur also alone. Furthermore, another form of the watermark, a thick crown (Drake's type 15) occurs also together with the watermark depicting a crown. It is unclear if this variant indicates another type of paper. According to Drake, the earliest dates that these watermarks occur on dated letters is August 1607 (crown C12) and May 1608 (crossbow 15).

According to paper type, paper size and watermark, the following folios seem to be closely related to folio 107: f089b, f115, double page f116/117, double page f126/127, f129, f130, f131, f132, double page f134/235, double page f136/137, f138, double page f140/141, double page f142/143, f144, f145, f147, f148, f153, f155, double page f156/157, double page f158/159, f161, f165, f166, double page f168/169, f174, f176, f179, f185, f186, f187, double page f190/191, f192.

zero point at the bottom. One of the two curves continues symmetrically up to the top, while the second curve reaches only to the zero point at the bottom; it is similar to the first one but apparently deviates from it by a different curvature.

A set of partly corrected figures is written along the curves, a second set of partly corrected figures is contained in a small table at the right side of the curves. The figures in the table obviously represent the differences between the figures written along the curves. Three sequences of increasing integers are written along the base line of the curves. The first sequence represents square numbers, the third one cubic numbers. The intermediate sequence is constructed by the rule that the difference between the figures is each time increased by four.

The reverse side of folio 107 contains in its center a geometrical drawing. In one corner a table of figures is written representing a series of empirical data which are close to square numbers, the deviations being marked by plus and minus signs behind some of the figures.

Near this table there are several calculations giving figures which can also be identified, partly corrected, in the table. These calculations can easily be interpreted as conversions of measured figures into the sixty times smaller unit used in the table. Furthermore, the page seems to have been used as scratch paper. Some small drawings and columns of figures are written in different orientations near the upper right edge of the page

Although there is no text on the folio which could explain the function of the curves, drawings, and the figures, a meticulous investigation of the folio[1] made it possible to accomplish a detailed reconstruction of its contents and purpose. Precise measurements have established beyond any doubt that the symmetrically completed curve is a parabola whereas the deviating curve is a catenary, that is the curve of a hanging chain (see figure 15).

Close inspection of the original folio has furthermore revealed a considerable number of construction lines drawn without ink by means of a stylus or by compasses (see figure 16). These construction lines are invisible as long as the page is not illuminated with light under an extremely small angle. Moreover a great number of tiny impressions and wholes in the surface of the paper have been detected which result from transferring distances by means of compasses or from indicating points by pressing the sharp end of a stylus into the paper. The function of the figures along the curves could be identified as representing horizontal distances of the catenary from the middle axis. Thus, the figures in the table representing the differences between these measures provide a check of the extent to which the catenary deviates from a parabola. The corrections of the figures could be interpreted as resulting from manipulations performed in order to fit them to some rule for the sequence of the differences.

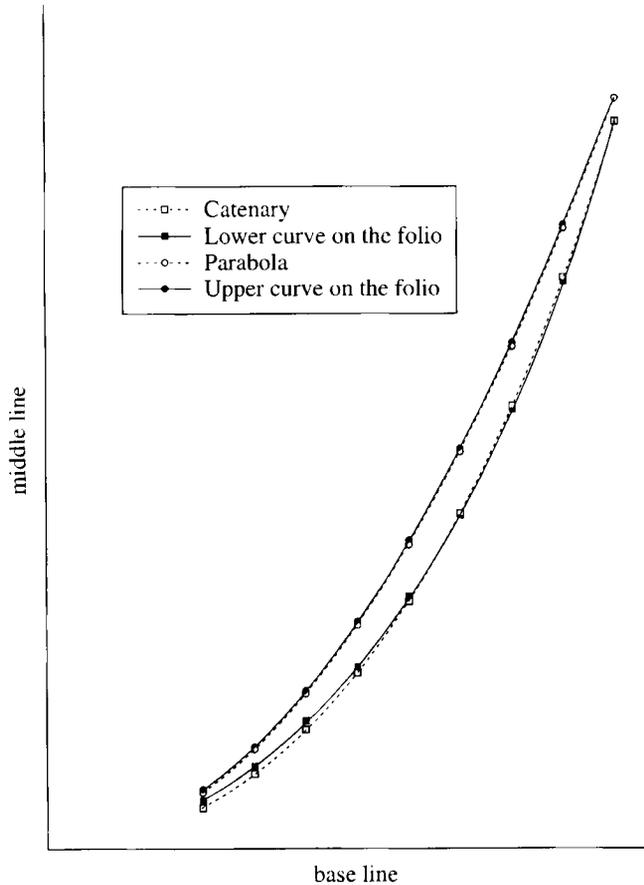[1]   Our detailed reconstruction of folio 107 will be published elsewhere.

Figure 15. Comparison of the two curves on folio page 107 recto with the parabola and the catenary

The investigation of the reverse side of the folio has unquestionably revealed, as will be shown in the next sections, that the drawing in the middle is part of an unfinished attempt to construct a proof for the alleged parabolic shape of the catenary. The empirical figures in the table on the reverse side have been analyzed by fitting mathematical curves to the sequence.

It turned out that the figures fit perfectly to a catenary which is stretched to such an extent that the width is about twenty times the height which the chain is hanging down, whereas there are slight systematic differences of the data to a parabola.[1] In the case of such a stretched hanging chain, the difference of the catenary to a parabola is, however, so small that it cannot be excluded that the better fit of the catenary is just accidental.
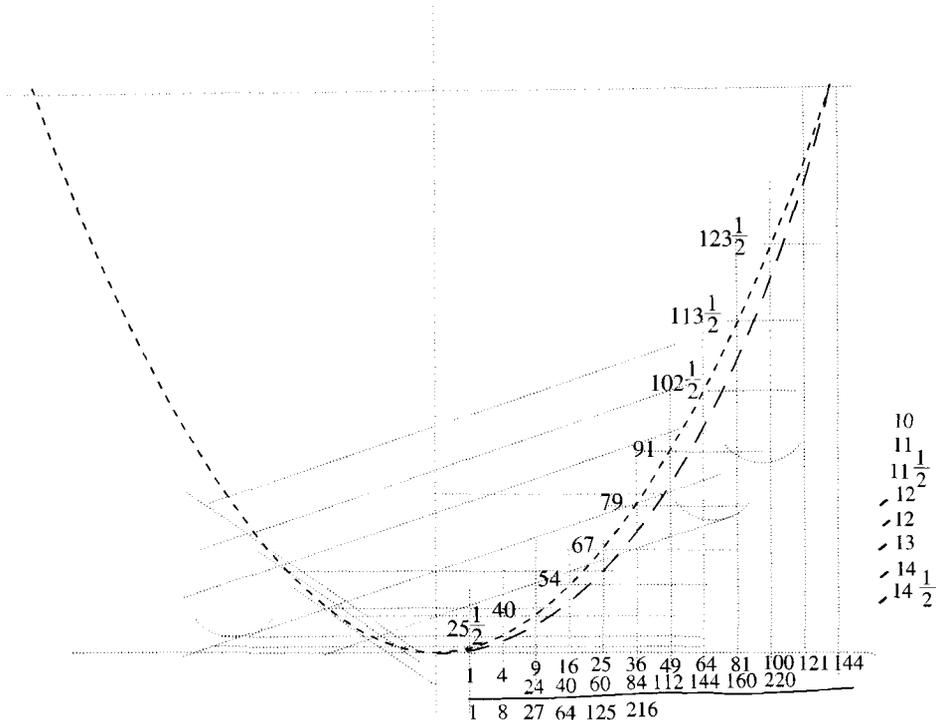

Figure 16. Uninked construction lines on folio page 107 recto

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[1]   In contrast to the parabola, the catenary does not scale along a single axis. Therefore, the fit of the catenary implies a definite relation between length and width of the hanging chain. Assuming that the highest value of 2123 in the calculations for Galileo's table on folio page 107v is the maximal vertical measure of the hanging chain, the best approximation is achieved for a width of about 24000. This corresponds to a hanging chain with an inclination of 20 degrees with respect to the horizontal at the suspension point, which is in perfect agreement with the angles used on the template folio 41/42 discussed above. The measure used by Galileo was probably the "point," so that the experiment must have been performed with a hanging chain of about 24 meters width and 2 meters height. This estimation of the measures must, however, have been taken with caution. The determination of the width by means of the best fit of a catenary to the data involves a very high error variance. Furthermore, any real chain will systematically differ from the catenary with the result of a systematic error in the estimation of the width. Even a much smaller width still gives a very good approximation. In any case, it has to be taken into account that a catenary with the relation of width to height as it is reconstructed here differs so little from a parabola that unfortunately the better fit of the catenary cannot be taken as a proof for the given interpretation of the data as resulting from an experiment with a hanging chain.

In addition to its careful investigation, folio 107 has been included in an ongoing project in which the composition of inks in Galileo's manuscripts is analyzed using methods developed in nuclear physics.[1] The aim of these investigations is to determine ink differences on the folio pages which may provide clues for dating the entries. For folio 107 these investigations showed no substantial ink differences between the main entries on both sides of the folio.[2] In particular, the ink points of the parabola, the uncorrected figures along the curves and in the table beneath the curves, the quadratic and the cubic scale at the base line of the curves, the calculations and the table of measured figures on the reverse side, and the drawing in the middle of the reverse side show no differences in the ink composition (see figure 17). Although this result does not prove that the entries were made at the same time, this result can be nevertheless interpreted as a strong indication for a close connection of the entries.
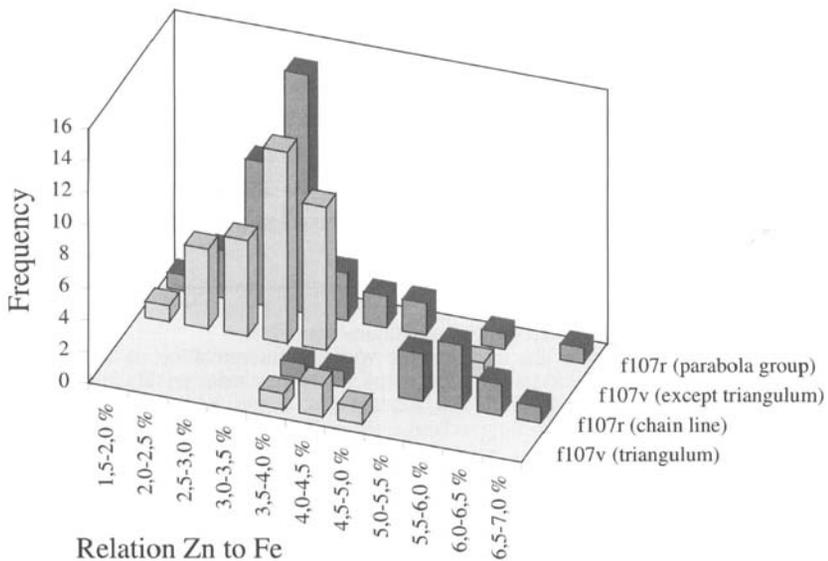

Figure 17. Result of the ink analysis of folio 107 recto. The diagram contains (from back to front): frequency distribution of the relation of zinc to iron at most points measured on the recto page (parabola, quadratic scale, cubic scale, uncorrected figures), on most points measured on the verso page (all points except the small triangle), at the chain line on the recto page, and at the small triangle on the verso page. Three slightly differing inks can be distinguished. The distributions show that all entries on both sides of the folio with the exception of the chain line and the triangle have been written with the same ink.

It is remarkable that some entities on the two pages show a slightly different amount of zinc in its ink composition. Time-line measurements on another manuscript with dated entries have shown that during the use of the same ink there are sometimes characteristic changes in the ink composition which result in such slight differences of individual components. Two such ink differences could be identified on folio 107. First, the dashed line of the catenary, the intermediate scale at the bottom with constantly increasing differences, and partly the ink of the corrections show a higher amount of zinc than the main entries on both sides, which is an indication that they were added on a somewhat later occasion.[1] Similarly, a small triangular drawing on the reverse side shows a much less but still detectably higher zinc component.[2]

Taking all these results of the investigation of folio 107 together, the following scenario for its origin and purpose seems to be the most plausible one.[3] In order to check whether the curve of a hanging chain is in fact, as he believed, a parabola, Galileo at some point in his work at Padua constructed a parabola and superposed it with a catenary produced, as later described in the *Discorsi*, by means of a fine chain. The right side of the parabola was constructed by drawing a horizontal base line with ink and erecting on it without ink a set of perpendicular lines in equal distances from each other.[4] In order to draw the left side, the constructed curve was mirrored at the vertical middle line by means of a ruler.[5] Horizontal lines were drawn without ink with distances to the base line representing a sequence of squares.[6] A corresponding sequence of square integers was written as

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[1] Relation to Fe: Zn 5.5%.

[2] Relation to Fe: Zn 4.5% (based on four measurements only)

[3] The results presented here are incompatible with the interpretation of folio 107 given by Drake. In his presentation of the manuscript he assumes that the entries on this folio were written at quite different times; see Drake 1979, 23, 40, 82, and 124, as well as his commentaries on pages XXXf, XXXV, and XXXVIII. According to Drake, the earliest entries are the figures in the table on the reverse, written in 1604 and representing empirical data of the discovery of the law of fall by means of the inclined plane experiment, see Drake 1990. Next, still in 1604, Galileo supposedly drew the curves and figures on the obverse, interpreted by Drake as a comparison of parabola and catenary, with the exception of the scales at the bottom which Drake considered as being added in 1609. Finally, according to Drake, Galileo added, also in 1609, the drawing on the reverse. He considered this drawing first as the depiction of an apparatus for measuring percussion, later as representing a water tube used by Galileo for the inclined plane experiment as a device for precise time measurements, see Drake 1990, 9-12.

[4] This distance may correspond to 1 uncia, but the measures reported in Zupko 1981, 174-179 are generally higher. The figures on the folio page show, in any case, that distance between the vertical lines was divided into 12 smaller units as it was the rule for the uncia throughout Italy in early modern times. It is remarkable that the measures on the page differ from that on folio 41/42 and the related folios, indicating that these folios, although also dealing with the catenary (see below), were probably written at a different place and time.

[5] The points which Galileo used to mirror the curve can easily be identified. These points were first indicated by impressions of compasses or stylus and then marked with ink in a particular way different from the way the rest of the curve is drawn.

[6] The scaling of the parabola was obviously chosen so that the parabola had about the size of the hanging chain. At the eight vertical the distance between the base line and the horizontal comes close to the horizontal distance to the middle axis, but does not fit perfectly. The difference is about 3%. It is therefore unclear if the scaling factor was consciously chosen or if it was implicitly determined by some procedure that was used to adjust the parabola to the hanging chain.

a scale at the bottom base line. The parabola was drawn with ink through the intersection points of the network of the inkless vertical and horizontal lines.

Next a hanging chain was matched to the parabola so that the curves coincide in the suspension points of the hanging chain and its lowest point.[1] The intersection points with the sequence of inkless horizontal lines with squared distances, which deviate from the intersection points through which the parabola was drawn, were marked by inkless stylus impressions, their distances to the middle axis were measured, and the results were written near the parabola, each of them at the level of the corresponding horizontal line. In order to check the parabolic shape the differences between the measurements were calculated and written down in a table to the right of the drawing. Since these differences would be equal if the curve were a parabola, Galileo must have realized immediately that the curve of the hanging chain did not only fail to match the constructed parabola, but any parabola that could be constructed (see table 1).

Distance to middle axis	Difference to next value	Distance of parabola to middle axis
$123^1/_2$	10	120
$113^1/_2$	$10^1/_2$	108
103	12	96
91	12	84
79	13	72
66	$12^1/_2$	60
$53^1/_2$	$14^1/_2$	48
39	$13^1/_2$	36
$25^1/_2$	---	24

Table 1. Table of the uncorrected distances of the catenary from the middle axis written along the curves and the uncorrected differences between the measurements, compared with the corresponding values of the parabola assuming a unit of one twelfth of the distance between the vertical lines

Facing this result, Galileo probably decided to repeat the attempt with a much longer chain in order to minimize possible errors caused by the limited flexibility of the material of the chain. He used a chain of several meters length which was stretched to such an extent that the curve was rather flat, thus necessarily produc-

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[1] The suspension points of the chain do not coincide with constructed points of the parabola. It is therefore more likely that the chain was empirically matched to the constructed parabola (as described in the *Discorsi*) and not the parabola scaled as to match the chain. The latter procedure would be much more difficult to execute with precision.

ing a better fit with a corresponding parabola, because the differences between the two curves increase with increasing relation of height to width as we know today from the correct formula of the catenary which was discovered much later.

The vertical height over the base line of the curve of the hanging chain was measured at eight points at regular distances from the lowest point of the curve. These measurements should increase in quadratic sequence. By calculations partly written down on the reverse side of the folio, the measurements were converted into the smallest unit, which was surely the same as the one used for measuring the catenary on the obverse side, and the results were written down in a small table left to the calculations in correspondence to a sequence of integers from 1 to 8 and their squares from 1 to 64 (see table 2).

1	1	32	
4	2	130	–
9	3	298	–
16	4	526	+
25	5	824	
36	6	1192	–
49	7	1620	
64	8	2104	

Table 2.   Galileo's table of measurements taken at a long chain

There are two remarkable differences between the measured values used in the calculations converting the units and the values Galileo actually entered into the table. While Galileo measured 1189 he wrote 1192, and while he measured 2123 he wrote 2104. The reason is obvious. Already when Galileo entered the data into the table he checked whether they correspond to his belief that they represent a parabola. A measurement at the double distance should yield a value which is four times greater. This rule applied to the first and the second measurement shows that the second value should be four times 32, that is 128; Galileo wrote this number down to the right of the table using tiny characters, obviously in order to keep this figure in mind. Applied to the third and the sixth measurement, the rule shows that the sixth value should be four times 298 which is 1192, and applied to the fourth and the eights value the rule gives four times 526 which is 2104. In both cases Galileo entered directly these theoretical instead of the slightly differing empirical values into the table.

Galileo finally attempted to check the deviations from the parabolic shape by calculating all values from the first value in the sequence. Since four times as well as nine times 32 fall short of the empirical values in the table, Galileo indicated

these deviations by minus signs behind the values in the table. Realizing that all other values would also fall short, Galileo now probably changed the basic value from 32 to 33. Consequently, the next calculated figure of 16 times 33 is greater then the value measured; this is indicated by a plus behind the figure. Furthermore, the next calculated value nearly matches the measured value (824 instead of 825) so that this one remained unmarked. The next calculated value 36 times 33 again falls short, which is indicated by adding a minus sign. At this point Galileo stopped the procedure and left the last two figures without any indication of their deviation from the theoretical values. In spite of these small deviations, however, Galileo will, for good reasons, have considered the results to fit much better the expected parabolic shape than in the case of the small chain recorded on the other side of the folio.

   Then Galileo started to work on a proof for the alleged parabolic shape of the catenary with a drawing below the table and the calculations, but left it unfinished. This proof attempt will be discussed in the next section.

   According to the ink difference, Galileo must have returned some time later again to the comparison of the curves on the obverse side of the folio. In order to be better able to compare the curves he now inked the chain line, added the third scale with differences increasing each time by four, and tried to modify the figures along the chain line so that their differences would decrease with increasing distance from the middle axis monotonously, intending to find a simple rule for their decrease (see table 3).

Corrections of the distances to the middle axis	Differences to next values
$123\frac{1}{2}$ remained unchanged	10 remained unchanged
$113\frac{1}{2}$ remained unchanged	$10\frac{1}{2}$ changed in 11
103 changed in $102\frac{1}{2}$	12 changed in $11\frac{1}{2}$
91 remained unchanged	12 remained unchanged
79 remained unchanged	13 changed in 12
66 changed first in $66\frac{1}{2}$, then in 67*	$12\frac{1}{2}$ changed in 13
$53\frac{1}{2}$ changed in 54	$14\frac{1}{2}$ changed first in $13\frac{1}{2}$, then in 14
39 changed in 40	$13\frac{1}{2}$ changed in $14\frac{1}{2}$
$25\frac{1}{2}$ remained unchanged	---

*   If Galileo did not make a calculation error, the original entry cannot have been $66\frac{1}{2}$ because the unchanged previous value of 79 and the difference 13 which was changed in 12 make this impossible.

Table 3.   Table of corrections of the distances of the catenary to the middle axis and of the differences between the measurements

The result of these trial-and-error corrections[1] is a sequence of corrected numbers close to his original measurements. No value was changed more than one unit of his scale, that is less than one millimeter. The differences between the values decrease now continuously from $14^{1}/_{2}$ to 10, but still failing to show a simple rule of their decrease. At that point Galileo must have given up his attempts to improve the data further.

This scenario of how Galileo wrote the entries on both sides of folio 107 is necessarily in part speculative, but it keeps close to the results of a careful investigation of the details of the page. It leaves unexplained several oblique construction lines on the obverse side and the scratch notes and drawings on the reverse.[2] The equally unexplained scratch notes and diagrams on the reverse page are too simple to allow for any substantial reconstruction of their purpose. However, they may well belong to the context of the other entries on the folio representing attempts to find an explanation for the puzzling difference between parabola and catenary by ascribing them to modifications of the "moments" in Galileo's terminology by which, as will become clear soon, he tried to explain the shape of the catenary.

Did Galileo Trust his Experiments?—Galileo's First Attempt of a Proof

The purpose of the drawing in the center of the reverse of folio 107 in the context of an attempt to find a proof for the parabolic shape of the catenary would have remained undiscovered if folio 132 had not been preserved. On this folio Galileo worked more explicitly with two different geometrical constellations on a closely related aspect of the same proof attempt, carrying it further than he did on folio 107. The most elaborate version is contained on the obverse of folio 132 (see figure 18), while another attempt which, however, was abandoned earlier is found

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[1] Probably starting from the top he changed first the 103 into a $102^{1}/_{2}$ and corrected correspondingly the differences $10^{1}/_{2}$ into 11 and 12 into $11^{1}/_{2}$ by writing first the new differences to the right of the old ones, but then striking them out and writing the new figures directly over the old ones. The result was a somewhat more regular sequence at the top. Then he started from the bottom where the irregularities were greater. He changed now directly by overwriting the old figures, first the 39 into a 40, correcting correspondingly the differences $13^{1}/_{2}$ into $14^{1}/_{2}$ and $14^{1}/_{2}$ into $13^{1}/_{2}$. He continued by changing the 66 into $66^{1}/_{2}$ and started to correct the differences by changing the $12^{1}/_{2}$ into a 13. Before he corrected the second difference, however, he must have realized that the differences decreased too quickly so that they would not nicely match the upper part of the table. Thus, he left the difference for the moment unchanged and started again from the figure below by changing now also the $53^{1}/_{2}$ into 54, followed by changing back the already changed difference $13^{1}/_{2}$ into 14 which is in the middle between the original value and the first change. He left the other difference which had already been changed into 13 untouched and corrected instead again the next value from $66^{1}/_{2}$ into 67, correcting the corresponding next difference from 13 into 12.

[2] No explanation has been found so far for the two sets of oblique construction lines on the obverse side. Since these lines have only very few coincidences with intersection points of the explained parts of the drawing, it may well be that these lines belong to an unrelated, unfinished earlier drawing which was not yet inked so that the folio could be used again for a different purpose.

on the reverse of the same folio. As folio 107, this folio, too, can be dated by its watermark into the Paduan period of Galileo's work.

In contrast to the precise drawing in the center of the reverse of folio 107, the drawing in the left upper corner of folio page 132r is only a rough sketch, but essentially complete. It depicts two constellations of a hanging string fixed with its ends to two points a and c of a horizontal line, the middle of the line between these two suspension points being designated by the point d.

In the first position the hanging string is pulled down by a neatly drawn weight, fixed in its middle at point b, so that the string forms a triangle abc. In the second constellation two further weights have been fixed precisely in the middle of each half of the string at points e and f pulling the string outwards on circles around the suspension points a and c towards two points which are both designated by the letter g.

These additional weights raise the first weight from point b to point o. In addition to theses two constellations of the string, Galileo sketched a third constellation which results from pulling the points e and f, where the additional weights are fixed, symmetrically outwards to the extremes so that the string now goes vertically down from the points a and c.

A small table to the right of the drawing contains the intended measures of the drawing from which the actual dimensions of the drawing, however, deviate considerably. The table furthermore contains some calculated lengths which can easily be inferred from the given measures by applying simple geometry. In particular, the table gives for the distance of the suspension point a to the middle point d between the suspensions a and c the measure 30, so that the total distance between the suspension points is hence assumed to be 60.

Furthermore, the table gives for the distance ab, which is half of the total length of the string, the measure 90, and for the distances ae and eb to the point e, where one of the two additional weights had to be fixed, the measure 45 which is half of the length 90 of half of the total string.

The next entry in the table is an approximate measure of db, the height of the first constellation from the middle d of the horizontal connection of the suspension points a and c to the point b in the middle of the string. The value $84^{7}/_{8}$ given for this height has been calculated using the theorem of Pythagoras. The calculation of the square root of 7200 (that is, of the difference between 8100 and 900, the squares of the lengths 90 and 30 of ab and ad, respectively) can be found above the table, resulting in $84^{144}/_{169}$, rounded in the table to $84^{7}/_{8}$. From this value Galileo calculated its half $42^{7}/_{16}$, denoted as the measure for the distances bi and di from the end points b and d of the height to its middle point i which is also the middle point of the horizontal line ef connecting the points e and f where the additional weights have to be fixed.
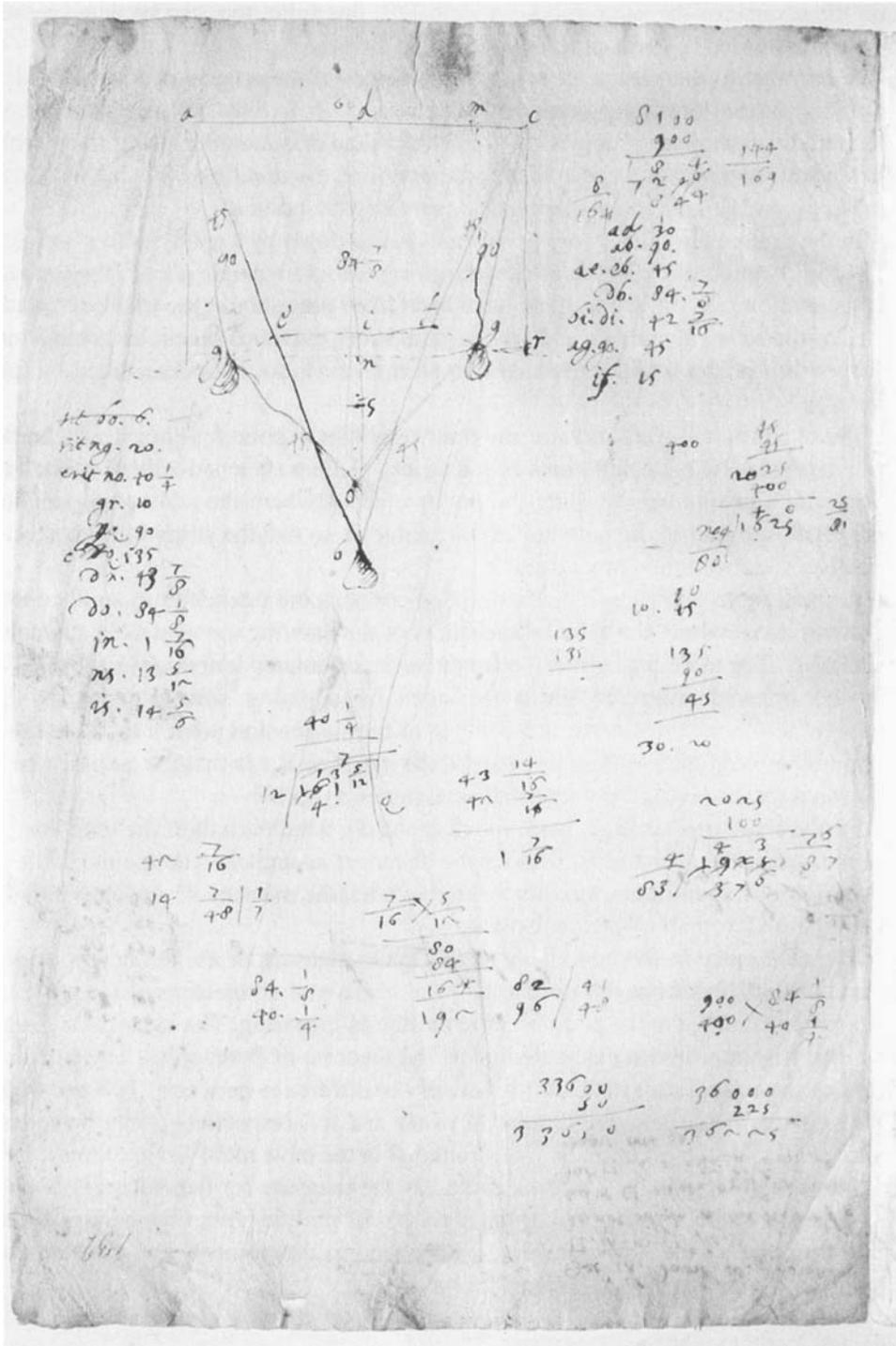
Figure 18. Ms. Gal. 72, folio page 132 recto

The table continues with the measure 45 for the parts *ag* and *go* of the string between the suspension point *a* and the points where the weights have been fixed in the second constellation – after they have been moved from *f* to *g* and from *b* to *o*. The next and final entry of the table gives the measure 15 for the distance *fi* between the end point *f* and the middle point *i* of the horizontal line *ef*, connecting the points *e* and *f* where the additional weights have to be fixed. The measure 15 for this length follows immediately from the similar triangles in the constellation to be half of the horizontal *dc*.

At this point the table stops, obviously because Galileo now encountered an obstacle which hindered him from calculating further the measures of the second constellation: How could he know how far the additional weights would move the string outwards from the points *e* and *f* to the points both designated as *g*, and how could he know how far, as a consequence, the first weight at point *b* would be raised to point *o*? In fact, the calculation of the measures of the second calculation resulting from the static equilibrium between the three weights fixed to the string requires knowledge about the compounding of static forces and algebraic techniques that were not available at Galileo's time. Galileo denoted the middle point of the line connecting the unknown positions of the additional weights in the resulting second constellation with the letter *n*. The only consequence that could be drawn was that the horizontal line *ng* had to be *longer* than the measure 15 of the corresponding line *if* in the original position, which he had recorded in the last line of the table, and that this line had to be *shorter* than the corresponding line in the extreme third constellation, that is, shorter than 30. In a similar way Galileo could have done all his calculations also for the other extreme constellation. This would have shown that the maximum distance by which the weight at point *b* could have been raised to point *o* was little more than 6.[1]

What Galileo really did in this situation is documented by a second table written to the left of the drawing near its bottom and by several calculations distributed all over the page. He bridged the unsolvable problem by a hypothesis written at the beginning of the second table. First he wrote "let *bo* be 6", but then cancelled this entry substituting it by the more realistic assumption "let *ng* be 20", that is, in between the original distance of the added weights of 15 and the extreme distance in the third constellation of 30. The entries on the folio page do not give any clue that might help to decide whether this assumption was based on an experiment or was simply guessed by Galileo. Fortunately, the drawing on the reverse of folio 107, to which we will turn below, shows that Galileo found an ingenious solution to this problem of determining the equilibrium position.

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[1] If the string goes vertically down 45 units from the top horizontal measuring 30 units from the suspension point to the middle line, the remaining string length of 45 units to the middle line results in an additional height which can be calculated as 15 times the square root of 5, that is, little more than $33^{1}/_{2}$ units. Added to the vertical distance of 45 units, this results in little more than $78^{1}/_{2}$ units, the value for the total height in the extreme position. Compared with the height of the first constellation calculated by Galileo as being $84^{7}/_{8}$ units, it follows that the maximum length by which the weight at the bottom can be raised is little more than 6 units.

The subsequent steps of Galileo's procedure on page 132r are well documented by the calculations on this page. Based on his hypothesis about the distance ng, which represents the distance of the added weights from the middle line after moving the string to the second constellation, he calculated the lengths of a number of lines belonging to this new constellation. The calculations were performed in the sequence of their arrangement on the page and the results were subsequently entered into the table. The general purpose of these calculations can be reconstructed from the last calculations on that page: Galileo checked whether the *squares* of the distances ng and ad, representing the horizontal distance of the assumed position of the additional weights from the middle line and the horizontal distance of the suspension points from that line, respectively, are proportional to the distances no and do, representing the corresponding vertical distances from the lowest point of the string. In other words, Galileo performed a "parabola check," i.e. he checked whether the suspension points and the weights lie on a parabola. This check is realized by comparing $ng^2 \times do$ and $ad^2 \times no$.

The calculations performed in order to make this comparison possible are straightforward and need not be reported here in detail, all the more as all calculations are explicitly given on the page and all rounding operations can be inferred from the values Galileo actually entered into the table of results. Having assumed ng to be 20 and having given at the beginning the length ad to be 30, Galileo needed to calculate only the values no and do using elementary geometry. That is what he actually did on the page via some intermediate steps.[1] He arrived at the rounded values $40^1/_4$ and $84^1/_8$ for the distances no and do respectively. Multiplying them with the squares 900 and 400 he achieved at the bottom of the pages as a result of his parabola check the values 36225 and 33650 which should have been equal if the points he checked would really be on a parabola.

After having finished the calculation of the values needed for the parabola check, Galileo calculated the distance in, that is, the vertical distance by which the additional weights moved downwards. Next Galileo added a point s below point n at $^1/_3$ of the distance no, that is, at the center of gravity of the second constellation, and calculated the distances ns and is. Finally, Galileo calculated the length of $^1/_3$ of the distance bi without entering, however, this last value of $14^7/_{48}$, which he rounded to $14^1/_7$, into the table. Obviously, he calculated this value in order to compare it with the length $14^5/_6$ of is, that is, he checked, whether the center of gravity of the two constellations would be at the same place, with the result that this is only approximately true.

This last operation provides an answer to the problem of how Galileo could know how far the additional weights would pull the string outwards. Guidobaldo del Monte's as well as Galileo's treatises on mechanics show that they both were

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[1]   Using the length cg (given) and ng (assumed), Galileo calculated subsequently the lengths no, gr, gq, dq (cancelled), cq, and dn to arrive at do, the value he needed in addition to the value no for the parabola check. The calculated values gq, dq (cancelled), and cq, which are distances to the intersection q of the upper part of the string with the middle line through d and b, are not necessary for the calculation of do. The purpose of the calculation of these lines remains unclear.

well aware of the fact that such a constellation cannot be in equilibrium as long as its center of gravity does not reach the lowest possible point. In his notebook, Guidobaldo even considered the case that a change of the center of gravity of the earth, achieved by putting an additional weight on its surface, causes the earth to move as a whole

> because the center of gravity tends by its nature to the center of the world (...). (del Monte ca. 1587-1592, 54)

A similar consideration might have suggested to Galileo that he had to study how the center of gravity moves when the weights along the string are swinging into their equilibrium position.
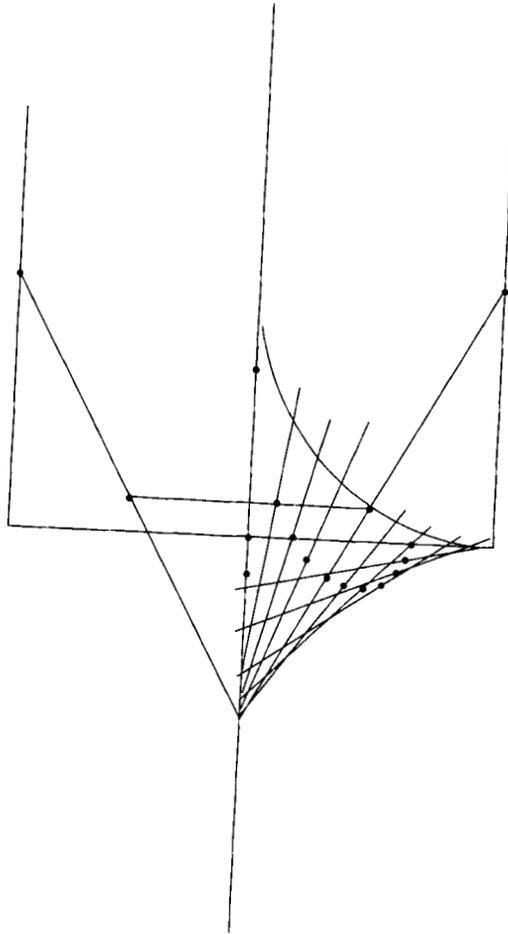

Figure 19. Drawing on folio page 107 verso with uninked construction lines

That this is, in fact, how Galileo solved the problem is documented by the precise drawing in the center of the reverse of folio 107 (see figures 14 and 19). In this drawing, particularly simple measures are used: the length of the string is exactly twice the horizontal distance of the suspension points[1] so that the extreme position of the two additional weights vertically below the suspension points can be reached only if the string between them is stretched out horizontally with all three weights hanging on the same level, the center of gravity being in the middle. Thus the drawing represents a constellation involving the shortest string that still makes it possible to pull the additional weights vertically below the suspension points. The centers of gravity of both constellations are marked by bold points, the original one where the added weights did not yet change the configuration and the extreme one where the weights are pulled vertically below the suspension points.

A great number of construction lines which are not drawn with ink can be identified on the folio page as being part of the drawing. They show that on the right hand side of the symmetrically hanging weights, Galileo systematically constructed a set of constellations. These constellations reach from the one extreme, in which the weights are pulled to the middle line, to the other, in which the weights are pulled vertically below the suspension points. From the circle along which the additional weight on the right-hand-side is moving, lines with the fixed length of the cord between the weights are drawn towards the middle line where the lowest weight is hanging. On each of these lines Galileo marked with ink by a bold point the distance of one-third of the total length. Since these points are necessarily on the same height as the center of gravity of the whole constellation, Galileo reached in an ingenious way a solution to the question of which constellation has the lowest possible center of gravity thus representing the equilibrium position.

What may have been the purpose of this arrangement of weights in equal distances on a string and of the check whether these weights lie on a parabola after reaching an equilibrium? An answer to this question is provided by a comparison with the strategy discussed above which Galileo used when he attempted to find a proof for the isochronism of the pendulum. Following the example of the Archimedian approximation of the circle by polygons, he tried to derive the isochronism of motions along the circle from the isochronism of motions along inclined planes which are chords in that circle by progressively substituting the chord by a sequence of smaller ones, thus treating the circle as polygon consisting of an infinite number of chords.[2] The comparison with this strategy suggests that Galileo tried to prove the alleged parabolic shape of the hanging chain by considering first a string with just one weight attached to its middle and then by

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[1] Nine units of twelve points each, if measured by the same units as the drawing on the other side.

[2] Galileo's letter to Guidobaldo del Monte from November 29, 1602, Galilei 1890-1909, X: 97-100, shows that Galileo already at that time considered a proof of the isochronism of the pendulum along this line.

subsequently adding weights at intermediate points, thus treating the string as a weightless cord with an infinite number of equal weights attached to it in equal distances. If Galileo would have been able to prove that, for any arrangement with a finite number of equal weights attached in equal distances to a string, the weights would lie on a parabola, it would have been reasonable for him to conclude that the links of a hanging chain also lie on a parabola. Galileo's attempt to decide whether a constellation of three weights attached in equal distances to a string fulfills this condition finds a reasonable explanation if it is interpreted as the first step of realizing such a strategy for finding a proof for the parabolic shape of the catenary.
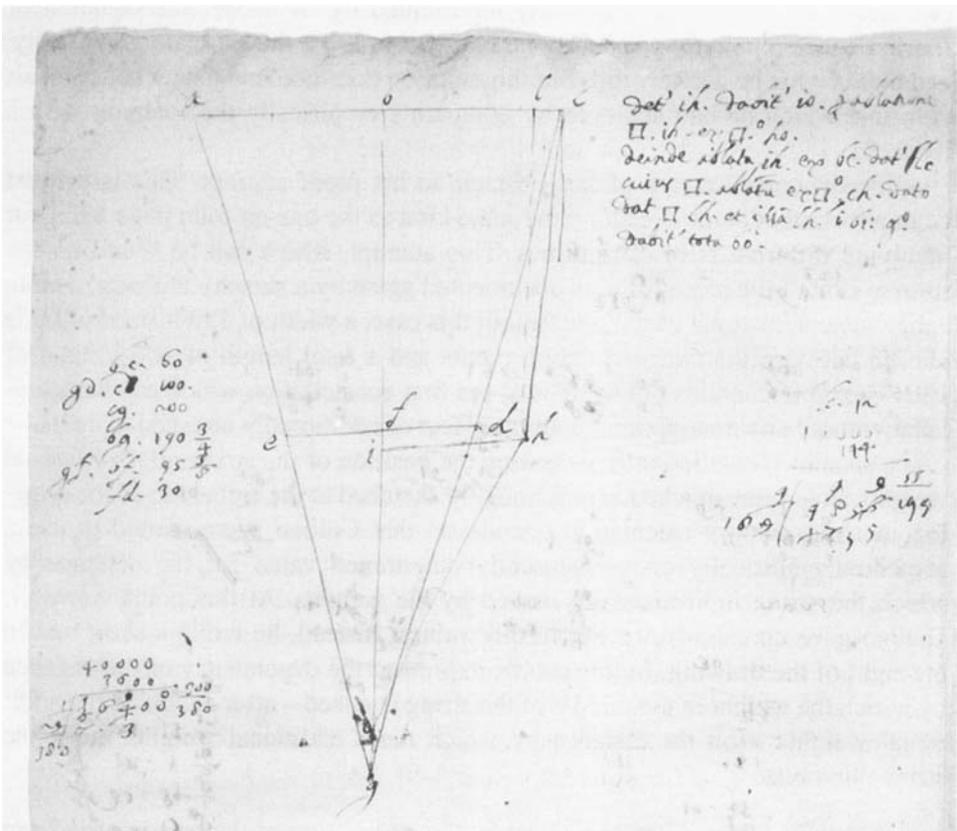

Figure 20. Ms. Gal 72, upper part of folio page 132 verso

This strategy, however, had no chance of being successfully pursued. Galileo could in fact not conclude anything from the fact that his calculation provided him with the result that his parabola check was nearly, but only nearly, fulfilled, i.e. the check whether the squares of the vertical distances of the weights from the bottom are proportional to the horizontal distances of the weights from the middle line. His attempt to prove the parabolic shape of the catenary ended up in a similar way as did his attempt to check the shape empirically. The least which Galileo would have needed to derive from the definition of the constellation representing the equilibrium was a proof that in this case the weights lie indeed on a parabola; only then could he have reasonably tried to generalize this result to more than three weights. However, the structure of the problem gave him no chance. Although his definition of the equilibrium position reduced the problem to a purely geometrical one, the relation between the motion of the added weights on their circles and the center of gravity of the entire constellation is far too complex as to be investigated by the mathematical tools available to Galileo; in modern terms, the relation is adequately represented by an irreducible equation of fourth degree. The only possibility he had was to solve the problem graphically, and that is what he actually did. But this solution does not lead to any better result than that which he had achieved by comparing empirically the catenary with a parabola.

Galileo was well aware of this obstacle to his proof attempt. This is evident from a second attempt of exactly the same kind as the one on folio page 132r, but involving different numerical values. This attempt, which can be found on the reverse side of the same folio, is documented again by a sketchy drawing, a table and some calculations (see figure 20). In this case, a width of 120 instead of 60 is chosen between the two suspension points and a total length of 400 instead of 180. The table contains only values of the first constellation which can be calculated without any assumption about the effect of additionally attached weights.

The second constellation representing the position of the string after additional weights have been attached is only roughly sketched at the right side of the drawing. A short cursory calculation documents that Galileo again started to use a somehow empirically or geometrically determined value for the distance by which the string is horizontally moved by the weights. At this point, however, Galileo gave up calculating any further values. Instead, he wrote a short text to the right of the drawing. In this text he expressed the dependency of the distance by which the weight in the middle of the string is raised – after attaching the additional weights – on the distance by which these additional weights move the string outwards.

> Let *ih* be given. *io* will be given by the subtraction of the square of *ih* from the square of *ho*. Hence by subtracting *ih* from *bc*, *lc* will be given, whose square, subtracted from the given square of *ch*, gives the square of *lh* and *lh* itself, that is *bi*: Therefore, the entire *bo* will be given.

Apparently, this text and the procedure of folio page 107 verso represent Galileo's *ultima ratio* in view of the poor outcome of his attempt to reduce the catenary to a sequence of strings with a finite number of weights attached. It turned out that, even in the simplest case, he was unable to theoretically determine the equilibrium position of the string needed for any further elaboration of the proof of the parabolic shape of the catenary which he probably intended to develop. As we will see in the next section, this failure to attain a proof of the alleged parabolic shape of the catenary did, however, not prevent Galileo from taking up the basic idea of this attempted proof in order to demonstrate another property of the chain which he intended to connect with his analysis of projectile motion, the impossibility to stretch a chain to a perfectly horizontal position.

Returning to the Dynamical Argument—The Final Proof

The failing early attempts of Galileo in Padua to empirically validate and theoretically prove the parabolic shape of the catenary are, however, not the last ones documented by his manuscripts and other contemporary sources. There is overwhelming evidence that the futile search for a satisfactory proof of the symmetry of the projectile trajectory directed his attention again to the alleged close relation between the projectile trajectory and the curve of a hanging chain due to the assumed common dynamical constellation.

Near the end of his life, Galileo composed the *Discorsi*, the final publication of the results of his life-long work on mechanics. It is known from Galileo's letters concerning this publication that the book was not really completed. As it was printed in the first edition, it ends with the *Fourth Day* which actually is the last part he managed to bring into a satisfying form to be published, apart from an appendix essentially reproducing a treatise on centers of gravity which Galileo had composed in his youth. In the following, we will argue that Galileo planned to complete the *Discorsi* with a *Fifth Day* which, among other topics, was intended to comprise a proof of the alleged parabolic shape of the catenary and an explanation of the practical utility of chains for determining projectile trajectories in artillery. It will also be shown that this *Fifth Day* eventually remained incomplete, not because Galileo had doubts about this demonstration but because of his failing health which hindered him from writing this final part of the *Discorsi* until the practical utility of chains for artillery was superseded by the introduction of another instrument, designed by Galileo's disciple Evangelista Torricelli.

As in the case of the *First Day*, the *Second Day*, and the *Third Day*, at the end of the *Fourth Day* the discussants postponed a topic for their meeting on the next day which, due to the rambling around which was characteristic of their extensive elaboration of the various topics, they were not able to complete on that day. At the end of the *First Day*, it was the main question of the resistance which bodies

have to fraction that, due to the numerous digressions, had to be postponed to the next day. The dialogue at the end of the *First Day* is typical for such announcements in the *Discorsi*:

> *Salviati*. (...) But gentlemen, where have we allowed ourselves to be carried through so many hours by various problems and unforeseen discussions? It is evening, and we have said little or nothing about the matters proposed; rather, we have gone astray in such a way that I can hardly remember the original introduction and that small start that we made by way of hypothesis and principle for future demonstrations.

> *Sagredo*. It will be best, then, to put an end for today to our discussions, giving time for our minds to compose themselves tranquilly at night, so that we may return tomorrow (if you are pleased to favor us) to the discussions desired and in the main agreed upon.

> *Salviati*. I shall not fail to be here at the same hour as today, to serve and please you. (Galilei 1974, 108)

In the case of the *Second Day*, it was the reading of the book of the "Academician" that Galileo's spokesman announced[1] and that, indeed, became the issue of the following two days. And again, at the end of the *Third Day*, they postponed the treatment of projectile motion to the *Fourth Day* .[2] Hence, in all these cases, the postponed topics indeed became the center of the discussions on the next day.

Similarly, at the end of the *Fourth Day* a topic was brought up by Sagredo and Simplicio insistently, but kept dangling by Salviati. This time it is the very topic which had inspired Galileo's work on a new mechanics at its beginning: the alleged common parabolic shape of the projectile trajectory and the curve of a hanging chain, its dynamical foundation, and the resulting utility of the chain for drawing parabolic lines.

Sagredo introduces the topic into the discussion of the projectile trajectory referring to Galileo's interpretation of the trajectory as resulting from the composition of horizontal and vertical motion:

> *Sagredo*. I observe that with regard to the two impetuses, horizontal and vertical, as the projectile is made higher, less is required of the horizontal, but much of the vertical. On the other hand, in shots of low elevation there is need of great force in the horizontal impetus, since the projectile is shot to so small a height. (Galilei 1974, 255)

In the course of the discussion of the consequences of this composition Sagredo returns to the utility of the chain for drawing parabolic lines as it was raised

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[1]   Galilei 1974, 142.
[2]   Galilei 1974, 215f.

already at the end of the *Second Day*, and Salviati announces further explanations:

> *Sagredo*. Then with a chain wrought very fine, one might speedily mark out many parabolic lines on a plane surface.

> *Salviati*. That can be done, and with no little utility, as I am about to tell you. (Galilei 1974, 257)

However, the discussion then turns away from this topic and returns instead to an argument that was used earlier in order to show that a projectile can never travel along a straight line along the horizontal, no matter how strong the impressed force driving it is (the "straightness question" which has been discussed also by other historians of science[1]). This argument is also based on the dynamical similarity between projectile motion and hanging chain. Galileo relates this property of projectile motion to the fact that it is similarly impossible to stretch a chain horizontally by whatever immense force may be applied; this claim remained to be proven and hindered Salviati for the moment from giving the announced further explanation on the relation of the projectile trajectory and the curve of a hanging chain.

The following proof deserves attention because it shows that Galileo's failed first attempt did not at all impel him to give up the underlying idea. It furthermore provides an experimental setting which makes Galileo's idea about the composition of a horizontal and a vertical force explicit which was conceived by him as to be the common dynamical basis of the projectile trajectory and the curve of the hanging chain. His proof uses a small weight hanging from the middle of a chain which is supported by two nails and stretched by two immense weights hanging from the loose ends of the chain; the proof essentially follows a line of reasoning similar to that discussed in the previous section. After the completion of this proof, the discussants return to the question of the utility of chains, which is now, however, definitely deferred to the next day, the *Fifth Day* of the *Discorsi*:

> *Simplicio*. (...) And now Salviati, in agreement with his promise, shall explain to us the utility that may be drawn from the little chain, and afterwards give us those speculations made by our author about the force of impact.

> *Salviati*. Sufficient to this day is our having occupied ourselves in the contemplations now finished. The time is rather late, and will not, by a large margin, allow us to explain the matters you mention; so let us defer that meeting to another and more suitable time. (Galilei 1974, 259)

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[1] See Drake and Drabkin 1969, 103f.

A first clue for answering the question of what Galileo had in mind when he announced further explanations of the raised topic is provided by later comments of Vincenzio Viviani.[1] When he became Galileo's assistant in the second half of 1638, he began to study Galileo's works and must have carefully read the *Discorsi* as soon as they became available. When Viviani came across the first of the above quoted passages on the utility of the chain, he made the following marginal note in a copy of Galileo's book:[2]

> By means of this small chain perhaps Galileo found the elevations to hit a given target.

Viviani's note hits the nail on the head. It points to a problem that remained essentially unsolved in the *Fourth Day* of the *Discorsi*, that is, the lack of a satisfying theory of oblique projection.[3] Viviani assumed that the utility of the chain for Galileo must have been related to projectile motion and, in particular, to practical purposes of artillery. In the *Fourth Day* of the *Discorsi* Galileo presented, precisely in view of such practical purposes, tables relating the angles of shots to the amplitudes, altitudes, and sublimities of the resulting parabolic trajectories. These tables were, however, of limited use for artillery – even leaving aside problems such as air resistance etc. – since they do not relate the position of a given target to the properties of a shot. Indeed, the tables do not give the full trajectory but only certain key parameters. It was therefore plausible to supplement them with a way of constructing the trajectory that would be of more direct use to gunners. Viviani's remark suggests that Galileo still intended to use the alleged relation between parabola and catenary and the technique suggested by the projectile experiment reported in Guidobaldo des Monte's notebook, which has been discussed already,[4] in order to precisely fill this gap.

In another manuscript note of Viviani, he even considered the possibility that Galileo made use of chains also for more theoretical purposes, again in the con-

[1] Viviani's comments discussed in the following were the basis of an earlier analysis of the role of the chain in the planned *Fifth Day* of the *Discorsi* by Raffaello Caverni, see Caverni 1972, V: 137-154. Caverni even claims to have found a substantial part of a dialogue on the chain supposedly written either by Galileo or by Viviani following Galileo. The authenticity of this alleged text has, however, been questioned, see Favaro 1919-1920. But in spite of the dubious character of this document, it has nevertheless also been taken seriously by modern historians of science, see Galilei 1958, 834-837. The text given by Caverni appears authentic in particular because it refers to a number of actually existing manuscripts in Galilei ca. 1602-1637, including folio 41/42 discussed above. The dialogue published by Caverni describes even how the curves on this folio page were drawn by Galileo with the help of carbon powder. Our analysis of the inks used on this folio page has shown, however, that these curves have actually been drawn by ink so that Caverni's dialogue on the utility of the chain is now definitely identified as a forgery, see Working-Group 1996.
[2] This statement was written by Viviani on the margin of page 284 of the first edition of the *Discorsi* next to the passage in the first edition of the *Discorsi*: "potersi et ancora con qualche utilità non piccola come appresso vi dirò." Viviani's copy with this remark is kept in the Biblioteca Nazionale Centrale in Florence as Ms. Gal. 79.
[3] See the analysis of Galileo's failed attempt in Damerow, Freudenthal, McLaughlin, and Renn 1992, chap. 3.
[4] See the discussion of the folios 113r and 41/42 in note 2 above.

text of projectile motion. Referring to the same passage of the *Discorsi*, Viviani wrote:[1]

> See at page 284 [sic! not 384 as written by Caverni], the last phrase, which benefit Galileo meant, whether of measuring the parabolic line, or whether of the way of finding the propositions concerning projectile motion.

Viviani thus took into account the possibility that Galileo might still have in mind the theoretical program suggested by the projectile trajectory experiment of exploiting the alleged common dynamical foundation of the trajectory and the curve of a hanging chain as a heuristic device in order to find propositions on projectile motion.

In contrast to the modern historian, Viviani was in the unique position to simply ask Galileo what these obscure references to the utility of the chain in the *Discorsi* really meant – which is what he actually did when he was living with Galileo from late 1638 until the latter's death in 1642.[2] When he later included recollections of this period in a book which he published in 1674, Viviani explained what he had learned from Galileo himself about the utility of the chain and its relation to projectile motion:

> Now all I have left to say is how much I know about the use of chains, promised by Galileo at the end of the *Fourth Day*, referring to it as he intimated when, he being present, I was studying his science of projectiles. It seemed to me then that he intended to make use of some kind of very thin chains hanging from their extremities over a plane surface, to deduce from their diverse tensions the law and the practice of shooting with artillery to a given objective. But of this Torricelli wrote adequately and ingeniously at the end of his treatise on projectiles, so that this loss is compensated.

> That the natural sac of such chains always adapts to the curvature of parabolic lines, he deduced, if I remember well, from a reasoning similar [to this]: Heavy bodies must naturally fall according to the proportion of the momentum they have from the places from which they hang, and these momentums of equal weights, attached to points of a balance [which is] supported by its extremities, have the same proportion as the rectangles of the parts of that balance, as Galileo himself demonstrated in the treatise on resistances. And this proportion is the same as the one between straight lines, which from the points of that same balance [taken] as the base of a parabola, can be drawn in parallel to the diameter of this parabola, according to the theory of conic sections. All the links of the chain, that are like so many equal weights hanging from points on that straight line which con-

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[1] See Ms. Gal. 74, folio page 33r, kept in the Biblioteca Nazionale Centrale in Florence. For a transcription (erroneously referring to folio page 23r) see Caverni 1972, V: 153.
[2] See Drake 1987, 394.

nects the extremities where this chain is attached and serves as base of the
parabola, have in the end to fall as much as permitted by their momentums
and there [they have to] stop, and [they] must stop at those points where
their descents are proportional to their momentums from the places where
those links hang, in that last instant of motion. These then are those points
which adapt to a parabolic curve as long as the chain and whose diameter,
which raises from the middle of the said base, is perpendicular to the hori-
zon. (Viviani, Vincenzo 1674, 105f)

The first part of Viviani's text confirms what we have discussed above, that Gali-
leo intended to introduce the chain as an instrument by which gunners could
determine how to shoot in order to hit a given target. The main part of Viviani's
text represents the sketch of a proof based on the determination of the moments
of weights hanging from a beam supported at its two ends and on the assumption
that the links of the chain descend according to these moments. If Viviani's report
on that proof is reliable, it implies that Galileo had planned to crown his life-long
reflections about the relation between projectile trajectory and chain with a proof
of the alleged parabolic shape of the catenary, a proof that would have become a
key subject of the never-finished *Fifth Day* of the *Discorsi*.

The reliability of Viviani's report is confirmed by a folio page in Galileo's own
hand, folio page 43r, in which one finds a chain line (marked by little rings) rep-
resenting a projectile trajectory, short texts, and some calculations (see figure 21).
The texts perfectly confirm Viviani's report on Galileo's intentions to continue
the ideas developed at the end of the *Fourth Day*. They deal with three issues
related to the end of the *Discorsi* and contain:

- a brief explanation of the utility of chains for purposes of artillery
- a sketch of the resolution of the "straightness question" based on the
  dynamical justification of the alleged relation between projectile trajec-
  tory and hanging chain
- a note on the main idea of the proof sketched by Viviani.

The first text concerning the practical utility of the chain reads:

Let the little chain pass through the points f and c, and, given the target z,
stretch the chain so much that it passes through z, and you will find the dis-
tance sc and the angle of elevation etc. (Galilei ca. 1602-1637, folio page
43r)

This text fits the interpretation given above that, for Galileo, the practical utility
of the chain consisted in determining the shooting angle necessary in order to hit
a given target if other parameters, in this case the amplitude of the parabola, are
given. Immediately underneath Galileo sketched how he planned to resolve the
"straightness question":

> It is to be demonstrated that, just as it is impossible to stretch a chain into a straight line, it is likewise impossible that the projectile ever travels along a straight line, if not along the perpendicular upwards, just as also the chain as a plumb-line stretches itself along a straight line.

The text sketches the line of argument followed by Galileo at the end of the *Fourth Day.* The third text on folio page 43r is a short note written next to the chain line and evidently referring to magnitudes in Galileo's diagram:

> The heavy body in *g* presses with less force than in *s* according to the proportion of the rectangle *fgc* to the rectangle *fsc*.

Just as in the longer explanation by Viviani, this short text by Galileo also focuses on the "rectangle" which corresponds to the product of the two parts of the baseline of the catenary, whose division is obtained by vertically projecting a given point of the catenary onto this base-line. Clearly, Galileo's note refers to the same proof-idea as Viviani's sketch. We can therefore indeed rely on this more explicit text in order to reconstruct the proof of the parabolic shape of the catenary which Galileo intended to incorporate into the *Fifth Day* of the *Discorsi.*
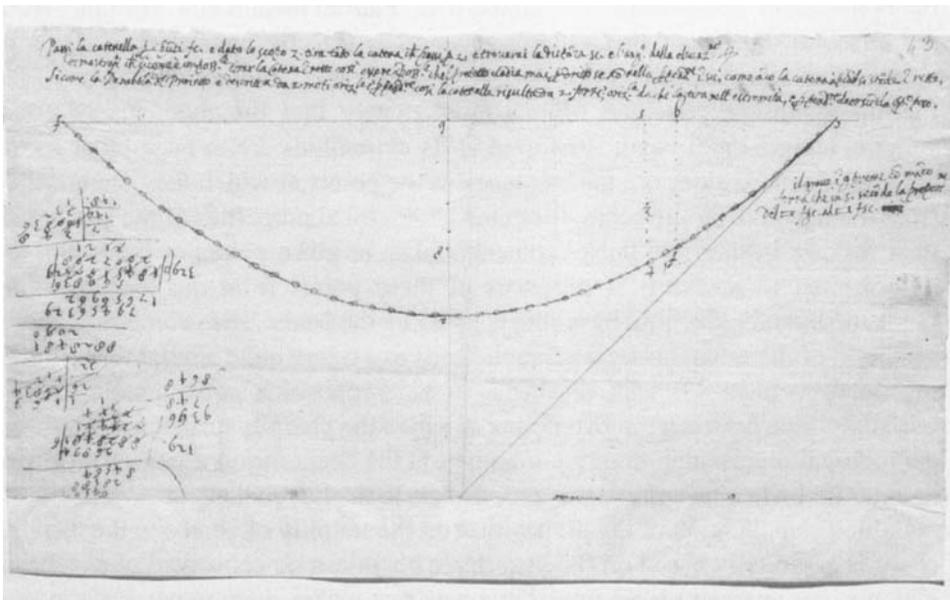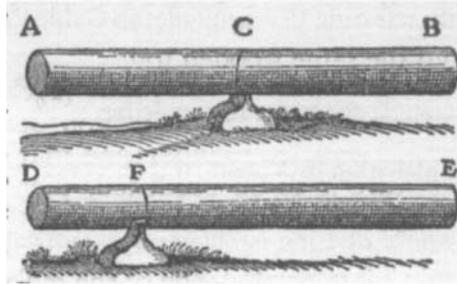

Figure 21. Ms. Gal. 72, folio page 43 recto

Viviani's text implies that the basis for the proof is a theorem on the resistance of a beam supported at both ends, proven by Galileo in his theory on the strength of materials. The theorem determines the proportion between limit resistances to breaking of a cylinder supported at both ends by the inverse proportion of the rectangles whose sides are the distances of the breakage points from the two ends:

If two places are taken in the length of a cylinder at which the cylinder is to be broken, then the resistances at those two places have to each other the inverse ratio [of areas] of rectangles whose sides are the distances of those two places [from the two ends.][1]

In the proof of this proposition the forces, represented by weights hanging down from the beam, are determined which are necessary at any particular place to break the beam. In a manuscript version (the "Pieroni manuscript") of this part of the *Discorsi*, the proof of Galileo's theorem is directly expressed in terms of moments of weights, just as it is done in Viviani's text.[2] In the formulation of the Pieroni manuscript, it then follows immediately that the moments of equal weights hung from a beam supported at its extremities are to each other as the rectangles whose sides are the distances of the points at which they are attached from the two ends of the beam. From the geometrical properties of the parabola it then follows further that these moments, taken at given points of the beam, are proportional to the vertical distances of these points from the corresponding points of a parabola whose base-line is given by the beam. The geometrical representation of the moments by a parabola leads to a figure quite similar to the drawing on folio page 43r with the beam of the proposition corresponding to the horizontal line between the two points at which the chain is suspended. Thus, the geometrical representation of the moments at the beam makes clear what Galileo had in mind when he applied this proposition to the hanging chain.

  Galileo's application of the proposition on the stability of beams to the hanging chain is essentially based on the idea that a chain can be conceived of as a beam which is cut in small pieces linked in a way that makes them move down in pro-

1   Galilei 1974, 133, corresponding figure taken from Galilei 1890-1909, VIII: 175.
2   Galilei 1890-1909, VII: 176; for a discussion of the concept of moment see Galluzzi 1979.

portion to the moments or forces acting on them as if they were weights suspended from the corresponding points of the beam. Viviani indeed argues that the links of a chain can be considered as so many little weights suspended from the beam and that the descent of these little weights from the horizontal is proportional to their moments. The latter proportionality follows from that between effect and cause if the moments of the links of the chain are considered to be the causes of their descents, a conclusion familiar also from other parts of Galileo's mechanics.

What is wrong with this proof of an evidently fallacious statement? The basic idea of the proof is correct also from the viewpoint of classical mechanics, only that Galileo did not take into account that actually the number of links of a hanging chain is not equally distributed over the horizontal. Given a fixed distance of the suspension points at the ends of the chain, Galileo's basic assumption deviates the more from the real situation the greater the length of the chain is, so that it is more steeply hanging down at its ends.

From the viewpoint of classical mechanics, Galileo's assumption merely amounts to the determination of an approximation. His construction can fairly well be justified by the fact that he, of course, did not have available the mathematical means necessary to take the length of the hanging chain within a given horizontal interval into account, instead of using the length of the horizontal beam as a model.

In principle, this type of approximation is characteristic of all physical laws, which indeed at some time in a revised theory of the future appear, at best, as approximations. There is no greater difference between Galileo's chain that stands for real chains as that between mass points that stand for extended rigid bodies, ideal gases that stand for real gases, and Newtonian masses that stand for rest masses as conceived in relativity theory.

From the viewpoint of Galileo, however, there was no reason for considering his argument as dealing only with an approximation, or even more, to consider it as fallacious. He was simply arguing, as any physicist does, in a framework of a given mechanical theory. His proof involves basic concepts of his mechanics, such as the concept of momentum, and does not contain any obvious "errors" if taken within his conceptual system. His conceptualization of the chain as consisting of links that are able to move independently from each other along the vertical according to his mechanical model of the hanging chain seems problematical from a modern point of view but must have appeared quite natural to Galileo in view of the dynamical justification he could give for the comparison with the case of projectile motion.

In view of the striking success that this proof of the parabolic shape of the catenary must have represented for the closure of Galileo's theory of projectile motion, it is all the more surprising that it was neither incorporated into the version of the *Discorsi* published in 1638 nor into the drafts for the *Fifth Day* that have survived. In order to reconstruct the fate of this proof and the role Galileo

intended for it in this last day of the *Discorsi*, we have to briefly recapitulate the history of the final composition of this book. The reconstruction of this final composition will also allow us to determine at least approximately in which period this proof was first formulated by Galileo.

After Galileo's condemnation in 1633 he returned to the scientific work that had been central to his concerns before he made his telescopic discoveries and engaged in his struggle for Copernicanism, that is, the theory of motion and the strength of materials. Even shortly after his condemnation, when he was still in Siena as a guest of the archbishop Ascanio Piccolomini, he began to write on the strength of materials, composing most of the treatise later contained in the *Second Day* of the *Discorsi*.[1] While a substantial part of the insights to be incorporated into this treatise had been attained already during Galileo's Paduan time, it was only now that he derived the propositions making up the final part of that treatise, including the crucial proposition quoted above for the derivation of the parabolic shape of the catenary.[2]

By mid-1635 much of the material later to be incorporated into the first two Days of the *Discorsi* had been completed.[3] Meanwhile, Galileo had formed the idea of composing four dialogues dealing with both the strength of materials and the theory of motion.[4] In the same year 1635, he probably also began to rework and edit his material on motion, dealing with motion along inclined planes, projectile motion, and the force of percussion for inclusion into this larger publication. After various failed efforts to find a publisher for the planned book, Galileo finally reached an agreement with Elzevir in May 1636.[5] In mid-1636, he managed to complete the *Third Day* (dealing with motion along inclined planes) and sent it to Elzevir.[6] Next Galileo turned to working on projectile motion, to be treated in the *Fourth Day*; by the end of 1636 he also had decided to amplify the original project of the book by an appendix containing his early work on centers of gravity.[7] In March 1637 Galileo sent an incomplete version of the *Fourth Day* to Elzevir, comprising the announcement of a further *Day* treating the force of percussion.[8] By that time, Galileo must have been working already on a dialogue on this topic.[9] An extensive draft of this part of the *Fifth Day* has survived and

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[1]   See e.g. Galileo to Andrea Arrighetti, September 27, 1633, Galilei 1890-1909, XV: 283f and for historical discussion Drake 1987, 356.
[2]   How far Galileo's work on the strength of materials was progressing can be inferred from his contemporary correspondence; see e.g. Niccolò Aggiunti to Galileo, September 10, 1633, Galilei 1890-1909, XV: 257f.
[3]   See Fulgenzio Micanzio to Galileo, April 7, 1635, Galilei 1890-1909, XV: 254, and Galileo to Elia Diodati, June 9, 1635, Galilei 1890-1909, XV: 272f.
[4]   Galileo to Elia Diodati, June 9, 1635, Galilei 1890-1909, XV: 272f.
[5]   See e.g. Galileo to Fulgenzio Micanzio, June 21, 1636, Galilei 1890-1909, XVI: 441f, and for historical discussion Drake 1987, 374.
[6]   Galileo to Fulgenzio Micanzio, June 28, 1636, Galilei 1890-1909, XVI: 445, and Galileo to Fulgenzio Micanzio, August 16, 1636, Galilei 1890-1909, XVI: 475.
[7]   Galileo to Elia Diodati, December 6, 1636, Galilei 1890-1909, XVI: 524.
[8]   Galileo to Elia Diodati, March 7, 1637, Galilei 1890-1909, XVII: 41f, and Fulgenzio Micanzio to Galileo, March 7, 1637, Galilei 1890-1909, XVII: 42.
[9]   See Drake 1987, 383.

was first printed in a later edition of Galileo's collected works; when exactly this draft was written remains unknown.[1] This draft dialogue on percussion was clearly written without the intention to include the topic of the chain. It thus corresponds to the original announcement of a further Day early in the *Fourth Day*. The incomplete version of the *Fourth Day* which Galileo sent in March 1637 ended with the tables on projectile motion and still lacked the treatment of the chain at the end of the printed version; it also still lacked the reiteration of the announcement of a further Day at the end of the printed version, now amplified by the theme of the catenary. In other words, by March 1637 Galileo did not yet dispose of the proof of the parabolic shape of the catenary.

In May and June of 1637, Galileo sent further material to Elzevir, probably comprising the Appendix on centers of gravity and also material for the *Fourth Day*.[2] In September 1637, the Dutch printers complained that they still had not received the manuscript of the *Fifth Day* and sent Galileo a memorandum to that effect.[3] In November Elzevir acknowledged having received from Galileo another folio related to the *Fourth Day*.[4] By the latest at this point, but possibly already in June, the *Fourth Day* was concluded in the way it later appeared in print, that is, comprising the treatment of the chain. Since we know from the analysis of folio page 43r that the main argument of this concluding section, dealing with the "straightness" question, was sketched at a time when Galileo possessed the proof idea for his demonstration of the parabolic shape of the catenary, it seems plausible to assume that this proof was conceived at some point between March and November 1637.

Since February 1637 Galileo suffered from problems with his sight which delayed his work on the *Fifth Day*.[5] In November Elzevir informed Galileo that he would go on with the printing but, if possible, await the completion of the *Fifth Day*.[6] In the beginning of January 1638 Elzevir offered to complete the *Discorsi* according to Galileo's orders if he should be unable to do it himself:

> Concerning the treatise on percussion and on the use of the chain, if you cannot bring it to perfection, I will complete it according to your order. (Louis Elzevir to Galileo, January 4, 1638, Galilei 1890-1909, XVII: 251)

By the end of January, however, the fate of the *Fifth Day* was sealed, at least for the first edition of the *Discorsi*. At that point in time, Elzevir requested from Galileo everything that was still needed in order to finalize the book and suggested to

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[1] See Galilei 1974, 281-306 and for a discussion of the chronology of Galileo's work on the *Fifth Day* see Galilei 1890-1909, VIII: 26-33.
[2] Fulgenzio Micanzio to Galileo, May 2, 1637, Galilei 1890-1909, XVII: 71f, Fulgenzio Micanzio to Galileo, May 9, 1637, Galilei 1890-1909, XVII: 76f, and Fulgenzio Micanzio to Galileo, June 20, 1637, Galilei 1890-1909, XVII: 114f.
[3] See Justus Wiffeldich to Galileo, September 26, 1637, Galilei 1890-1909, XVII: 187f.
[4] Fulgenzio Micanzio to Galileo, October 17, 1637, Galilei 1890-1909, XVII: 199f and Louis Elzevir to Galileo, November 1, 1637, Galilei 1890-1909, XVII: 211.
[5] See Drake 1987, 384.
[6] Louis Elzevir to Galileo, November 1, 1637, Galilei 1890-1909, XVII: 211.

him to add an explanation concerning the absence of material on the force of percussion, if none was to be included.[1]

What was the fate of the *Fifth Day* after the *Discorsi* had been finally published in 1638? Galileo continued to work on his theory of motion, clearly also because he was dissatisfied with the exposition of this theory in his book. Following a suggestion of his disciple Viviani, who became his assistant in the second half of 1638, Galileo first turned to a problem in the logical foundation of his theory of motion, whose solution he intended to insert into the second edition. He focussed his attention to problems of the deductive structure of his theory also because he found it difficult to elaborate new theorems given his problems of sight, as he wrote in a letter to Baliani of 1639.[2] But in the same letter, he also wrote that he planned to enrich a future edition of the *Discorsi* with material on other scientific subjects, including the force of percussion, had he ever a chance to do so. This plan must have comprised also the promised treatment of the catenary.

This plan remained, however, unrealized. Not only was it difficult for Galileo to bring his numerous hitherto unpublished scientific results into a publishable form in view of his failing health, but he must have definitely abandoned his original plan to deal with the utility of the chain for artillery in a separate day of the *Discorsi* when he discovered that his theory of projectile motion had meanwhile been substantially elaborated by somebody else, Evangelista Torricelli.

When Galileo, in March 1641, saw Torricelli's treatise on motion, he not only found there that Torricelli had succeeded in solving some of the key problems of Galileo's theory of projectile motion, such as the derivation of the parabolic shape of the trajectory in the case of oblique projection, but also that Torricelli had himself designed an instrument that would make these insights useful for gunners, thus effectively replacing the chain in its presumed utility for artillery.[3] Instead of elaborating another treatise dealing with novel physical problems such as percussion, Galileo thus settled for securing what he had already achieved and pursued his approach to polish the deductive foundation of his theory of motion. He therefore began, in October 1641, to compose a dialogue on the theory of proportions which he dictated to Torricelli.[4] From its beginning, it is clear that this dialogue was intended to replace the earlier plan of a *Fifth Day* on the catenary and on percussion and was supposed to directly follow the Appendix and the *Fourth Day* of the *Discorsi* as they were published in 1638.

The unfortunate fate of the *Fifth Day* of the *Discorsi* apparently definitely sealed also the fate of the chain as a key subject of the theory of motion inaugurated by Galileo. Definitely? Well, not quite. It saw a striking revival in a context we had to neglect here, that of Galileo's study of the motion of the pendulum.

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[1] Louis Elzevir to Galileo, January 25, 1638, Galilei 1890-1909, XVII: 265.
[2] Galileo to Giovanni Battista Baliani, August 1, 1639, Galilei 1890-1909, XVIII: 78.
[3] Torricelli 1919 and Benedetto Castelli to Galileo, March 2, 1641, Galilei 1890-1909, XVIII: 303.
[4] See Drake 1987, 421f and Giusti 1993.

Figure 22. Vincenzio Viviani's Application of a hanging chain for determining the length of a pendulum with a given time period

We have seen above that Galileo's faithful disciple Viviani made sure that Galileo's proof of the parabolic shape of the catenary was preserved for posterity by including it in a book reporting on Galileo's unpublished achievements. But Viviani also thought of giving practical significance to Galileo's discovery, even after the chain had lost, as we have also pointed out above, its practical utility for gunners which Galileo had in mind due to a new instrument introduced by Torricelli. Viviani considered instead the practical utility of the supposedly parabolic shape of the catenary for determining the lengths of pendulums swinging with a certain desired period of time.[1] He designed an instrument consisting of a horizontal rod whose one half is divided into 60 equal parts with a chain hanging underneath the entire rod (see figure 22). If the distance between the vertex of the chain and the rod is chosen to be such that a pendulum of that length would swing with a period of one second, then the length of a pendulum swinging with any given fraction of a second can be found by first selecting the corresponding value on the scale along the horizontal rod. The distance between the rod at that point and the corresponding point of the chain underneath gives the desired length of the pendulum. The line of a hanging chain, supposedly of parabolic shape, is hence used by Viviani as a mechanical representation of the functional relation between times and lengths of the pendulum.

When and How did Galileo Discover the Law of Fall?

At the beginning of this investigation the problem has been raised whether the standard answer to the question of when and how Galileo made his celebrated discoveries of the law of fall and of the parabolic shape of the projectile trajectory is correct. But even after our extensive examination of the historical evidence, comprising some hitherto neglected or unknown documents, the difficulties of answering this question did not disappear. On the contrary, the apparent answer to this question achieved here makes it evident that this question does not really hit the point. Whatever answer will be given, it necessarily reduces the origin of a new conceptual structure, which is the outcome of a complex human interaction determined by both tradition and innovation, to the activities of one individual subject, Galileo Galilei. If the reconstruction of the history of the discovery as it is given here is reliable, an answer to this question necessarily detaches his activities from the context which made them meaningful in the social process of emerging knowledge.

At least in the case of the discovery of the law of fall and the parabolic shape of the projectile trajectory, the context of discovery is indistinguishably intermingled with the context of its justification. Neither the statement of the law of fall

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[1]   See Viviani, Vincenzio after 1638, folio page 64r. For a transcription see Caverni 1972, IV: 428.

nor that of the parabolic shape of the trajectory are *per se* meaningful. Since they do not correspond to any immediate experience they have to be theoretically justified in order to attain their exceptional status within the body of mechanical knowledge. Without any theoretical context they can not be related to practical experiences with falling or projected bodies, a context on which the truth of these statements heavily depends. They were only the results of a collective process which created classical mechanics and thus provided Galileo's discoveries with that meaning which is implicitly presupposed in the question of when and how Galileo made them. It is this context that gives to statements such as the law of fall the appearance of being empirical facts which are relatively independent of their theoretical justification by means of proofs within a particular representation of mechanical knowledge.

However, this context of later developments was not the historical context of Galileo's discoveries. The context which made up the stage for Galileo was rather determined by a moderately anti-Aristotelian conception of motion which Galileo shared with contemporaries such as Benedetti and Sarpi and which formed the basis of his treatise *De Motu*. It is obvious that none of them had at the beginning any idea of the law of fall. Consequently, they could not draw any conclusion from such a law concerning the shape of the projectile trajectory. Moreover, the question of according to which law acceleration takes place would probably not even have been a meaningful question to them. Galileo, at least, explicitly discussed in *De Motu* the acceleration of a falling body as an accidental phenomenon. The issues which actually bothered Galileo and some of his contemporaries at that time were problems such as the question of what ratio exists between the speeds of different falling bodies. This question, for instance, was generally considered to make perfect sense, only the correct answer to this question being a controversially disputed, open problem. Another problem of this type was the question of what the true cause of speed and slowness of natural motion really was. A further question was, how exactly the process of impressing motive force into a body functions. How can natural and forced motion interact? In contrast to these questions which were considered to be questions that could unambiguously be settled, a variety of different explanations for the initial acceleration of the motion of a falling body were conceivable, non of them making the question of what the law is according to which this acceleration takes place into a meaningful question. In short, with regard to acceleration, there really seemed to be nothing to discover in this context!

Galileo's *De Motu* contains, on the other hand, a number of arguments which can well be considered as important discoveries, although they do only make sense in the historical context they were raised. We have shown above, for instance, that the concept of neutral motion which in *De Motu* complements the concepts of natural and forced motion and which to a certain extent contradicted assumptions on motion which, in spite of his anti-Aristotelian attitude, he shared with Aristotle, was an essential precondition that permitted Galileo (contrary to

Guidobaldo who did not share Galileo's preoccupation with a science of motion) to identify the parabolic shape of the trajectory and to infer from this shape the law of fall. In a similar way, the proposition that the ratio of speeds of bodies moving down inclined planes with the same vertical heights is equal to the inverse ratio of the corresponding lengths of the planes, became an important pre-condition for the development of the theory which Galileo finally published in his *Discorsi*. Given that neither Galileo's concept of motion nor his concept of speed corresponds to the notions of classical mechanics, both examples do not represent discoveries in the sense which is inherent in the question of when and how Galileo discovered the law of fall. Even if Galileo would have derived this law from observations as they are analyzed in *De Motu*,[1] or if he would have found the law experimentally he could not have recognized it as *the* important achievement he considered it later.

When and how has the law of fall been discovered? Is the discovery to be dated to the moment when somebody for the first time happened to stumble upon the idea that the spaces traversed by a falling body might increase in the same proportion as the squares of the times? We have argued that in the context of a theory of motion as the one Galileo and several of his contemporaries adhered to at the time of *De Motu*, a discovery of the law of fall in that sense could not have been recognized as a substantial challenge of this *De Motu* theory. According to our reconstruction, this situation occurred indeed a short time later when Galileo performed together with Guidobaldo del Monte the projectile trajectory experiment recorded in Guidobaldo's notebook. The outcome of this experiment appears with hindsight as a pathbreaking achievement which forced Galileo to give up his misconception of the projectile trajectory and to develop the idea of its parabolic shape. It seems therefore that the now available evidence for Galileo's role in performing this experiment, which is provided by the note about its outcome in Paolo Sarpi's notebook, demonstrates unambiguously that the discovery must be dated into the year 1592.

But does it really make sense to attribute a discovery to somebody who gives this discovery quite a different meaning? What Galileo reported to Sarpi must have been perceived by him as a discovery. But what he considered to be the discovery was probably the observation – amazing in the context of the *De Motu* theory – that the trajectory seemed to be symmetrical like a hyperbola or a parabola. The fact that in his protocol Guidobaldo left open whether the curves achieved by the experiment were parabolic or hyperbolic and also the fact that Sarpi's note does not say anything at all about the precise mathematical shape of the trajectory makes it, on the other hand, questionable that the precise shape of

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[1]   This would not have been absolutely impossible. Galileo came close to the discovery of the isochronism of chords in a circle which, in fact, he discovered only shortly afterwards. Together with his considerations on the speeds and forces on inclined planes, this discovery makes it nearly possible to infer the law of fall. He only needed the relation between speeds and times for motions along inclined planes in the form of his later "postulate" as he used it in the *Discorsi*; see Damerow, Freudenthal, McLaughlin, and Renn 1992, 156-158.

the trajectory was the point that mattered to them, even though Galileo could not have had any trouble in recognizing its close connection to the dynamical interpretation of the experiment. The discovery of the symmetry, however, was amazing because it seemed to indicate that natural and forced motion, although allegedly quite different in nature, were symmetrically exchanging in ascending and descending their roles. This was not to be expected, but could nevertheless easily be made compatible with the prevailing conception of natural and forced motion.

Another aspect of the "discovery" was surely considered by both Galileo and Guidobaldo to be even more important, namely, the (fallacious) conclusion that the same explanation can be given for the symmetrical shape of the catenary and that of the projectile trajectory; they assumed that both curves result in the same way from the interaction of a horizontal and a vertical force. As we have shown, Galileo believed that to be true till the end of his life when the law of fall for a long time already had become the cornerstone of his new science of mechanics and he regarded the discovery of the parabolic shape of the trajectory—as he claimed in the conflict with Cavalieri—as "the first fruit" of his studies and as a "flower (...) broken from the glory." This re-evaluation of the discovery of the parabolic shape of the projectile trajectory was not accompanied by a reinterpretation of its early dynamical justification. On the contrary, he even underlined once more the importance of the common dynamical explanation of catenary and trajectory when he planned to make the proof of the parabolic shape of the catenary a final highlight of the *Fifth Day* of the *Discorsi*.

Given that neither Guidobaldo nor Galileo initially and partly even later fully recognized the theoretical consequences of the outcome of their projectile trajectory experiment, does it then make more sense to date the discoveries of the parabolic trajectory and the law of fall not to the year when they performed it but rather to a later time when Galileo realized its implications? Should the discovery, for instance, be attributed to his first interpretation of the curve generated in the experiment as a parabola and not as a hyperbola or any other similar curve, thus accepting as a consequence the validity of the law of fall? Or does it make more sense to date the discoveries even later to the time when he was able to prove the law of fall and the parabolic shape of the trajectory? Should perhaps even stronger criteria be applied? Can he, for instance, be credited already with the discovery of the law of fall as long as he was still erroneously proving it from the assumption that the "degrees of velocity" increase proportional to the spaces traversed?[1] Or should the discovery be attributed to him only when he had found the proof which he finally published in the *Discorsi*? Is it relevant for this attribution that even this proof is still not a proof valid in classical mechanics? Or is the famous inclined plane experiment as an empirical demonstration of the validity of the law of fall a better indication of the discovery of the law of fall than any

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[1] For this assumption see the letter by Galileo to Paolo Sarpi, October 16, 1604, Galilei 1890-1909, X: 115f and the discussion in Damerow, Freudenthal, McLaughlin, and Renn 1992, chap. 3.

theoretical speculation? Galileo later claimed that when he had repeated this experiment "a full hundred times, the spaces were always found to be to one another as the squares of the times." (Galilei 1974, 170) Does it depend on whether such a claim is true that Galileo can be credited to have discovered the law of fall? Any dating later than 1592 when the projectile trajectory experiment was performed for the first time has obviously to take an act of interpreting this experiment as an indication of the discovery and is thus liable to doubts whether such a discovery can really be established as an indubitable historical fact.

What date after the day in the year 1592 when Galileo and Guidobaldo performed the experiment might be considered as the day when Galileo discovered the parabolic shape of the trajectory or the law of free fall? It has been shown that the "practical turn" in Galileo's life after his move to Padua made him look at the "discovery" in a specific way. Knowledge such as the recognition of the parabolic shape of the trajectory seemed at that time to be relevant to him only insofar as it was applicable to the solution of technological problems. He made, for instance, the "discovery" the basis of his intended treatise on artillery which, however, was never written. But even if he had written this treatise, it would probably have added nothing to what he knew already before. Galileo's experiments and studies in that period made him an experienced engineer-scientist, but did not substantially change his interpretation of the projectile trajectory experiment in the theoretical framework of a revised *De Motu* theory.

Turning to the time when he returned to the study of motion, we are apparently better off. The letter to Guidobaldo of 1602 which attests to this reorientation may be regarded as a testimony for a changed relation to Guidobaldo del Monte and his way of drawing the boundaries of mechanics and of validating a discovery. At that time the law of fall was used already substantially as a means of proving other propositions in order to assure the validity of observations such as the isochronism of inclined planes in a circle or, without success, the isochronism of the pendulum. What else should be necessary to credit him with the "discovery" of the law? The available evidence does, however, in no way indicate a dramatically new interpretation of the projectile trajectory experiment that could justify taking this reorientation as representing the real "discovery" of the law of fall.

The following period when Galileo already intensively worked on a deductive theory of motion based on the law of fall as its core theorem provides good reasons to worry whether stronger criteria should not perhaps be used for accepting a scientific activity as attesting to the discovery of a central theorem of classical mechanics such as the law of fall. In fact, at that time Galileo still had no answer to the problem raised by Sarpi who objected against Galileo's dynamical interpretation of the symmetry of the trajectory that an arrow shot vertically upwards has a much greater force than an arrow falling down from the maximum height of the shot. The theoretical program Galileo offered instead, as it becomes visible in his letter to Sarpi written in October 1604, is far from being convincing from the viewpoint of classical mechanics. Galileo intended to built up a theory based

essentially on one fallacious principle which he expected to be able to cover "the other things," which probably refers to such diverse topics as those of the former *De Motu* theory enriched by his experiences as a practitioner, the theoretical discussions in his early years in Padua, in particular those with Sarpi about natural motion, the force of percussion, the isochronism of the pendulum, the length-time relation of the pendulum, and, of course, the law of fall and the projectile trajectory.

By returning to the origin of the "discovery," that is the experiment performed together with Guidobaldo del Monte, Galileo topped his ambitious theoretical program with the challenge of still another issue, that is, the derivation of the catenary. The situation became worse when Galileo realized that his claim that the catenary is a parabolic curve just as the projectile trajectory could not be verified empirically except for very flat hanging chains. For understandable reasons Galileo did what he apparently always did in such a situations, he trusted a proof which he believed to be true within his theoretical framework more than the outcome of an experiment. He did so for good reasons. As an experienced practitioner he knew many reasons why an experiment could fail. It would have been silly to give up such beliefs as the truth of the law of fall, the parabolic shape of the projectile trajectory, and the parabolic shape of the catenary only because he could only approximately demonstrate their validity by some experimental arrangements. Given such circumstances, what can then be a reliable distinction between a discovery and an error? What meaning can be ascribed to a statement such as "Galileo discovered the law of fall," or "Galileo discovered the parabolic shape of the projectile trajectory" when he "discovered" in exactly the same way also the parabolic shape of the catenary? Galileo finally stuck to everything he thought to be able to prove; his deductive theory in the *Discorsi* is the final outcome of his discoveries, representing an integration that legitimately can be considered as the outset of the development of a new science of motion although the development of this theory was completed only long after his death. As we have shown extensively in our study of the origins of classical mechanics, it was not individual achievements of Galileo designated as "discoveries" but their participation in a collective process of constructing a new body of knowledge that made his activities meaningful in spite of the seemingly chaotic path of his stumbling from one error into the other.[1] We hope that the internal consistency of the activities of Galileo, which is rather denied than confirmed by a description of them as a series of "discoveries," has been made evident by the reconstruction presented here.

According to our opinion, the conclusion from this reconstruction can be generalized. Independent of the contemporary systematic contexts of a developing body of knowledge and the contexts of its practical application, single elements of a structured body of knowledge such as the mental representations of the

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[1]   Damerow, Freudenthal, McLaughlin, and Renn 1992.

accelerated motion in free fall or of the trajectory of projectile motion are just meaningless tokens that trigger the phantasy of those who are separated from these contexts by a historical distance. Outside of their own contexts which make them meaningful, the actions of discoverers such as Galileo appear erratic and confront the historian of science only with a series of unsolvable riddles. The activities of the heroes in the history of science as well as the activities of the numerous practitioners on whose shoulders they stand regain their meaning only from the reconstruction of the continuity and change of the processes that transform these contexts. Hence, trying to solve the riddles of the history of science by determining the exact points in time in which the great discoveries of human history were made and describing how precisely they took place is nothing else but hunting the white elephant.

Appendix A: Selected Letters

The appendix comprises four letters which are partially quoted in the main text, a letter by Guidobaldo del Monte to Galileo from 21 February 1592, a letter by Galileo to Guidobaldo del Monte from 29 November 1602, a letter from Paolo Sarpi to Galileo from 9 October 1604, and Galileo's response to Sarpi from 16 October 1604. Although most of these letters are well-known and have often been discussed in the literature, they are here presented in a new English translation prepared by Fiorenza Z. Renn and June Inderthal. Previous translations into English were either partial translations or have misrepresented key passages of these letters. According to the argument of our paper, these letters have to be read as being closely related to each other. They provide a glance at two crucial intellectual contexts for Galileo's research on the law of fall.

Letter of Guidobaldo del Monte to Galileo in Pisa, February 21, 1592.

My Most Magnificent and Honorable Lord.

Because I did not have news of your Lordship for many days, I got my son Horatio to ask you [about them]. From what I see, I realize that your Lordship has written [letters] to me on previous occasions and I did not receive them, just as I did not receive that one which your Lordship told me you have written to me concerning your father's death. Indeed, when I heard about it, I was very sorry, both for the love of him and for the love of your Lordship; he did not seem so old to me that he could not have lived many more years. I condole with your Lordship, but we must be content with these upsets which the world deals us.

It also saddens me to see that your Lordship is not treated according to your worth, and even more it saddens me that you are lacking good hope. And if you intend to go to Venice in this summer, I invite you to pass by here so that for my part I will not fail to make any effort I can in order to help and to serve you; because I certainly cannot see you in this state. My forces are weak but, whatever they may be, I will employ them all in serving you. And I kiss your hands, as well as those of S.r Mazzone if he happens to be in Pisa. May the Lord grant your wishes.

In Monte Baroccio, 21st February 1592.
From your Lordship's Servant

Guidobaldo dal Monte. (Galilei 1890-1909, X: 46f)

Letter of Galileo to Guidobaldo del Monte in Montebaroccio, November 29, 1602.

Your Lordship, please excuse my importunity if I persist in wanting to persuade you of the truth of the proposition that motions within the same quarter-circle are made in equal times, because having always seemed to me to be admirable, it seems to me [to be] all the more so, now that your Most Illustrious Lordship considers it to be impossible. Hence I would consider it a great error and a lack on my part if I should allow it to be rejected by your speculation as being false, for it does not deserve this mark, and neither [does it deserve] being banished from your Lordship's understanding who, better than anybody else, will quickly be able to retract it [the proposition] from the exile of our minds. And because the experiment, through which this truth principally became clear to me, is so much more certain, as it was explicated by me in a confused way in my other [letter], I will repeat it here more clearly, so that you, by performing it, would also be able to ascertain this truth.

So now I take two thin threads of equal length, each being two or three braccia long, and let them be *AB*, *EF*. [I] hang them from two small nails, *A* and *E*, and at the other ends, *B* and *F*, I tie two equal lead balls (although it would not matter if they were unequal). Then, by removing each of the above-mentioned threads from its perpendicular, but one very much [so], as through the arc *CB* and the other very little, as through the arc *IF*; I let them go freely at the same moment of time. The

one begins to describe large arcs, like *BCD*, and the other describes small ones, like *FIG*; but yet the mobile *B* does not consume more time moving along the whole arc *BCD* than the other mobile *F* in moving along the arc *FIG*. I make absolutely sure of this in the following way:
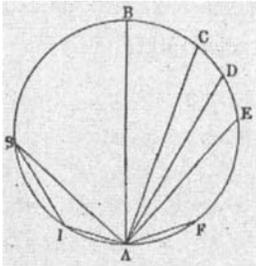
The mobile *B* moves along the large arc *BCD*, returns along the same *DCB*, and then comes back towards *D*, and it does this 500 and 1000 times, reiterating its oscillations. Likewise, the other one goes from *F* to *G*, and from here returns to *F*, and will likewise make many oscillations; and in the time that I count, let us say, the first hundred large oscillations *BCD*, *DCB* etc., another observer counts another hundred very small oscillations through *FIG*, and he does not count even a single one more: a most evident sign that each particular of these very large [oscillations] *BCD* consumes as much time as each particular of those minimal ones [through] *FIG*.

Now, if all *BCD* is passed [through] in as much time as *FIG*, then, in the same way, half of them, these being descents through the unequal arcs of the same quadrant, will be done in equal times. But even without staying on to enumerate other [oscillations], your Most Illustrious Lordship will see that the mobile *F* will not make its very small oscillations more frequently than the mobile *B* [will make] its larger ones, but rather, they will always go together.

The experiment which you tell me you have done in the box can be very uncertain, either because its surface has perhaps not been well cleaned or maybe because it is not perfectly circular, and because one cannot observe so well in a single passage the precise moment in which the motion begins. But if your Most Illustrious Lordship still wants to take this concave surface, let the ball *B* go freely from a great distance, such as from point *B*. It will pass to *D*, at the beginning producing its oscillations with large intervals, and at the end with small ones; but the latter, however, [will not be] more frequent in time than the former.

With regard now to the unreasonable opinion that, given a quadrant 100 miles in length, two equal mobiles might pass along it, one the whole length, and the other only a span, in equal times, I say it is true that there is something wondrous about it; but [less so] if we consider that a plane can be at a very slight incline, like that of the surface of a slow-moving river, so that a mobile will not have traversed naturally on it more than a span in the time that another [mobile] will have moved one hundred miles over a steeply inclined plane (namely being equipped with a very great received impetus, even if over a small inclination). And this proposition does not involve by any adventure more unlikeliness than that in which triangles within the same parallels, and with the same bases, are always equal [in area], while one can

make one of them very short and the other a thousand miles long. But staying with the subject, I believe I have demonstrated this conclusion to be no less unthinkable than the other.

In the circle *BDA*, let the diameter *BA* be erected on the horizontal, and let us draw from the point A to the circumference any lines *AF, AE, AD, AC*: I demonstrate identical mobiles falling in equal times both along the perpendicular *BA*, and along the inclined planes of the lines *CA, DA, EA, FA*; so that, by starting at the same moment from the points *B,C,D,E,F*, they will reach the end point *A* at the same time, and let the line *FA* be as small as we want it to be.

And maybe even more unthinkable will appear the following, also demonstrated by me; that wherever the line *SA* being not greater than the chord of a quadrant, and [given] the lines *SI* and *IA*, the same mobile, starting from *S*, makes the journey *SIA* quicker than just the journey *IA*, starting from *I*.

Until now I have demonstrated without transgressing the terms of mechanics; but I cannot manage to demonstrate how the arcs *SIA* and *IA* have been passed through in equal times and it is this that I am looking for.

Please do me the favor of kissing the hand of Sig.r Francesco in return, telling him that when I have a little leisure, I will write to him about an experiment which has already entered my imagination, for measuring the moment of the percussion. Regarding your question, I consider that what your Most Illustrious Lordship said about it was very well put, and that when we begin to deal with matter, because of its contingency the propositions abstractly considered by the geometrician begin to change; since one cannot assign certain science to the [propositions] thus perturbed, the mathematician is hence freed from speculating about them.

I have been too long and tedious for your Most Illustrious Lordship: please excuse me, with grace, and love me as your most devoted servant. And I most reverently kiss your hands.

In Padua, 29th November 1602
From Your Illustrious Lordship's Most Obliged Servant

Galileo Galilei. (Galilei 1890-1909, X: 97-100)[1]

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[1] Since the original of this letter has not been preserved the diagrams may not be reliable.

Letter of Paolo Sarpi to Galileo in Padua, October 9, 1604.

My Most Excellent Lord and Most Respected Master.

With the occasion to send you this enclosure, I thought of proposing to you an argument to be resolved, and a problem which keeps me in doubt.

We have already concluded that a body cannot be thrown up to the same point [termine] if not by a force, and, accordingly, by a velocity. We have recapitulated – so Your Lordship lately argued and originally found out [inventò ella] – that [the body] will return downwards through the same points through which it went up. There was, I do not remember precisely [non so che], an objection concerning the ball of the arquebus; in this case, the presence of the fire troubles the strength of the argument. Yet, we say: a strong arm which shoots an arrow with a Turkish bow completely pierces through a table; and when the arrow descends from that height to which the arm with the bow can take it, it will pierce [the table] only slightly. I think that the argument is maybe slight, but I do not know what to say about it.

Here is the problem: if there are two bodies different in species, and any of them receives a force that is smaller than that of which it is capable; if now the force is communicated to both of them, will they receive the same amount of it? Thus, if gold were able to receive from a maximum force [an amount of] 20 and not more, and silver of 19 and not more; if they are now moved by a force of 12, will they both receive a force of 12? It seems that this is the case because the force is entirely communicated, the body is capable [of receiving it], hence the effect is the same. It seems [on the other hand] that this is not the case because, [if it were so], two bodies of different species, driven by the same force, would reach the same point with the same velocity.

If someone said: a force of 12 will move silver and gold to the same point but not with the same velocity. Why not [we may respond] if both of them are capable of receiving even more than that which [the force of] 12 can communicate to them?

I do not want to oblige Your Lordship to answer. Just in order not to send this paper blank, which had a peripatetic appetite of being filled with these characters, I wanted to satisfy it, acting like the agent does with the prime matter. And now, I stop here and kiss your hands.
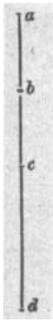
In Venice, 9th October 1604
Your Most Excellent Lordship's Most Affectionate Servant

Brother Paulo from Venice (Galilei 1890-1909, X: 114)

Letter of Galileo to Paolo Sarpi in Venice, October 16, 1604.

Very Honorable Lord and Most Cultivated Father.

Thinking again about the matters of motion, in which, to demonstrate the phenomena [accidenti] observed by me, I lacked a completely indubitable principle which I could pose as an axiom, I am reduced to a proposition which has much of the natural and the evident: and with this assumed, I then demonstrate the rest; i.e., that the spaces passed by natural motion are in double proportion to the times, and consequently the spaces passed in equal times are as the odd numbers from one, and the other things. And the principle is this: that the natural moveable goes increasing in velocity with that proportion with which it departs from the beginning of its motion; as, for example, the heavy body falling from the point a along the line $abcd$, I assume that the degree of velocity that it has at c, to the degree it had at b, is as the distance ca to the distance ba, and thus consequently, at d it has a degree of velocity greater than at c according as the distance da is greater than ca.

I should like your Honorable Lordship to consider this a bit, and tell me your opinion. And if we accept this principle, we not only demonstrate, as I said, the other conclusions, but I believe we also have enough in hand in order to show that the naturally falling body and the violent projectile pass through the same proportions of velocity. For if the projectile is thrown from the point d to the point a, it is manifest that at the point d it has a degree of impetus sufficiently powerful to drive it to the point a, and not beyond; and if the same projectile is in c, it is clear that it is linked with a degree of impetus sufficiently powerful to drive it to the same point a, and, in the same way, the degree of impetus in b is sufficient to drive it to a, whence it is manifest that the impetus at points d, c, b goes decreasing in the proportions of the lines da, ca, ba; whence, if it goes acquiring degrees of velocity in the same (proportions) in natural fall, what I have said and believed up to now is true.

Concerning the experiment with the arrow, I believe that it does acquire during its fall a force that is equal to that with which it was thrown up, as we will discuss together with other examples orally, since I have to be there in any case before All Saints. Meanwhile, I ask you to think a little bit about the above mentioned principle.

Concerning the other problem posed by you, I believe that the same bodies receive both the same force, which, however, does not create the same effect in both; as for example the same person, when rowing, communicates his force to a gondola and to a larger boat, both being capable of assuming even

more of it, but it does not result in one and in the other [boat] the same effect concerning the velocity or the distance-interval through which they move.

I am writing in the dark, this little may rather suffice to satisfy the obligation of answering than that of finding a solution of which to speak orally I reserve to a meeting in the near future.

And with all respect I kiss your hands.

In Padua, 16th October 1604
Your Very Honorable Lordship's Most Obliged Servant,

Galileo Galilei (Galilei 1890-1909, X: 115f)

Appendix B: Galileo's Claims from the Perspective of Modern Physics

BY DOMENICO GIULINI

Part 1. On Galileo's Exaggerations

That Galileo somewhat exaggerated the outcome of experiments described in his *Discorsi* is often suspected. Leaving alone the question as to why this might happen, it seems useful to also produce some precise quantitative estimations of such suspected exaggerations. This we shall do in the present part of this appendix for the famous case concerning the isochronism of the pendulum. Compare e.g. Drake 1990, chapters 1 and 14.[1]

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[1] With respect to this example S. Drake states on p. 210-211 that "when the arc to the vertical for the pendulum having the wider swing is no more than 25°, the difference in times for it and the other pendulum is not very great and it keeps on diminishing." After all, the following quantitative estimation shows that such differences are observable after at most 20 swings.

Our estimations will be based on the *exact* formula for the period of a pendulum *without friction*.[1]

In a famous part towards the end of the first 'day' of the *Discorsi* (Galilei 1974, 87-88), Galileo (i.e., Salviati) gives the following account of an experiment:

> Ultimately, I took two balls, one of lead and one of cork, the former being at least a hundred times as heavy as the the latter, and I attached them to equal thin strings four or five braccia long, tied high above. Removed from the vertical, these were set going at the same moment, and falling along the circumferences of the circles described by the equal strings that were the radii, they passed the vertical and returned by the same path. Repeating their goings and comings a good hundred times by themselves, they sensibly showed that the heavy one kept time with the light one so well that not in a hundred oscillations, nor in a thousand, does it get ahead in time even by a moment, but the two travel with equal pace. The operation by the medium is also perceived; offering some impediment to the motion, it diminishes the oscillations of the cork much more than those of the lead. But is does not make them more frequent, or less so; indeed, when the arcs passed by the cork were not more than five or six degrees, and those of the lead were fifty or sixty, they were passed over in the same times.

Taking for the *braccio* 0.6 meters and hence the length of the pendulum between 2.4 and 3.0 meters, we see that we talk about periods certainly larger than 3 seconds.

The amplitude α is taken to be the angle between the thread of the pendulum and the vertical (direction of the gravitational field). The exact expression for the period T as function of α is an elliptic integral of first kind whose expansion in terms of $\sin(\alpha/2)$ begins as follows (Sommerfeld 1994 Reprint, Mechanik):

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[1] Friction has two effects:1) It leads to an exponential damping of the amplitudes, 2) it enhances the period by an amount depending on the damping. The first affects our considerations insofar as we will calculate accumulated phase differences for pendulums of substantially different amplitudes. Hence we must check that the actual damping indeed allows to maintain such a difference in amplitudes for the considered periods of accumulation. Regarding 2) we need to estimate this effect since it threatens to level our calculated phase difference which is solely based on the enhancement of the period with amplitude.

Applied, as below, to a situation of two pendulums, one with large amplitude and small damping, the other with smaller amplitude because of stronger damping, we see that *both* pendulums will suffer an enhancement of their periods, albeit from different sources. However, the estimation of the enhancement due to damping is easily done and shows that a levelling of these two effects does not occur. To see this, let σ denote the number of full swings after which the amplitude has dropped by a factor of e^{-1}, the difference ΔT to the undamped period T is then given by $\Delta T/T = (8\pi^2\sigma 2)^{-1}$ (plus higher powers in $(2\pi\sigma)^{-2}$, which we can safely neglect). Hence the corresponding number of swings after which a phase difference of $2\pi/n$ to the undamped pendulum has occurred is given by $N_n = \sigma^2 8\pi^2/n$. Note in particular the *quadratic* dependence on σ and the relatively large prefactor $8\pi^2 \approx 79$. They imply that even for a considerable damping, like $\sigma = 5$, we would have to wait around 200 full swings to see a phase difference to the undamped pendulum of $2\pi/10$. This is a much smaller effect than the one discussed below.

$$T(\alpha) \; = \; 2\pi \sqrt{\frac{l}{g}} \left\{ 1 + \frac{1}{4} + \sin^2\frac{\alpha}{2} + \frac{9}{64}\sin^4\frac{\alpha}{2} + \dots \right\}. \qquad (A1)$$

Hence the period increases with the amplitude resulting in the lead-pendulum falling behind the cork-pendulum. We denote by $N_n(\alpha)$ the smallest integer number of full swings beyond which a pendulum of constant amplitude α will have fallen behind a time of at least T/n against a pendulum of period sufficiently close to $T := 2\pi\sqrt{l/g}$ (i.e. of sufficiently small amplitude, like 3°). After N_4 full swings the phase difference is at least $\pi/2$ and certainly detectable by be naked eye, since then the pendulums start to move in opposite directions. More careful but still unsophisticated observations should reveal deviations from synchrony by, say, one tenth of T, that is, after N_{10} swings.[1]

By definition of $N_n(\alpha)$ we have

$$N_n(\alpha) \; = \; \text{smallest integer} \geq \frac{T}{n \cdot (T(\alpha) - T)}. \qquad (A2)$$

Using $(A1)$ we get for the various values of α and $n = 4$ or $n = 10$:

α	10°	15°	20°	25°	30°	35°	40°	70°	80°
N_4	132	59	33	21	15	11	9	3	2
N_{10}	53	24	14	9	6	5	4	2	1

From these values we infer that a situation with amplitudes $\alpha_{\text{lead}} = 25°$ and $\alpha_{\text{cork}} = 3°$ certainly cannot have appeared synchronous for longer than about 20 full swings.

The situations becomes even more drastic in a later description of a similar experiment, reported shortly after the beginning of the fourth day (Galilei 1974, 226). In this second experiment two balls of lead are suspended on equally long strings of 4-5 braccia and the periods compared for amplitudes $\alpha_1 = 5°$ and $\alpha_2 = 80°$ (!). Here again the assertion is that no deviations from synchrony could be detected, whereas our values for N_4 show that it must have been clearly apparent after 3 full swings the latest. After 4 full swings the two pendulums will even cross the origin approximately simultaneously with oppositely directed velocities.[2]

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[1] For example, by letting two separate experimenters count and voice the passages of zero amplitude for the two pendulums respectively. Such a method is in fact suggested in the *Discorsi* (Galilei 1974, 227).

[2] In Galilei 1974, footnote 12 on page 227, S. Drake states that "a disagreement of about one beat in thirty should occur with pendulums of length and amplitudes described here." Unfortunately he did not state how he arrives at this result, which, seen from our analysis, seems to be an underestimation of the real effect by more than a factor of 3.

Part 2: Theory of the Hanging Chain and its Parabolic Approximation

In this part we first describe the usual theory, $\mathbf{T}_{ex}$, of the hanging chain in terms of Newtonian concepts, and then its approximation, $\mathbf{T}_{ap}$, for small slopes y', which leads to parabolic shapes as would be the case for constant mass distributions along the horizontal projection of the chain. On the level of physical quantities ("Observables") this approximation corresponds to expansions in terms of d/D to various degrees, depending on the observable, where

$$2D = \text{horizontal distance of the suspension points}$$

and d = sag, i.e. the distance between the lower apex and the horizontal line joining the suspension points.

The Exact Theory $\mathbf{T}_{ex}$

We will think of the hanging chain as being given by a function $y(x)$ in a Cartesian xy-plane. A point in this plane is denoted by its coordinates (x,y), so that the curve is the set of points $(x,y(x))$, where x ranges over an interval which we take to be $[-D,D]$. y' *and* y'' denote the first and second derivatives of y with respect to x.

The fundamental equation for the theory of the chain is obtained from a simple and typical argument based on a local application of the principle of *balance of forces*. To do this, we imagine the chain being cut at $(x, y(x))$ and consider one end. We denote by $F(x)$ the strength of the force that one would have to apply to one end in order to keep the corresponding part of the chain in its place. This is also called the chain's tension. We can decompose $F(x)$ into a horizontal component, $F_h(x)$, and a vertical component, $F_v(x)$. Since by definition a chain can only support tangential forces, these components must satisfy

$$y'(x) = \frac{F_v(x)}{F_h(x)}. \tag{B1}$$

If the external force (gravitation) has no horizontal component, $F_h(x)$ must in fact be independent of x. Otherwise a piece of chain with different strengths of the outward pointing horizontal forces could not stay at rest; hence

$$F_h(x) = F_h = \text{const.}$$

Differentiating $(B1)$ once more then leads to

$$y''(x) = \frac{F'_v(x)}{F_h}. \tag{B2}$$

It is now easy to express the right hand side of (B2) as function of x and $y'(x)$, since $F_v(x + dx) - F_v(x)$ must clearly be equal to the weight of the piece of chain between $(x, y(x))$ and $(x + dx, y(x + dx))$. If we denote by μ the mass per unit length of the chain, which we take to be constant[1], then its weight is given by $\mu g \, ds(x)$, where $ds(x) = \sqrt{1 + [y'(x)]^2} \, dx$ is the length of the (infinitesimal) piece of chain that we consider. Hence (B2) results in

$$\frac{y''}{\sqrt{1 + [y']^2}} = \frac{1}{h} := \frac{\mu g}{F_h}, \qquad (B3)$$

which is our fundamental equation defining $\mathbf{T}_{ex}$ ['ex' for exact].

Upon integration with boundary data $y(x = \pm D) = 0$ one obtains the famous cosh-form[2]

$$y(x) = h \left[\cosh(x/h) - \cosh(D/h)\right]. \qquad (B4)$$

For an engineer, say, it would be more appropriate to eliminate the non geometric parameter h in favour of the *length* of the chain, given by

$$L := \int_{-D}^{D} dx \sqrt{1 + [y']^2} = 2h \sinh(D/h), \qquad (B5)$$

or its sag

$$d := -y(x = 0) = -h[1 - \cosh(D/h)] = 2h \sinh^2(D/2h), \qquad (B6)$$

i.e., to solve (B5) for $h(L, D)$ or (B6) for $h(d, D)$ respectively and insert this into (B4). But, unfortunately, this cannot be done in terms of elementary functions. Hence (B4) (B5) or (B4) (B6) should be thought of as *implicit* representation of the hanging chain as function of the parameters L, D or d, D respectively.

Finally, the total tension $F(x)$ of the chain is easily computed:

$$F(x) = \sqrt{F_h^2 + F_v^2(x)} = F_h \sqrt{1 + [y'(x)]^2} = \mu g h \cosh(x/h), \qquad (B7)$$

which, using (B4), can also be read as saying that F grows linearly in y.

The Approximating Theory $\mathbf{T}_{ap}$

Galileo's approximative modelling of the hanging chain by a parabola can be understood within the larger context of an *approximation of theories*. It is

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[1] The following formula (B3) remains valid for variable μ. It then implies that the hanging chain can be made to assume *any* convex shape by letting $\mu > 0$ vary appropriately along the chain.
[2] An equivalent form, obtained by applying the addition laws for cosh-functions, is

$$y(x) = 2h \sinh((x + D)/2h) \sinh((x - D)/2h). \qquad (B4')$$

obtained as *first* approximation of the fundamental equation (*B*3) for small slopes *y'*. Such approximations clearly make sense only for *y'* < 1, which is just the regime for which the parabolic approximation of the hanging chain is claimed in the relevant part of the *Discorsi* (Galilei 1974, 256-257; see The Neglected Issue: Trajectory and Hanging Chain). Hence we expand the square-root in (*B*3) in terms of powers of *y'* and truncate the second and all higher powers. But since *y'* appears already in squared form under the square-root, this amounts to simply replacing this square-root by 1. *From the derivation of* (*B*3) *it is clear that this is equivalent to taking the mass-distribution as homogeneous along the horizontal projection* (*x-axis*) *rather than being homogeneous along the proper length. This, in turn, is precisely the [implicit] assumption that underlies the application of Galileo's results on the distribution of moments along a solid and homogeneous cylindrical beam which rests horizontally supported at both ends; see the main text.* In first approximation one simply expands (*B*3) in terms of powers of *y'* discarding the second and all higher powers.

The fundamental equation that defines the approximating theory $\mathbf{T}_{ap}$ is now simply given by:

$$y'' = \frac{1}{h} \qquad (B3')$$

and for the same boundary data as above one obtains

$$y(x) = \frac{1}{2h}(x^2 - D^2). \qquad (B4')$$

Formally this corresponds to a quadratic expansion of the cosh-function in (*B*4) in terms of the dimensionful parameter $1/h$, which should be understood as expansion in terms of a dimensionless parameter $(\frac{1}{h}) \times$ (intrinsic length) $\cong D/h$. The sag, *d*, is now given by the simple formula

$$d = \frac{D^2}{2h}, \qquad (B6')$$

which, in contrast to the exact theory, can now be easily solved for *h*. This allows us to explicitly parameterise the curve by the geometric quantities *d* and *D*. An expansion in terms of *D/h* is hence equivalent to an expansion in terms of *d/D*.

Note that in general it will not be the case that the exact expressions of an approximating theory correspond to certain approximations of the exact theory, but only that simultaneous expansions in both theories coincide up to some order. For example, the expression for the length *L* in $\mathbf{T}_{ap}$ has the complicated structure

$$L = \int_{-D}^{D} dx \sqrt{1 + (x/h)^2} = D[\sqrt{1 + (D/h)^2} + (h/D)\operatorname{asinh}(D/h)] \quad (B5')$$

but the quadratic expansions of $(B5)$ and $(B5')$ in terms of d/D (i.e. in terms of D/h and then h eliminated using $(B6')$ both lead to

$$L = 2D\left[1 + \frac{2}{3}\left(\frac{d}{D}\right)^2\right]. \qquad (B5'')$$

The same holds for the total tension, which in $\mathbf{T}_{\mathrm{ap}}$ takes the form

$$F(x) = \mu g h \sqrt{1 + (x/h)^2}, \qquad (B7')$$

whereas the quadratic expansions of $(B7)$ and $(B7')$ in terms of d/D coincide in the following "engineer-formula"

$$F(x) = \mu g \frac{D^2}{2d}\left[1 + 2\left(\frac{x}{D}\right)^2\left(\frac{d}{D}\right)^2\right]. \qquad (B7'')$$

Finally we can raise the question of how to grade the quality of the approximation of $\mathbf{T}_{\mathrm{ex}}$ by $\mathbf{T}_{\mathrm{ap}}$. This can be done for each observable (here observables are e.g. $y(x)$, d, $F(x)$ and L) by looking at the orders of the first non-vanishing correction to $\mathbf{T}_{\mathrm{ap}}$ by $\mathbf{T}_{\mathrm{ex}}$. Let O_{ex} and O_{ap} be the values of an observable on "corresponding" [see below] solutions of the fundamental equation of $\mathbf{T}_{\mathrm{ex}}$ and $\mathbf{T}_{\mathrm{ap}}$ respectively. Then one considers

$$\Delta(O) := \frac{O_{\mathrm{ex}} - O_{\mathrm{ap}}}{O_{\mathrm{ex}}} \qquad (B8)$$

and defines as usual $o(\Delta(O))$ to be that integer which characterises the leading order in the expansion of $\Delta(O)$ with respect to the expansion parameter (here d/D). The grade $g(O)$ of the expansion can then be defined as

$$g(O) := o(\Delta(O)) - 1. \qquad (B9)$$

In our case we obtain

$$g(y(x)) = g(d) = 1, \quad g(F(x)) = g(L) = 3. \qquad (B10)$$

From the definition of $\mathbf{T}_{\mathrm{ap}}$ together with $y' = \sinh(x/h) = x/h + \dots$ one could not have expected a grade of approximation better than 1 [linear approximation]. But, as we just saw, the approximation might come out to be much better. This mirrors a well known phenomena in physics: that some formulae "are better than their

derivation". In our case this is for example true for the tension (formulae $(B7')$ $(B7'')$), which deviates from the exact expression only in *fourth order* in d/D, thereby slightly *underestimating* the real tension.

Finally we wish to comment on the notion of *corresponding solutions*. In order to define a correspondence one has to make a choice of preferred observables whose values uniquely fix solutions of the fundamental equations $(B3)$ and $(B3')$. Solutions with coinciding values on these observables are then defined to correspond to each other. Such a definition should therefore always be thought of as *relative* to the choice of preferred observables. So, for example, for given horizontal distance $2D$ of the suspension points one may either take the horizontal tension F_h (as we did) or the length L or the sag d to fix the solution. A non-trivial consequence of this general observation is that the grade of an approximation of some observable will in general *depend* on the choice of preferred observables which are used to fix the solutions.

Acknowledgment

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