Galileo’s Experiments with Pendulums: Then and Now

Contents

1. INTRODUCTION

The pendulum was crucial throughout Galileo's career. The properties of the pendulum that Galileo was fascinated with from very early on concern especially time. A 1602 letter (cf. T1) is the earliest surviving document in which Galileo discusses the hypothesis of the pendulum's *isochronism*.[1] In the letter, Galileo claims that all pendulums are isochronous. He says that he has long been trying to demonstrate isochronism *mechanically*, but that so far he has been unable to succeed (see T1, for the qualification of the proof as *mechanical*). From 1602 onwards Galileo referred to pendulum isochronism as an admirable property but failed to demonstrate it.

The pendulum is the most "open ended" of Galileo's artefacts. After working on my reconstructed pendulums for some time, I became convinced that the pendulum had the potential to allow Galileo to break new ground. But I also realized that its elusive nature sometimes threatened to destabilize the progress that Galileo was making on other fronts. It is this ambivalent nature that, I thought, might prove invaluable in trying to understand the crucial aspects of Galileo's innovative methodology.

To explore Galileo's innovative methodology, I have repeated most of his path-breaking experiments with pendulums; I have investigated the robustness of pendulum effects, otherwise difficult

[1] Isochronism is the property of certain physical systems to oscillate at a constant frequency regardless of the amplitude of the oscillations. We now know that simple pendulums are not isochronous. Note that *isochronism* is not a Galilean word. As the reader will see (cf. the texts in Appendix 3) Galileo uses other, revealing turns of phrase to refer to this property. I will keep to *isochronism*, though, since it has become common in the literature.

to capture, with computer simulations; and I have repeated crucial calculations done by Galileo. In this paper, I will relate the discoveries that I made, and emphasize their significance for our understanding of Galileo's innovative methodology.

I am not the first to have been beguiled by Galileo's pendulums.[2] Ronald Naylor, who contributed the most to our understanding of Galileo's work with pendulums, reconstructed Galileo's experiments long ago. He summarizes his findings as follows. "One of Galileo's most renowned discoveries was the isochronism of the simple pendulum. In the *Discorsi*, Galileo used this discovery to good effect – though his claim that the pendulum was isochronous for all arcs less than 180° has created something of a puzzle for the history of science. The question arises as to how far the evidence available to Galileo supported his claims for isochronism."[3] Naylor concludes that "Galileo was almost certainly familiar with a much wider range of evidence than he indicated in the *Discorsi*. The examination of the evidence available to Galileo indicates that, though it provided ample support for his thesis, it was certainly not as conclusive as he implies in the *Discorsi*. It also seems clear that Galileo was bound to be aware of this".[4] What did the "much wider range of evidence" really consist in? What did Galileo know that he was not

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[2] Cf., for example, Koyré 1953, Ariotti 1968, 1972, Drake 1975, 1995, 1999, Naylor 1974, 1977, 2003, MacLachlan 1976, Costabel 1978, Hill 1994, Machamar & Hepburn 2004, and Matthews 2004.

[3] Naylor 1974, p. 23.

[4] Naylor 1974, p. 23. The *Discorsi* referred to by Naylor are the *Two new sciences*.

willing to make public? One scholar has gone so far as to indict Galileo of knowingly publishing false assertions.[5]

Isochronism is only one pendulum property that fascinated Galileo. Indeed he put the pendulum to many different and ingenious uses. He experimented with lead and cork bobs, for example, in order to investigate the naturally accelerated motion of different materials. The historian and physicist, James MacLachlan, who also reconstructed Galileo's experiments, argued that Galileo's "observations with balls of cork and lead" are "an imaginary experiment", and that Galileo's "claim that the period of a pendulum is independent of amplitude" was "based more on mathematical deduction than on experimental observation".[6] So did Galileo not really perform his experiments, as also Alexandre Koyré thought?[7]

More or less the same conclusion was reached by Pierre Costabel, but the embarrassment that Galileo caused to him might have been avoided, if Costabel had repeated the experiments; for, he would have seen that there is nothing impossible in what Galileo has to say about cork and lead pendulums (as we shall see in the next section).[8]

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[5] "Galileo […] published some things (the isochronism of the circular pendulum) which he knew to be false" (Hill 1994, p. 513). In my view, Hill's conclusion is untenable, and will be discussed in section 3.

[6] MacLachlan 1974, p. 173.

[7] Koyré 1966.

[8] "It is also clear that Galileo deceived himself: if he had followed step-by-step , rigorously, the constitutive elements of his experiment, he should have concluded that theoretical presuppositions implied the expectation of the disagreement of the pendulums; by claiming that the experiment is negative, Galileo embarrasses us: has he really posed a question to nature, but an ill-posed

The scholars mentioned so far have all been intrigued by what I call the "matching" problem, i.e., the question of whether Galileo's reports really match the outcome of his experiments. In my view, this approach has been too narrowly focused, and has restricted the scope of inquiry. Further, the matching problem rests on arbitrary, often anachronistic assumptions about what constitutes good or bad empirical evidence for a theoretical claim— a question which has hardly ever been raised in this Galileo literature (perhaps not even thought to be urgent). There is another dubious assumption, often underlying this literature, namely, that texts reporting experimental results can be understood by a perspective solely internal to the texts (obviously except in cases, such as Naylor's and Thomas Settle's studies[9], where the experiments have been repeated by the authors).

In my approach I try to avoid these pitfalls. I have suspended the question of the matching problem, and similar evaluative questions. Further, I make an effort not to presume I understand

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one? It is better to think that he did not do the experiment [Il est ainsi très claire que Galilée s'est abusé: s'il avait suivi davantage pas à pas, en rigueur, les élements constitutifs de son expérience, il aurait dû conclure que les présupposés théoriques imposaient l' attente d' un désaccord des pendules. En affirmant que l' expérience est négative, Galilée nous met dans un profond embarrass: a-t-il réellement mis à l' épreuve une question posée à la nature, mais une question mal posée? Mieux vaut penser que l' épreuve précise n'à pas été faite]" (Costabel 1978, p. 6). Costabel doubts that Galileo could have done the experiments with cork and lead pendulums since, in Costabel's view, Galileo's reasoning about the experiment is erroneous. Costabel's argument only shows his misunderstanding of what Galileo's experiments with cork and lead pendulums are really about and intended to give evidence of (cf. section 2).

[9] Cf. Settle's ground-breaking reconstruction of the inclined plane experiment (Settle 1961).

texts evoking experience, and whose meanings appear at first glance to be problematic, without first forming a sense of whether what is being evoked can possibly be experienced, and under which circumstances. In fact my methodology was inspired by the historiographical approach to past medical texts developed by the Italian historian of medicine, Luigi Belloni, in his pioneering studies of Marcello Malpighi (1628-1694).[10] Belloni realized that the history of the novel anatomical structures described in the seventeenth century by Malpighi under the microscope, could not be done by simply reading Malpighi's texts. It had to be supported by a historically accurate reconstruction of the observations made by Malpighi. This was accomplished by replicating the procedures for preparing the specimens that Malpighi recounts in his writings. I have tried to apply the lesson I learned from Belloni.

Furthermore, I have tried to remedy the problem of the lack of robustness that besets the observations made by others with pendulums. By "robustness" I mean *repeatability* and *consistency* of outcome over a wide range of the parameters that control the experiment. The pioneer replications done by Naylor and MacLachlan focused on too narrow a set of parameters arbitrarily fixed by the operator. Since Galileo does not tell us much about the setup of his experiments, we face formidable indeterminacies, which may affect our interpretation of the texts to a point that we risk failing to see what Galileo might have seen, and vice versa. To resolve the indeterminacies we need to make the experiments robust over as wide a range of parameters as possible. This can be

[10] Belloni 1970, and Belloni's commentaries in Malpighi 1967. In recent years, more and more scholars have embraced the experimental approach to the history and philosophy of science. Cf., for instance, Settle 1961, Wilson 1999, and Renn and Damerow 2003.

obtained today with computer simulations of mathematical models (which I describe in appendix 1).

I hope that, in this way, I will de-emphasize, if not resolve, the numerous puzzles and the embarrassment that Galileo's pendulums have created for scholars (at times, myself included).

A NOTE ON THE MATERIAL QUOTED IN THIS PAPER

I shot a large number of videos which I will quote in this book. The reader will find the video material freely downloadable at www.exphs.org, a website which is devoted to *Experimental History of Science*, a research project that I have started at the University of Pittsburgh.

Translations are mine. I have collected the relevant texts by Galileo in Appendix 3 for easy reference.

ABBREVIATIONS

EN The so-called *Edizione Nazionale* of Galileo's works in twenty volumes (Galilei 1890-1909), the main edition of Galileo texts and letters. I quote this as EN, followed by a Roman numeral indicating the volume, and by Arabic numerals preceded by "p." or "pp.", indicating page numbers.

T1 (EN, X, pp. 97-100).

Letter of Galileo to Guido Ubaldo dal Monte. Padua, 29 November 1602. (See Appendix 3).

T2 (EN, VII, pp. 256-257).

Excerpt from the *Dialogue on the two chief world systems (1632)*. (See Appendix 3).

T3 (EN, VII, pp. 474-476).

Excerpt from the *Dialogue on the two chief world systems (1632)*.

(See Appendix 3).

T4 (EN, VIII, pp. 128-129).

Excerpt from the *Two new sciences (1638)*. (See Appendix 3).

T5 (EN, VIII, pp. 139-140).

Excerpt from the *Two new sciences (1638)*. (See Appendix 3).

T6 (EN, VIII, pp. 277-278).

Excerpt from *Two new sciences (1638)*. (See Appendix 3).

2. INGENIOUS ARTEFACTS

In this section, I discuss my reconstruction of Galileo's experiments. I will focus not only on the outcomes of the experiments, but also on experimenter's activities that form the outcome of an experiment.

2.1 On the scene of experience

An experiment is hardly an event in isolation. It is more like the performance of a set of interrelated activities on a scene of experience. However, it is not always easy to define, or rather design, the activities in advance. At times they are dictated by the very nature of the artefact around which they start evolving.

I assembled a slender and light wooden frame in the form of a gallows (cf. Appendix 2, for further details). The main vertical post I made movable up and down, so that I could quickly raise the horizontal arm in order to make more vertical room for longer pendulums. I prepared the horizontal arm with many holes and hooks, so that I could hang pendulums at different distances from each other. I wanted to observe two pendulums swinging behind each other. In this way I made sure that I had a good perspective, i.e., that I could see two equal pendulums marching synchronously and in parallel, and moreover that my point of view was freely movable all around. Figure 2.1.1 shows the scene of the pendulum experience. Figure 2.1.2 shows a detail of the apparatus.

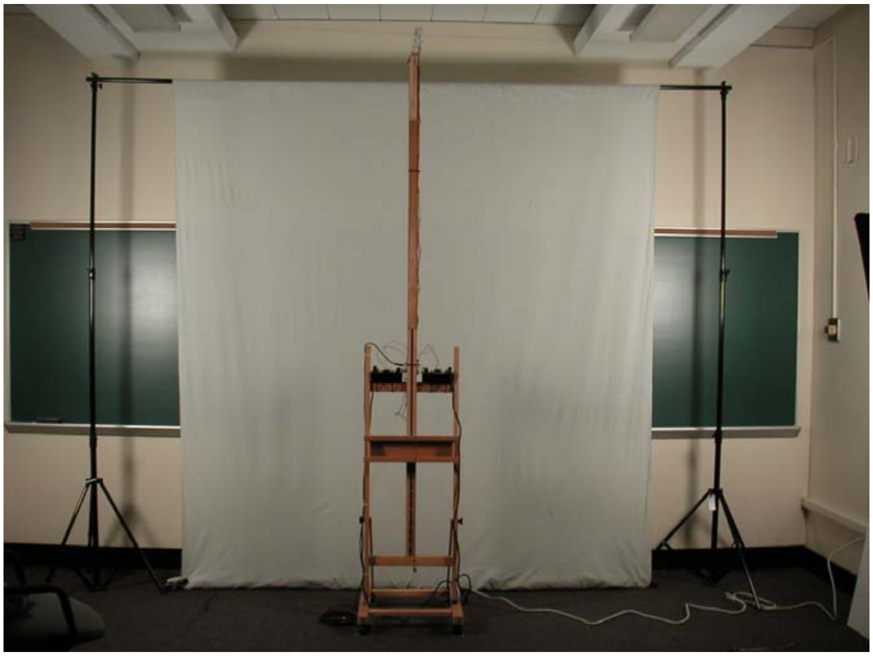

Fig. 2.1.1 The scene of the pendulum experience. The wooden gallows-like frame is at the centre. Behind it is a white backdrop for improving our direct observations and also shooting movies. The use of modern electronics is explained in Appendix 2.

Fig. 2.1.2 A detail of the upper part of the pendulum apparatus; the horizontal bar is made of Plexiglas. The load cells used for measurements of the tension in the strings can be spotted on the Plexiglas bar (cf. Appendix 2).

I begin by recounting how I measured the lengths of two equal pendulums, or rather how I decided that my two pendulums were of the same length.

For starters I cut two hemp strings[11] of the same length (about 92 inches); first of all, I cut one and then the second after making their ends coincide. I hung two pendulums made with the hemp strings knotted to two 1-ounce lead balls, adjusting the knots until I was satisfied by visual inspection that the two pendulums were of the same lengths.[12] The pendulums were removed from the perpendicular and let go at the same instant. They started losing synchronism after a short while, contrary to my expectations. So I thought that there might be something wrong with the apparatus or with the way I let the bobs go. I repeated the test with the same results. I tried from the other side of the apparatus with the same results. Finally it dawned on me that the lengths of the pendulums might in fact not be the same. By *length of the pendulum* I mean the resulting length of the string plus the radius of the ball to which the bob is attached, once the pendulum has been hung from the frame. The operation of mounting the pendulum affects its length. In fact visual inspection failed as a criterion of equality for the lengths of pendulums.

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[11] Hemp is a material that would have been easily available in Galileo's time. Linen also would have been available. Galileo says "spago" or "spaghetto" , i.e., thin string, which implies that the string's material would probably have been hemp or linen. Cf. Appendix 2, for further details.

[12] The choice of the lead balls is somewhat problematic. Galileo does not specify the size of the lead balls he uses, but the words he chooses seem to indicate that he used very small balls, roughly the size of musket balls, or little more. Musket balls at that time would have been in the range of 1-2 ounces (about 28-56 grams). Cf. Appendix 2, for further details.

There is no way of making sure that the two lengths are the same other than letting the pendulums swing together for as long a time as possible, and observing that they will oscillate synchronously, i.e., keep pace with each other. The two pendulums function together as a combined accumulator of the delays due to the inevitable difference in lengths of the pendulums. Therefore, in order to guarantee that pendulum experiments are significant one has to work patiently until one is satisfied that for an arbitrarily defined interval of time the pendulums will swing synchronously. I finally settled on a *reasonable* time window of five minutes. Beyond that time limit I knew I would have to expect the results to become less and less reliable since the pendulums would slowly start losing synchronism. I spent my first morning on the scene of experience working this problem out.

Since the pendulums are supposedly identical, by "oscillating synchronously" I simply mean that they appear to march together to the observer. "Togetherness" here is unproblematic because the two pendulums will have to be in the same position at the same time all the time.

Videos 1, 2, 3, 4 (*The length of the pendulum*) document the phenomenon of the lengths of the pendulums. They show longer and longer synchronism between two pendulums of "equal" lengths. I achieved this result during my first morning on the scene of experience. The astonishing fact is that pendulums will always tend to go out of sync and (relatively) quickly. How quickly? When I started, I had in mind the hundreds of *oscillations*[13] that

[13] I will consistently use "oscillation" to indicate a complete swing of the pendulum, forth and back, from starting point to maximum height on the other side with respect to the perpendicular and return. Note that Galileo's language is

Galileo seems to claim to have counted (cf. T1, T4, T6, Appendix 3). But with the lengths of pendulums that Galileo approximately indicates (in the range of the 4-5 braccia, as suggested by T4, T6, i.e., 2-3 meters, according to what one chooses as the equivalent of Galileo's braccio), it was problematic for me to observe more than about one hundred "good" oscillations, since the time window allowed by my two "equal" pendulums was precisely about five minutes (about one hundred oscillations for pendulums of 92 inches).

This of course does not imply that it is impossible in principle to fine-tune the lengths of the pendulums until they will swing more than one hundred oscillations synchronously. It means, however, that the fine-tuning becomes more and more difficult and tedious to carry out, since there are obvious limitations in the way the hands of a human observer operate while, for example, trying to adjust a knot around the hook of the lead ball.[14] Keep also in mind that when doing the adjusting the operator cannot be helped by visual inspection, since the lengths of the pendulums will tend to look the same all the time.

Furthermore, there is another practical limitation to the manual procedure of adjusting the lengths. Hemp strings tend to coil and are also very flexible. In order to straighten them up one has to apply a modicum of tension, which however will tend to extend

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not always clear when referring to pendulum swings, at times suggesting oscillations, other times perhaps half oscillations.

[14] This is how I performed the adjusting of the lengths of the pendulums. It is of course possible to think up better fine-tuning methods, but they are trial-and-error procedures, since the lengths of the pendulums will always look the same all the time (obviously within the limits of technologies that would have been available to Galileo).

them a little. In sum, the adjusting must be done by trial and error. Tension in the string is an ineliminable problem that the pendulum experimenter learns how to live with pretty quickly. In fact when grabbing the lead balls in preparation for a launch the operator is constantly adjusting, more or less consciously, as I have realized, the tension applied to the strings. This tension does not sensibly affect the outcome of the experiment, as I have concluded, yet creates a sort of anxiety that the outcome will in fact be affected.

We can virtually-experiment with the effects of slight differences in the lengths of *supposedly* equally long pendulums with the help of a computer model of the pendulums used in real experiments (see Appendix 1). I ran a few simulations and noticed that in order to end up with a visually discernible lack of synchronism between the two pendulums, after a number of oscillations comparable to that of Videos 1, 2, 3, 4 (*The length of the pendulum*), a difference of about 5 millimetres is required (cf. S-Video 1 *Two lengths*). This suggests that once the pendulums are mounted on the wooden frame the perspective of the observer's point of view determines a margin of error in the estimated equality of the lengths of the pendulums of at least about that size.

Why do pendulums stop swinging? The question is far from naïve, since resistance due to impediments, such as air and/ or mechanical friction, might not be the sole factor, or even "the" factor, responsible for the slowing down of pendulums. Further, in early seventeenth-century Padua, Aristotelian natural philosophers, such as, for example, Galileo's friend, Cesare Cremonini (1550-1631), would have assumed that the medium was responsible for keeping the pendulum going, not for slowing it down. Galileo came up with an ingenious hypothesis, as we will see now.

2.2 Artefact's modes

T2 is a fascinating text. In it Galileo expounds an elaborate theory of the intrinsic tendency of pendulums to slow down and eventually stop regardless of all external impediments. Galileo also draws a figure of the shape of an oscillating pendulum made with a bob and a rope (Fig. 2.2.1).

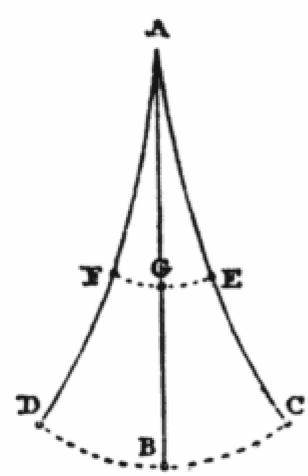

Fig. 2.2.1 The non-rectilinear shape that a pendulum made with a rope would show during an oscillation.

He argues that if the pendulum's suspension is a "corda" [rope], i.e., a thicker and heavier suspension than the thin "spago" [string], or "spaghetto", as generally reported in other texts (such as, for example, T4, T6), then the rope's parts behave like many pendulums distributed along the rope. These will have their own well-determined frequency, higher and higher, as their distance from the centre of oscillation becomes smaller and smaller. Therefore, Galileo argues, they will slow down the oscillating bob, since the latter will be "restrained" by the many pendulums that the rope really amounts to, and which will want to oscillate faster and faster than the bob. This effect, Galileo continues, will be even more manifest to the senses if the rope is replaced with a chain. Thus, Galileo concludes, all pendulums will inevitably stop, even if all external impediments were removed.

Why a chain?

I replicated Galileo's experiments with chain pendulums in order to observe the shape taken by the pendulum while oscillating. The results were astounding; possibly, in my view, the most important finding on the scene of experience. These experiments with chain pendulums revealed the existence of so-called *latent modes of oscillation*. A pendulum's latent modes of oscillation are infinite though only some can be observed. Latent modes of oscillations, as they are sometime referred to in the technical literature, are a well known phenomenon to structural engineers of the twentieth century. There are infinite possible shapes that a continuous mechanical system, such as, for example, a heavy rope, or a chain, can assume while oscillating. What Galileo shows us (Fig. 2.2.1) is what structural engineers call the *fundamental* mode of oscillation, i.e., the shape of the oscillation that occurs at the lowest possible frequency. In reality, the motion of the pendulum is always a composite of all possible shapes, though we can normally observe only a few, since those at higher frequencies are more difficult to perceive clearly and distinctly (no matter how Cartesian you are).

The observation of the latent modes of the chain pendulum at last explains why Galileo insists on using pendulums made with thin strings (cf. T1, T4, T6). Thin strings de facto eliminate the problem of latent modes of oscillations. I have no doubt that Galileo was well aware of the fantastic display of the latent modes of oscillation early on. Before further discussing the implications of this finding I need to present the latent modes of oscillation more graphically.

First of all, I hung brass-chain pendulums of different lengths and did what I had been doing with string pendulums, i.e., simply removed them from the perpendicular and let them go. If the starting angle is relatively small, the shape the chain pendulum takes is the simple shape of the fundamental mode, exactly like that shown by Galileo. But, if the angle increases, more modes start kicking in and the fireworks begin.

Videos 22, 23, 24 (*The chain pendulum*) show the fantastic behaviour of the latent modes of oscillations. The chain pendulums oscillating at a sufficiently wide angle clearly display the superimposition of a few latent modes, which give the chain pendulum's motion its characteristic, apparently chaotic, serpent-like shape.

The following table (Table 2.1) shows approximations of the latent modes of oscillation of a chain pendulum, starting from the lowest frequency. I have calculated the modes with a 100-mass linear model of the chain pendulum.[15] There are exactly one hundred modes for a 100-mass model. The table shows eight modes, corresponding to the eight lowest frequencies, in order of ascending frequency.

[15] Cf. Braun 2003, for a discussion of a multiple mass pendulum model. I have adopted Braun's linear approximation for my 100-mass model of the chain pendulum.

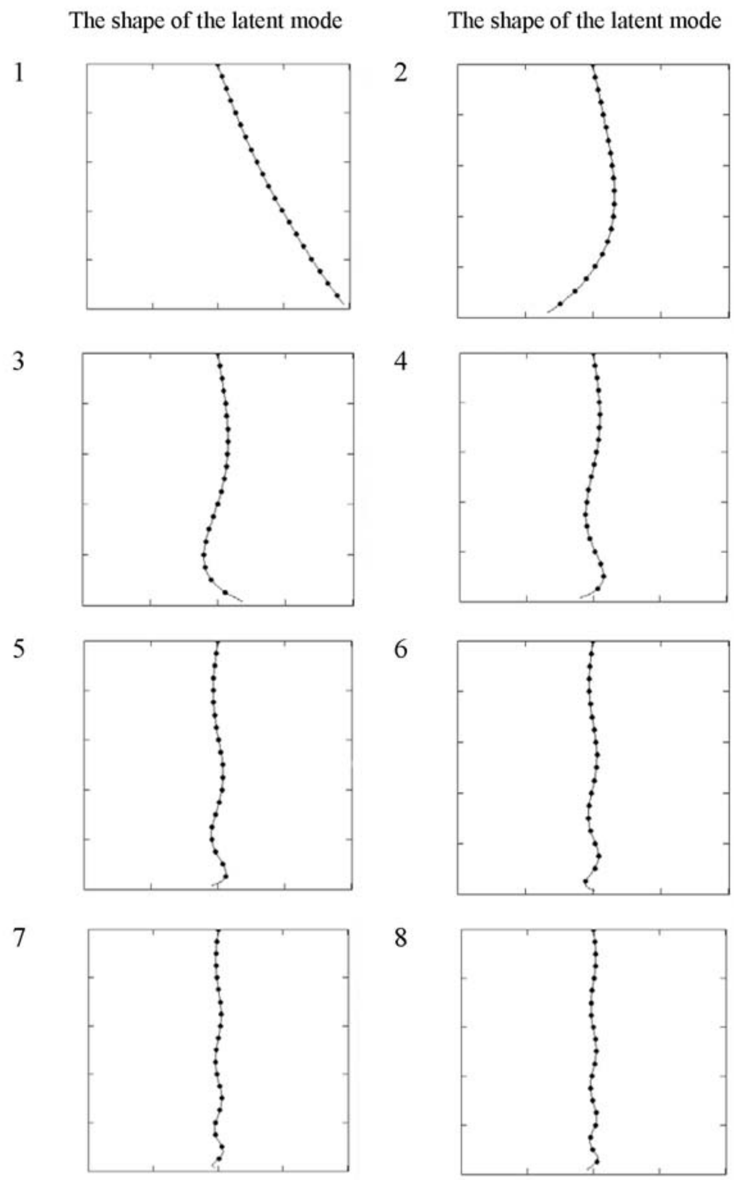

Table 2.1 This table shows the eight latent modes corresponding to the eight lowest frequencies, in order of ascending frequency, for a 100-mass linear model of the chain pendulum (the black dots represent some of the masses). Mode number 1 practically matches Galileo's picture. During a real oscillation all the modes contribute, with different weight factors, to the formation of a complex shape.

The discovery of the latent modality present in the chain pendulum—which explains why Galileo otherwise always emphasizes the use of thin strings (which do not show latent modality)—, has further important implications.

First, as already noted, the behaviour of the chain pendulum for wider and wider angles shows less and less regularity. It is obvious that the chain pendulum does not have a proper period of oscillation. Indeed it is hard to decide what the period of oscillation of the chain pendulum should be. The reason is that it is difficult to fix the meaning of *oscillation* since the chain assumes different shapes all the time. It looks like a continuously changing shape. Yet any theoretical use of the *pendulum* presupposes that a pendulum has by definition a period of oscillation. As all the texts translated in Appendix 3 suggest, Galileo was unswerving in repeating that simple pendulums are either isochronous, or at least quasi-isochronous (cf., for example, T3).

Second, Galileo chooses to focus on the regularity which the chain pendulum displays for lower angles of oscillation and lower frequencies. There is a tendency in the behaviour of the chain pendulum. The operator controls the unruliness of the pendulum by decreasing the angle of the initial release of the pendulum. Thus the operator learns that regularity tends to manifest itself within the range of small angles of oscillation. Oscillations are observable, somehow, though the operator wonders more and more what the meaning of oscillation for a chain pendulum is. Regularity and oscillation manifest themselves together.

I speculate that if (and, I must stress, this is a big "if") Galileo started his pendulum experiment with chain and/or rope pendulums and heavy masses, he would have encountered the unruly latent modality. He would subsequently have decided to go on to experiment with lighter apparatus, such as strings and light masses, in order to control the phenomena of latent modality.

Finally, and most importantly, what would Galileo's reaction have been to the latent modality of the chain pendulum? It is impossible to say exactly when the chain pendulum experiments were really carried out by Galileo, since the only reference to the chain pendulum is given in T2, which is a passage from the 1632 *Dialogue*. But, on the assumption, commonly accepted by scholars, that most experiments would have been done in the Padua period, we can speculate on the basis of T1. The latter is a very early, Padua text, in which Galileo tells us something important about the predicting power of mathematical demonstration, when the latter is put to the test of experience. He argues that, when it comes to matter, the propositions abstractly demonstrated by the geometer are altered, and that of such altered propositions there cannot be science [*scienza*]. The geometer, Galileo concludes, is exempt from the responsibility of dealing with the many alterations introduced by matter in the outcome of the effects predicted by geometrical demonstrations.

Thus, I would argue that the 1602 Galileo would have shown little or no concern for the intriguing *imperfection* of experience (T1). Matter is solely responsible for the unruly modality displayed by the chain pendulum. Why bother?

2.3 Experience in the limit

In this section, I tackle the crucial issue of the isochronism of simple pendulums (i.e., a property of simple pendulums to oscillate at a constant frequency regardless of the amplitude of the oscillations), and that of the synchronism of pendulums of different materials (i.e., a property of simple pendulums whose bobs are of different materials, but whose lengths are the same, to oscillate at the same frequency).

Readers should familiarize themselves with the texts in Appendix 3, especially with the terminology that Galileo adopts when addressing the issue of isochronism (a word, let us recall, which Galileo does not use).

Isochronism vs. discrepancy. Before starting the discussion about the isochronous behaviour of simple pendulums, a fundamental question has to be answered concerning the material nature of Galileo's equipment (a question rarely addressed by Galileo scholars). What kind of materials would Galileo have used in his pendulum experiments? First of all, we now know that there is a profound reason explaining why Galileo always proposes pendulums made with thin strings (*spaghetti*). Thins strings are light and therefore their latent modality remains in fact *latent*. Thin string pendulums de facto behave like masses moving along the circumference of a perfect circle whose radius is the length of the string. The string, in other words, remains perfectly rectilinear during the oscillations of the pendulum. In consequence, it is obvious that the masses that Galileo would have preferred are relatively small. It becomes immediately clear, even to a casual experimenter, that heavy weights are too dangerous when operating with thin strings, since the speed acquired by the bob of a pendulum 4-5 braccia long, swinging from a wide angle, is indeed very high. But Galileo never tells us anything precise about the weight of the lead balls he uses. There is only one exception. In *Two new sciences* he tells us that the lead ball in the experiment of the pendulum twisting around the peg is 1-2 ounces [*una palla di piombo d' un' oncia o due*].[16] This makes perfect sense. Further, in his time, commonly available musket balls would have weighed precisely 1-2 ounces.[17]

If Galileo used musket balls, or, at any rate, small balls of 1-2 ounces, as well as thin strings, then his pendulum's patterns of behaviour would have been heavily dependent on the aerodynamic resistance acting on the string and the ball. The literature on Galileo's pendulum experiments consistently misses this important point, i.e., light pendulums behave in a way that is governed by aerodynamic resistance. This is not to say that Galileo never extended his range of operations to pendulums heavier than 1-2 ounces. Although we lack any textual evidence in this regard, it is likely that a curious experimenter, such as Galileo, would have tried different materials, different sizes and different lengths (see, for example, T1, where Galileo argues that the experience he made was done with two equal bobs, but that it makes no difference if the bobs were different). The point is that by experimenting with different sorts of weights and lengths Galileo would have been exposed to the significant effects of aerodynamic resistance on pendulums. In other words, he would have been exposed to the range of patterns of behaviour that real pendulums display.

[16] EN, VIII, pp. 205-208.

[17] It is rather easy to cast lead balls since lead's liquefying temperature is not too high. It is therefore possible that Galileo would have cast his own lead balls, in which case he would have been free to cast balls of different weights than musket balls (see Appendix 2, for more on lead balls and other materials).

A simple pendulum, as we know, is not isochronous (Fig. 2.3.1).

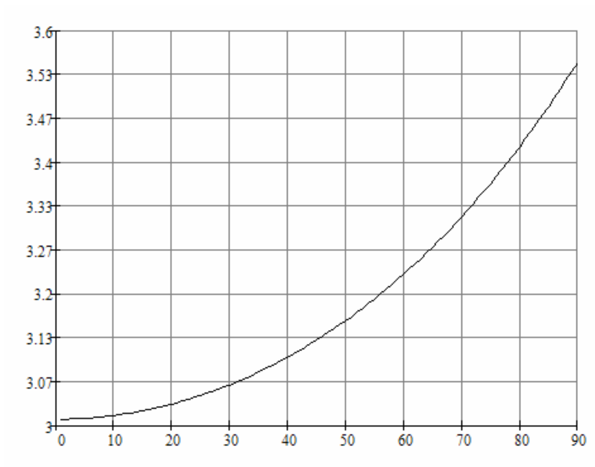

Fig. 2.3.1 The non-isochronism of the simple pendulum. The curve shows the period of oscillation of a simple pendulum (i.e., the time required to complete a whole oscillation back and forth) as a function of its length. On the vertical axis, the period is represented in seconds. On the horizontal axis the angle of the swing is represented in degrees, for angles between 0^{o} and 90^{o}. The curve has been calculated for a pendulum's length of 2.25 meters. The variability in the period is about one half of a second.

Videos 5, 6, 7 (*The isochronism of the pendulum*) show two light pendulums of about 92 inches loaded with 1-ounce lead balls. The synchronous behaviour of the pendulums is evident. As Galileo would have said, the two pendulums go together. In the third video, the angle of release was slightly wider, so the pendulums start showing some discrepancy. When experimenting with pendulums oscillating along not too wide angles, say, up to about 30-35 degrees, and in the range of 4-5 braccia, the whole oscillation can be observed quite easily. The speed of the bob when crossing the vertical is not too high.

Videos 9, 10, 11, 12 (*The discrepancy of the pendulum*) show the change in the visual appearance of the pendulums as the angles of release of the bobs is progressively increased. I call this phenomenon *discrepancy*. As I experimented, the best way to get to grips with this gradual change in visual appearance (at least the way that worked best for me) is to focus observation on the stopping points of the two bobs, the points where the bobs invert their motions. At those instants the bobs have zero speed. And at that time it is easy to ascertain if their motions do not start again at the same instant, or, conversely, if they do not arrive at the inverting point at the same instant.[18]

Before discussing the implications of these findings I need to stress that the results obtained with my apparatus, i.e., the phenomena of isochronism and discrepancy, are very robust, in the sense that they occur over a wide range of the pendulum's parameters. Robustness is difficult to test in practice, since these tests are very time consuming (and resource consuming, in that one has to find numerous pieces of equipment, such as lead balls of different weights, etc.). To test robustness, I repeated a whole set of experiments concerning the *discrepancy* phenomenon with 2-ounce balls. The results, consistent with the 1-ounce pendulums,

[18] At one point in the course of the experiments, the impression emerged that the two pendulums might somehow interfere with each other. I will discuss the possible effects of interference further on in this section. For the time being, I note that the two pendulums did not interfere in any appreciable way. I tested this conclusion by leaving one of the two pendulums at rest and operating the other in order to see if the motion of one of the two might excite some movement in the other. Video 8 *The stability of the pendulum* shows that the pendulum left at rest remains at rest while the other oscillates for a long period of time, well beyond the 4-5 minutes of the time window allowed.

are documented in Videos 13, 14, 15, 16 *The discrepancy of the pendulum 2oz*. Can we go further?

Robustness is where the power of *computer modelling* kicks in. With the pendulum model discussed in Appendix 1, calibrated on the real data acquired from the real apparatus, I have tested the phenomena of isochronism and discrepancy over a range of plausible parameters. I report one example of the phenomenon of discrepancy, in the form of so-called *time histories* of the angles of two pendulums. This was obtained with the pendulum model for a pendulum of 1.5 meter, the range of 2-3 braccia that Galileo talks about in T1 (Fig. 2.3.2). The diagram only gives an approximate sense of the phenomenon since it is difficult to visualize the data mentally. In order to form a visual sense of the pendular motion, the reader should observe the animation of the phenomenon, in S-Video 2 *The pendulum model discrepancy*. The pendulum model is so accurate that the whole phenomenon could hardly be distinguished from a real case.

In sum, both in real tests and in simulations with the pendulum model, the phenomena of isochronism and discrepancy appear to be robust over a wide range of parameters. Thanks to the robustness assessed with a computer model, I feel confident in concluding that Galileo could not have missed the gradual change in behaviour, from isochronism to discrepancy, as he experimented with different pendulums and different angles.

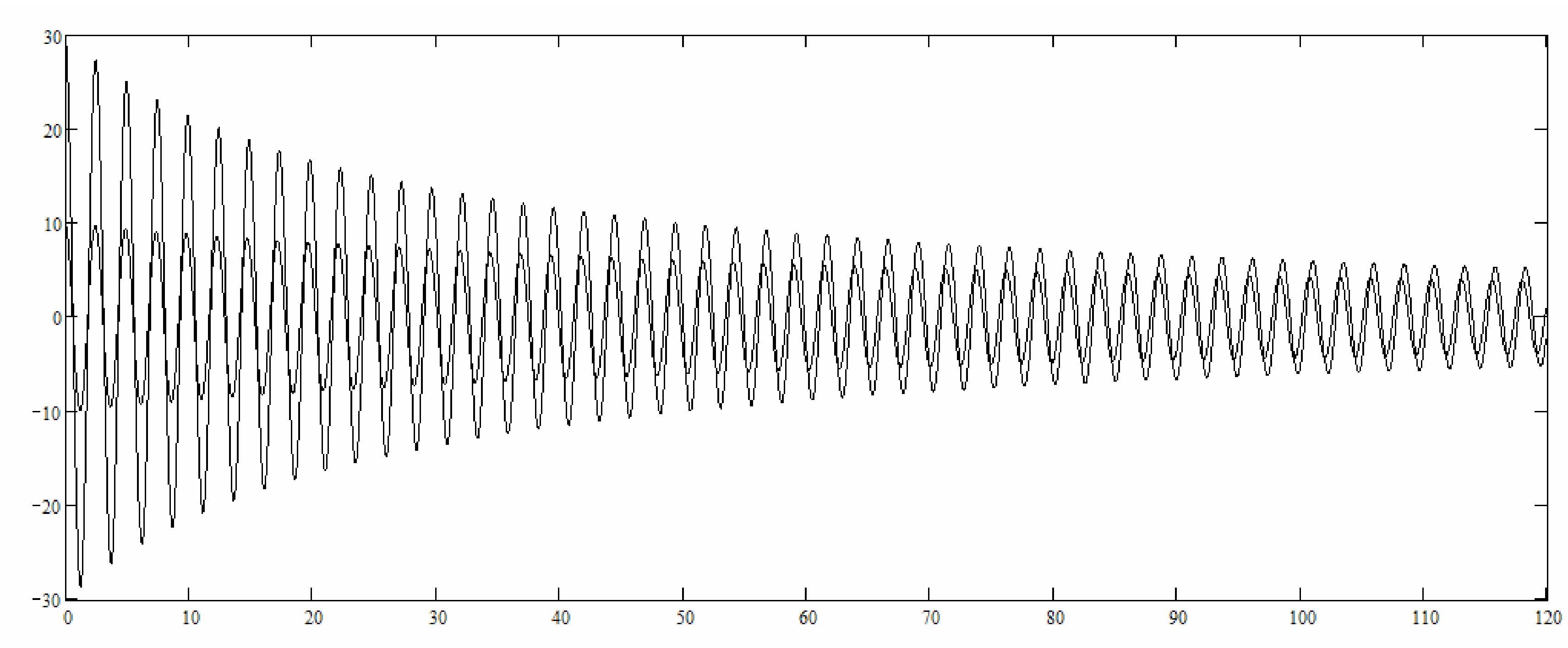

Fig. 2.3.2 The time histories of the angles of two pendulums obtained with the pendulum model, for a pendulum of 1.5 meter, which is the range of 2-3 braccia that Galileo talks about in T1. On the vertical axis, the amplitudes of the two pendulums are represented in degrees. On the horizontal axis, time is represented in seconds. The two pendulums started from 30 and 10 degrees. The simulation was calculated for 2 minutes. This diagram only gives an approximate sense of the phenomenon. In order to form a better opinion, the reader should observe the animation of the phenomenon, in S-Video 2 *The pendulum model: discrepancy*.

The gradual shift from isochronism to discrepancy challenges any observer of the pendulum. What is, in Galileo's view, the cause of the gradual shift from isochronism to discrepancy? A few preliminary comments are necessary before answering the question.

The two most revealing texts in this regard are T1 and T6. In these texts, Galileo stresses the fact that he numbered the oscillations of pendulums (note that it remains unclear whether he actually means *complete* oscillations back and forth, or rather only *half* oscillations, from one point of maximum elongation to the other). Direct observation of the isochronism of the pendulums, Galileo seems to say in between the lines, is arduous, but numbering the oscillations, or vibrations (Galileo uses both words interchangeably), is more secure. It is true that, in T1 (the 1602 letter to Guido Ubaldo), Galileo shows a cavalier attitude, when concluding that even without bothering to number the vibrations, Guido Ubaldo would easily ascertain the property of isochronism by simply observing the two pendulums. But the fact that he stresses the counting of the oscillations in T6, which he published in *Two new sciences*, to my mind betrays the fact that he realized that counting was the most secure way of ascertaining the fact of the matter about the isochronism of pendulums. Counting, however, has its own serious problems, as I will show presently.

Most Galileo scholars have worried about the problem of whether Galileo's claims as regards isochronisms are justified, and to what extent, by the outcome of the experiments. This form of historiography, in my opinion, founders on the problem of the arbitrary assumptions usually made in order to define what *justified*, or *supported* by empirical evidence, should mean. You decide. On the other hand, all of these approaches to the question of defining *justified*, or *supported* by empirical evidence, have strangely neglected Galileo's emphasis on counting, which is the very claim that Galileo consistently makes. It is a most interesting claim.

The numbers of oscillations that Galileo claims he could count seem exorbitant. He speaks of hundreds of oscillations. On the other hand, if he meant half oscillations— as some of his descriptive language sometimes seems to imply—, the order of one hundred oscillations is not impossible. In fact, the human eye, as I discovered, has the power to discern accurately even very small, tiny oscillations. At any rate, it is easy to count one hundred full oscillations, or 200 hundred half oscillations, with pendulums in the range of the 4-5 braccia, and masses in the range of the 1-2 ounces (the reader can easily verify this by counting the vibrations while looking at the videos). For example, with the light pendulums used in my tests, it is easy to count more than one hundred full oscillations, without a 1-count discrepancy, for angles up to about 70 degrees, which is more or less the maximum angle I could reliably test with my apparatus. This accords with Galileo's claims that it is indeed possible to do so (cf. T6).

In addition, I used the pendulum model to test Galileo's T6 claim, for 4-5 braccia pendulums, swinging from initial amplitudes of 80 and 5 degrees. Galileo says "hundreds" but allowing for the fact that he may have meant one-half oscillations, the count of one hundred full oscillations becomes two hundreds.[19] The results are

[19] In T6 Galileo actually seems to argue that not only would the count not disagree by one oscillation, but not even by a fraction of an oscillation, and for

impressive. It is possible to count up to one hundred full oscillations while observing a stable discrepancy between the two pendulums. I conducted two virtual tests with pendulums of 1 and 2 ounces. The two videos, *S-Videos 80 5 degrees 1 oz pendulums* and *S-Video 80 5 degrees 2 oz pendulums*, show that one hundred full oscillations are possible without a difference of one count. In the second case, however, the discrepancy increases to almost ½ full oscillation (in which case, if Galileo meant half oscillations, the count would amount to a difference of one).

The discrepancy has one very peculiar characteristic. Since the motions of light pendulums slow down rather quickly, because of aerodynamic resistance, the pendulums after a short time from the start enter a region of oscillations where the difference in their periods diminishes, so that the discrepancy, after accumulating for a while, plateaus and appears to be rather constant over the remaining interval of observation. This, once again, is consistently true over a wide range of parameters. The discrepancy, in other words, does not "explode" leading to chaotic patterns of behaviour. It remains clearly visible at a level which seems to be perfectly stable over a long period of observation.

Further, counting the same number of oscillations for the two pendulums, and assuming, as Galileo does, that this is a basis for concluding that all the oscillations are isochronous seems to be faulty reasoning. One can divide the same line, or the same quantity, for example, into the same number of parts, but this does not mean that all the parts will be of the same length. In fact Galileo is well aware of this problem, I think. In T1, he says that

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angles up to more than 80^{o}, which, however, I think is exaggerated, since the discrepancy produces that sort of difference of a fraction of oscillation.

the counting's turning out the same for both pendulums is a *sign* of isochronism. And in T6, where he concludes the same, he actually says that the experience of counting makes us certain of isochronism. So, at most, experience furnishes a sign, in the first version of the argument, or makes us certain of the fact, in the second.

We are now in a better position to appreciate the question of the cause of the gradual shift from isochronism to discrepancy.

I believe that Galileo cannot have failed to confront the isochronism vs. discrepancy phenomenon. It is all too evident across the whole spectrum of parameters. Yet we are told nothing in the published record, and unfortunately no surviving manuscripts illuminate this issue. But why would Galileo have otherwise emphasized counting so consistently, and not direct visual inspection of the phenomenon, as the means of ascertaining the facts of the matter? I think because of the gradual shift from isochronism to discrepancy. Also note that in the published *Dialogo* text (cf. T3), where Galileo in fact dispenses with counting, he hastens to underline that experience shows that the pendulums are isochronous, or *if not perfectly isochronous, at least quasi-isochronous*. He does not say that experience shows perfectly isochronous pendulums.

Galileo must eventually have asked himself: *What is the cause of the gradual shift from isochronism to discrepancy*? For the early Galileo, the implications of the gradual shift from isochronism to discrepancy were far more serious than the imperfection of the pendulum experience. We need to go back to the *De motu* writings to understand why.

In the *De motu* writings, Galileo vehemently opposes the Aristotelian theory of rest at the point of inversion. The problem was addressed by Galileo under the heading of *point of reflection* [*punctum reflexionis*]. Galileo opposes the Aristotelian view that in order for motion to be inverted, such as, for example, in the case of a stone thrown upwards, which will invert its upward motion before starting its downward motion, a rest occurs at the point of inversion.[20] Galileo's reconstructs Aristotle's main line of argument as follows. What moves nearing a point and leaving the point, while making use of the point both as an end and a beginning, does not recede unless it stays on that point: but that which moves towards the end point of a line and is reflected by that [end point], makes use of that [point] both as an end and as a beginning; therefore, between access and recess, it is necessary that [what moves] rests.[21]

Galileo's counter-argument to the theory of rest at the point of inversion is based on five distinct and independent strategies. We need not dwell on the first four, since the fifth is really the most elaborate, and the one whose relevance for the young Galileo's theory of motion was now at stake, in the face of the new empirical evidence from the pendulum discrepancy. I will only relate the gist

[20] EN, I, pp. 323-328, is the most elaborate version of the battery of counter-arguments levelled by Galileo at the theory of the *punctum reflexionis*, in the whole of *De motu*.

[21] The argument reconstructed by Galileo is rather obscure. Galileo's original Latin is as follows. "Quod movetur ad aliquod punctum accedendo et ab eodem recedendo, ac ut fine et principio utendo, non recedet nisi in eo constiterit: at quod ad extremum lineae punctum movetur et ab eodem reflectitur, utitur eo ut fine et principio: inter accessum, ergo, et recessum ut stet, est necessarium" (EN, I, pp. 323-324).

of the fifth strategy. Two assumptions are introduced by Galileo preliminarily.[22] First, mobiles will only rest outside of their own place, when the virtue prohibiting their descent is equal to the gravity of the mobiles pushing them downwards. Second, the same mobile can be sustained in the same place by equal virtues for equal intervals of time. Now, if a stone rests for some time at the point of inversion, then, for the same duration, there will be equality of impelling virtue and resisting gravity; but this is impossible since, Galileo argues, it has already been shown in another chapter of *De motu* that the impelling virtue must diminish continuously. He then moves on to re-frame the argument in the form of a stronger reductio ad absurdum. We need not follow Galileo in the details of the proof. What is at stake is clear; it is the theory of the impelling virtue, i.e., the theory of *impetus*. Let us now return to the discrepancy.

Would it not be attractive to explain the cause of the gradual shift from isochronism to discrepancy, by saying that at the point of inversion the bobs *will indeed rest for a short while*? But one could make a step further. On the assumption that the wider the oscillation the slightly longer the time of rest at the point of inversion, one could explain why the discrepancy seems to accumulate faster at the beginning of the phenomenon, before plateauing and eventually becoming virtually constant. And what better experience could confirm the cause of the gradual shift from isochronism to discrepancy, than observing the discrepancy becoming more and more evident, as the operator removes one pendulum more and more from the vertical? Aristotle's theory of the rest at the point of reflection could be correct after all.

[22] EN, I, pp. 326-328.

I speculate that Galileo did not reject isochronism out of the window, and remained steadfast in rejecting Aristotle's theory of the rest at the point of reflection because in *De motu* he stated that "experience does not teach us the causes".[23] The norm that "experience does not teach us the causes", which Galileo followed at this early stage, became a stabilizing factor in Galileo's search for a mathematical-mechanical theory of isochronism. Galileo's commitment to that norm thwarted the threat posed by the discrepancy and by the lure of explaining the discrepancy via Aristotle's theory of the rest at the point of reflection. The isochronism vs. discrepancy phenomenon was destabilizing not only for a mathematical-mechanical theory of isochronism, but for the whole *De motu*— so decisively staked upon the theory of *impetus* as the explanans of the impossibility of rest at the point of inversion.

In sum, experience with pendulums presented the early Galileo with two lures. One is the possibility of explaining the regularity of isochronism, which must have appealed to Galileo the mathematician. The second is the possibility of explaining the gradual shift from isochronism to discrepancy with the abhorred theory of rest at the point of inversion, possibly rejecting isochronism altogether. The second possibility must have appealed to Galileo the natural philosopher. Two souls competed for the same body. Paradoxically enough, a norm regulating the philosopher's quest for causes aided and abetted the mathematician in the pursuit of proof.

[23] Experience does not teach causes, says Galileo. "…quaerimus enim effectuum causas, quae ab experientia non traduntur" (EN, I, p. 263).

The synchronism of pendulums with bobs of different materials but same lengths. Cork and lead pendulums. In T4, a text published in *Two new sciences*, Galileo reports pendulum experiments with cork and lead balls. Galileo claims that two pendulums, one made with a cork ball, and another with a lead ball one hundred times heavier than the cork ball,

> "reiterating a full hundred times their forward and backward motions (*le andate e le tornate*), have sensibly shown that the heavy goes under the time of the light, in such a way that, neither in a hundred nor a thousand vibrations, the heavy is ahead of time for a moment, and both go at the same pace".[24]

Galileo's wording is so carefully calibrated that it almost defies translation. Galileo seems to claim that what happens is the reiteration of one hundred comings and goings (*reiterando ben cento volte per lor medesime le andate e le tornate*), so that experience shows the going at the same pace of the two bobs, for a hundred or even a thousand vibrations. Clearly the thousand vibrations claim is no more than a conclusion based on reasoning, since Galileo has just claimed that what happens is a hundred comings and goings. He also claims that never is the heavy ball ahead of time for a moment. Further, that this experiment is not supposed to show isochronism is emphasized by the disclaimer at the end of the passage, where Galileo specifies that "on the other hand, when the arcs traversed by the cork were no more than 5-6 degrees and those of the lead no more than 50-60 degrees, they are traversed under the same times [*anzi quando gli archi passati dal*

[24] "*reiterando ben cento volte per lor medesime le andate e le tornate, hanno sensatamente mostrato, come la grave va talmente sotto il tempo della leggiera, che né in ben cento vibrazioni, né in mille, anticipa il tempo d'un minimo momento, ma camminano con passo egualissimo*". Cf. T4.

sughero non fusser più che di cinque o sei gradi, e quei del piombo di cinquanta o sessanta, son eglin passati sotto i medesimi tempi]".

Galileo's claim about lead and cork pendulums concerns the synchronism of pendulums with bobs of different materials but same lengths. The context of Galileo's claim, the theory that all bodies fall at the same speed regardless of weight and material, I have explored in another paper.[25] In what follows, I will discuss the findings of my experiments with lead and cork pendulums.

The most serious problem is Galileo's assertion that the lead ball is one hundred times heavier than the cork ball. This does not seem problematic but it really is. In fact, cork's specific weight is so much smaller than lead's, that in order for a lead ball to be one hundred times heavier than a cork ball, either the cork ball must be very light, or the lead ball must be very heavy. Both cases present problems. A cork ball too light will not oscillate long enough. A lead ball too heavy is hard to reconcile with Galileo's indication that he is still using thin strings (*due sottili spaghetti*). I have been able to count about fifty full oscillations with balls that weigh approximately in the ratio given by Galileo.

Videos 17, 18 cork and lead, show tests made with balls in a ratio close but not exactly equal to the ratio of 1 to 100 (cork ball = 8 grams, lead ball = 670 grams). In *Videos 19, 20 cork and lead*, I changed the ratio, cork ball = 18 grams, lead ball = 670 grams, and cork ball = 7 grams, lead ball = 670 grams. *Video 21 cork and lead* shows that there is no interference between the pendulums. The lengths of the two pendulums were about 94 inches.

[25] Palmieri 2005.

It is possible that the ratio indicated by Galileo is not realistic. But it is impossible to rule out that Galileo could operate with balls exactly in that ratio. We are left with an indeterminacy here.[26] At any rate, the pendulums show a gradual shift from synchronism to discrepancy, with the cork ball moving ahead. This is due to the fact that, because it decelerates rapidly, the cork ball enters the region of small oscillations, thus starting to move ahead of the lead ball.

So, once again, experience issues a fantastic challenge to the experimenter, the transition from *passo egualissimo* to discrepancy. Galileo is on safe ground while claiming that neither in a hundred nor a thousand vibrations is the heavy ball ahead of time. In fact the opposite happens with the gradual emergence of the discrepancy. Do the balls go at the same pace (*camminano con passo egualissimo*)? For some time they do. Since Galileo is careful not to say from what angle the balls are supposed to be released, it is quite possible, for small oscillations, to see the balls go *con passo egualissimo* for some time.

If such experiments were carried out early on (as T1 seems to suggests, when Galileo claims that it would not matter if the bobs were of *different* weights), then we can ask what Galileo would have made of these results, in the framework of his early *De motu*

[26] In order to get closer to the 1-100 ratio, I repeated the tests with a heavier lead ball, in *Video 26 cork lead 4lb interference*, and *Videos 27, 28, 29 cork lead 4lb discrepancy*. The lead ball was 1812 grams and the cork ball 18.5 grams, very close to the exact ratio claimed by Galileo. As *Video 26* shows, the heavy lead ball causes some interference which somehow affects the results. The origin of this interference is mechanical, as will be explained further on in this section. Unfortunately, the experiments with the heavy lead ball are affected by an interference which precludes drawing further conclusions.

writings. According to the *De motu* Archimedean framework, in fluid mediums, specifically heavier bodies will move faster than specifically lighter bodies.[27] Galileo candidly admits, in concluding *De motu*, that experience contradicts the proportions of motions calculated on the basis of the Archimedean framework.[28] This claim by Galileo has remained a mystery so far. What are the experiments he alludes to?[29]

I hypothesize that the experiments that Galileo alludes to (but never discusses in *De motu*) are the experiments with lead and cork

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[27] Palmieri 2005, 2005a, 2005b.

[28] EN, I, p. 273. Galileo is clear and honest. The proportions of motions calculated according to the Archimedean rules of the specific gravities do not pass the test of experience. Galileo does not say more about the tests.

[29] A possible and intriguing answer might be given by the tests made by Thomas Settle and Donald Miklich (even though the experiments made by Settle and Miklich only aimed at determining the plausibility of the empirical basis underlying another theory espoused by Galileo in *De motu*, namely, the theory according to which light bodies fall faster than heavy bodies at the beginning of a free fall). A few decades ago Settle and Miklich tried the following test (Settle 1983). Participants were asked to drop two balls at the same time, an iron ball of about ten ponds, and a wooden ball of about one pound. The iron ball preceded the wooden ball after release rather consistently. In his paper, Settle documented this phenomenon by printing eight frames after release from a 24-frame per second 8-mm camera. Recently, in a private communication, Settle confirmed to me that the phenomenon can also be observed directly, and that he obtained the same results again in 2005-06, though less formally, in Florence. The phenomenon can be explained physiologically. The fatigue of grabbing the heavy iron ball with one hand causes a delay in the execution of the drop even though experimenters believe that they are actually executing the drop of the two balls at the same time. The result is fascinating but I feel that it is not robust. In other words, you need a very narrow set of parameters to replicate the effect (balls enormously different in weight), and I think that this, somehow, makes it less likely that experiments of this sort were those alluded to by Galileo.

pendulums. They seem to suggest that specifically lighter bodies will move faster than specifically heavier bodies. So, how could a mighty lead ball lag behind the cork ball? Unfortunately the Archimedean framework that innervates *De motu* is silent about the resistive role of the medium. For Aristotle and the Aristotelians, attacked by Galileo in *De motu*, the medium is the cause of, not a hindrance to, the motion of projectiles. If we suspend the belief that mediums resist motion, we can see that the latter question must have been deeply troubling for the young Galileo. Could the medium be more effective in pushing the cork ball than the lead ball?

A cork ball starts moving ahead of a lead ball. Why? What is the cause of such a bizarre phenomenon? Might a new theory of the resistance exerted by the medium be brought to bear on the Archimedean framework, so as to prevent the latter from collapsing, in the face of negative evidence from the cork and lead pendulums? The cork and lead pendulums somehow seem to teach us something about fluid mediums which is missing in both the Aristotelian and Archimedean frameworks. Fluid mediums can resist motion. They can be the cause of resistance. Does experience tell us more about causes than we might have suspected, after all, contrary to the *De motu*'s stern statement that "experience does not teach causes"? Here we see the challenges of pendulum behavior behind the scenes of Galileo's crucial transition from the *De motu* writings to a more mature theory of motion.

To summarize, pendulums show isochronism, synchronism, and discrepancy along a continuum of patterns of behaviour. There is no such thing, then, as "the" pendulum experience. I would argue that the pendulum experience is *an experience in the limit*, in the sense that isochronism and synchronism tend to manifest themselves better and better, as the parameters that control the outcome of the experiment tend to certain values. This is also true of latent modality. Latent modality tends to disappear as the parameters that control it tend to certain values.

2.4 Artefact restrained

Artefacts may be unpredictable in interesting ways. Latent modality was discovered in the chain pendulum. In the gallows-like apparatus that I built, I discovered *coupling*. This phenomenon was present from the very beginning. Coupling is an interference of some sort between the two pendulums. Coupled pendulums may mislead the observer; for example, by tuning themselves to each other, or by driving each other. Coupling, therefore, may determine quite bizarre oscillatory patterns. It is a fascinating phenomenon that may affect all pendulum experiments. So far as I know, it has never been investigated in the literature concerning Galileo's experiments. In this section, I will briefly discuss coupling, its consequences for pendulum experiments, and argue that we can reasonably exclude that coupling had a serious impact on Galileo's experiments.

There are at least two forms of coupling, mechanical coupling and aerodynamic coupling. Let me say at the outset that I made sure that coupling did not affect my experiments by checking that the two pendulums do not interfere with each other. This is the reason why, as noted above, I controlled the outcome of my experiments by always checking, in a preliminary test, that while one pendulum was going the other remained at rest. However, in the case of the very heavy lead ball, which I used to test the 1-to-100 ratio in the cork and lead pendulums, a coupling phenomenon was clearly observable. Its origin is mechanical. I will focus firstly on mechanical coupling, and then briefly on aerodynamic coupling.

As already noted, Galileo tells us virtually nothing about the set up of his pendulums. Indeed it is perfectly possible that he simply hung pendulums from the ceiling of his workshop, or even of his bedroom, or, on a wall, adjacent to each other, as shown in the diagram accompanying T1.[30] I have imagined the gallows-like structure on the basis of two considerations. First, it is a structure which allows for the two pendulums to be seen in front of each other, thus also allowing for a better observation of their relative motions. Second, I was inspired by a similar structure devised later on by the Galilean experimenters at the *Accademia del Cimento* in Florence (cf. Appendix 2, for further details). The horizontal arm of the structure has some flexibility that allows the arm to bend under the forces exerted by the oscillating masses. If the masses are modest then the forces will be modest, but if the masses are greater then the forces might be considerable. The flexibility, though, can be controlled by connecting the horizontal arm with little, or non, extensible cables to fixed points on walls. This is how I practically eliminated the unwanted flexibility. I say practically because in the case of the heavy lead ball I was unable to eliminate all the flexibility. This residual flexibility explains why coupling was observable with the heavy lead ball. The heavy lead ball was able to drive the light cork ball for dozens of oscillations, through mechanical coupling induced in the flexible structure (cf. *Video 26 cork lead 4lb interference* and *Videos 27, 28, 29 cork lead 4lb discrepancy*).

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[30] See Appendix 2, where I discuss the reasons why I think this arrangement is unconvincing.

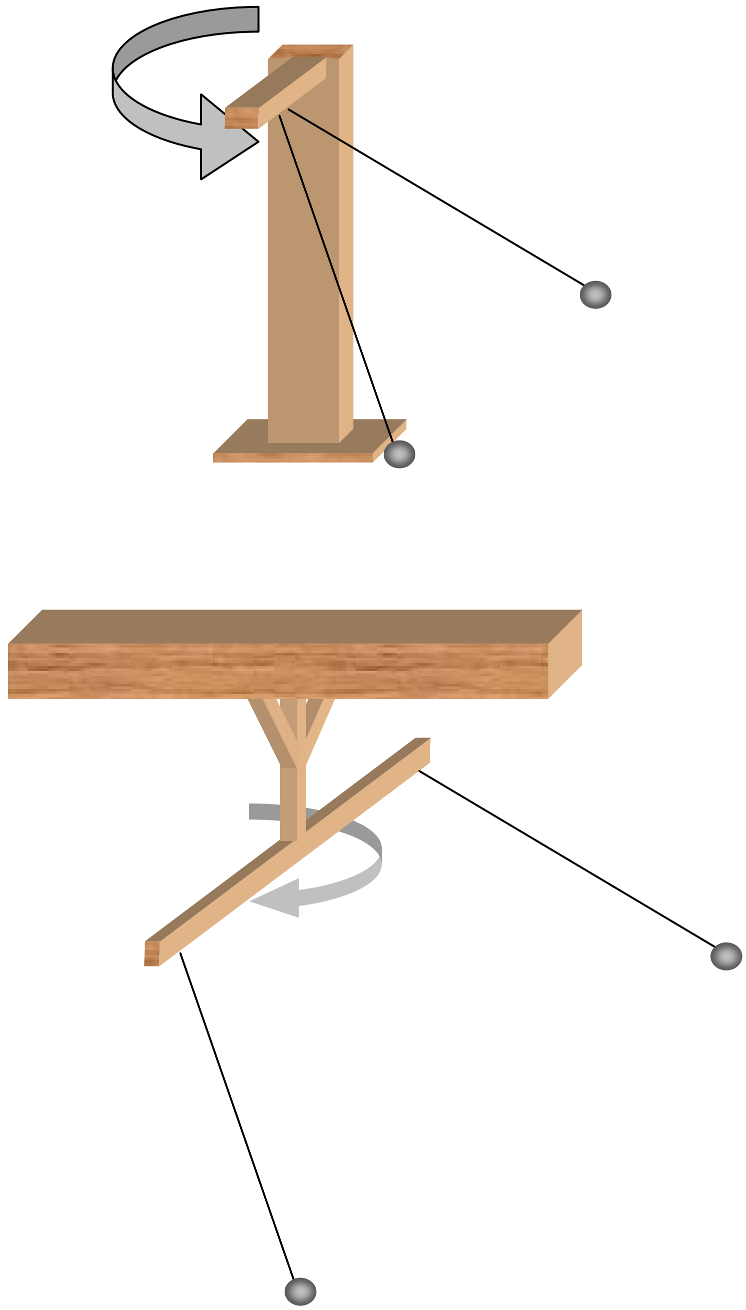


Fig. 2.4.1 *Above*: A simplified sketch of the structure I built, with a light horizontal arm rigidly attached to a stronger vertical frame. Mechanical coupling is possible if the horizontal arm of the structure bends. In this case, the points at which the strings are attached will move accordingly. *Below*: An imaginary structure where a flexible horizontal arm is connected to a sturdier, fixed beam above.

Under these circumstances, on the other hand, the horizontal arm quite visibly and regularly bends (in the order of 2-3 inches at the farthest point from the joint), right and left, under the regular, alternate pulling of the heavy ball. If Galileo ever used a structure similar to the one I have re-invented, and if this kind of mechanical coupling was observable when he experimented, I conclude that he must have realized that something in the set up was seriously flawed, and that a corrective was needed. Therefore, I do not believe that his accounts, particularly of the cork and lead experiments, can be explained away in terms of significant mechanical coupling phenomena, of which he might have been unaware.

But can we exclude that mechanical coupling was not affecting Galileo's pendulums under all possible circumstances, even when operating with small masses?

While it is impossible to rule out mechanical coupling absolutely, we can at least investigate the case of isochronism for small masses, like the 1-2 ounces lead balls that I think Galileo used, once again with the help of computer models. Consider now the imaginary set up represented in Figure 2.4.1 (part below). Suppose, in other words, that Galileo hung his pendulums from a structure that, unknown to him, at least initially, allowed for some flexibility. Might he have been misled by mechanical coupling into believing that two pendulums are perfectly isochronous? In other words, might structural flexibility subtly (and viciously) couple the pendulums in such a way as to make them oscillate in tune with each other?

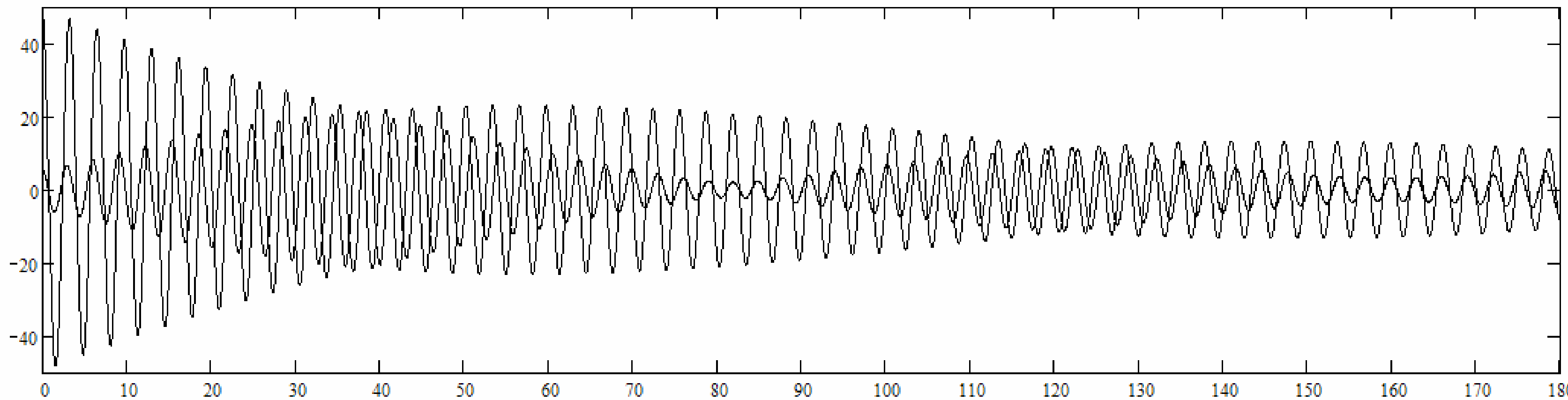
Consider Fig. 2.4.2 (diagram above). A simulation was carried out for three minutes, with pendulums of 1 oz masses, and strings of 92 inches, in order to observe an example of the highly complex patterns of behaviour which may develop because of coupling. The two pendulums drive each other. The pendulum which is started from the higher angle, which has more energy, initially pushes the other, but the latter responds, because of the interaction through the structure, and slows down the first pendulum. The pattern at one point shows that the phenomenon is reversed. This kind of weird pattern of behaviour is too evidently an artefact of the mechanical structure, I am convinced, for it to be mistaken, even by a naïve observer, as genuine. Indeed, the horizontal arm's ends move alternately back and forth, with aplitudes up to ± 2 cm, a fact which should alert any observer. Therefore, I would argue that we can confidently exclude that such weird examples might have been mistaken for anything more than an exceptional result due to the particular setup.

However, there are more subtle possibilities. Consider now Fig. 2.4.2 (diagram below). Another simulation was carried out for three minutes, again with pendulums of 1 oz masses, and strings of 92 inches. The result is subtly different. After an initial phase of energetic interaction, the two pendulums tune to each other so well that they go on oscillating, as if they were perfectly superimposed on each other. However, even in this more subtle case, the visible motion of the ends of the horizontal arm should alert the observer to the possibility that something in the mechanical structure of the arrangement is affecting the oscillations of the pendulums.

How do these results translate into dynamic visual appearances? We can form an idea of the real dynamics of the phenomenon of coupling, by animating the numerical results obtained with the coupled pendulums. In *S-Video Coupling 1* and *S-Video Coupling 2*, I have animated the two patterns of behaviour diagrammatically presented in Figure 2.4.2. Note, however, that only the pendulums are shown, but in reality, on the scene of experience, the oscillations would appear in their natural setting with the horizontal arm flapping back and forth.

I will now say something about the possibility of aerodynamic coupling. If the pendulums are placed very close to each other, in an arrangement such as, for instance, that shown above in Figure 2.4.1, or when simply hanging from a ceiling, the question arises whether aerodynamic forces, which are generated especially by the strings, could make the two pendulums interfere with each other. First of all, the observations made with one pendulum at rest and the other oscillating, again, confirm that such a phenomenon was not affecting the results in my experiments. Moreover, it is somewhat difficult to place the pendulums very close to each other, because their planes of oscillation often tend to rotate, one way or other. The danger of collisions discourages the idea of such an arrangement.

In conclusion, I feel that we can exclude the possibility that coupling phenomena might have consistently vitiated the results that Galileo obtained with pendulums.



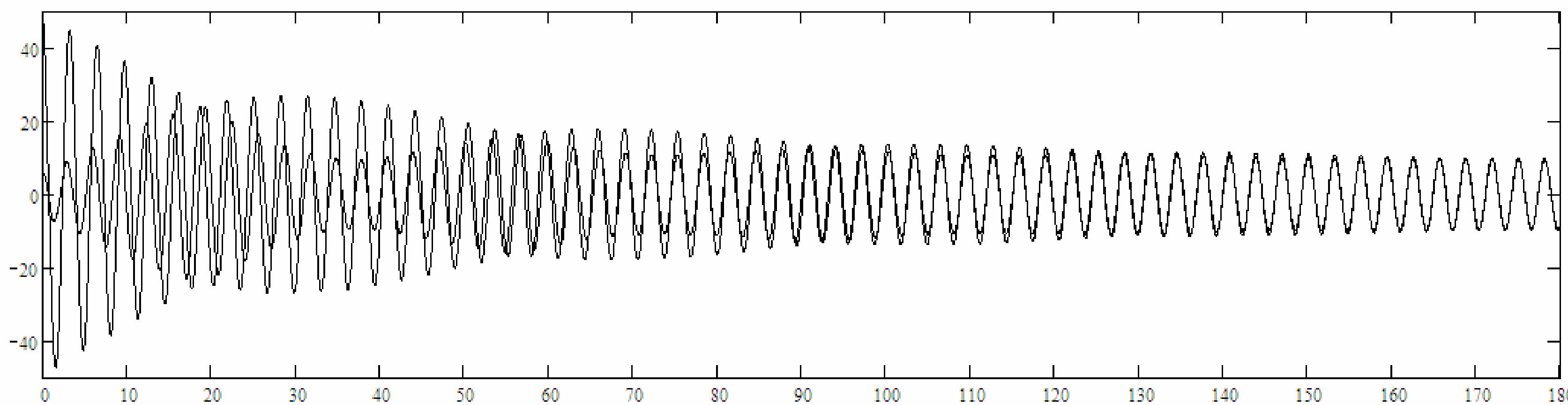


Fig. 2.4.2 Above: a spectacular yet bizarre case of coupling (on the vertical axis angles are represented in degrees, while on the horizontal axis time is represented in seconds). The pendulums drive each other while developing a highly complex pattern of behaviour. One mass was started from 50^o and one mass from 5^o. The pendulum starting from the higher angle "pushes" the other pendulum, which in turn responds by amplifying its oscillation while slowing down the other. The pattern is then almost reversed. Below: a much more ambiguous and perplexing pattern. After a initial phase of energetic interaction, the two pendulums tune to each other so well that they go on oscillating as if perfectly superimposed to each other.

3. THE BRACHISTOCHRONE PUZZLE

In this section, I discuss the significance of the brachistochrone calculations (carried out by Galileo on folios of so-called *Manuscript 72*), in relation to our understanding of Galileo's pendulum theory.[31]

In 1994 David Hill argued that Galileo knew a great deal more about pendulums than he was willing to publish.[32] In particular, some manuscript folios of *Manuscript 72* have been interpreted by Hill as evidence of experiments and calculations performed by Galileo with pendulums. Hill's challenging conclusions are that, as the calculations confirmed, Galileo was well aware of the non-isochronic behaviour of pendulums, and consciously published false assertions in *Two new sciences*. In what follows, by reconstructing and repeating the calculations done by Galileo in *Manuscript 72*, I will offer a counter-argument to Hill, showing that his interpretation of the calculations is erroneous, and his conclusion that Galileo consciously published false assertions untenable.

More specifically, I will show that the main evidence Hill brings in support of his thesis cannot in fact support it. His whole argument hinges on the assumption that by calculating times of descent along rectilinear chords of a circle and extrapolating the results to the arcs of the circle, Galileo must have realized the non-isochronism of swinging bobs. There is no evidence, though, that Galileo might have allowed such an arbitrary extrapolation. Furthermore, the

[31] See Galilei 1999, for *Manuscript 72*. The calculations have been published also in EN, VIII, pp. 420-422.

[32] Hill 1994. Cf. also Drake 1990, pp. 9-31, who proposes a different interpretation of the manuscripts studied by Hill.

crucial calculations that would have been necessary to extrapolate the results, even though they are perfectly possible in principle, were not carried out by Galileo, presumably, as we shall see, because of the insurmountable amount of operations needed.

First of all, it is important to realize that the original context of the calculations was not the pendulum, as Hill believes, but the problem of the brachistochrone curve (i.e., a curve joining two points such that a body travelling along it under the sole action of gravity takes a shorter time than along any other curve between the points). At the end of his treatment of accelerated motion along straight paths, in *Two new sciences*, Galileo conjectures that an arc of circumference is the brachistochrone (a word not used by Galileo).[33] Galileo does not prove his conjecture. The conjecture is in fact false. We now know that the brachistochrone is the cycloidal arc. However, Galileo sought to prove his conjecture, as the many remnants of related theorems and problems surviving in the folios of *Manuscript 72* suggest.[34] That the calculations originally relate to this problem I will try to make clear presently (Fig. 3.1).

[33] Cf. Galileo's *Scholium*, in EN, VIII, pp. 263-264.

[34] Wisan 1974.

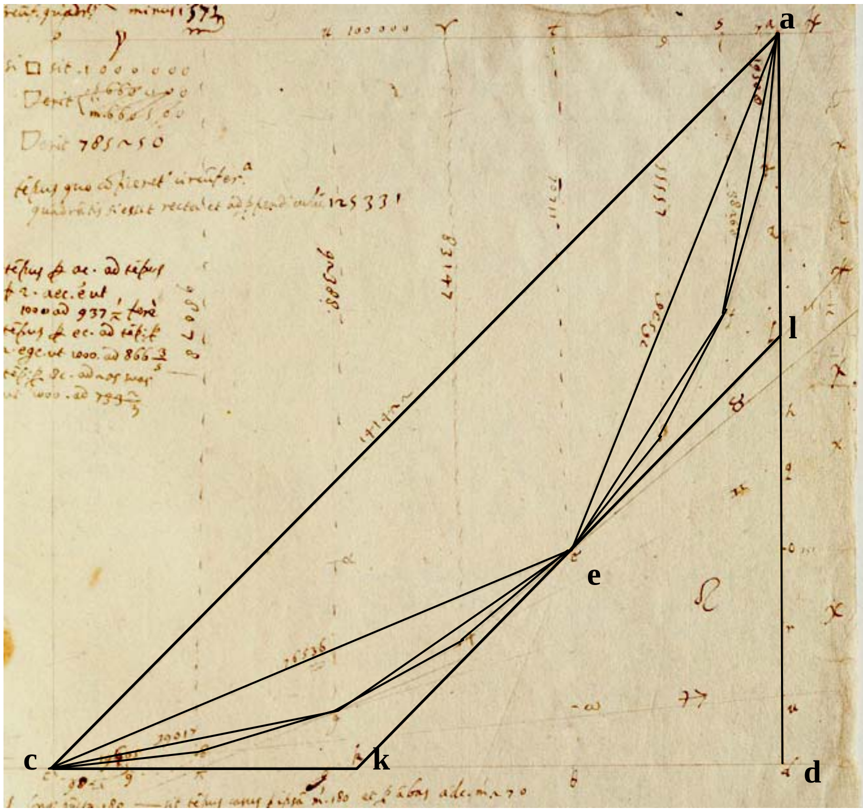


Fig. 3.1 How Galileo defines the boundaries of the problem of a numerical analysis of the brachistochrone conjecture. I have highlighted the relevant segments and the arc. From *Manuscript 72*, preserved at the National Library in Florence, at folio 166 recto (see Galilei 1999).

To begin with, Galileo defines the boundaries of a numerical analysis of the brachistochrone conjecture. His units are very simple. He assumes that the vertical path *ad* is equal to 100000 units. He also assumes that the time of descent along this vertical path is equal to 100000 units. He knows that the time of descent along the 90^{o} chord, i.e., a chord corresponding to an angle of 90^{o}, is to the time along the vertical path as the length of the 90^{o} chord is to the length of the vertical path.[35] He therefore calculates for the

[35] EN, VIII, pp. 215-217.

latter time the value of 141422 (which, given Galileo's choice of units, is also the length of the 90^o chord). You can spot this figure in the diagram close to the middle of the 90^o chord. He then calculates the time of the multiple-segment trajectory *alkc*, finding 135475, and the time of the two-chord trajectory, *aec*, finding 132593. These values, 132593 and 135475, define the problem, in that the time of the multiple-segment trajectory *alkc* is greater than the time of the two-chord trajectory *aec*, and both in turn are smaller than the time along the 90^o chord, thus suggesting that among the paths inside the quadrilateral figure, *alkc*, there must be a path of minimum time. Galileo's brachistochrone conjecture therefore amounts to the following question: might the arc of circumference, inside the quadrilateral figure, *alkc*, be the trajectory of minimum descent time?

But Galileo does not know how to calculate the time of descent along the circular path, except by successive approximations of multiple-segment trajectories, which approach the arc in the limit. He then has no choice but to embark on a computing journey. The further calculations that he performs are for a 4-chord approximation, and finally for an 8-chord approximation. In the next table I summarize Galileo's results.

Trajectory	Calculated time
1 chord (90^o)	141422
2 chords	132593
4 chords	131319
8 chords	131078

The times of descent decrease, as expected, thus presumably confirming, in Galileo's mind, the validity of his conjecture. These are all the calculations that Galileo carried out, or, at least, all those which have been preserved among his folios.

The amount of operations needed for this numerical approach is exorbitant, since for each rectilinear path of descent one has to calculate the time by considering not only the different inclination of the path, but also the initial speed gained by the body while falling along the whole preceding trajectory. It comes as no surprise that, with no other means of calculation than his own human computing power, Galileo gave up on this approach, after succeeding in crunching the 8-chord approximation. So far as documented history is concerned, then, this is all we can say about the extant calculations.

But the interesting possibility is that this numerical approach to the analysis of conjectures might immediately be turned to another use, that is, an exploration of the isochronism of pendulums, as Hill has intuited. Since the isochronism of the pendulum predicts that all times of descent along circular arcs are isochronous (up to a *quarta*, in Galileo's terminology, i.e., an arc of 90^{o}), one way to explore this prediction is to calculate the times of descent along different arcs by the method of approximating the arcs with multiple-segment trajectories, such as those used for the brachistochrone analysis. The above table already furnishes one such case, for the time of descent along the whole arc of 90^{o}. The best approximation, i.e., the 8-chord approximation, predicts a time of 131078.

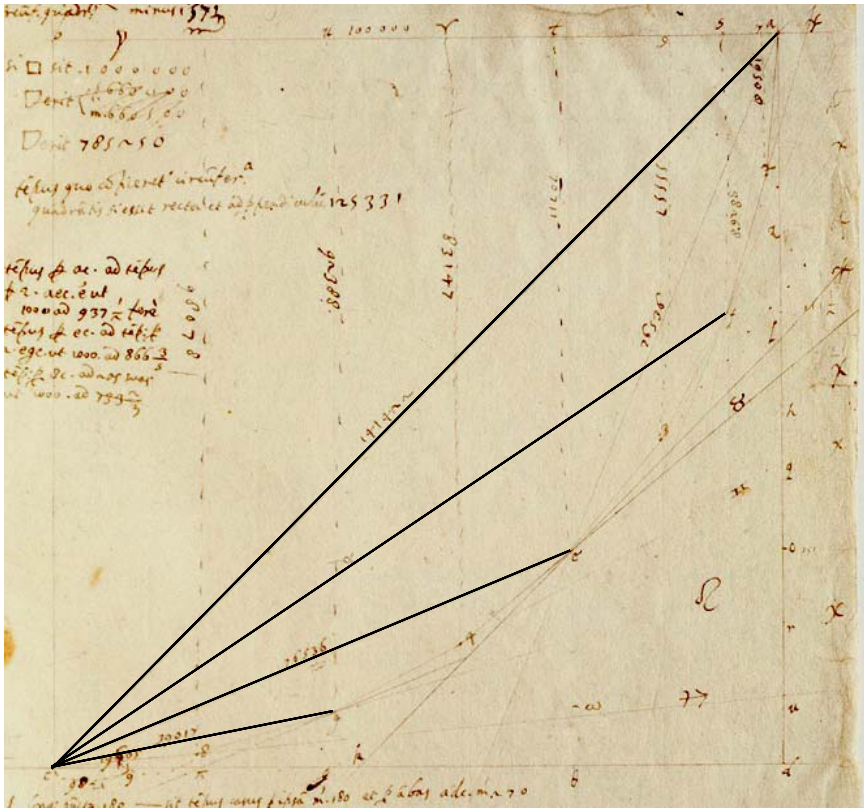


Fig. 3.2 The multiple-segment trajectories that I have calculated, according to Galileo's procedure, in order to investigate numerically the conjecture that all times of descent along a circular arcs are isochronous. I have highlighted the four initial chords. From *Manuscript 72*, preserved at the National Library in Florence, at folio 166 recto (see Galilei 1999).

In order to investigate this possibility, I have repeated Galileo's computations for all of the four multiple-segment trajectories, approaching in the limit the four arcs, $\frac{\pi}{8},\frac{\pi}{4},\frac{3\pi}{8},\frac{\pi}{2}$, and for different approximations (Fig. 3.2). I started from the one-chord approximation, as Galileo did, for each of the four trajectories. The initial one-chord approximations (Fig. 3.2) are all isochronous, as Galileo has proven, and their time of descent is, in his units, 141422 (the same figure he calculated). Now consider the calculations diagram (Fig. 3.3).

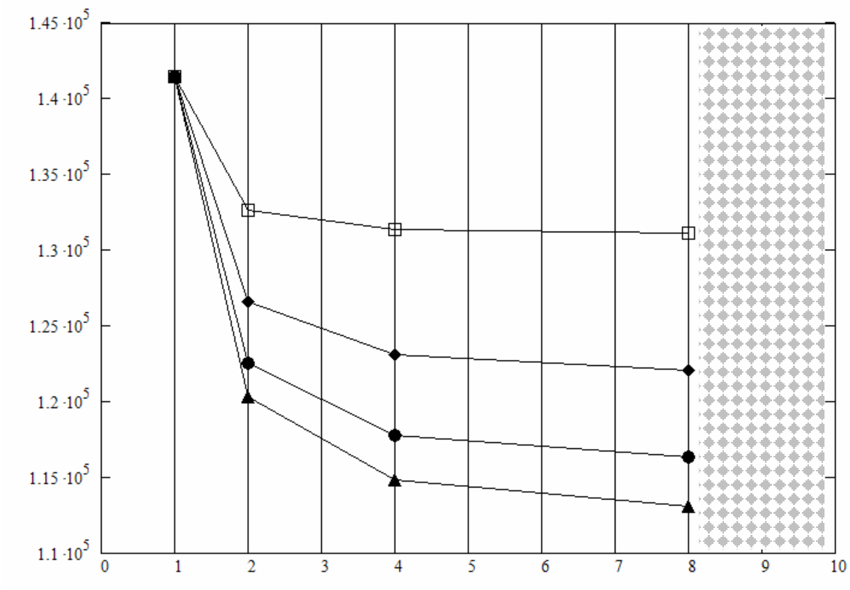


Fig. 3.3 The results of the multiple-segment trajectories that I have calculated according to Galileo's procedure. The uppermost curve is Galileo's calculation (but I have re-calculated the values) corresponding to the 1, 2, 4, 8 chords (the curve is identified by the square-like symbol □). The grey arrow suggests the decrease in the times of descent for smaller and smaller arcs. The next curves below, are arcs of $\frac{3\pi}{8}, \frac{\pi}{4}, \frac{\pi}{8}$, and identified by a black diamond-like symbol, a black circle symbol, and a black triangle symbol, respectively. The large question mark underlines the lack of evidence about the behaviour of the four curves for higher order approximations, as the number of chords is increased.

Along the horizontal axis, I represent the number of iterations, corresponding to the number of chords in each approximation (I have calculated 1, 2, 4, 8 chords, for each of the four arcs, as Galileo originally did for the arc of 90^{o}). On the vertical axis, I represent the times of descent in Galileo's units. So, for a one-chord approximation, all curves in the diagram start from the same point, since, as already noted, all such chords are isochronous. As expected, the times of descent decrease with the decreasing amplitude of the arc, as we now know that the arcs are not isochronous. The four curves do not converge, as the number of chords increases, to a common value, contrary to expectations on the isochronism hypothesis.

However, as far as the isochronism hypothesis is concerned, even if we assume that Galileo might have repeated his efforts three more times, in order to calculate the three curves, for $\frac{3\pi}{8}, \frac{\pi}{4}, \frac{\pi}{8}$, the evidence of the above diagram must still be considered inconclusive, since we must also assume to know nothing beyond the limit of 8-chord approximations. In other words, to make the calculations significant we would need to extend the diagram, by computing approximations for far greater numbers of chords. We can easily supply these calculations today, of course, and satisfy ourselves that the curves do not in fact converge to a single value, thus casting doubt on the pendulum's isochronism hypothesis.

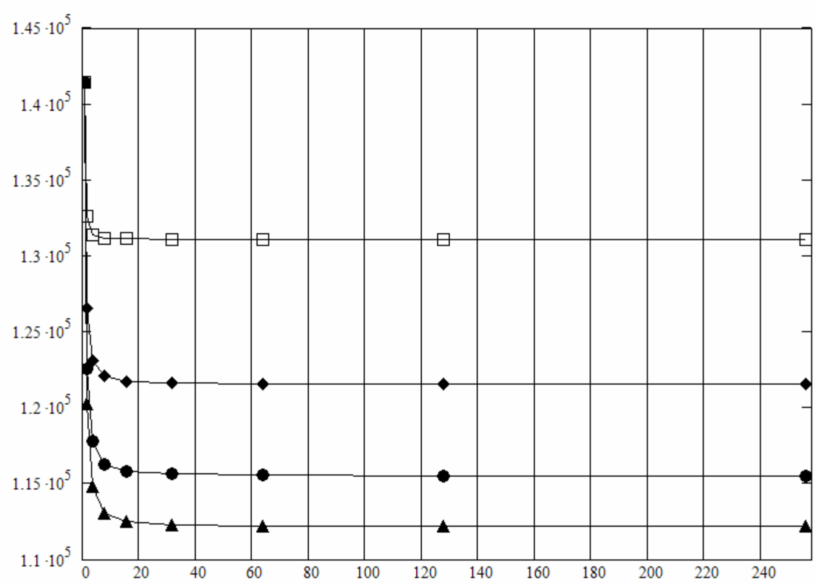


Fig. 3.4 The results of the multiple-segment trajectories that I have calculated, according to Galileo's procedure, extending the approximations, in powers of 2 (hence the uneven distribution of the data along the horizontal axis), up to 256 chords for each of the four arcs.

Indeed, I have repeated the calculations for approximations of the four arcs up to 256 chords, according to powers of 2 (Fig. 3.4), as Galileo did for the 90^o arc. The extended diagram can strengthen our confidence that the four curves do not in fact converge to a single value. But it is also clear that the evidence of the data at Galileo's disposal, namely, only one curve (the uppermost one in Fig. 3.3), or, hypothetically, the four curves— had he repeated the calculations three more times—, would not have been significant, for the extrapolation of any conclusion about the isochronism of pendulums, i.e., about the convergence of the four curves. Galileo had to content himself with the values for only one curve, and up to his best approximation of only eight chords.

To conclude, the calculations carried out by Galileo concerned originally the brachistochrone hypothesis, not the pendulum. It is true that these calculations can in principle be repeated for other arcs, so as to construct a numerical analysis of the isochronism hypothesis. But Galileo did not go so far as to arrive at a series of data which might call into question the isochronism hypothesis. The computations needed were simply far beyond the power of a human computer. Therefore, Hill's conclusion that Galileo consciously published false assertions in *Two new sciences* is, in my view, untenable.

A technical note.

It is easy to replicate Galileo's calculations with a computer. Galileo's units determine the gravity constant that must be used in order to obtain results comparable to Galileo's. Thus by assuming the radius of the quadrant to be equal to 100000 units, the gravity constant for vertical descent must be $g = 2 \cdot 10^{-5}$, and the formula for the time of descent along a rectilinear path will be $T = -\frac{V_0}{g} + \sqrt{\left(\frac{V_0}{g}\right)^2 + \frac{2 \cdot L}{g}}$, where T is the time of descent, V_0 the speed at the beginning of the descent, and L is the length of the rectilinear path. By programming a series of iterations for all the chords of any approximation to an arc, adjusting g according to the inclination of the path, and taking V_0 as the speed reached after fall along the preceding portion of the trajectory, one can calculate the total time for a descent along a multiple-segment trajectory.

4. CONCLUSION

The pendulum was an open-ended artefact. Its elusive features challenged the norms that presided over the tentative construction of a new methodology for science. However, the destabilization potential also acted as a invitation to explore new investigative pathways.[36] Galileo's detour around the pendulum was path-breaking; it was a risky enterprise. The element of risk is inherent in Galileo's new science. To calculate something of the risk he was taking with the pendulums, Galileo might have embarked on formidable calculations. However, as we have seen, the iterations that he needed would have been beyond his computing power.

Much has been made, in the philosophical literature, of the relation of empirical facts to theory, and ever since Pierre Duhem's seminal studies emphasis has been placed on theory-laden phenomena. I find that this emphasis is misplaced. On the scene of experience, I lived-through nothing but the stubbornness of phenomena. The scene of experience is robust. For, try as I might, weird phenomena would resist all sorts of attempts to explain them away. Facts of experience are not easily concocted out of theoretical commitments. You may be struck dumb by the unexpected behaviour of artefacts, and wish it to go away by the magic of theory, but you can do nothing about it. You are stuck with it.

Galileo's new science erupted out of a willingness to negotiate and trade theoretical norms for stubborn facts of experience, and sometimes, no doubt, the other way round (with consequent discomfort of later scholars). Praxis and theory were placed on an equal, unstable footing.

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[36] "Pathway" in the sense articulated by Holmes 2004.

APPENDIX 1. The computer models

In this appendix, I discuss the computer models used in the course of my investigations. All models have a mathematical component, i.e., a system of simultaneous ordinary differential equations which describe the physics of the phenomenon under scrutiny, and a computer component which solves the system of equations. For the systems of equations we are interested in do not in general have known solutions in analytic form. In other words, in general there is no known formula which solves the system of equations. This is the reason why we need a computer. We need to study the solutions of the systems of equations in numerical form.

But there is another reason why computers are so spectacularly apt for this type of investigation. Most of the difficulties with replicating historical experiments are due to the paucity of information about the original setting and conditions of the experiments. To repeat the test in practice requires reconstructing many possible settings, a time-consuming requirement which sometimes is impossible to meet, or otherwise economically unfeasible. In other words, there is often a huge range of uncertainty about the original experiments. Thus replicating an experiment is in a way reinventing it. How do we make sure that, this limitation notwithstanding, we can capture something of the original which is true? With computer models we can run tens of virtual experiments in a few minutes by adjusting the parameters, so that we can explore the range of uncertainty, and draw very robust conclusions relative to a range of variability of a parameter. We cannot claim that this or that specific event happened but we can claim that it was plausible that a specific event happened under physical circumstances described by a range of variability of certain parameters. But of course a final verdict can only come from real experimentation. How this approach translates into practice we shall see better below in the discussion of each individual case.

There are nowadays efficient algorithms to carry out the computations needed for solving systems of differential equations. There is a specialized literature on this subject, which in effect has become a branch of mathematics in its own right. There are also numerous software packages that implement the algorithms. Given the huge computational power of today's personal computers we are in the fortunate position of being able to carry out computations that not long ago would have required expensive supercomputers accessible only to a few scientists around the world.

The computer models presented in this section have all been studied with *Mathcad, version 13*, by http://www.mathsoft.com/, a sowftware package for solving mathematical problems numerically and symbolically. I myself have written the systems of equations. The algorithms for solving the equations are already coded in the package so there is no need to write additional code. The feature which I find most attractive about this package is that it allows the writing of mathematics in a natural format, as though on a piece of paper, without need for learning a programming language.

I will discuss the nature of the physical phenomena I have modelled, the systems of equations I have written in order to capture the phenomena, and the results I have obtained.

1.1 The pendulum computer model

A simple pendulum, such as one of those hinted at by Galileo (see texts T1, T3, T4, T5, T6), consists of a thin string hanging from some kind of support and a ball somehow tied at one end of the string (except in the case of the chain pendulum, cf. text T2). The equations of motion of a simple pendulum depend on many factors, most important of which are the length of the string, the weight of the ball, and the aerodynamic forces acting on the string and the ball. Since the pendulum's freedom of movement is due to the flexible string deforming under the driving force of the ball's weight, and since there is virtually no rotation of the string around the point to which it is knotted, unlike the case of a rigid pendulum which rotates around a pivot, mechanical friction is practically negligible. I will assume that the pendulum does not rotate in three-dimensional space, i.e., that the oscillation is constrained to a plane of oscillation, and further that the plane of oscillation does not rotate. Under these circumstances the angle of oscillation of the string suffices to describe the dynamics of the pendulum. Consider the following schematic diagram (Fig. A1.1.1).

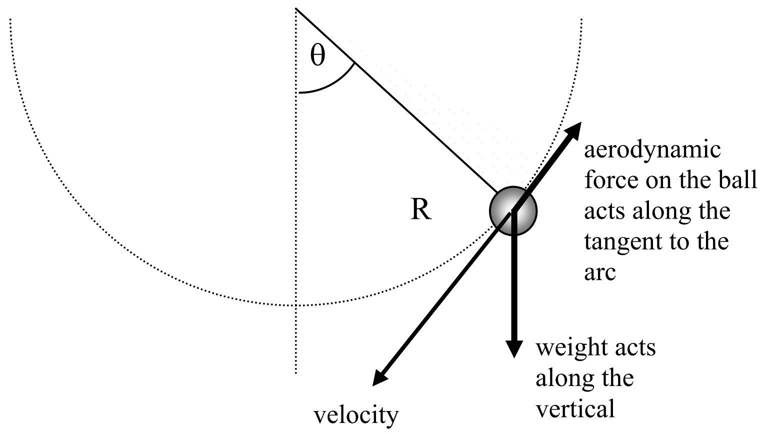

Fig. A1.1.1 A simple pendulum. The only paramter needed to describe the oscillatory motion of the simple pendulum is angle θ. The triangular shaded area approximately represents the distributed aerodynamic force acting on the string. Note, however, that the correct distribution of the string's aerodynamic resistance is not a linear function of the distance from the centre of oscillation.

The most uncertain factor we need to capture in the model is the aerodynamic force, specifically resistance, acting on both the ball and string (lift is negligible since we assume that the ball and the string are sufficiently symmetrical with respect to the direction of the velocity). An approximate description of the form of these forces can be given by common, simple formulas for spheres and slender cylindrical bodies available in the literature. The problem, as we shall see, is that these approximate formulas contain an empirical coefficient that needs to be adjusted to the operating conditions of the real apparatus.

As for the ball, on the assumption that it is a perfect sphere, we can express the aerodynamic resistance with the following formula,

$$R = C_d \cdot \frac{\rho}{2} \cdot S \cdot V^2 \ ,$$

where R is the resistance, C_d the drag coefficient of a sphere, ρ the density of air, S a reference surface, in our case the surface of a diametrical section of the sphere, and V the speed of the ball.

As for the string, we can represent an infinitesimal element of resistance acting on an infinitesimal portion of the string, assumed to be perfectly cylindrical, as follows,

$$dF = C_x \cdot \frac{\rho}{2} \cdot \delta \cdot dr \cdot V^2 \quad ,$$

where dF is the infinitesimal force acting on an infinitesimal portion of the string, C_x the drag coefficient of the cylinder, δ the diameter of the cylindrical string, dr the infinitesimal length of the infinitesimal portion of the string (so that $dr \cdot \delta$ is the string's infinitesimal reference surface), and V the speed at that point of the string. By integrating the previous formula along the radius one obtains the total resistance caused by the string.

Since $V = \dot{\theta} \cdot R$, the equation of motion of a simple pendulum, with aerodynamic forces acting on both ball and string, and on the assumption that the string has no mass (i.e., that the mass of the string is negligible compared to the mass of the ball), is as follows,

$$\ddot{\theta} + \frac{g}{R} \cdot \sin(\theta) - R(C_d) \cdot \dot{\theta}^2 - F(C_x) \cdot \dot{\theta}^2 = 0 \qquad ,$$

where $\ddot{\theta}$ is the angular acceleration, g is the gravity constant, $R(C_d)$ and $F(C_x)$ are functions which depend on air density, R, the mass of the ball, the reference surfaces of ball and string, and the empirical coefficients C_d, C_x. I have evidenced only the latter coefficients as arguments of $R(C_d)$ and $F(C_x)$, since these coefficients constitute the most delicate part of the model.

As anticipated C_d, C_x have to be determined empirically, since the model will work really well only when the coefficients are adjusted to the real operating conditions. This is especially true since both the ball and the cylindrical string are blunt bodies, and even the best aerodynamic theories we have today do not furnish good theoretical approximations of resistance forces in the case of blunt bodies. This is the most challenging part of modelling the simple pendulum.

In order to solve this adjustment problem I have equipped the pendulum apparatus with a computerized data acquisition system (see Appendix 2, for a description of the pendulum apparatus, and of the data acquisition system), which basically allows me to collect very precise time-histories over a certain period of time. The time-histories can subsequently be compared with simulated time-histories of the theoretical model. By adjusting the C_d, C_x in the theoretical model until the simulation, i.e., the calculated dynamics of the modelled system, accords well with the acquired time-histories, I succeeded in tuning the pendulum model. In other words, I succeeded in fixing the values of the C_d, C_x which work best under the operating conditions of my real apparatus. I proceeded as follows.

First of all, note that it is rather difficult to read the position of the strings accurately using an affordable technology. Therefore, I have used high-precision load cells in order to read the tension in the strings of the pendulums. So the time-history collected is the actual tension present in the string at any given moment during the motion of the pendulum. Load cells are wonderful engineering masterpieces which can output precise and reliable data. The following diagram, Fig. A1.1.2, shows a comparison between the data collected with my apparatus, and the calculated dynamics (which is easy to perform starting from the above equation of motion of the pendulum).

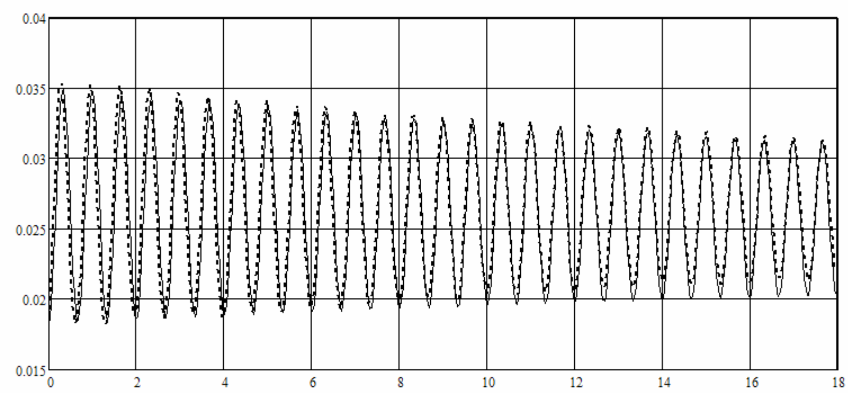

Fig. A1.1.2 A comparison between the data collected with my apparatus, and the calculated dynamics.

On the horizontal axis time is given in seconds over a period of 18 seconds. On the vertical axis tension in the string of the pendulum is given in kilograms. The dotted curve represents the time-history collected during the test with the real pendulum. The solid line represents the calculated result with the tuned model. The match is satisfactory, in that the overlap of the two time-histories tells us that the tuned model captures well the two fundamental characteristics of the pendulum's oscillatory motion, i.e., its frequency and its oscillatory "decay" due to aerodynamic resistance.

During the trial-and-error tuning of the model I easily hit upon the values that best fit the acquired data. In so doing I discovered that the approximations for the C_d, C_x coefficients, which can be found in the scientific literature, dramatically underestimate the real resistance in my apparatus. But this was no surprise.

To sum up, the tuned pendulum model represents, I believe, a significant improvement of the prediction power of the simple pendulum equation discussed above. With some caution, and over a range of parameters not too different than those adjusted on the tests carried out with real apparatus, we can use the tuned pendulum model in order to investigate conditions of operation, which, for whatever reason, might not be realizable in the real apparatus.

1.2 The coupled-pendulums computer model

In the previous section I presented a model for one pendulum. We can now develop this into a more complex model for two pendulums mechanically coupled. Consider the structure represented below (Fig. A1.2).

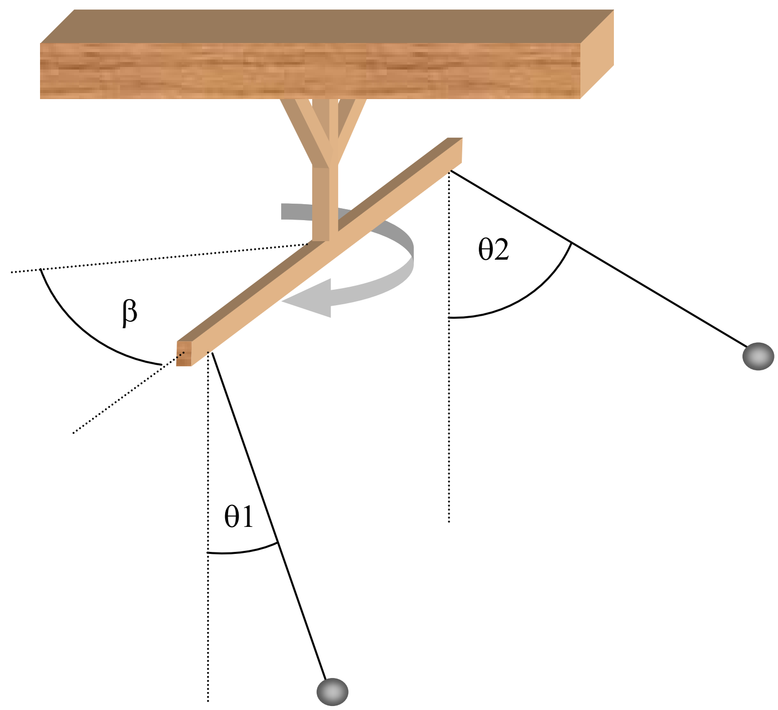

Fig. A1.2 A simple structure allowing for mechanical coupling. To describe the equations of motion of this structure three degrees of freedom are required, which can be assigned by means of three angles.

Three degrees of freedom are required to write the equations of motion of the two pendulums interfering through the mechanical structure. Two angles are sufficient to describe the oscillations of the two pendulums (imagined as occurring on a plane of oscillation that does not change orientation in space), but a third angle must be introduced to describe the rotation of the horizontal arm around a vertical axis. By neglecting not only all of the structural characteristics of the material of which the horizontal arm is supposed to be made, but also the mechanics of its joint with a fixed support, the horizontal arm can be modelled quite simply, on a first approximation, with a second-order ordinary differential equation whose two parameters are frequency and damping. These two parameters, in other words, summarily take into account all the complex effects due to structure and friction in the joint. This reducing of the complex structural and mechanical behaviour of the arm to two parameters is not a serious limitation, since by varying the parameters in the model, within a plausible range of values, we can explore the effects that they generate.

The following system of equations contains the three equations of motion of the two pendulums and the horizontal arm, coupled by the flexibility of the structure represented above.

$$\begin{cases} m_1 \cdot R_1^2 \cdot \ddot{\theta}_1 = -m_1 \cdot g \cdot \sin(\theta_1) \cdot R_1 + FA_1 - R_1 \cdot m_1 \cdot \dfrac{L}{2} \cdot \ddot{\beta} \cdot \cos(\theta_1) \\ m_2 \cdot R_2^2 \cdot \ddot{\theta}_2 = -m_2 \cdot g \cdot \sin(\theta_2) \cdot R_2 + FA_2 + R_2 \cdot m_2 \cdot \dfrac{L}{2} \cdot \ddot{\beta} \cdot \cos(\theta_2) \\ \ddot{\beta} + 2 \cdot \xi \cdot \omega \cdot \dot{\beta} + \omega^2 \cdot \beta = \tau_1 \cdot \sin(\theta_1) - \tau_2 \cdot \sin(\theta_2) \\ \tau_2 = m_2 \cdot g \cdot \cos(\theta_2) + m_2 \cdot R_2 \cdot \dot{\theta}_2^2 + m_2 \cdot \dfrac{L}{2} \cdot \ddot{\beta} \cdot \cos(\theta_2) \\ \tau_1 = m_1 \cdot g \cdot \cos(\theta_1) + m_1 \cdot R_1 \cdot \dot{\theta}_1^2 - m_1 \cdot \dfrac{L}{2} \cdot \ddot{\beta} \cdot \cos(\theta_1) \end{cases}$$

The symbols have the following meaning.

- m_1, m_2 masses of the pendulums
- R_1, R_2 lengths of the strings of the pendulums
- θ_1, θ_2 angles of the pendulums
- FA_1, FA_2 aerodynamic forces on pendulums
- L length of horizontal arm
- g gravity constant
- β angle of the horizontal arm
- τ_1, τ_2 tension in the strings of the pendulums
- ξ damping parameter of horizontal arm
- ω frequency parameter of the horizontal arm

Note that there are five equations but only three degrees of freedom, i.e., only three variables are required to describe the motion of the pendulums and horizontal arm, since two equations are used only to calculate the tensions in the strings of the pendulums. However, since the latter two equations are coupled with the third, it is necessary, to speed up the calculation process, to resolve algebraically the system of the last three equations in order to make explicit the tensions and the angle of the horizontal arm. Once this is done a system is obtained of three equations of motions which can be easily implemented numerically, and efficiently solved.

APPENDIX 2. The pendulum apparatus

In the collective book reporting their experiments (published in 1666), the Galilean experimenters of the *Accademia del Cimento* presented a pendulum in the form of gallows-like structure. It was depicted in the historiated capital of the first paragraph of the section on time measurements. It struck me. It is a simple but elegant structure. In the marginal note, we read, "Experiences that require the exact measure of time [*Esperienze, che richiedono la misura esatta del tempo*]".

The real instrument, built and used by the *Accademici*, is then depicted in detail in a full-page table (Fig. A2.1). I was inspired by the pendulum of the *Accademici*, which might in turn have been inspired by Galileo via the mediation of Galileo's pupil, Vincenzo Viviani, himself a member of the Cimento Academy.

Unfortunately the only suggestion we get from Galileo concerning his pendulum set-up is the diagram accompanying the 1602 letter to Guido Ubaldo (cf. T1). The arrangement suggested in T1 is unconvincing. It is of course quite possible that Galileo initially had two 2-3 braccia pendulums adjacent to each other on a wall. But if we consider that later on he refers to 4-5 braccia pendulums, I think that the T1 arrangement becomes implausible. In order for the pendulums not to hinder each other, they would have to be hung at a distance such as to allow their full extension. This means a wall of at least 20 braccia, about 7 meters, which would then require the observer to stand at a considerable distance, thus making the observations rather difficult. Further, the 1602 letter is preserved only in a third-hand copy of the nineteenth century. The diagram of the two pendulums looks suspicious since, in the text, Galileo says that one of the pendulums is removed from the perpendicular “a lot [*assai*]”, while the other is removed “very little [*pochissimo*]”, yet the two arcs drawn in the diagram are almost of the same amplitude. Moreover, it is possible that, even if accurate, the diagram only had an illustrative function, and was not intended to describe a real set-up.

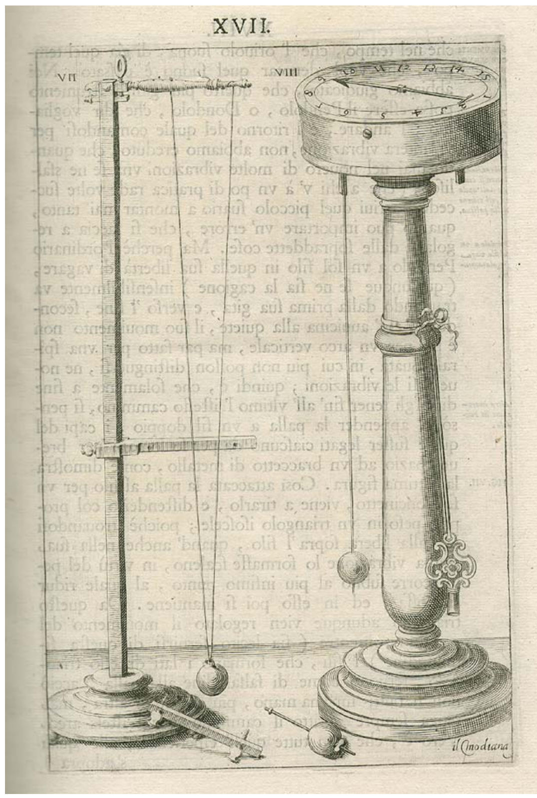

Fig. A2.1 The pendulum used by the *Accademici del Cimento*. The shape of the pendulum support (left) initially inspired the gallows-like structure shown in section 2, Fig. 2.1.1. The *Accademici*’s pendulum, though, seems to be a very small instrument. See Magalotti 1666, p. 21.

I therefore came to the conclusion that a different structure might have been used by Galileo. The *Accademici* offered me a plausible solution. The gallows-like structure has the advantage of allowing for easier observation of the oscillations of two pendulums relative to each other. Since isochronism and synchronism, as explained in section 2, were the focus of the investigation, this seemed an ideal arrangement for the pendulums.

I modified a wooden easel in order to build a structure which could easily accommodate the electronics needed to acquire the data for the computer model of the pendulum (see the picture shown in section 2). The suspicion that a phenomenon of mechanical coupling might be lurking in structures like this, as I had already predicted on the basis of simple computer models, suggested that I build a light horizontal arm which could subsequently be stiffened by means of cables fixed to the walls of the lab. This solution worked well and allowed for a certain freedom in adjusting the cables, so as to control the amount of residual flexibility left in the structure.

The data acquisition system is based on miniature, high precision load cells capable of measuring the tension in the strings of the pendulums. Ideally one could measure directly the angular position of the pendulums, but in practice, since the strings are not rigid, easy and economically affordable solutions, based on angular position sensors, such as, for instance, rotation potentiometers, are unavailable. On the other hand, the measurement of tension made possible by load cells is very precise.

Fig. A2.2 The horizontal arm, made of Plexiglas, showing the two load cells, above it, two hemp strings directly connected to the load cells, a few hooks, and the stiffening cables fixing the horizontal arm to the wall of the lab.

The load cells are then connected to a data acquisition system, not shown above, which converts the analogical signals from the cells into a numerical format. The acquisition system is connected to a laptop computer, on which special software allows for further elaboration of the acquired data.

The load cells I used are capable of reading loads up to 100 grams, with virtually infinite sensitivity, only limited by the finite length, i.e., number of bits, of the output from the analogue to digital converter inside the data acquisition system (a 14-bit A/D converter in the system I used). The range and high precision of the load cells is perfectly suited to measurements with pendulums carrying light lead balls, in the range of the 1-2 ounces that Galileo might have used.

The tension generated in the string of the pendulum can easily be calculated while simulating the mathematical model, so that an accurate comparison can then be made between acquired tension data and calculated tension data. This comparison is the basis for the fine-tuning of the aerodynamic parameters of the mathematical model of the pendulum (see the discussion of the pendulum computer model in Appendix 1).

As for the materials of strings and balls, let us first note that often Galileo refers to the strings of his pendulums as “spago” or “spaghetto”. A “spago” would presumably have been made of either hemp or linen. The strings of my pendulums were thin and made of natural hemp or of linen (about 1.5 mm in diameter, though diameter varies, especially for hemp strings). Natural hemp tends to be more fluffy and knotty than linen, so I initially thought that this might be a factor in determining aerodynamic forces, but it turned out that there is no appreciable difference between strings having more or less fluff or little irregularities such as small knots. In fact the aerodynamic forces are due basically to the length and thickness of the string. The 1623 edition of the *Vocabolario degli Accademici della Crusca*, says that “canapa [hemp]” was used to make “corde, funi, e anche tele”, and that a “spago” is a “funicella”, i.e., a thin “fune”, which therefore suggests to me that hemp would likely have been used by Galileo for his “spago” or “spaghetto”.[37]

As far as lead balls are concerned, I investigated the possibility that Galileo used musket bullets, since they were roughly the size of 1-2 ounces (as he indicates on one occasion, in the experiment of the pendulum twisting around the peg is 1-2 ounces, as we have seen).

[37] Cf. Accademici della Crusca 1623, ad vocem.

Bullets would have been easily available to him, I guess, since he was in contact with military students and engineers, especially in Padua. I purchased a few historic musket bullets, relics of the English Civil War, and noticed that they tend to be rather irregular both in size and weight. If this is any indication of the quality of early seventeenth-century bullets, and if Galileo used anything like bullets, then we must make allowances for possible differences in what he tends to call lead "balls [*palle*]". On the other hand, Galileo might also have cast his own lead balls. The technology would have been available to him since the melting temperature of lead is not too high.

At any rate, I eventually decided to do my experiments with modern lead balls, which are commonly used in fishing. They come in different sizes and have a hook, so that it is easy to experiment with different weights. I noticed no difference between a modern lead ball and a historic bullet of roughly the same size and weight, though the bullet, not having a hook, has to be tied to the string differently. As for the cork balls, I need to point out that, since cork is a natural material somewhat variable in specific weight, it is impossible to say exactly how large Galileo's cork balls would have been, even when the weight can be estimated with some accuracy (and the same could be repeated of early modern lead, though perhaps to a lesser extent).

Finally, as for the video material, I used a camera with wide-angle lens to shoot the videos in the lab, since the amplitude of the oscillations of the pendulums can be very large. Subsequently, the videos were downloaded into a laptop computer, reviewed, edited, and finally converted to a suitable format for delivery via the internet or other digital media.

APPENDIX 3. Galileo's pendulum texts

In this appendix, I furnish translations of Galileo's most relevant texts concerning the pendulum.

T1 (EN, X, pp. 97-100).

Letter of Galileo to Guido Ubaldo dal Monte, from Padua, dated 29 November 1602.

Most Illustrious Sir and Revered Master

I beseech You to excuse my insistence in persuading You that the proposition of the motions done in equal times in the quadrant of circle is true; for, it always having seemed admirable to me, now I fear that it might be considered impossible by You. Thus, I would consider it my grave error and deficiency, if I allowed it to be repudiated by your thinking, as if it were false, since it does not merit this qualification, let alone deserve to be banished from Your mind; especially since You are the one, more than anyone else, who could retrieve it most quickly from exile. Given that the experience by which I became clear about this truth is certain, although I must have explained it rather confusingly in my previous letter, I will repeat it now, so that You can ascertain this truth by replicating this experience.

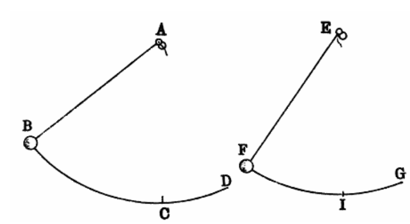

Fig. T1.1

I take two thin strings (Fig. T1.1), equally long about two or three braccia, let them be AB, EF, and attach them to two nails, A, E, while at the other ends I tie two equal lead balls (although it would make no difference if they were different). After removing the strings from the vertical, one a lot, as along arc CB, the other very little, as along arc IF, I let them go at the same moment. One begins to describe great arcs, similar to BCD, while the other describes small arcs, similar to FIG; yet mobile B does not employ more time traversing the whole arc BCD, than the other mobile, F, traversing arc FIG. I make sure that this must be the case as follows.

Mobile B traverses the great arc, BCD, and comes back along the same DCB, and returns towards D, reiterating 500 and 1000 times its reciprocations. The other equally will make many reciprocations. During the time while I count, for example, the first hundred great reciprocations BCD, DCB etc., another observer will count a hundred other reciprocations along FIG, very small, without counting one more: a most evident sign that each single one of the greatest [oscillations] BCD takes as much time as any single one of the smallest [oscillations] FIG. Now, if the whole BCD is traversed in as much time as FIG, then their halves, too, which are the falls along the unequal arcs of the same quadrant, will be traversed in equal times. But even without bothering to count, You will see that mobile F will not make its smallest reciprocations more frequent than mobile B its own greatest, but they will always go together.

The experience, which you tell me you did with the box, may be most uncertain, perhaps because its surface is not very smooth, perhaps because it is not perfectly circular, and perhaps because it is impossible to observe the moment of the beginning of motion in one passage. However, if You still desire to use this concave surface let ball B go from a great distance, for example from point B, which ball will reach point D, and at the beginning will make its reciprocations large, but at the end small, and yet the latter will not be more frequent than the former.

As to the fact that it seems unreasonable that, of a quadrant 100 miles long, two mobiles might traverse, one of them the whole of it, the other only a palm's width, in the same time, I admit that it is admirable. But let us consider that a plane might be so little inclined, as that of the bed of a river which flows so very slowly, that a body along such a plane would not traverse more than a palm's length, in the time that another body on a much more inclined plane (or even on a modest inclination if conjoined with a great impetus) will have traversed one hundred miles. Yet this is not more incredible than the geometric proposition stating that triangles between two parallels and on equal bases are equal, even though one can make a triangle very short, and another as long as a mile. But to go back to the our subject, I think I have demonstrated this conclusion, which is no less certain than the other.

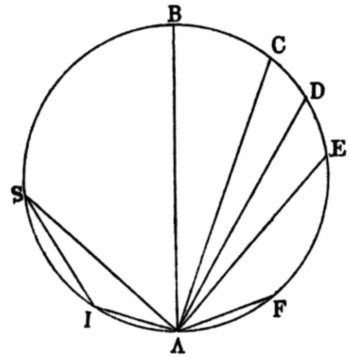

Fig. T1.2

Let diameter AB, in circle BDA (Fig. T1.2), be perpendicular to the horizon, and from point A let lines be drawn to the circumference, such as AF, AE, AD, AC: I prove that equal bodies fall in the same time along the vertical BA and the inclined planes CA, DA, EA, FA. Thus, if they start at the same moment from points B, C, D, E, F, they will arrive at the same moment at point A, no matter how small is line FA.

The following, which I have also demonstrated, may perhaps appear even more incredible. If the line is not greater than the chord of a quadrant, and if the lines, SI, IA, are taken as one pleases, the same body will more quickly traverse path SIA, starting from S, than the single path IA, starting from I.

So far I have demonstrated without transgressing the boundaries of mechanics; but I am unable to demonstrate that arcs SIA and IA are traversed in the same time, which is what I seek.

Please send my regards to Signor Francesco, and tell him that as soon time allows me I will describe to him an experiment, which I imagined, in order to measure the moment of percussion. As to his question, I concur with Your opinion, namely, that when we begin to concern ourselves with matter, because of its contingency, the propositions demonstrated by geometers in the abstract begin to be altered. As for these propositions, thus perturbed, since no certain knowledge of them can be assigned, the mathematician is absolved from speculating.

I have been too prolix and tedious, please forgive me and consider me as Your very devoted servant. With my utmost reverence to You.

T2 (EN, VII, pp. 256-257).

Excerpt from the *Dialogue on the two chief world systems* (1632).

SALV. Tell me: of two different pendulums, does not that which is attached to the longer string make its vibrations less frequently?

SAGR. Yes, if they moved along equal distances from the vertical.

SALV. This removal from the vertical, being more or less, does not matter, since the same pendulum always makes its reciprocations, whether they are through very long or short arcs, under the same times, regardless of the pendulum's being removed from the vertical a great deal or very little; or, if they are not exactly equal, they are negligibly different, as experience can show you. However, even though they were quite different, this would not go against our opinion.

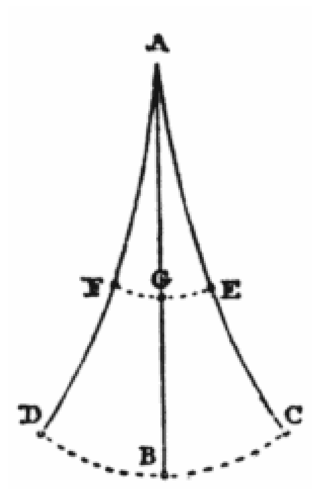

Fig. T2

Let us consider perpendicular AB (Fig. T2), and let a weight C hang from point A by rope AC, and let us consider another weight, E, above. Now, after removing rope AC from the perpendicular, let it be released. Weights C, E will move along arcs CBD, EGF. Weight E, insofar as hanging from a shorter distance, and also less removed from the perpendicular (as you suggested), wishes to go back sooner, and make its vibrations more frequent than weight C, so that it will prevent the latter from running towards point D as much as if it were free. In this way, by impeding it in every vibration, it will make it stop.

Now, the rope itself (if one does not consider the weights) is a composite of many heavy pendulums; that is, each part of the rope is a pendulum, attached closer and closer to point A, and therefore such that it will make its vibrations more and more frequently. In consequence, a part can impede continuously weight C. A sign of this is that if we observe rope AC, we will see it not elongated rectilinearly but curved. If instead of a rope we take a chain, we will see this effect more evidently, and most of all when we remove weight C from the perpendicular a great deal. For, since the chain is compounded of many disjointed particles, each of which is very heavy, arcs AEF, AFD will be seen very curved. For this reason, namely, that the parts of the chain want to make their vibrations more frequent, according as they are closer to point A, thus preventing the parts below from running as far as they would naturally do, which continuously diminishes the vibrations of point C, the parts of the chain will ultimately stop weight C, even if the impediment of air could be removed.

T3 (EN, VII, pp. 474-476).

Excerpt from the *Dialogue on the two chief world systems* (1632).

SALV. Thus, I say that it is true, natural, and necessary, that the same body, moved circularly by the same moving virtue, will run its course along a greater circle in a longer time than along a smaller circle. This truth is accepted by everybody, and confirmed by all sorts of experiences, of which we will put forward some. […] Further: let a weight be attached to a rope which twists around a nail fixed in a wall, and keep the other end of the rope with your hand; after letting the pendulum go, while it makes its vibrations, pull the end of the rope with the hand, so that the weight will be lifted. You will see that while it rises the frequency of its oscillations will increase, as if they were made continuously along smaller circles. Here I want you to take notice of two things worth knowing. First, the vibrations of such a pendulum are made so necessarily under so determined times, that it is impossible to make the pendulum vibrate under different times, except by lengthening or shortening the rope. You can ascertain this by experience, by tying a stone to a string and keeping the other end in your hand. Try as you might, with any stratagem whatever, you will be unable to make it oscillate under another time than its own, unless you lengthen or shorten the string. The second fact, really marvellous, is that the same pendulum makes its vibrations with the same frequency, or very little, almost negligibly different, regardless of whether they are made along the greatest or the smallest arcs of the same circumference. I say that if we remove a pendulum from the perpendicular, one, two, or three degrees, or if we remove it 70, 80, or even 90 degrees, once released, it will in both cases make its vibrations with the same frequency, both those where it traverses an arc of 4 or 6 degrees, and those where it traverses arcs of 160 degrees, or more. This will be most clearly seen by suspending to equal weights by two strings of the same length, and by removing one from the perpendicular a small distance, and the other a much greater distance. When let go they will go back and forth under the same times, one along very long arcs, the other along very small ones.

T4 (EN, VIII, pp. 128-129).

Excerpt from the *Two new sciences* (1638).

SALV. The experiment made with two mobile bodies as different in weight as possible, by letting them fall from a height to observe if their speed is the same, labours under some difficulties. For, if the height is great, the medium that must be cleaved and laterally pushed by the impetus of the falling body, will impede much more the small moment of the light body than the violence of the heavy body, so that the light body will lag behind by a long distance. If the height is small, it would be doubtful whether there truly is any difference, or perhaps if there were one, it would be unobservable. Thus, I thought I could reiterate many times the fall from small heights, so as to accumulate many small differences of time, which might occur between the arrivals of the heavy and the light bodies, in such a manner that once conjoined these differences would add up to an easily observable time. Furthermore, to take advantage of motions as slow as possible, in which the resistance of the medium alters much less the effect dependent on simple gravity, I thought I might have the bodies descend on an inclined plane, but slightly elevated above the horizon, for how heavy bodies of different weight behave would be observable both on an inclined plane and along a vertical descent. Then, wishing to eliminate the impediments that might be due to the contact between the mobile bodies and the plane, I decided to take two balls, one of lead and the other of cork, the former more than a hundred times heavier than the latter. I attached both to thin strings, equally long, about 4-5 braccia, and fixed above. After removing both from the perpendicular, I let them go at the same instant. While descending along the circumference of the circles described by the equal strings, the balls, after passing beyond the vertical, have come back along the same paths. By reiterating their goings and comings a full hundred times, on their own, they have shown to the senses that the heavy one goes so nearly under the time as the light one, that neither in one hundred, nor in one thousand vibrations, does it anticipate the time by a moment; for, both move with exactly the same pace. Further, the operation of the medium can be discerned, since, by somewhat impeding motion, the medium much more decreases the vibrations of the cork ball than those of the lead ball; yet, it does not make them more or less frequent. On the contrary, if the arcs traversed by the cork ball are no more than 5-6 degrees, and those traversed by the lead ball no more than 50-60 degrees, they will be traversed under the same times.

T5 (EN, VIII, pp. 139-140).

Excerpt from the *Two new sciences* (1638).

SALV. We will see if from these pendulums of ours, we can extract the solutions to our difficulties. As to the first doubt, which is, if truly and most exactly the same pendulum makes all its vibrations, the greatest, the intermediate, and the smallest, under precisely equal times, I will refer you to what I heard from our Academician. He proves clearly that a mobile body that descends along the chords of any arc would traverse all of them necessarily in equal times, not only that corresponding to 180 degrees (i.e., to the diameter), but also those corresponding to arcs of 100, 60, 10, 2, ½, degrees, or even a fraction of a degree, on the understanding that all chords converge to the bottommost point which touches the horizontal plane. As far as the bodies descending along the vertical arcs of the chords, which are no greater than a quadrant, i.e., 90 degrees, experience shows that all the arcs are traversed in the same times, but shorter than the times of the chords; which effect is marvellous, in that, superficially, it seems that the contrary would have to happen, for, given that the starting and ending points of motion are the same, and given that the straight line is the shortest line between two points, it would seem reasonable that the motions along the straight chords would be the shortest; but this does not happen. Indeed, the shortest time, and therefore the fastest motion, is that along the arc of the chord. As for the ratio of the times of mobiles hanging from strings of different length, they are in the subduple ratio [i.e., *the square root*] of the ratio of the lengths of the strings. In other words, the lengths of the strings are in the duplicate ratio of the times, i.e., they are as the squares of the times. Thus, for example, in order for the vibration time of one pendulum to be double the vibration time of another pendulum, the length of the string of the former must be quadruple the length of the string of the latter. In this way, during one vibration of a pendulum another will make three, if the string of the former is nine times that of the latter. From this it follows that the lengths of the strings have to one another the same ratio as the squares of the numbers of vibrations made in the same time.

T6 (EN, VIII, pp. 277-278).

Excerpt from *Two new sciences* (1638).

SALV. As for the other part, i.e., to show that the impediment that the same mobile body receives from air, while the mobile moves with a great speed, is not much greater than that which it receives when moving slowly, the following experience will afford certainty that this is indeed the case. Let two equal lead balls be attached to two equally long strings, of about 4-5 braccia, and after fixing these strings above, let the two balls be removed from the perpendicular, but one 80 degrees or more, the other no more than 4-5 degrees, in such a way that, when released, one will descend, and after crossing the perpendicular, will describe great arcs of 160, 150, 140 degrees, etc., which diminish little by little; but the other, while oscillating, will go along small arcs of 10, 8, 6, etc., which also diminish little by little. I now say that the first ball will traverse its degrees of 180, 160, etc., in the same time as the second ball traverses its degrees of 10, 8, etc. From this it is manifest that the speed of the first ball will be 16 and 18 times greater than the speed of the second. Thus, if the greater speed were impeded from the air more than the smaller speed, the vibrations in the great arcs of 180 and 160 degrees, etc., would have to be less frequent than those in the small arcs of 10, 8, 4, and even 2 and 1. But experience does not agree with this, for, if two observers counted the vibrations, one the smallest and the other the greatest, they will count not only tens of them, but hundreds, without a discrepancy of one count, or even of a fraction of a count. This observation makes us certain of two propositions, namely, first, that the greatest and smallest vibrations are all made under equal times, and, second, that the impediment due to air does not operate more in the fastest motions than in the slowest, contrary to what we thought a short while ago.

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