About Galileo Galilei

An orderly summary of thirty years of intermittent and disorderly work

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Editor's foreword

Did Galileo actually do all the experiments he talked about?

Alexander Koyré, who was its first historian, came to deny it, arguing that Galileo Galilei did not perform **all** the experiments he spoke of. Subsequently, however, many other historians have dismissed Koyré's statement as a biased personal opinion.

Our author, Juan Luis, will demonstrate,
repeating the same ones,
including their inaccuracies,
that Galileo did have to perform the
experiment of the sphere
falling on an inclined plane

Our author throughout this text makes a detailed dynamic analysis of a sphere falling down an inclined plane and explains how he put Galileo's experiments into practice, also involving many of the students he had throughout his career as a high school physics teacher. Juan Luis makes us enjoy the observation of physical phenomena and teaches us how we can manage with few technical resources to obtain reliable data capable of supporting a whole theory.

We move in time and mentality. We repeat Galileo's briefly described original experiments; we obtain measurements, we fill in the gaps in Galileo's pre-scientific documentation. This leads us to see details that Galileo himself missed when measuring distances. But above all we come to only one possible conclusion: the results that Galileo scribbled on his parchments could not have been deduced or invented, they are numerical results that can only correspond to real experiments.

The author of our book passionately defends not only Galileo's word, but also infects that healthy experimental spirit that objective observers have. Galileo's physical setups are accessible, they can easily be repeated in first person. The main consequence of Galileo's and Juan Luis' numbers is the natural phenomenon that the velocity of the sphere increases continuously with the time of fall. If the velocity were constant, the space travelled would be directly proportional to the time. As the velocity increases linearly, the space increases with the square of the time. This is the first physical law of dynamics, which gave rise to all subsequent physical science. This analysis was made by a 16th century thinker, in a world dominated by religious beliefs and Aristotelian physics created and closed 200 years before Christ. To understand Galileo's analysis, one must, with him, analyse details and adopt that humble attitude, free of prejudice, which undoubtedly made Galileo the first scientist, the first in history, who had the courage to defend what one can see for oneself against imposed inherited ideas.

This book will leave that aftertaste in the reader: it will turn us into observers of details, it will teach us to analyse and measure them, it will make us deepen our respect for nature, not based on the blind admiration of the authority of the one who describes it, but on the personal discovery of its surprising laws and connections. Connections and laws that Galileo was passionate about throughout his life. Galileo broke with the pre-scientific dogma and opened the doors to the age of science. Not that of technology, which always existed, and to which he also sold his knowledge, but that of scientific observation. Galileo, then, and Juan Luis, now, open the doors to the beauty of the almost magical behaviour of nature, of the behaviour that can be communicated and experienced objectively without added interests or interpretations, of the knowledge that comes from our senses.

Our author, Juan Luis, agrees with Professor José Romo ((Romo, 2005)), although he never knew it, in the theoretical analysis of the data we have from Galileo; but Juan Luis also checks them and even sees the mistakes made in the original experimental setups. Juan Luis taught his students, among whom I count myself, to perform this type of experiments throughout a brilliant career as a high school teacher.

'The stimulating power of experiment' as Juan Luis calls it, is motivation enough for any curious researcher to try to test with reality what theories can predict, and in Galileo's case, it would have been strange indeed for him to combat the prejudices of his time without experimental basis. He made many observations with his improved telescope, and it was much easier to measure times and lengths to verify revolutionary dynamical theories that anyone could test for themselves.

On the problem of the credibility of the experiments

Interestingly, and I think it sums up the situation rather well, Naylor's opinion of the importance Galileo attached to the accuracy and predictability of his experiments is expressed here (Naylor, 1976):

> 'Historians have attempted to determine in which situations Galileo offered an experiment as a didactic device and in which he regarded the experiment as crucial evidence. As this has hitherto been attempted without any clear knowledge of which experiments were actual and which were imaginary, it is hardly surprising that there exists such a vast spectrum of opinion on the subject. It has even resulted in a widely accepted view that Galileo had little interest in actual experiment at all. This is largely a result of the influence of Alexandre Koyré, who argued very persuasively that good physics is made a priori. Though there is much to support this thesis, it is becoming increasingly clear that Koyré's philosophical predisposition led him to underestimate the role of experiment in Galileo's mechanics.'

In some of the texts, and even series of texts, by some authors, replications of experiments are detailed. However, the lack of specifications of the original experiments has made the replication of them more of an art than any meticulous procedure based on following a series of steps. Let us remember that we are in the 16th century. Our author reasons what angles and lengths Galileo must have used. He manages to put himself in the place of the first experimenter with a level of precision that most historians have not achieved. Throughout this text different forms of experiments are repeated, presenting their results and comparing them with those of Galileo. The final conclusion is inevitable.

In particular, the hardness of Koyré is surprising, who goes so far as to deny the realization of the experiments and, with it, part of Galileo's genius and who, for some mysterious reason, gives more merit to people born much later and who had access to the work, approaches and conclusions of the previous characters. Precisely this historical order of characters, in which many geniuses from many countries participated, would place Galileo at the top of a mountain to which the bravest ones ascended, those who confronted the most deeply rooted prevailing prejudices. From him, the path was already laid. In Florence, not far from the museum of Leonardo da Vinci, the museum dedicated to Galileo represents a tribute not only to the great genius, but to the birth of science itself. Many books have been dedicated to Galileo. One of the most complete and exhaustive books is the recent one by Caffarelli (Caffarelli, 2009), in which all his work is meticulously detailed. This museum can only dedicate one room to the few instruments that Galileo used, the other many rooms are full of devices such as telescopes, microscopes, scales,

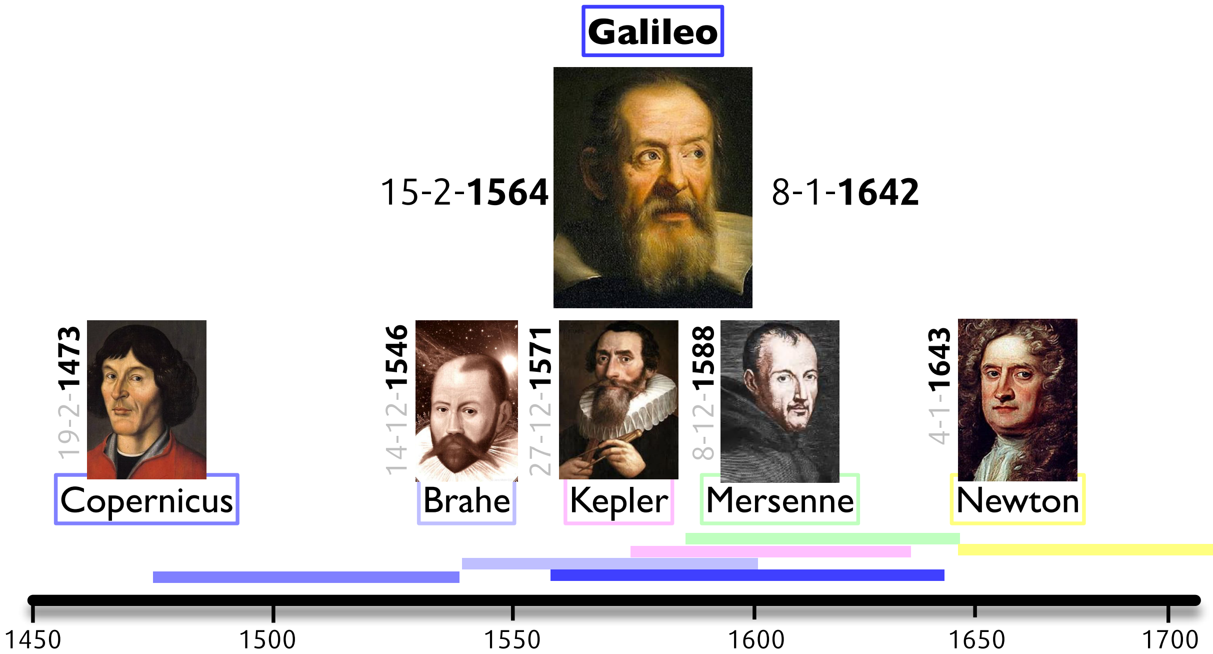

Figure 1: Lives of personalities related to Galileo

etc, of so many other characters of the time, reflecting how the birth of science was the result of small contributions and, above all, a change of mentality in the history of mankind. The indisputable great role of Galileo was in the confidence that Nature followed the logic of mathematics, and the confidence that experiments, without prejudice, would lead us to find the relationships between natural phenomena, thus being the first to experiment in as many areas of physics as he could: astronomy, mechanics, fluids, and many others, as long as he could make measurements and find mathematical relationships in them. In astronomy his description of Jupiter's main satellites on January 6, 1610, Io, Europa, Ganymede, and Callisto was so accurate, as was his mapping of moon spots, that it is still valid today.

References to Galileo by other great physicists

Stephen Hawking (Hawking, 1989):

> "Galileo, perhaps more than any other single person, was responsible for the birth of modern science. His renowned conflict with the Catholic Church was central to his philosophy, for Galileo was one of the first to argue that man could hope to understand how the world works, and, moreover, that we could do this by observing the real world"

Albert Einstein (Einstein, 1953; Galilei, 2017):

> "The most important single step in the history of physics was Galileo's realization that natural laws can be discovered by systematic observation and rational deduction, rather than reliance on metaphysical assumptions"

> "Propositions arrived at by purely logical means are completely empty as regards reality. Because Galileo realised this, and particularly because he drummed it into the scientific world, he is the father of modern physics – indeed, of modern science altogether."

Steven Strogatz (Strogatz, 2019):

> "Galileo was the first practitioner of the scientific method. Rather than quoting authorities or philosophizing from an armchair, he interrogared nature through meticulous observations, ingenious experiments, and elegant mathematical models. His approach led him to many remarkable discoveries. One of the simplest and most surprising is this: The odd numbers 1, 3, 5, 7, and so forth are hiding in how things fall.
>
> Before Galileo, Aristotle had proposed that heavy objects fall because they are seeking their natural place at the center of the cosmos. Galileo thought these were empty words. Instead of speculating about *why* things fell, he wanted to quantify *how* they fell. To do so, he needed to find a way to measure falling bodies throughout their descent and keep track of where they were moment by moment.
>
> It wasn't easy. Anyone who has dropped a rock off a bridge knows that rocks fall fast. It would take a very accurate clock, of a kind not available in Galileo's day, and several very good video cameras, also not available in the early 1600s, to track a falling rock at each moment of its rapid descent.
>
> Galileo came up with a brilliant solution: He slowed the motion. Instead of dropping a rock off a bridge, he allowed a ball to roll slowly down a ramp. In the jargon of physics, this sort of ramp is known as an inclined plane, although in Galileo's original experiments, it was more like a long, thin piece of wooden molding with a groove

cut along its length to act as a channel for the ball. By reducing the slope of the ramp until it was nearly horizontal, he could make the ball's descent as slow as he wished, thus allowing him to measure where the ball was at each moment, even with the instruments available in his day.

To time the ball's descent he used a water clock. It worked like a stopwatch. To start the clock he would open a valve. Water would then flow steadily, at a constant rate, straight down through a thin pipe and into a container. To stop the clock, he would close the valve. By weighing how much water had accumulated during the ball's descent, Galileo could quantify how much time had elapsed to within 'one-tenth of a pulse-beat.'

He repeated the experiment many times, sometimes varying the tilt of the ramp, other times changing the distances rolled by the ball. What he found, in his own words, was this: 'The distances traversed, during equal intervals of time, by a body falling from rest, stand to one another in the same ratio as the odd numbers beginning with unity.'

To spell out this law of odd numbers more explicitly, let's suppose the ball rolls a certain distance in the first unit of time. Then, in the next unit of time, it will roll *three* times as far. And in the next unit of time after that, it will roll *five* times as far as it did originally. It's amazing; the odd numbers 1, 3, 5, and so on are somehow inherent in the way things roll downhill. And if falling is just the limit of rolling as the tilt approaches vertical, the same rule must hold for falling.

We can only imagine how pleased Galileo must have been when he discovered this rule. But notice how he phrased it— with words and numbers and proportions, not letters and formulas and equations. Our current preference for algebra over spoken language would have seemed cutting-edge back then, an avant-garde, newfangled way of thinking and speaking. It's not how Galileo would have thought or expressed himself, nor would his readers have understood him if he had."

Acknowledgements

*To **Juan Ignacio Ramos Sobrados**, thank you for detecting what escapes all eyes but yours. Your attentive and generous reading was a true gift for this book.*

About the edition

I have tried to respect the original text as much as possible, but I have inevitably modified the formatting since I found it as a series of files in *MS-Word* format that certainly no longer reflected the careful style of our author. On the other hand, its conversion to LaTeX/Ti*k*Z made me try to enrich and correct the way of referencing formulas, tables and graphs. Apart from all this aesthetic issue, there is the issue of content. The way in which the book was initially presented responds, as the author tells us, more to memoirs than to a book to be read. I have respected the order of the chapters, but I thought it necessary to add appendices and an introduction to adapt it to what we understand today as a book, perhaps not so much a book to be read, but one that allows easy access to readers with a high school level of knowledge of physics. So, we must recognize that yes, it is inevitable, that it is necessary to know physics at least at high school level to understand many of the details described in this book. However, we believe that what is most interesting is not the author's inevitable physical analysis of the experiments, but the author's actual repetition and description of the experiments, as well as the conclusions and criticisms of the historians' work. The editor understands that this work may in itself constitute a sufficient basis to demonstrate beyond doubt that Galileo did perform these experiments, and from the analysis he made of these and others, he most probably did not fail to enjoy doing them all with the greater skill than in those times, when there were not even units of measurement whose standard could be more distant than the limits of the city where one lived.

Since the chapters have been developed at very different times, although with very different objectives, it is impossible that they do not contain repetitions of some ideas. This has made me collect all the relevant equations in an Appendix and some experiment, already mine, in another. I hope that the mathematics and physics at high school level that is handled in this book will refresh the memory of some and encourage others to enter into it.

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Preface by the author

Juan Luis Alcántara speaking at a book presentation. **October 22, 2009**

The eight chapters that follow contain an orderly summary of thirty years of intermittent and disorderly work.

In the first one I develop my personal theoretical reflections on the rolling of a rigid sphere on a deformable plane. These reflections began in 1994 and have lasted until 2008. The orderly and logical exposition that I present may make them seem to be the result of a short and intense period of work. Such an impression is misleading.

In the second chapter I compile the most significant aspects of my experimental work on the same subject, carried out between 1995 and 2002. The design of the experimental setup with which the chapter begins may appear as a logical consequence of the theoretical development presented in the previous chapter. This impression is also misleading. The truth is that both works – the theoretical and the experimental – were intertwined over the years. The experimental work – as we shall see later – began in 1979, much earlier than the theoretical work.

The decision to summarize in these two chapters the basic knowledge acquired by me on this subject responds to the desire to put the reader in a position to get the maximum benefit from reading the remaining six chapters. I do not believe, however, that it is necessary for the reader to study them in depth on a first reading. In any case, he will be able to return to them later if the subject treated in the remaining six chapters becomes of interest to him.

I begin the third one by reproducing Galileo's account in the 'Discorsi' of his famous experience with the inclined plane, as well as the arguments put forward by Alexandre Koyré to deny its plausibility. I continue by quoting I.B. Cohen, who is in favor of granting validity to the experience carried out by T.B. Settle in 1961 replicating the one described by Galileo in his famous text. Then I summarize and comment on the excellent experimental work carried out by T.B. Settle, citing the objections that – according to Federico di Trocchio – Ronald Naylord makes to him. At the end, and after drawing my own conclusions on all the above, I expose my first experimental incursion in this same subject, which dates back to 1979, when I was just a curious and undocumented novice.

In the fourth chapter I detail my own experiences in 1979 aimed at verifying first hand the principle of superposition of motions proposed by Galileo. These experiences provided me with very pleasant and enriching surprises, both from a personal and professional point of view.

In the fifth chapter I develop my research on an unpublished folio of Galileo, the folio 116^v, discovered and interpreted in 1973 by Stillman Drake. When I started these researches -at the end of 2003- I had already retired several months before, finally getting access to a computer, a technology that I refused to use during my active life as a teacher, but I was still far from becoming an internaut. When I finally gave up -at the end of 2006- I discovered on the net the existence of another unpublished folio of Galileo, the folio 81^r. I realized then that my theoretical/practical work on the *rolling of a rigid sphere on a deformable plane* could serve to justify and complete the interpretation that Stillman Drake offered in his day of the folio 116^v as well as to propose interpretations of my own invention for the folios 114^v and 81^r. That is what I set forth in chapter six.

The seventh begins with a description of a small chance finding made while using my computer's drawing program. I based myself on that finding to thread an account of how Galileo was able to initiate and order his empirical inquiries about motion, beginning with the one he describes in his famous account of the 'Discorsi' and continuing with those suggested in the three enigmatic unpublished folios. In the eighth chapter I give a detailed account of my own home experiences made in support of all that has been stated in the three preceding chapters.

In the eighth chapter, I give a detailed account of my own home experiences in support of the previous three chapters.

Juan Luis Alcántara López
La Línea de la Concepción, 14–IX–2009

Chapter 1

Friction in the rolling of a sphere. Theory

1.1 Introduction

From 1979 until 2002 -the year I retired as a Physics and Chemistry teacher- one of the resources I used with my students to introduce them to *kinematics* consisted of reproducing with modern material the experience of the sphere rolling on an inclined plane described briefly by Galileo in the third day of the third day of the 'Discorsi'. One problem on which I concentrated my attention from the beginning was of the *dynamic* type: *How to evaluate the frictional forces that the plane and the sphere exert on each other*?

In January 1994 I began my personal research on this matter, since the subject, perhaps because it is considered of little interest, does not appear treated in depth in any of the Physics textbooks I have consulted throughout my life. In the following sections I will try to develop my theoretical reasoning on this issue.

1.2 Coefficient of rolling friction

If a metal sphere rolls over a wooden plane both will undergo deformation in the mutual contact zone, but the deformation of the plane - represented in Figure 1.1 by the arc $\widehat{A'B'}$ subtending an angle λ at the centre of the sphere - should be more pronounced than that experienced by the sphere itself as the metal is stiffer than the wood. The mutual interaction will be located in the small *contact zone* around the arcs $\widehat{AB}$ of the sphere and $\widehat{A'B'}$ of the plane, but if we consider that λ should be very small, we can symbolise it by means of two vectors applied at B and B'. In pure rolling without sliding, points A and A' must remain at relative rest, while B' sinks as the sphere rolls to the right. Therefore the sphere is transferring its kinetic energy to the plane by working on it through a force applied at B'. We will refer to Figure 1.2 below. In it we can see that $\beta = \lambda/2$ as the sides that form both angles are perpendicular to each other.

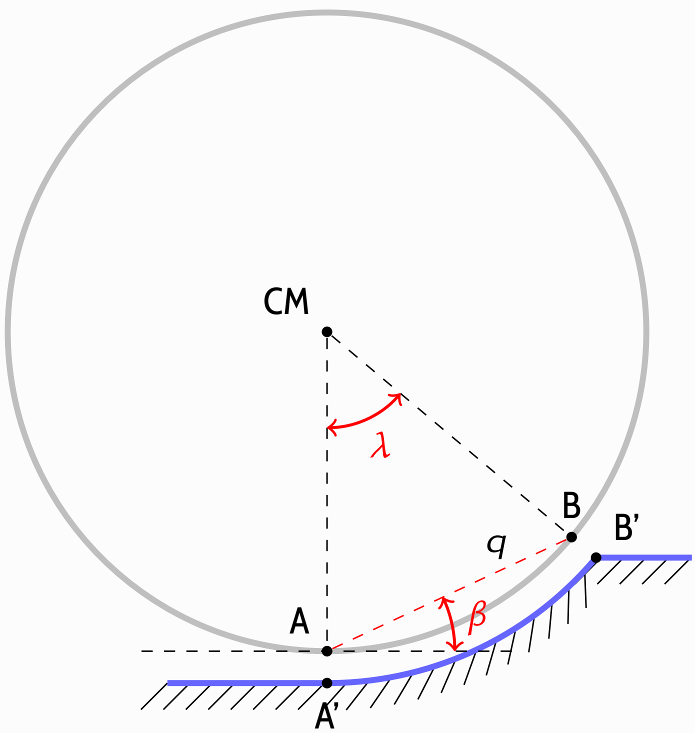


Figure 1.1: Deformation of the plane

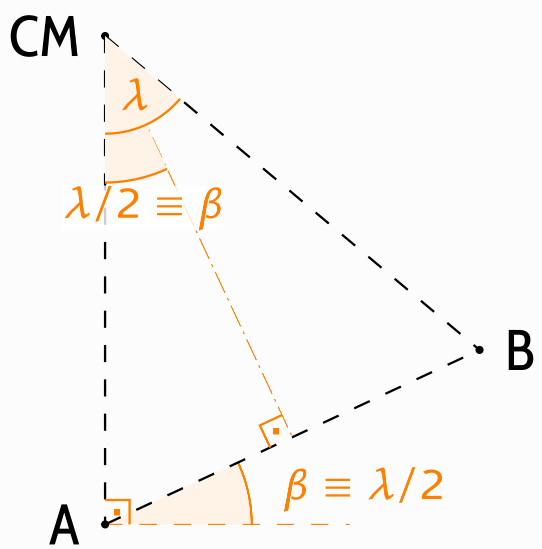


Figure 1.2: Angles $\beta = \lambda/2$

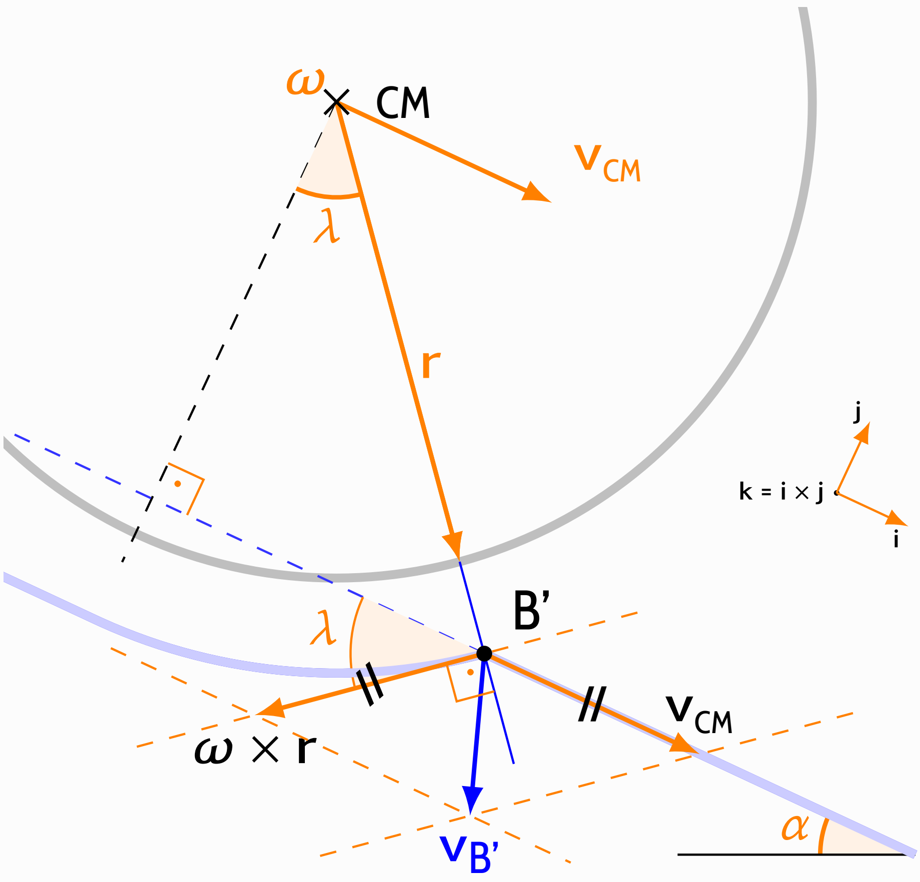


Figure 1.3: Velocities

In Figures 1.3 and 1.4 we show the same sphere rolling on the same plane, now with an inclination of α. The instantaneous velocity with which point B' sinks as a consequence of the advance of the sphere will be given by the equation:

$$\mathbf{v}_{\mathrm{B'}} = \mathbf{v}_{\mathrm{CM}} + \boldsymbol{\omega} \times \mathbf{r}$$

Note in Figure 1.3 that the angles λ with vertices at CM (Centre of Mass) and at B' are equal to each other because their sides are perpendicular. This allows us to express the result of the vector product $\boldsymbol{\omega} \times \mathbf{r}$, in Cartesian coordinates, by means of:

$$\boldsymbol{\omega} \times \mathbf{r} = -v_{\mathrm{CM}} \cos\lambda\, \mathbf{i} - v_{\mathrm{CM}} \sin\lambda\, \mathbf{j}$$

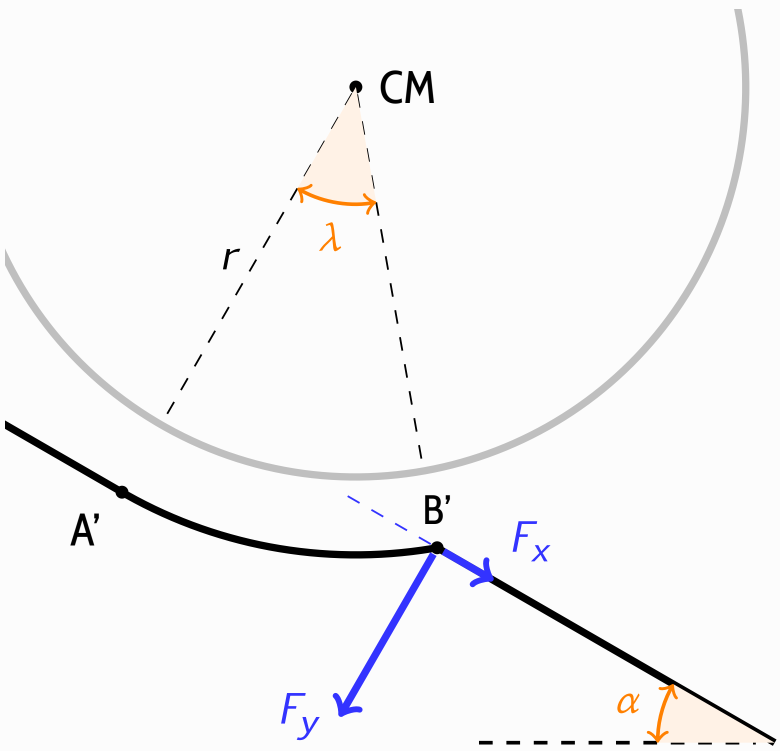


Figure 1.4: Forces

and since, on the other hand, $\mathbf{v}_{\text{CM}} = v_{\text{CM}}\,\mathbf{i}$, the instantaneous velocity of B' will be given by the vector:

$$\mathbf{v}_{\text{B'}} = v_{\text{CM}}\,[(1 - \cos\lambda)\,\mathbf{i} - \sin\lambda\,\mathbf{j}]$$

Figure 1.4 shows the rectangular components of the force that the sphere applies to the same point B' as agreed above.

The instantaneous power P developed by a constant force – as in our case – is calculated by the scalar product of that force by the instantaneous velocity at which its point of application moves.

Therefore the instantaneous power $P = dE/dt$ with which the plane receives energy E from the sphere will be given by the scalar product[1]:

$$P = (\mathbf{F_x} + \mathbf{F_y}) \cdot \mathbf{v_{B'}}$$

In other words,

$$P = v_{\text{CM}} \left[F_x (1 - \cos\lambda) + F_y \sin\lambda \right]$$

But how

$$v_{\text{CM}} = a_{\text{CM}}\, t$$

being a_{CM} the acceleration, constant, of the centre of mass, the amount of energy transferred to the plane during the interval dt will remain:

$$dE = \left[F_x (1 - \cos\lambda) + F_y \sin\lambda \right] a_{\text{CM}}\, t\, dt.$$

If the sphere starts from rest, a simple integration with respect to time tells us that the energy E that the sphere transfers to the plane during rolling will be given by

$$E = \left[F_x (1 - \cos\lambda) + F_y \sin\lambda \right] \frac{1}{2} a_{\text{CM}}\, t^2$$

or

$$E = \left[F_x (1 - \cos\lambda) + F_y \sin\lambda \right] s \qquad (1.1)$$

where s is the path of the CM of the sphere during the considered time interval.

If we remember that $\lambda \to 0$, so that $1 - \cos\lambda \to 0$ and $\sin\lambda \to \lambda$, the above equation will look like this:

[1] **Editor's Note:** $dE/dt = \mathbf{F} \cdot d\mathbf{s}/dt = \mathbf{F} \cdot \mathbf{v} = F_x v_x + F_y v_y$

$$\lambda \to 0 \Rightarrow \qquad E = \lambda F_y s \tag{1.2}$$

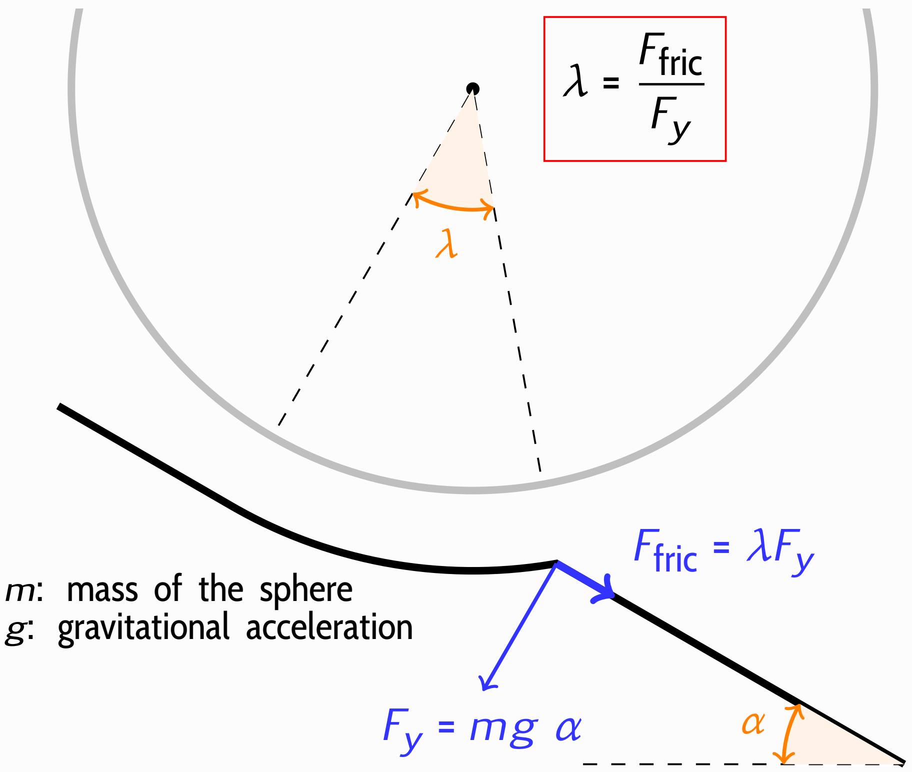


Figure 1.5: Angle λ in the plane deformation

Therefore we can consider that λ plays in the equation 1.2 the function of a '*coefficient of rolling friction*' according to the usual definition of such coefficients[2]. See Figure 1.5.

The F_{fric} shown in Figure 1.5 and the F_x shown in Figure 1.4 play completely different roles in terms of the energy transfer that the sphere makes to the plane: The role of F_x is zero in practice, by virtue of the equation 1.1, so that the energy transfer is almost exclusively taken care of by F_{fric}, which depends,

[2] **Editor's Note:** The frictional force that slows down the sliding of one body over another, which has been widely studied, is $F_{fric} = \mu F_y$, and its work, the energy that is transferred to the plane due to this force: $E = F_{fric}\, s = \mu F_y\, s$. Where μ is the dynamic friction coefficient which, for example, for glass-wood surfaces has a value of 0.2

as we have just shown, on λ and F_y.

1.3 How can the value of this coefficient be obtained?

To achieve this we will pose the equation of motion of the sphere considering the simultaneous rotation/translation as a pure rotation about the instantaneous axis of rotation passing through point A and perpendicular to the plane of the paper. See Figure 1.6. We will have:

$$\mathbf{r} \times \mathbf{w} + \mathbf{q} \times \mathbf{F} = I_A \boldsymbol{\gamma}$$

being I_A the moment of inertia of the sphere and γ the angular acceleration. According to Steiner's theorem (Wikipedia, 2023), the expression for the moment of inertia I_A with respect to A will be:

$$I_A = I_{CM} + mr^2 = \frac{2}{5}mr^2 + mr^2$$

or:

$$I_A = \frac{7}{5}mr^2 \tag{1.3}$$

being m the mass of the sphere and r its radius.

Let us now consult Figure 1.2 to express the vector $\mathbf{q}$ and we will find that:

$$\mathbf{q} = q_x\,\mathbf{i} + q_y\,\mathbf{j} = q\sin\frac{\lambda}{2}\,\mathbf{i} + q\cos\frac{\lambda}{2}\,\mathbf{j}$$

Having made the pertinent substitutions and operations in the equation of

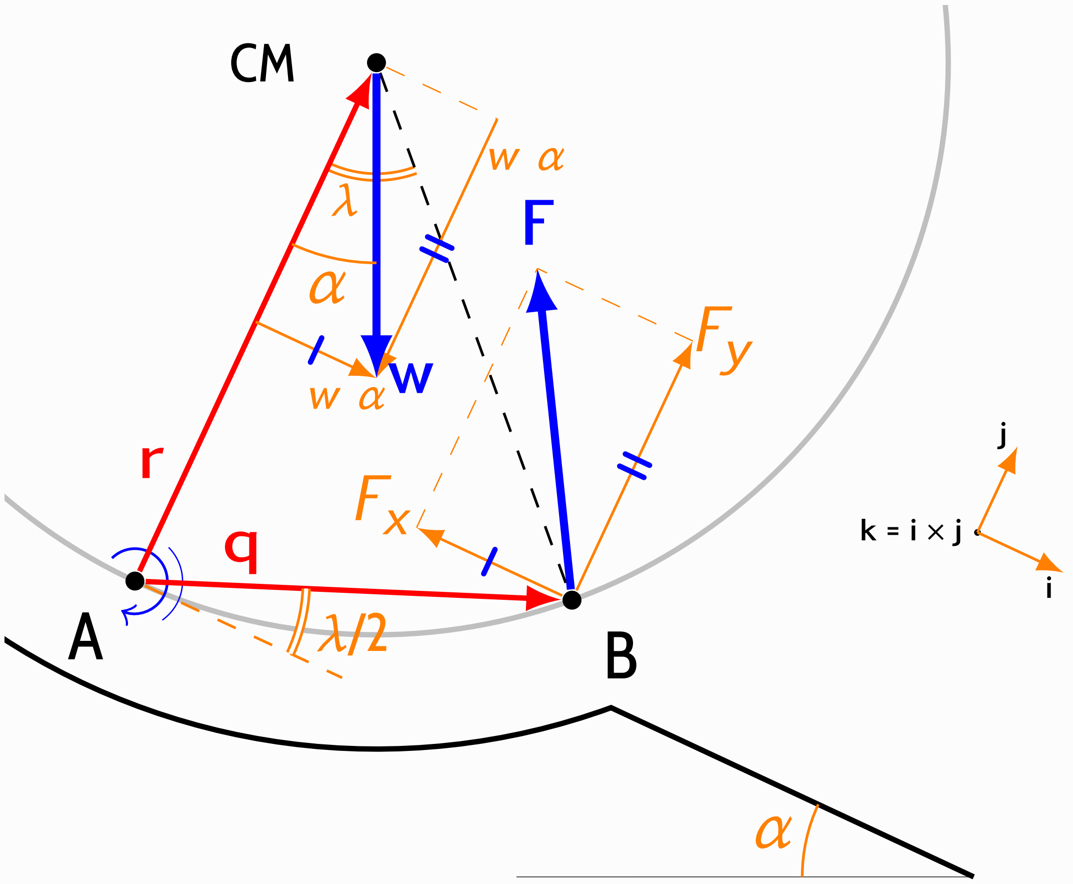


Figure 1.6: $\mathbf{r} \times \mathbf{w} + \mathbf{q} \times \mathbf{F} = I_{\mathrm{A}}\, \boldsymbol{\gamma}$

motion we will have:

$$r\,mg \sin\alpha - q\,F_x \sin\frac{\lambda}{2} - q\,F_y \cos\frac{\lambda}{2} = \frac{7}{5}\,m\,r\,a_{\mathrm{CM}}$$

where a_{CM} represents the linear acceleration of the centre of mass since $\gamma = a_{\mathrm{CM}}/r$.

Since $\lambda \to 0$, we will have $\cos\frac{\lambda}{2} \to 1$ and $\sin\frac{\lambda}{2} \to \frac{\lambda}{2}$, so that the above equation can be written as follows:

$$r\,mg \sin\alpha - \frac{\lambda}{2} qF_x - qF_y = \frac{7}{5}\,m\,r\,a_{\mathrm{CM}}$$

If both members of the above equation are divided by r and the approximation, also acceptable, is made that $\frac{q}{r} \to \lambda$, the above equation will be:

$$mg \sin\alpha - \lambda^2 \frac{F_x}{2} - \lambda F_y = \frac{7}{5} m\, a_{\text{CM}}$$

or

$$\lambda^2 \frac{F_x}{2} + \lambda F_y + m\left(\frac{7}{5} a_{\text{CM}} - g \sin\alpha\right) = 0$$

This second degree equation allows the calculation of λ from two quantities whose values are experimentally determinable: The angle α that forms the plane with the horizontal and the acceleration a_{CM}. As for the rectangular components of the force that the plane exerts on the sphere we will have that they will be:

$$F_y = mg\ \cos\alpha \qquad F_x = m\,(g\ \sin\alpha - a_{\text{CM}})$$

It is evident that if the plane and the sphere were both perfectly rigid, so that $\lambda = 0$, the mutual contact would be reduced to a single point. In this case there would be no friction and the acceleration would be the theoretical acceleration, denoted by $\underline{a}_{\text{CM}}$[3]:

$$m\left(\frac{7}{5} \underline{a}_{\text{CM}} - g \sin\alpha\right) = 0$$

which leads to the well-known result for the acceleration that we call theoretical of the centre of mass $\underline{a}_{\text{CM}}$:

$$\lambda = 0 \Rightarrow \qquad \underline{a}_{\text{CM}} = \frac{5}{7} g \sin\alpha$$

[3] **Editor's Note:** The notation used for the theoretical values of K a v would be $\underline{K}$ $\underline{a}$ $\underline{v}$

equation valid in the ideal case of *absence of friction forces and, therefore, without dissipation of energy* $\lambda = 0$[4].

A realistic approximation without supposing λ totally null, – since the absolute rigidity does not exist – would be obtained by disregarding the quadratic term in the equation of second degree, since if $\lambda \to 0$ will do so with much more reason its square and substituting also F_y for the value obtained before, we would obtain:

$$\lambda = \tan\alpha - \frac{7\,a_{\mathrm{CM}}}{5g\cos\alpha},$$

or

$$\lambda = \sec\alpha\left(\sin\alpha - \frac{7\,a_{\mathrm{CM}}}{5g}\right) \tag{1.4}$$

mathematical expression that describes a functional relationship between λ and a pair of independent variables: the *angle of inclination of the plane* α and the *real acceleration of the centre of mass of the sphere* a_{CM}. That is to say: $\lambda = f(\alpha, a_{\mathrm{CM}})$.

1.4 A new interpretation of λ

If the metallic sphere rolls on the inclined wooden plane, part of its initial gravitational potential energy will be dissipated along the path, and when it reaches the foot of the plane, its kinetic energy content will be lower than expected.

[4] **Editor's Note:** See Appendix A

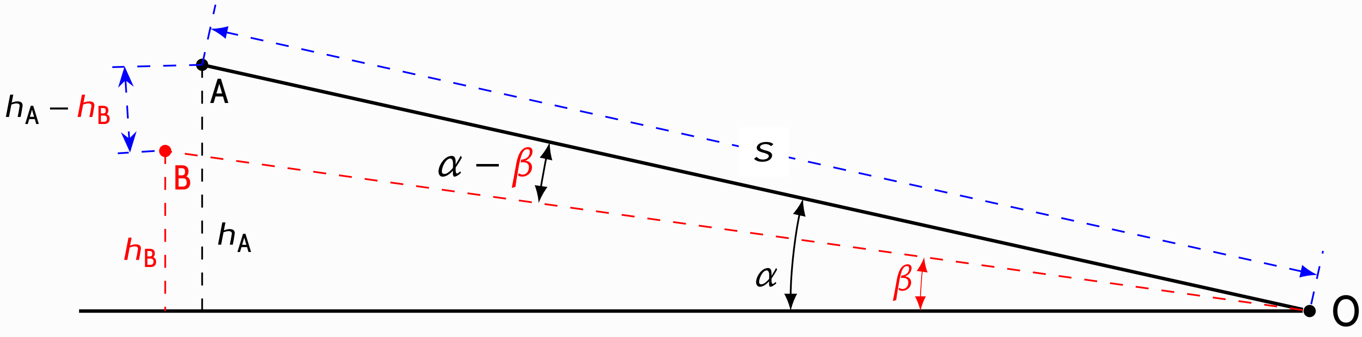


Figure 1.7: Nueva interpretación de λ

If the rolling sphere starts from rest from point A and travels the length s until it reaches O – the plane forming an angle α with the horizontal – the unavoidable dissipation of energy will cause a practical effect equivalent to that of having carried out *the same rolling without dissipation of energy*, but on a plane of inclination $\beta < \alpha$. See Figure 1.7. We can evaluate the dissipated energy by:

$$\Delta E_p = mg\,\Delta h$$

or

$$mg\,\Delta h = mg\,h_A - mg\,\Delta h = mg\,h_B$$

Simplifying,

$$\Delta h = h_A - h_B$$

which, multiplying and dividing by s, leaves:

$$\Delta h = s\left(\frac{h_A}{s} - \frac{h_B}{s}\right)$$

or, finally,

$$\Delta h = s\,(\sin\alpha - \sin\beta)$$

The parenthesis of the last relation above can be expressed in this way:

$$\sin\alpha - \sin\beta = 2\sin\frac{\alpha-\beta}{2}\cos\frac{\alpha+\beta}{2}$$

If we consider that $\lambda = \alpha - \beta$ *must be a very small angle* we can admit that $\beta \rightarrow \alpha$ and also that $\sin\lambda \rightarrow \lambda$, so that the above expression would be

$$\sin\alpha - \sin\beta = \lambda\cos\alpha$$

hence we are able to write

$$\Delta h = s\lambda\cos\alpha \tag{1.5}$$

But, since the *experimental value* of the linear acceleration of the centre of mass of the sphere must fit the equation

$$a_{\mathrm{CM}} = \frac{5}{7}g\sin\beta$$

we would have

$$\sin\beta = \frac{7a_{\mathrm{CM}}}{5g}$$

and it would also turn out to be true that we can express Δh in this other way:

$$\Delta h = s\left(\frac{h_{\mathrm{A}}}{s} - \frac{7a_{\mathrm{CM}}}{5g}\right) \tag{1.6}$$

Equating the second members of 1.5 and 1.6 and operating we obtain:

$$\lambda = \sec\alpha\left(\frac{h_{\mathrm{A}}}{s} - \frac{7a_{\mathrm{CM}}}{5g}\right)$$

o

$$\lambda = \sec\alpha\left(\sin\alpha - \frac{7a_{\text{CM}}}{5g}\right) \tag{1.7}$$

This equation 1.7 – identical to 1.4 – allows the evaluation of λ from α and a_{CM}. The novelty in this case is that λ now represents neither the plane *strain* nor the *coefficient of rolling friction* but the difference between the *real* α and *apparent inclinations* β of it.

An interesting consequence of the above is that we can and should distinguish between $\underline{a}_{\text{CM}}$, *theoretical or ideal acceleration* – that which would possess the rolling sphere's CM in the *ideal case* of *zero friction* – and a_{CM}, the *experimental or real acceleration*, that which presents in practice the sphere's CM, since the *friction forces* are *inescapable* causing the consequent dissipation of energy. Both accelerations can be calculated by:

$$\begin{aligned} \underline{a}_{\text{CM}} &= \frac{5}{7} g \sin\alpha \\ a_{\text{CM}} &= \frac{5}{7} g \sin\beta \\ \lambda &= \alpha - \beta \end{aligned} \tag{1.8}$$

1.5 Sphere rolling on a rectangular section groove

In a famous experiment carried out in 1961 by T. B. Settle – replicating the one described by Galileo – this researcher rolled his spheres on a rectangular section groove carved on the narrowest face of its inclined plane. In this experiment the rolling sphere has *two small zones* of contact with the support, as shown in Figure 1.8.

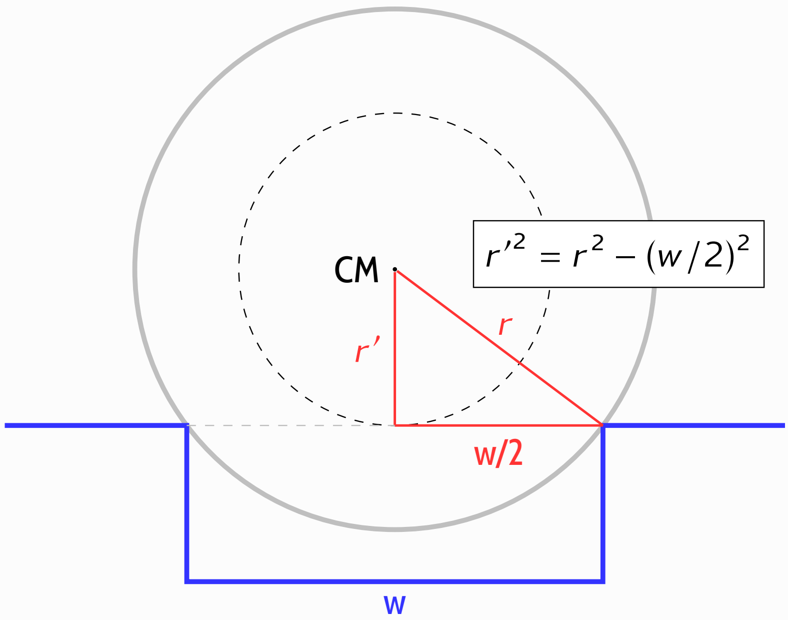


Figure 1.8: Apparent radius r'

In Figure 1.9 we present a side view of the same sphere rolling on the groove of the inclined plane at an angle α with respect to the horizontal.

Suppose that the sphere starts from rest, being its CM at a height h above the horizontal, and rolls along a length s. In the ideal assumption of no mechanical energy dissipation in the rolling, we would have that:

$$mgh = \frac{1}{2} m \underline{v}_{\mathrm{CM}}^2 + \frac{1}{2} I \underline{\omega}'^2$$

where m is the mass of the sphere and I its moment of inertia with respect to the axis of rotation.

As $I = \frac{2}{5} m r^2$ and $\underline{\omega}'^2 = \left(\frac{\underline{v}_{\mathrm{CM}}}{r'}\right)^2$ we will have:

$$gH = \frac{1}{2} \underline{v}_{\mathrm{CM}}^2 + \frac{1}{5} \left(\frac{r}{r'}\right)^2 \underline{v}_{\mathrm{CM}}^2$$

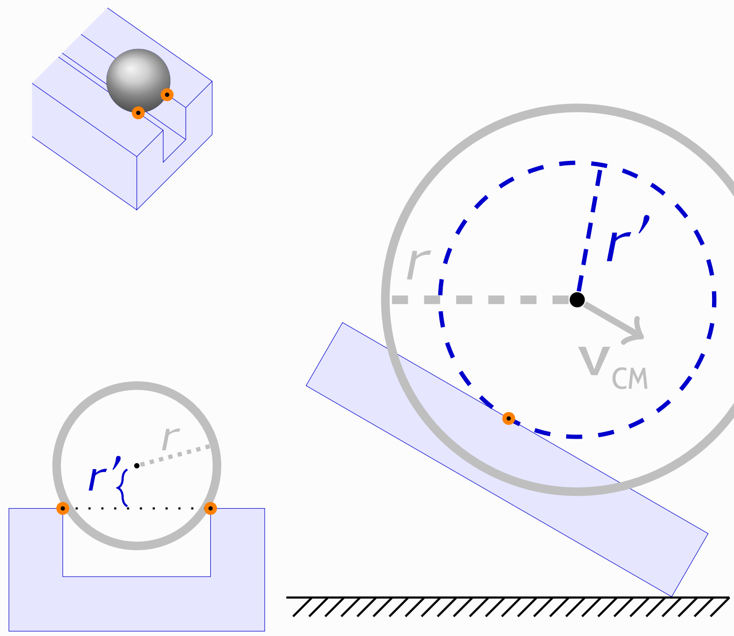


Figure 1.9: Angular velocity $\omega' = \dfrac{v_{CM}}{r'}$

Taking into account that: $\underline{v}_{CM}^2 = 2\underline{a}_{CM}s$ and that $\sin\alpha = \dfrac{h}{s}$ we get:

$$\underline{a}_{CM} = \frac{1}{1 + \frac{2}{5}\left(\frac{r}{r'}\right)^2} g \sin\alpha \qquad (1.9)$$

Expression that allows us to calculate the *ideal or theoretical acceleration* $\underline{a}_{CM}$ of the CM of the sphere rolling under these conditions. The coefficient enclosed in square brackets is easy to calculate (see Figure 1.8) y reduces to the familiar $\frac{5}{7}$ in the case where $r = r'$.

By virtue of the relations 1.8 this same equation 1.9 will serve to calculate the *experimental acceleration* a_{CM} by simply substituting $\sin\alpha$ for $\sin\beta$, since the expression enclosed in the bracket is a purely geometrical factor.

1.6 Calculation of the percentage of energy dissipated in the rolling movement

We will show that this percentage χ can be calculated by:

$$\chi = 100\left(1 - \frac{a_{\mathrm{CM}}}{\underline{a}_{\mathrm{CM}}}\right)$$

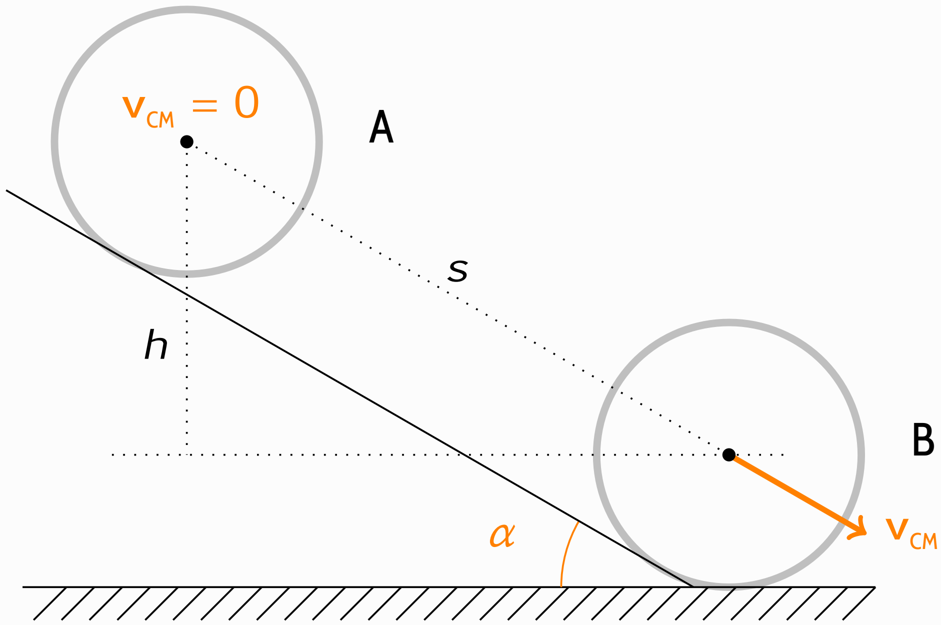


Figure 1.10: Conversion of E_p to E_{c_T} with a dissipation of dX.%

In *absence of friction* all gravitational potential energy in **A** would be conserved in **B** as kinetic, which we could express by

$$\underline{E} = K\,\underline{v}_{\mathrm{CM}}^2$$

being

$$K = m\left(\frac{1}{2} + \frac{1}{5}\left(\frac{r}{r'}\right)^2\right)$$

or

$$\underline{E} = 2\,s\,K\,\underline{a}_{\mathrm{CM}}$$

since

$$\underline{v}_{\mathrm{CM}}^2 = 2\underline{a}_{\mathrm{CM}}s$$

where $\underline{a}_{\mathrm{CM}}$ represents the *ideal or theoretical acceleration* of the centre of mass.

But in an analogous way the energy E can be expressed as kinetic energy E, which will be conserved in **B**:

$$E = 2s\,K\,a_{\mathrm{CM}}$$

being now a_{CM} the *real or experimental acceleration*.

The quotient $E\,/\underline{E}$, which reduces to $a_{\mathrm{CM}}/\underline{a}_{\mathrm{CM}}$ by simplifying it, *represents the percent for one of the conserved initial energy*.

The *so much per one of dissipated energy* will be obtained by means of

$$\frac{\underline{E} - E}{\underline{E}} = \frac{\underline{a}_{\mathrm{CM}} - a_{\mathrm{CM}}}{\underline{a}_{\mathrm{CM}}}$$

or:

$$\frac{\Delta E}{E} = 1 - \frac{a_{\mathrm{CM}}}{\underline{a}_{\mathrm{CM}}}$$

and the *percentage* χ of dissipated energy will be given by

$$\chi = 100\left(1 - \frac{a_{\mathrm{CM}}}{\underline{a}_{\mathrm{CM}}}\right) \tag{1.10}$$

It is very easy to prove that the equation

$$\chi = 100\left(1 - \left(\frac{v_{CM}}{\underline{v}_{CM}}\right)^2\right) \tag{1.11}$$

allows us the same calculation as the 1.10 from the relation between velocities (real and theoretical) of the centre of mass of the sphere.

References

Wikipedia (2023). *Steiner's theorem*.

Chapter 2

Friction in the rolling of a sphere. Experiences

2.1 Experimental setup

Either of the equations 1.4 or 1.7 suggests a laboratory setup that would allow one to calculate values of λ by empirically determining values of a_{CM} and h. Such a setup is depicted in Figure 2.1. A bubble level, a graduated ruler-square

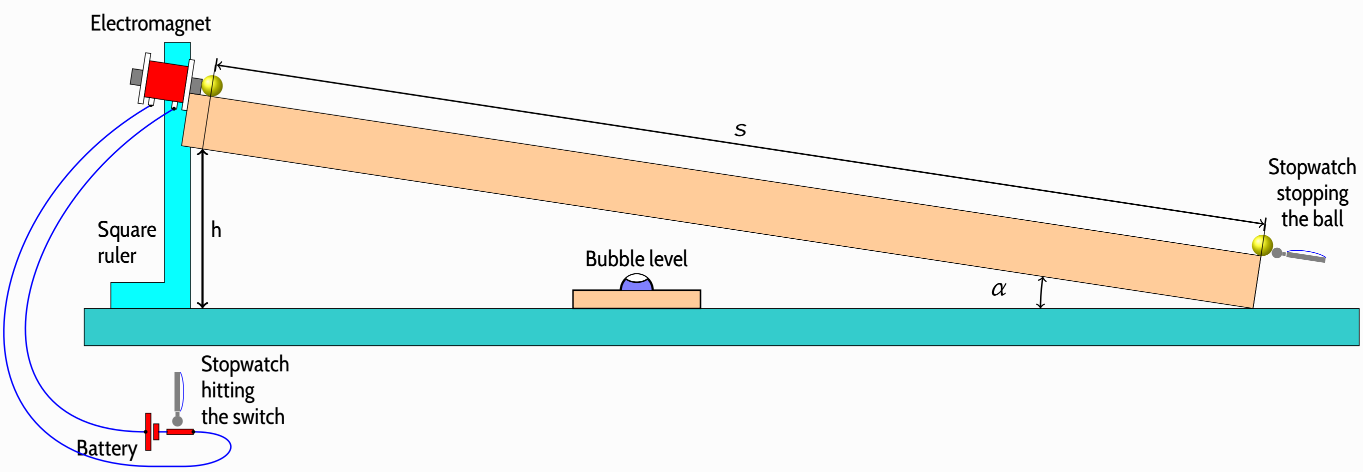


Figure 2.1: Experimental setup to calculate λ

and an aluminium square used in the manufacture of windows, together with nuts, clamps and supports – which are not shown in the figure – make assembly very simple and easy to transport. The aluminium square, placed on the laboratory table, must be adjusted with coins, for example, checking that it is perfectly horizontal using a bubble level.

Special mention should be made of the procedure used to measure the running times, which consists of starting the digital chronometer simultaneously with the departure of the sphere at rest (opening the electromagnet's power supply circuit by hitting the switch with the chronometer spring), and stopping the chronometer at the same time as the sphere reaches the end of its travel (waiting for it with the spring arranged so that it is hit by the sphere). It is necessary to arrange the electromagnet and the chronometer in such a way – suggested in the figure – that the time t timed corresponds to the travel s made by the centre of mass of the sphere.

2.2 Metal sphere on wooden plane

In this experiment the inclined plane consisted of a square of pine of $105\ \mathrm{cm}$ of length, $10\ \mathrm{cm}$ of width and $2\ \mathrm{cm}$ of thickness. Along the thickness, two parallel fishing lines were stretched – separated $5\ \mathrm{mm}$. from each other – delimiting a path where an iron sphere of $12.5\ \mathrm{mm}$ radius will roll, keeping a small contact area with the plane, as we have already described in the previous chapter. The fishing lines have the task of correcting the possible deviation of the sphere from its rectilinear trajectory with a minimum energy cost.

The length s travelled for each slope h was always the same ($100\ \mathrm{cm}$) and the rolling times were measured ten times for each h, taking the average value as representative. However, the timing procedure is so accurate that a single time measurement for each slope would suffice, so that the experiment can be carried out in the classroom during class time without boring the students.

Of course the *experimental acceleration* a_{CM} is calculated by:

$$a_{CM} = \frac{2s}{t^2}$$

and the slope h is measured with the ruler-square, as indicated in Figure 2.1. The data are collected in Table 2.1 and plotted in Graph 2.1.

The blue line in Figure 2.1 shows the *empirical relationship* between a_{CM} and h obtained in this way, while the red line shows the *theoretical relationship* that would be obtained between the *theoretical acceleration* $\underline{a}_{CM}$ and h in the *absence of energy dissipation*. We have calculated it by the simple equation:

$$\underline{a}_{CM} = 7h$$

which is obtained from:

$$\underline{a}_{CM} = \frac{5}{7} g \sin\alpha$$

remembering that:

$$\sin\alpha = \frac{h}{100}$$

The empirical equation: $a_{CM} = 6.85\,(h - 0.23)$ was obtained by relating the two data sets a_{CM}, h shown in Table 2.1 using the linear regression method. The correlation index turned out to be worth $r = 0.999$.

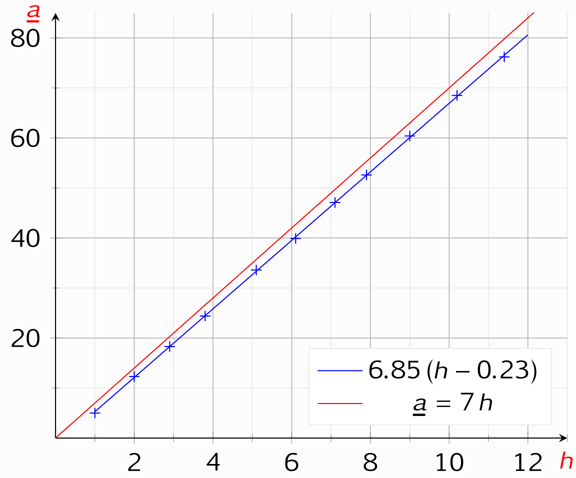
If we take this empirical relation to the theoretical equation 1.7 and perform the relevant operations we will obtain:

$$\lambda = \sec\alpha\,(2.20 \times 10^{-4}\,h + 2.23 \times 10^{-3})$$

which gives us the functional relation $\lambda = f(\alpha, h)$ in the particular case of our metallic sphere rolling on our inclined plane of pine wood.

#	α^o	h cm	t s	a_{CM} cm/s^2
1	0.57	1.0	6.32	5.0
2	1.14	2.0	4.04	12.3
3	1.66	2.9	3.31	18.3
4	2.18	3.8	2.86	24.4
5	2.92	5.1	2.44	33.6
6	3.50	6.1	2.24	39.9
7	4.07	7.1	2.06	47.1
8	4.53	7.9	1.95	52.6
9	5.16	9.0	1.82	60.4
10	5.85	10.2	1.71	68.5
11	6.54	11.4	1.62	76.2

Table 2.1



Graph 2.1: Results of a_{CM} and $\underline{a}_{CM}$ of metal sphere on wood

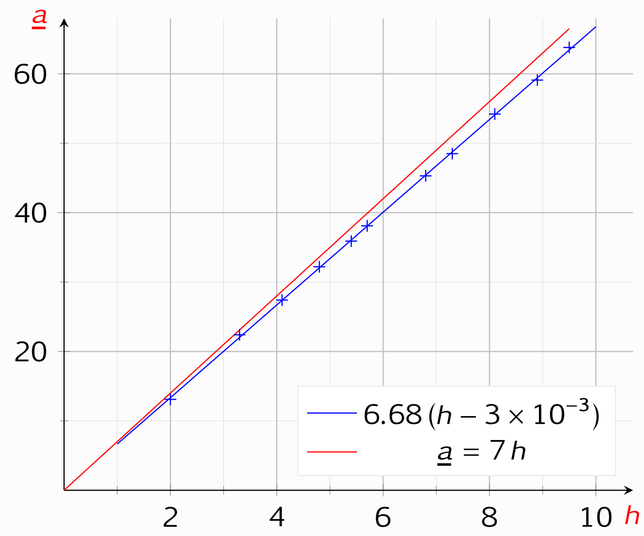
If the sphere rolls on the horizontally disposed plane, so that $\alpha = 0$, it follows from the above relation that the value $\lambda = 2.23\times10^{-3}$ radians will represent the *deformation of the plane* or also the value of the *rolling coefficient* in this case.

2.3 Metal sphere on metal plane

In another experiment, the same sphere was rolled on a duralumin ruler and made to travel 100 cm following the same procedure described in the previous section. The results obtained are shown in Table 2.2.

#	α^o	h cm	t s	a_{CM} cm/s^2
1	1.14	2.0	3.90	13.1
2	1.89	3.3	2.99	22.4
3	2.35	4.1	2.70	27.4
4	2.75	4.8	2.49	32.2
5	3.09	5.4	2.36	35.9
6	3.27	5.7	2.29	38.1
7	3.90	6.8	2.10	45.3
8	4.18	7.3	2.03	48.5
9	4.64	8.1	1.92	54.2
10	5.10	8.9	1.84	59.1
11	5.45	9.5	1.77	63.8

Table 2.2



Graph 2.2: Results of a_{CM} and $\underline{a}_{CM}$ of metal sphere on duralumin

The empirical equation:

$$a_{CM} = 6.68\,(h - 3 \times 10^{-3})$$

has also been obtained by the linear regression method, with both data series showing a correlation index $r = 0.999$. Taking this empirical relationship to the theoretical equation 1.7 we will have:

$$\lambda = \sec\alpha\left(4.6 \times 10^{-4}\,h + 2.8 \times 10^{-5}\right)$$

The data are represented in Graph 2.2.

In both cases – graphs 2.1 and 2.2 – the red lines pass, as is logical, through the point of coordinates $(0, 0)$, while the blue lines always cut the horizontal axis and have a positive abscissa at the origin.

2.4 Metal sphere on rectangular section groove

In the experiment I am going to present below, I used Boyle's ruler as an inclined plane. I named it this way because it is a thick metallic ruler belonging to an assembly destined to the verification of the laws of gases (Boyle-Mariotte, Gay-Lussac, etc.). It measures 110 cm in length, has a grooved face (11.0 mm of channel width) and the other smooth and flat faces, in one of which there is a scale of 100 cm that allows to appreciate up to millimetres. Figure 2.2 illustrates the double use that can be made of this Boyle's ruler: In A the sphere is shown rolling between two fishing lines (the little green circles) so that the rolling sphere has *one* area of contact with the plane. In B the same sphere is shown rolling along the groove or channel now having *two* contact zones.

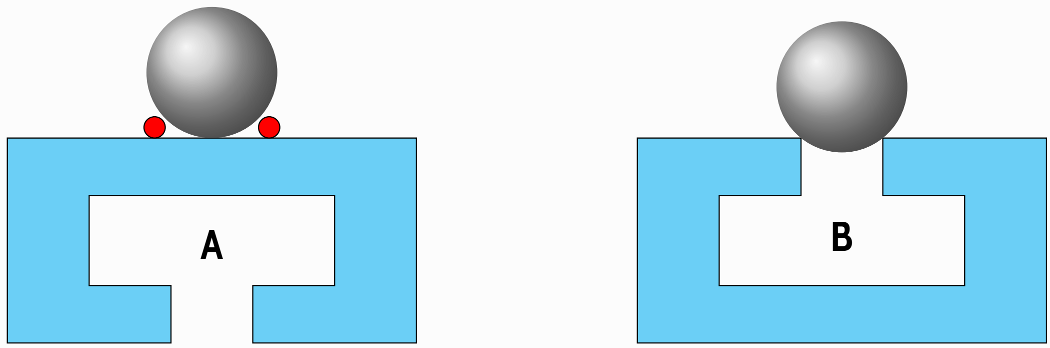


Figure 2.2: Boyle´s ruler

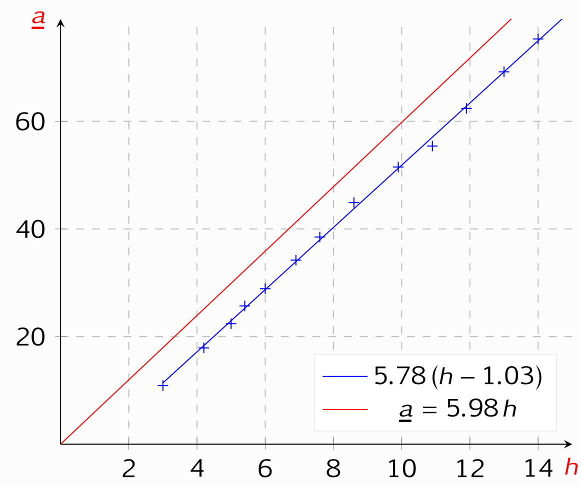
In the experience I am about to describe, the *Boyle's ruler* was used in the B form. Later I will present a couple of experiments in which it was used in both forms using the same rolling sphere.

In the one I describe below, an old iron sphere, of 23.84 g mass and 0.9 cm radius, which had lost its surface nickel plating and had completely rusted away, was used. The oxide layer was partially removed by rubbing with a cloth. The time taken to travel (s = 100.0 cm) over the rectangular groove of the rail (A =

11.0 mm) was timed by varying the slope h thirteen times, taking a single time measurement t for each value of the slope. The experimental acceleration a_{CM} was calculated for each h and the corresponding graphical representation of a_{CM} and $\underline{a}_{CM}$ versus h was plotted (Figure 2.3).

#	h cm	α^o	t s	a_{CM} cm/s^2
1	3.0	1.72	4.29	10.9
2	4.2	2.41	3.34	17.9
3	5.0	2.86	2.99	22.4
4	5.4	3.09	2.79	25.7
5	6.0	3.44	2.63	28.9
6	6.9	3.96	2.42	34.2
7	7.6	4.36	2.28	38.5
8	8.6	4.93	2.11	44.9
9	9.9	5.68	1.97	51.5
10	10.9	6.26	1.90	55.4
11	11.9	6.83	1.79	62.4
12	13.0	7.47	1.70	69.2
13	14.0	8.05	1.63	75.3

Table 2.3



Graph 2.3: Results of a_{CM} and $\underline{a}_{CM}$ of metal sphere on metal groove

The equation $\underline{a}_{CM} = 5.98\,h$ has been obtained using the formula 1.9 from the previous topic, while the $a_{CM} = 5.78\,(h - 1.03)$ has been obtained by relating the experimental data using the linear regression procedure with a correlation index $r = 0.999$.

Using the equation 1.7 from the previous topic we find that, in this case:

$$\lambda = \sec\alpha\left(1.7\times10^{-3}\,h + 8.5\times10^{-3}\right)$$

2.5 The percentage of energy dissipated as a function of h

If we apply the equation 1.10 from the previous topic to the experience we have just analysed we will have that:

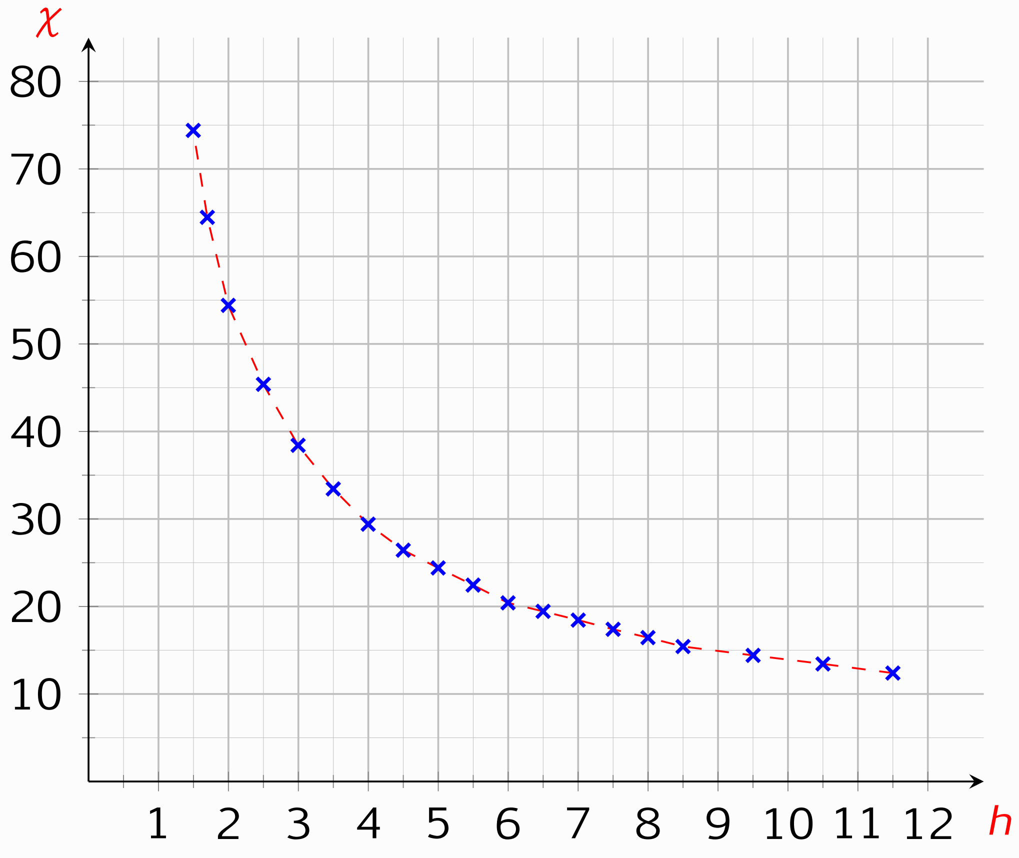
$$\chi = 100\left(1 - \frac{5.78\,(h - 1.03)}{5.98\,h}\right)$$

By performing the pertinent operations we arrive at:

$$\chi = 3.34 + \frac{99.5}{h}$$

valid for $h \geq 1.03$.

The equation that we have just obtained admits the representation that we offer in the Graph 2.4. This is an interesting result to which we will return in the future. It is clear that the total available energy increases as h increases, but also that the percentage of that which is dissipated decreases in the same direction.



Graph 2.4: χ versus h

2.6 Two experiences of January 2001

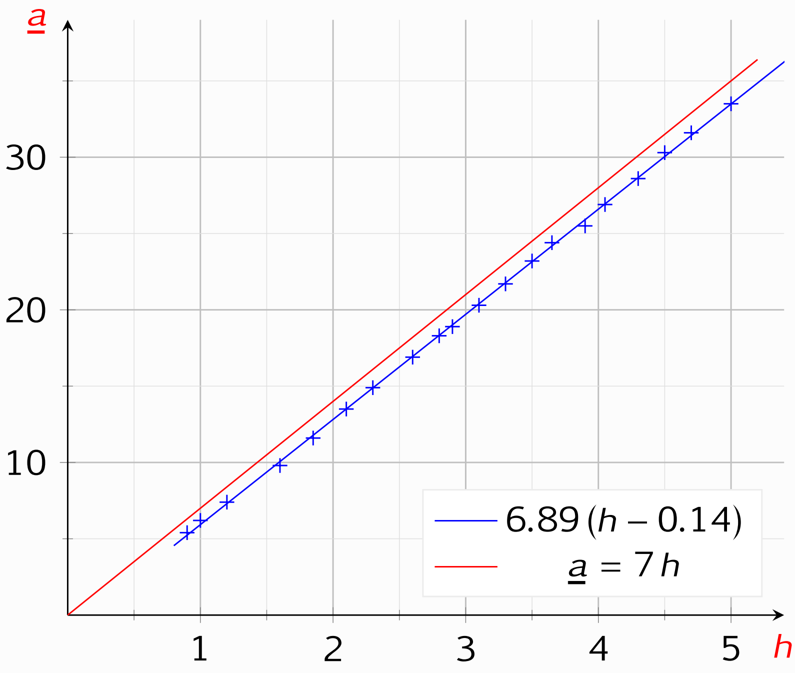
On January 22, 2001, I used the set-up described in Figure 2.2 by taking the Boyle's ruler as shown in Figure 2.2A. The next day I repeated the experience but using the Boyle's ruler as shown in Figure 2.04B. I rolled in both cases a nickel-plated sphere of radius 12 mm along the available 100 cm by measuring ten times the time corresponding to each *height* h to take the mean value of t in the calculation of the *experimental accelerations* a_{CM}.

The data related to both experiments are shown in Tables 2.5 and 2.6.

Those in Table 2.5 correspond to 20 rolls in which between the sphere and the support there is'*a single contact zone*' varying the angle of inclination between

#	h cm	t s	a_{CM} cm/s^2
1	0.90	6.11	5.4
2	1.00	5.69	6.2
3	1.20	5.19	7.4
4	1.60	4.51	9.8
5	1.85	4.14	11.6
6	2.10	3.84	13.5
7	2.30	3.66	14.9
8	2.60	3.43	16.9
9	2.80	3.31	18.3
10	2.90	3.24	18.9
11	3.10	3.14	20.3
12	3.30	3.03	21.7
13	3.50	2.94	23.2
14	3.65	2.86	24.4
15	3.90	2.80	25.5
16	4.05	2.72	26.9
17	4.30	2.64	28.6
18	4.50	2.57	30.3
19	4.70	2.51	31.6
20	5.00	2.44	33.5

Table 2.5



Graph 2.5: One contact zone

0.51° and 2.86°. Applying the linear regression method to establish the relationship between a_{CM} and h we obtain:

$$a_{CM} = 6.89\,(h - 0.14) \qquad \text{with a correlation index} \qquad 0.999$$

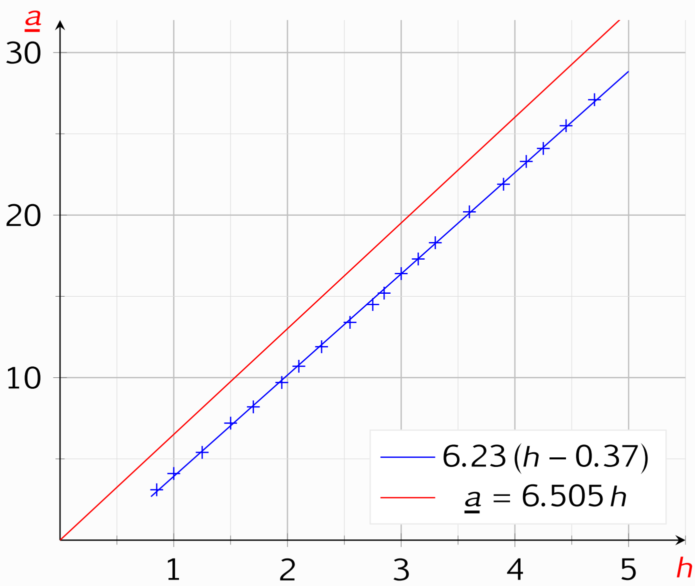
By the procedure already known we find that:

$$\lambda = \sec\alpha\left(1.38\times10^{-3} + 1.6\times10^{-4}h\right)$$

Those in Table 2.6 correspond to 20 rolls in which between the same sphere and the same support there are ‘*two contact zones*’, varying the angle of inclination between 0.48° and 2.69°. Applying the linear regression method

#	h cm	t s	a_{CM} cm/s^2
1	0.85	8.01	3.1
2	1.00	6.95	4.1
3	1.25	6.08	5.4
4	1.50	5.26	7.2
5	1.70	4.94	8.2
6	1.95	4.53	9.7
7	2.10	4.32	10.7
8	2.30	4.10	11.9
9	2.55	3.86	13.4
10	2.75	3.71	14.5
11	2.85	3.62	15.2
12	3.00	3.49	16.4
13	3.15	3.40	17.3
14	3.30	3.30	18.3
15	3.60	3.15	20.2
16	3.90	3.02	21.9
17	4.10	2.93	23.3
18	4.25	2.88	24.1
19	4.45	2.80	25.5
20	4.70	2.71	27.1

Table 2.6



Graph 2.6: Two contact zones

we obtain for the relationship between a_{CM} and h:

$$a_{CM} = 6.23\,(h - 0.37)$$

with a correlation index 0.999.

By the same procedure we find that:

$$\lambda = \sec\alpha\left(3.54 \times 10^{-3} + 4.2 \times 10^{-4}\,h\right)$$

Since for the interval of α in which we move $\sec\alpha \rightarrow 1$ we can dispense with this refinement in the above equations to compare with each other the *coefficients* of h and the *independent terms*, resulting:

$$\frac{3.54 \times 10^{-3}}{1.38 \times 10^{-3}} = 2.56 \approx 2.6$$

y

$$\frac{4.2 \times 10^{-4}}{1.6 \times 10^{-4}} = 2.62 \approx 2.6$$

In these two experiments the materials in contact *are the same*, the only difference being that in A the sphere rolls on the '*smooth plane*' and in B on '*a rail or groove*'. The calculations we have just made reveal that, very approximately:

$$\lambda_B = 2.6\,\lambda_A$$

The experiment was designed precisely to demonstrate this difference between the two coefficients of rolling friction. It was expected that λ_B would be at least *twice* as large as λ_A. Presumably the width of the rail, relative to the diameter of the rolling sphere, will also play a role.

The accelerations *ideal in the absence of friction* $\underline{a}_{CM}$ that would correspond in both cases would be:

$$\underline{a}_{CM} = 7.000\,h \qquad \text{(on the smooth plane)}$$

and

$$\underline{a}_{CM} = 6.505\,h \qquad \text{(on the rail)}$$

Using the equation 1.10 from the previous topic we can see that the energy dissipation rates are, on average, 2.6 times higher in the case of the sphere rolling on the rail.

Chapter 3

The much-discussed experience of the inclined plane

3.1 Galileo's experiment

He himself describes it as follows in (Galilei, 2017):

> 'A piece of wooden moulding or scantling, about 12 cubits long, half a cubit wide, and three finger-breadths thick, was taken; on its edge was cut a channel a little more than one finger in breadth; having made this groove very straight, smooth, and polished, and having lined it with parchment, also as smooth and polished as possible, we rolled along it a hard, smooth, and very round bronze ball.
> "Having placed this board in a sloping position, by lifting one end some one or two cubits above the other, we rolled the ball, as I was just saying, along the channel, noting, in a manner presently to be described, the time required to make the descent. We repeated this experiment more than once in order to measure the time with an accuracy such that the deviation between two observations never exceeded one-tenth of a pulse-beat. Having performed this operation and having assured ourselves of its reliability, we now rolled the ball only one-quarter the length of the channel; and having measured the time of its descent, we found

> it precisely one-half'of the former. Next we tried other distances, comparing the time for the whole length with that for the half, or with that for two-thirds, or three-fourths, or indeed for any fraction; in such experiments, repeated a full hundred times, we always found that the spaces traversed were to each other as the squares of the times, and this was true for all inclinations of the plane, i. e., of the channel, along which we rolled the ball. We also observed that the times of descent, for various inclinations of the plane, bore to one another precisely that ratio which, as we shall see later, the Author had predicted and demonstrated for them.
>
> For the measurement of time, we employed a large vessel of water placed in an elevated position; to the bottom of this vessel was soldered a pipe of small diameter giving a thin jet of water, which we collected in a small glass during the time of each descent, whether for the whole length of the channel or for a part of its length; the water thus collected was weighed, after each descent, on a very accurate balance; the differences and ratios of these weights gave us the differences and ratios of the times, and this with such accuracy that although the operation was repeated many, many times, there was no appreciable discrepancy in the results.'

Between 1937 and 1961 it was fashionable to doubt the efficacy and verisimilitude of the experience contained in this brief account. The prestigious historian of science Alexandre Koyré expresses himself in this respect as follows (Koyré, 1973):

'Une boule en bronze roulant dans une rainure «lisse et polie» taillée dans du bois! Un récipient d'eau avec un petit trou par lequel l'eau passe et que l'on recueille dans un petit verre pour la peser ensuite et mesurer ainsi les temps de descente (la clepsydre romaine, celle de Ctésibius, était un bien meilleur instrument): quelle accumulation de sources d'erreur et d'inexactitude!

Il est évident que les expériences de Galilée sont complètement dénuées de valeur : la perfection même de leurs résultats est une preuve rigoureuse de leur inexactitude. Les historiens modernes, accoutumés à voir les expériences de Galilée faites à l'intention des étudiants dons nos laboratoires scolaires, acceptent. cet exposé étonnent comme vérité d'évangile et louent Galilée d'avoir établi ainsi non seulement la validité empirique de le loi de chute, mois cette dernière aussi. (Cf., parmi beaucoup d'autres, N. Bourbaki, Élements de mathématique, 9, première partie, liv. IV, chap. I-III, Note historique, p. 150 («Actualités scientifiques et Industrielles» nº 1074, Paris, Hermann, 1949). Cf. Appendice 1.

Il n'est pas étonnant que Galilée, qui est sans doute pleinement conscient de tout cela, évite autant que possible (par exemple dans les Discours) de donner une valeur concrète pour l'accélération; et que, chaque fois qu'il en donne une (comme dans le Dialogue), celle-ci soit radicalement fausse. Tellement fausse que le P. Mersenne a été incapable de dissimuler sa surprise:

«Or il suppose, écrit-il à Peiresc 2, que le boulet tombe cent brasses dans cinq secondes, d'où il s'ensuit qu'il ne tombera que quatre brasses dans une seconde quoique je sois assuré qu'il tombe de plus haut.»

En effet, quatre coudées – pas même sept pieds 8 – sont moins que la moitié de la vraie valeur; et environ la moitié de la valeur que le P. Mersenne établira lui-même. Et pourtant, le fait que les chiffres donnés par Galilée soient grossièrement inexacts n'a rien de surprenant; tout au contraire: il serait surprenant, et même miraculeux, qu'ils ne le fussent pas. Ce qui est surprenant, c'est le fait que Mersenne, dont les moyens d'expérimentation n'étaient pas beaucoup plus riches que ceux de Galilée, ait pu obtenir des résultats tellement meilleurs.'

'A bronze ball rolling in a 'smooth and polished' groove cut in wood! A container of water with a small hole through which the water passes and which is collected in a small glass so that it can be weighed and the descent times measured (the Roman clepsydra, that of Ctesibius, was a much better instrument): what an accumulation of sources of error and inaccuracy! It is obvious that Galileo's experiments are completely worthless: the very perfection of their results is rigorous proof of their inaccuracy. Modern historians, accustomed to seeing Galileo's experiments done for students in our school laboratories, accept this astonishing statement as gospel truth and praise Galileo for having thus established not only the empirical validity of the law of falling, but the latter as well (Cf., among many others, N. Bourbaki, Eléments de mathématique, 9, première partie, liv. IV, chap. I-III, Note historique, p. 150!- Actualités scientifiques et Industrielles – nº 1074, Paris, Hermann, 1949). Cf. Appendix 1

It is not surprising that Galileo, who was no doubt fully aware of all this, avoided as much as possible (for example in the Discourses) giving a concrete value for acceleration; and that, whenever he did give one (as in the Dialogue), it was radically false. So wrong that P. Mersenne was unable to conceal his surprise:

'Now he supposes,' he wrote to Peiresc 2, 'that the cannonball falls one hundred fathoms in five seconds, from which it follows that it will only fall four fathoms in one second, although I am assured that it falls from a greater height.'

'In fact, four cubits – not even seven feet 8 – is less than half of the true value, and about half of the value that Father Mersenne himself will establish. And yet, the fact that the figures given by Galileo are grossly inaccurate is not surprising; on the contrary, it would be surprising, and even

> miraculous, if they were not. What is surprising is the fact that Mersenne, whose means of experimentation were not much richer than Galileo's, was able to obtain such better results.'

In another of his articles on the story of the Tower of Pisa, Koyré writes (Koyré, 1973):

> 'Il nous faudrait admettre que Galilée, qui ne s'est pas privé de nous conter et de nous présenter comme faites effectivement des expériences qu'il s'était borné à imaginer, nous aurait soigneusement caché une expérience glorieuse effectivement réalisée. C'est tellement improbable que l'on ne peut l'admettre sérieusement. La seule explication possible de ce silence est la suivante : si Galilée ne parle jamais de l'expérience de Pise, c'est qu'il ne l'a pas faite. Très heureusement pour lui, d'ailleurs. Car, s'il l'avait faite, en formulant le défi que, pour lui, formulent ses historiens, elle eût tourné à sa confusion.'
>
> 'We would have to admit that Galileo, who did not hesitate to tell us about and present to us as actually done experiments that he had merely imagined, would have carefully hidden from us a glorious experiment that had actually been carried out. This is so improbable that it cannot be taken seriously. The only possible explanation for this silence is as follows: if Galileo never mentions the Pisa experiment, it's because he didn't do it. Very fortunately for him, in fact. For, if he had done it, by formulating the challenge that his historians formulated for him, it would have turned out to be confusing.'

Does Koyré include the inclined plane experiment among the '*imagined*' ones that Galileo presents to us as '*realized*'? I personally find this comment offensive. What need did Galileo have to lie about this matter?

Let's continue with Koyré (Koyré, 1966, 1973):

> 'Mais, en fait, nous ne pouvons pas penser au mouvementdans le sens de l'effort et de l'impelus ; nous pouvons seulementnous l'imaginer. Nous devons donc choisir entrepenser et imaginer. Penser avec Galilée ou imaginer avecle sens commun. Car c'est la pensée, la pensée pure et sansmélange, et non l'expérience et la perception des sens,qui est à la base de la «nouvelle science» de Galileo Galilée.
> Galilée le dit très clairement. Ainsi, en discutant le fameux exemple de la balle tombant du haut du mât d'un navire en mouvement, Galilée explique longuement le principe de la relativité physique du mouvement, la différence entre le mouvement du corps par rapport à la Terre et son mouvement par rapport au navire; puis, sans faire aucune mention de l'expérience, il conclut que le mouvement de la balle par rapport au navire ne change pas avec le mouvement de ce dernier. De plus, quand son adversaire aristotélicien, imbu d'esprit empiriste, lui pose la question : «Avez-vous fait une expérience?» Galilée déclare avec fierté: «Non, et je n'ai pas besoin de la faire, et je peux affirmer sans aucune expérience qu'il en est ainsi, car il ne peut en être autrement»'
>
> 'But, in fact, we cannot think about movement in the sense of effort and impulse; we can only imagine it. So we have to choose between thinking and imagining. To think with Galileo or to imagine with common sense. For it is thought, pure and unadulterated thought, and not the experience and perception of the senses, that is the basis of Galileo Galilei's 'new science'.
>
> Galileo makes this very clear. Thus, in discussing the famous example of the ball falling from the top of the mast of a moving ship, Galileo explains at length the principle of the physical relativity of motion, the difference between

> the motion of the body relative to the Earth and its motion relative to the ship; then, without making any mention of experience, he concludes that the motion of the ball relative to the ship does not change with the motion of the latter. What's more, when asked by his empiricist-minded Aristotelian opponent, 'Have you done an experiment?' Galileo proudly declares: 'No, and I don't need to do it, and I can affirm without any experiment that it is so, because it cannot be otherwise.''

Koyré's attitude regarding the credibility that can be granted to Galileo is, at least, curious: He grants him credit in the case we have just cited, in which Galileo appears to us as a pure Platonist, and denies it when Galileo himself relates and ponders the accuracy and reiteration of the results obtained with the experiment of the inclined plane, in which he presents us with his experimentalist streak.

According to I. Bernard Cohen, another eminent historian of science, says in Supplement 4: "Galileo's Experimental Foundation of the Science of Motion" (Cohen, 1989):

> 'In the decades following World War II, many scholars— following the lead of Alexandre Koyré — had concluded that in the stages of discovery and development of the principles of motion, the role of true experiment was minimal. Galileo was seen as a thinker and analyst, not one who put direct questions to the test of experience. It was even doubted that Galileo had ever performed the inclined-

plane experiment described in the Two New Sciences as a confirmation of the conclusions arrived at by mathematical analysis. Most scholars agreed that the reported exactness of observations within "a tenth of a pulse-beat" far exceeded the capacity of this apparatus; here was apparent evidence that Galileo had probably never done this experiment. The best that could be said for Galileo was that he had boastfully exaggerated the results. This point of view seemed all the more justified to the degree that Galileo gave no numerical data. Doubts concerning the inclined plane were not voiced for the first time in the twentieth century. In Galileo's own time. Father Marin Mersenne wrote in 1636 (Mersenne, 1637) page 112:

"Je doute que le père Galilee ait effectué les expériences des chutes sur le plan, puisqu'il n'en parle nullement, et que la proportion qui donne un résultat contradictoire avec l'expérience. Je voudrais également que plusieurs personnes effectuent des expériences sur des plans différents, en prenant toutes les précautions nécessaires, afin qu'ils comparent leurs expériences aux nôtres et qu'ils puissent en tirer des leçons pour élaborer un théorème sur la vitesse de ces chutes obliques. Les effets de la gravité pourraient endommager les carreaux, ce qui serait d'autant plus grave que la pente est moins inclinée vers l'horizon et que la ligne perpendiculaire est approchée."

"I doubt that Professor Galilee carried out the experiments of falls on the plane, since he makes no mention of it, and that the proportion which gives a result contradictory to the experiment. I would also like several people to carry out experiments on different planes, taking all the necessary precautions, so that they can compare their experiments with ours and learn from them to develop a theorem on the speed of these oblique falls. The effects of gravity

> could damage the tiles, which would be all the more serious when the slope is less inclined towards the horizon and the perpendicular line is closer."
>
> Today our view of the matter has undergone a radical change. In 1961, Thomas B. Settle devised and performed an experiment that closely replicated the one described by Galileo in the Two Mew Sciences. In his report ("An Experiment in the History of Science," (Settle, 1961)), Settle showed that the results were, just as Galileo said, easily accurate to within a tenth of a pulse-beat. Others confirmed Settle's results. Another experimenter, James MacLachlan in 1973 (MacLachlan, 1973), then repeated an effect described by Galileo, which had been the subject of particular derision and had been used to underline the fact that Galileo's experiments were only "thought-experiments" and obviously could not possibly give the results described by Galileo. But MacLachlan found that this experiment, unbelievable at first encounter, accorded exactly with Galileo's description. We have seen (in Supplement 3) that in the early 1590's, while still at Pisa, Galileo was making experiments with falling bodies and that there is a reasonable explanation for the bizarre result he recorded that a light body starts out ahead of a heavy body when both are released "simultaneously."'

However, in 'The Lies of Science' (1993) Federico di Trocchio writes (Trocchio, 1993):

'Alexandre Koyré, uno dei più grandi storici della scienza, ha sostenuto la prima ipotesi, vale a dire che Galilei non ha mai fatto l'esperimento del piano inclinato. La cosa sembrò a molti incredibile sicché nel 1961 Thomas S. Settle decise di provare a farlo nelle stesse identiche condizioni indicate da Galileo. Egli constatò che Galileo avrebbe potuto ottenere risultati empirici «soddisfacenti», cioè vicini anche se non proprio identici a quelli da lui riferiti, nel modo da lui sostenuto. Le cose sembravano così tornare finalmente a posto e Stillman Drake, il più noto studioso americano di Galileo, poté affermare con soddisfazione che «le ben note asserzioni di Galileo circa i suoi esperimenti su piani inclinati erano state completamente convalidate»

Purtroppo nel 1973 Ronald Naylor, nel ripetere ancora una volta l'esperimento di Galileo, individuò delle discrepanze tra ciò che aveva fatto Settle e la descrizione di Galileo. Settle aveva innanzitutto fatto rotolare una palla non già dentro la scanalatura del piano inclinato ma sospesa sui bordi di essa. In questo modo riduceva notevolmente l'effetto della rotazione, che priva la palla di gran parte della sua accelerazione, fornendo così dati più strettamente concordanti con la legge. Ma Galileo non aveva fatto l'esperimento in questo modo. Il suo piano inclinato aveva una scanalatura abbastanza ampia da contenere la palla. Alcuni studiosi hanno supposto che il segreto del successo dell'esperimento galileiano stesse proprio nell'uso della pergamena che, essendo liscia, riduceva al minimo l'attrito. Secondo Naylor in realtà l'effetto fu contrario. Dal momento che la pergamena, essendo fatta con pelle di vitello o di pecora, non può superare la lunghezza di tre piedi, per quanto accuratamente si possano congiungere le estremità queste non possono essere abbastanza lisce da assicurare un passaggio senza ostacoli.

Insomma l'accelerazione della palla sarebbe stata periodicamente ridotta dalla necessità di superare i punti di giuntura tra i vari pezzi di pergamena e, se Galileo avesse eseguito veramente l'esperimento, si sarebbe subito accorto che l'uso della pergamena non solo non era di alcun aiuto ma era controproducente.'

'Alexandre Koyré, one of the greatest historians of science, supported the first hypothesis, namely that Galileo never performed the inclined plane experiment. This seemed incredible to many, so in 1961 Thomas S. Settle decided to try it under the exact same conditions as Galileo. He found that Galileo could have obtained 'satisfactory' empirical results, i.e. close if not quite identical to those he had reported, in the way he had claimed. Things thus seemed to finally fall into place and Stillman Drake, the best known American Galileo scholar, was able to state with satisfaction that 'Galileo's well-known assertions about his experiments on inclined planes had been completely validated'.

Unfortunately, in 1973 Ronald Naylor, in repeating Galileo's experiment once again, identified discrepancies between what Settle had done and Galileo's description. Settle had first rolled a ball not into the groove of the inclined plane but suspended on the edges of it. In this way he greatly reduced the effect of the rotation, which deprived the ball of much of its acceleration, thus providing data that more closely agreed with the law. But Galileo had not performed the experiment in this way. His inclined plane had a groove wide enough to hold the ball. Some scholars have assumed that the secret of the success of the Galilean experiment lay in the use of parchment which, being smooth, minimised friction. According to Naylor, the effect was actually the opposite. Since parchment, being made from calfskin or sheepskin, cannot exceed a length of three feet, no matter how carefully the ends are joined they cannot be smooth enough to ensure an unobstructed passage.

In short, the acceleration of the ball would have been periodically reduced by the need to overcome the joints between the various pieces of parchment and, if Galileo had actually performed the experiment, he would have im-

> mediately realised that the use of parchment was not only unhelpful but counterproductive.'

3.2 The experience by Thomas B. Settle

Original text from 2009

On January 17, 2008, through the Internet and on the website of the 'Fondazione Galileo Galilei', I found the text of Thomas B. Settle's article 'An Experiment in The History of Science' (1961) to which I refer above. With my precarious knowledge of English and the invaluable help of my son Juan Manuel, I set out to translate it.

The work of T.B. Settle is impressive. As the author uses the Anglo-Saxon system to express lengths, I decided to convert his data to the decimal metric system and speculate a bit about it.

The plan used by Settle measured

> 18 ft (5.48 m) long,
> 6" (15.0 cm) wide and
> 2" (5.0 cm) thick,

and endowed it with an inclination of $3°44'$. He tells us that he practised a rectangular section groove of $\frac{1}{4}$" (6 mm) width along the edge of the plane. He used a standard billiard ball of $2\frac{1}{4}$" (5.7 cm) diameter and a steel bearing ball of $\frac{7}{8}$" (2.2 cm) diameter to roll them. To measure time intervals he used an

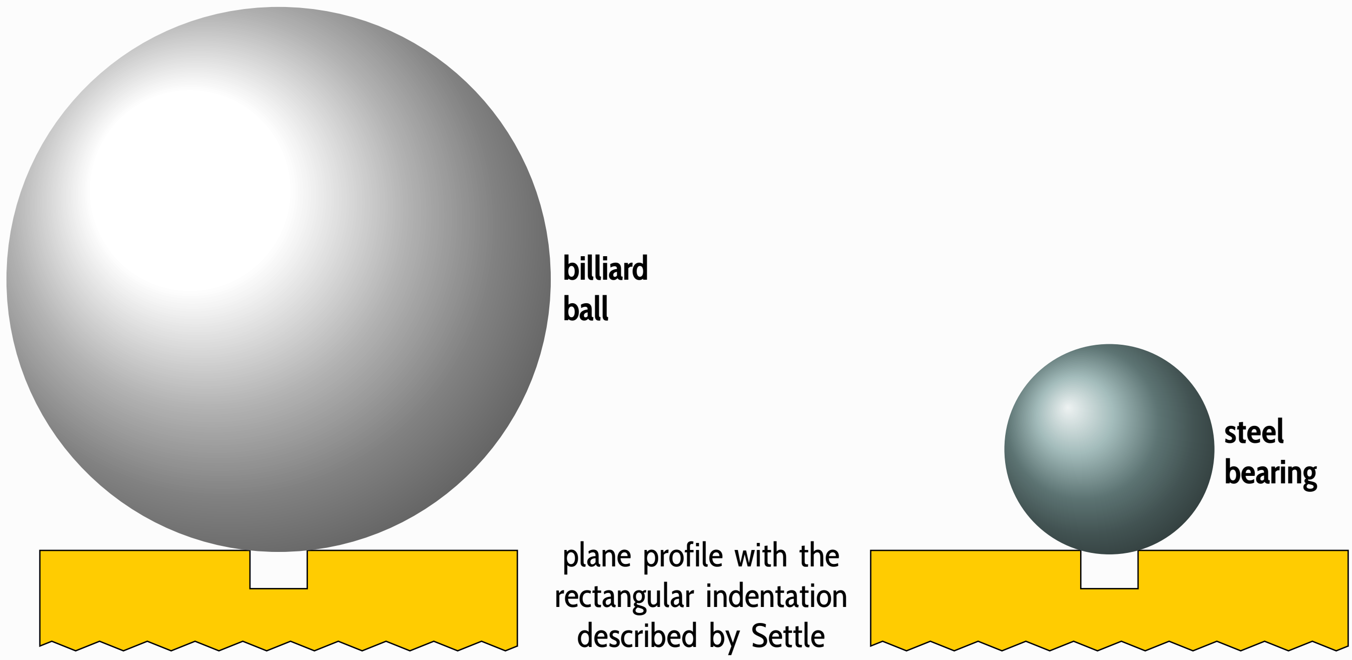


Figure 3.1: Settle experiment balls

'*ordinary pot*' as a water container with a glass tube threaded into the bottom hole for the water to flow into a graduated cylinder in millilitres placed underneath. The tube had a length of 4.5" (11.5 cm) and an approximate inner diameter of 0.18" (0.46 cm). The upper end of the tube could be capped and uncapped with a finger – to start or stop the flow of water – with the palm of the hand resting on the rim of the pot. By a series of operations, minutely described by the author, he succeeded in getting a uniform flow of water to flow through the tube, which he evaluated at 19.5 ml/s during the brief intervals of time that the rolls last. Since 1.95 ml $\approx$ 2.0 ml equals 0.1 s, it can be said that with this device one can be accurate up to '*1/10th of a pulse*', as Galileo presumed in his description of his own experiment.

As for the measurements of lengths and unevenness – operations for which Galileo does not describe the material he used – Settle resorts to rigid rulers, bubble levels and communicating vessels that could well have been used also by Galileo, to whom we must grant, at the very least, a similar ingenuity and initiative to those deployed by Settle three hundred and fifty years later.

In the article I am commenting on appears a table in which Settle records the spaces travelled (in feet, foot) and times spent (in millilitres, ml) by his *billiard ball* rolling on its inclined plane that forms an angle of $3.44°$ with the horizontal. The third column indicates – in the summary Table 3.1 that I make from his original one – the number of time *takes* made for each distance. Settle seems to choose the *mode* as the representative time, in the first four experiments, and the *arithmetic mean* in the last three. In experience No. (2) all seven shots taken agree in pointing out that 84 ml are spent in traversing the stipulated 13 ft. Settle therefore chooses that time as the *basis* for judging the goodness of the other experimental times. In Table 3.2 I repro-

#	Distances ft	Samples	Time ml
1	15	(10)	90
2	13	(7)	84
3	10	(6)	72
4	7	(7)	62
5	5	(12)	52
6	3	(7)	40
7	1	(15)	23.5

Table 3.1: Settle Table

duce these same data expressing the distances in centimetres (cm) and the times in seconds (s). I add a column where I calculate the acceleration corresponding to each descent. Within the limits of error imposed by the operative method, it can be assured that the *acceleration* remains constant, confirming the desired law $s \propto t^2$. But that is not the objective I am pursuing: *What I seek is to calculate the percentage of energy that has been dissipated, assum-*

ing that this dissipation is responsible for the most reliable experimental values obtained for the acceleration.

#	Distances cm	Time s	Acceleration cm/s
1	457.0	4.6	43.2
2	396.1	4.3	42.8
3	304.7	3.7	44.5
4	213.3	3.2	41.6
5	152.4	2.7	41.8
6	91.4	2.0	45.7
7	30.5	1.2	42.3

Table 3.2: Settle table with acceleration

To calculate the acceleration $\underline{a}_{CM}$ that would correspond to a *ideal frictionless rolling* – taking into account that the ball rolls along a rail maintaining *two contact zones* with the plane – we must apply the equation 1.9 obtaining:

$$\underline{a}_{CM} = 697.5 \sin 3°44' = 45.4\,\text{cm/s}^2$$

This theoretical result invalidates as absurd the value obtained in the experience number (6) of the Table 3.2, but does not invalidate the others which are perfectly admissible. The experimental value of the acceleration more trustworthy could be the one obtained in the experience number (1), *since the experimental values of departure are those affected by a smaller relative error*. Let us calculate in this case the percentage of dissipated energy, using the equation 1.10:

$$\chi = 100\left(1 - \frac{43.2}{45.4}\right) = 4.8\,\%$$

(If we were to take experience no. (2), chosen as *base* by Settle, it would turn out to be of the 5.7 %).)

It is of course *impossible* to control *all* the variables that can influence a rolling, so that – even if the slope and the path of *two* of them are the same – there will always be *other* variables – small differences in the initial position of the ball, irregularities in its shape and in its path on the plane – that will make them slightly *different*. So, the **friction forces** will also act a bit differently in two identical rollings.

The differences between Galileo's experience and that of Settle consist in the fact that the plane used by the former was somewhat longer; in that the angle which I attribute to it in the analysis which I shall make later of the folio *81*r, is somewhat smaller; and that – in my opinion – *the indentation or channel through which the ball would descend in Galileo's plane was not of rectangular section but an arc of circumference*, a criticism in which I agree with that expressed by Naylor.

Since Galileo does not inform us of the dimensions of the ball he used – limiting himself to say that it was '*of very hard bronze, well rounded and polished*' – I have allowed myself to represent it in Figure 3.2 similar to the steel bearing used by Settle. Another difference consisted in the fact that Galileo *weighed* the water collected in each operation instead of *measuring its volume*. But Settle himself informs us that by the sixteenth century there were balances capable of weighing weights up to $0.2\ \mathrm{g}$, thus acknowledging that the time measurements practised by Galileo may have been much better than his own.

The experience carried out by Settle shows that the law $s \propto t^2$ *could* be subjected to efficient empirical verification with the means described by Galileo in the famous passage of the '*Discorsi*'. But Galileo goes even further when he states that:

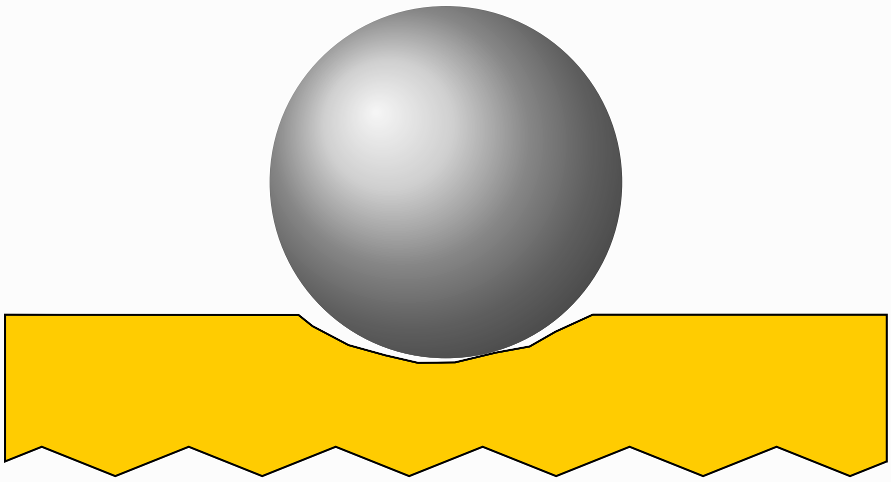


Figure 3.2: Idealisation of the profile of the plane with the semicircular channel. Galileo says that 'this channel, cut as straight as possible, was made extremely smooth and even by placing a piece of parchment paper inside it, polished to the maximum'. This observation leads me to believe that this channel, 'a little more than a finger's width', must have had a curved profile, obtained with a curved gouge. This profile would, if possible, provide a single zone of contact between the ball and the plane.

> *'This could be applied to all inclinations of the plane, i.e., of the channel through which the ball was lowered.'*

If by *inclination of the plane* we understand the *sine of the angle it forms with the horizontal* it is very easy *today* to propose an experiment to verify if the law $s \propto t^2$ is applicable '*to all the inclinations of the plane*', as Galileo assured. Let us see:

$$s \propto (\sin \alpha)\, t^2$$

$$s \propto \left(\frac{h}{L}\right) t^2$$

If by L we mean the length of the plane, *which is a constant*, we can simplify the above expression:

$$s \propto h\,t^2$$

Was Galileo able to experimentally verify the above statement?

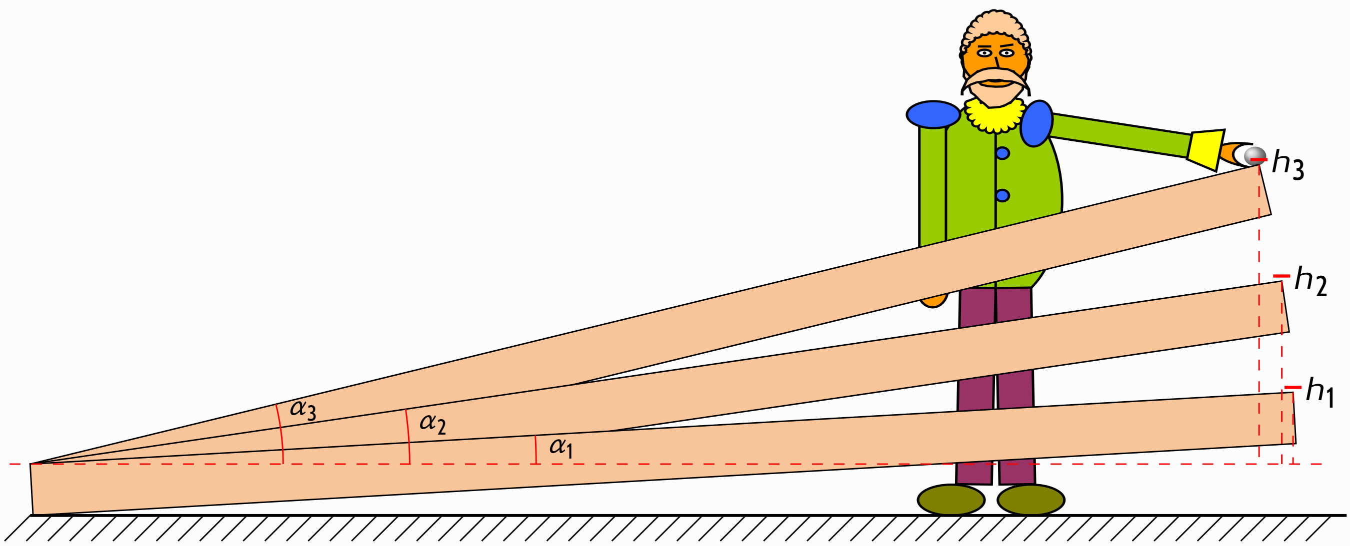


Figure 3.3: Was Galileo able to verify this experimentally?

There is an *empirically verifiable* consequence that follows from the above equation:

$$\frac{t_a}{t_b} = \sqrt{\frac{s_a\,h_b}{s_b\,h_b}} \tag{3.1}$$

where t_a and t_b represent the times spent by the ball in travelling *any* distances s_a and s_b on planes of different *inclinations* h_a and h_b.

Settle subjected this consequence to empirical verification. Let us clarify first that to determine the sine of the angle of inclination Settle chooses a fixed length ($L = 8$ ft $= 96$) and measures the heights h, corresponding to each angle, also in inches, so that:

$$\sin\alpha = \frac{h}{96}$$

I reproduce below (Table 3.3) the second and last table published by Settle in his article, in which the data obtained by him *with his billiard ball* are collected to prove experimentally whether Galileo's assertion is correct: The data

#	s ft	h "	t ml
1	12	2,92	117
2	13	6,25	84
3	9	11,47	53

Table 3.3: Settle table with billiard ball

of experience no. (2) are extracted from Table 3.1. Anyone who is curious can check that the data contained in Table 3.3 satisfy the condition expressed in the equation 3.1.

It seems obvious to me that if Settle successfully made these empirical verifications with such precarious means, so *could* Galileo three hundred and fifty years earlier.

I translate from Settle's article (Settle, 1961) a paragraph that I would like to comment on in some detail:

> *'The results of the tests made with the steel ball were just as good, but I found that they were not comparable with those made with the billiard ball. For instance, on the shallowest slope, the billiard ball made the 16-foot mark in 136 millilitres*

> *but the steel ball took 4 millilitres longer. This seemed odd; theoretically, neither the mass nor the radius should affect the acceleration. By the correct formula we can calculate that both balls should have traversed the distance in 132 millilitres. Actually, because the balls run on the two edges of the groove, their "running" circumferences are slightly less than their real ones, so they require more revolutions, and more time, to cover the same distance. A rough calculation shows that this fact probably accounts for most of the discrepancies. Had Galileo noticed similar differences between results for balls of different size, he probably would have ascribed them to frictional retardation. In any case, it appears that they would not have controverted his proportionalities.'*

This paragraph is interesting because in it Settle notes with acuity an experimental observation and suggests a theoretical explanation that is accurate only up to a point. It is curious that he downplays friction as the main cause of the observed retardation and attributes it to purely geometrical factors such as '*smaller-than-actual rolling circumferences*'. In this he aligns himself with historians who have addressed the enigma of Galileo's unpublished folios, which we will discuss later. *We shall now demonstrate that it is friction that is chiefly responsible for that strange observation made and sharply noted by Settle.*

When he alludes to '*the correct formula*', Settle is undoubtedly referring to the equation:

$$s = \frac{1}{2}\left(\frac{5}{7} g \sin\alpha\right) t^2$$

for, indeed, using it, we obtain that both balls – irrespective of their masses and radii – should take 6.77 s (equivalent to 132 ml) to travel 16 ft.

But a rigorous calculation – and not '*approximate*', as suggested by Settle – can be made from the equation 1.9. This calculation shows that the 16 ft should have been traversed by 6.78 s by the billiard ball and by 6.79 by the steel bearing. However, based on the experimental data, it turns out that the billiard ball takes 6.97 s (0.19 s more) and the steel ball 7.18 s (0.39 s more)...

No doubt this additional delay must be put down to rolling friction, and Galileo would have been fully correct in thinking that way.

It can be verified – applying the same equation – that the *ideal accelerations in frictionless rolling on the rectangular rail* carved by Settle should be:

$$\text{For the billiard ball:} \quad \underline{a}_{CM} = 21.18\,\mathrm{cm/s^2}$$
$$\text{For the steel ball:} \quad \underline{a}_{CM} = 21.13\,\mathrm{cm/s^2}$$

While the *experimental accelerations* turn out to be:

$$\text{For the billiard ball:} \quad \underline{a}_{CM} = 20.06\,\mathrm{cm/s^2}$$
$$\text{For the steel ball:} \quad \underline{a}_{CM} = 18.90\,\mathrm{cm/s^2}$$

From these data it is obtained that the billiard ball dissipates in rolling a 5.3 % of its energy, while the steel ball dissipates up to a 10.6 %. There is nothing strange about this: The billiard ball – see Figure 4.1 – rolls on the rail keeping the two contact zones relatively close to each other in relation to its own diameter, whereas this is not the case for the steel ball, hence the percentage of dissipated energy in the latter case is higher. This is in agreement with my own experimental results contained in the section 2.6 (page 56) entitled "Two experiences of January 2001".

The calculations we have just made also show that in Settle's experience – ball rolling on the edges of the groove – '*the sphere is not deprived of much of its acceleration*' relative to Galileo's experience – ball rolling on the bottom of the groove – as the latter describes it. The width of the groove practised by Settle is so small in relation to the diameter of the billiard ball used that there is no appreciable difference between the accelerations acquired by *the same ball* in both cases. The objection that Trocchio attributes to R. Naylor on this point seems to me to be meaningless.

More serious seems Naylor's objection concerning the parchment '*smooth and smooth*' with which Galileo claims to have coated the bottom of the groove. My opinion, however, is that Galileo '*if he had actually carried out the experiment*' would not only have noticed this detail but would have successfully solved it by sanding more thoroughly the '*joining zones between the different pieces of parchment*'...

In connection with all this I will allow myself to relate my first experience with the inclined plane, in which there was an unresolved '*zone of union*' or '*solution of continuity*', such as those adduced by Naylor in his critique.

3.3 My first inclined plane
Original text from 2003

During the summer of 1979 my colleague Adolfo Cruz acted as a member of an examining board for a physics and chemistry teacher of Bachillerato. On his return, he told me that in the report presented by one of the candidates he had seen an attempt to reproduce Galileo's experience with the inclined plane using a V-shaped profile channel. Adolfo was of the opinion that the friction at the two points of contact of the little sphere with the two walls of the channel would make the project unfeasible. I told him that I was going to try it, but looking for another procedure that would allow only one point of contact to reduce friction as much as possible.

My unconditional sympathy for Galileo was already old, but my information about his life and work, in 1969, was reduced to the biography of Cortés Pla (Pla, 1946) and the comments dedicated to him by John D. Bernal (Bernal, 1979). In spite of everything, I had the audacity to choose his figure as the subject for the inaugural lecture of the 69/70 academic year, which fell on me – poor interim – when the professors of the *Instituto Nacional de Bachillerato*, where I had been working for two years, avoided the commitment with various pretexts. Ten years later, when I made this experience, my knowledge of Galileo had been somewhat expanded by Gerald Holton's references (Holton, 1993).

When on October 28, 1979, I set out to measure the rolling times of a glass marble ($16\,\mathrm{mm}$ diameter), along a channel ($5\,\mathrm{mm}$ wide)-improvised with two taut, parallel fishing lines-over the narrowest faces ($2\,\mathrm{cm}$) of two aligned boards ($181.5\,\mathrm{cm}$ long each), slanting only $1°$, I could not even imagine that I had found an inexhaustible source of wholesome amusement and wisdom from which I would drink to this day. In Table 3.4 are the results as I consigned them, at the time:

The two boards functioned as side trims (Formica-covered chipboard) for my children's beds. My father-in-law (a retired carpenter) would release the marble and I would press the stopwatch. We achieved simultaneity (?) by the procedure of 'one, two... three'. At the end of its run, the marble would hit the bottom

x cm	t s	$K = x/t^2$
360	8.4	5.1
330	7.8	5.4
300	7.4	5.5
270	7.0	5.5
240	6.6	5.5
210	6.3	5.3
180	5.8	5.4
150	5.0	6.0
120	4.4	6.2
90	3.9	5.9
60	3.1	6.2

Table 3.4: Rolling of glass sphere over a channel

of an empty peanut can, next to which I was located, and the sound of this collision would serve (in addition to the direct vision) to stop the stopwatch. Luis (my oldest son of ten years) took note of the readings. To achieve the inclination of both planes, three wooden blocks of 32.5 mm each (coming from my son's and collaborator's architecture set) were used, two of them wedging the first plane, and the third wedging the *continuity solution* with the second plane, whose other end rested on the floor, so that the inclination was the same in both of them. Several measurements were taken (never less than three) of each time and the average values were recorded in the table. Three hours were invested in the experience – according to the report written at the time and which I now consult – and it was carried out in the dining room of this sixth floor where I live on the shores of the Bay of Algeciras. All the material used, except for the analogue chronometer capable of measuring double tenths of a second, was strictly home-made, within anyone's reach. As far as the chronometer is concerned, today digital models, capable of reading hundredths of a second, can be purchased very cheaply in any bazaar.

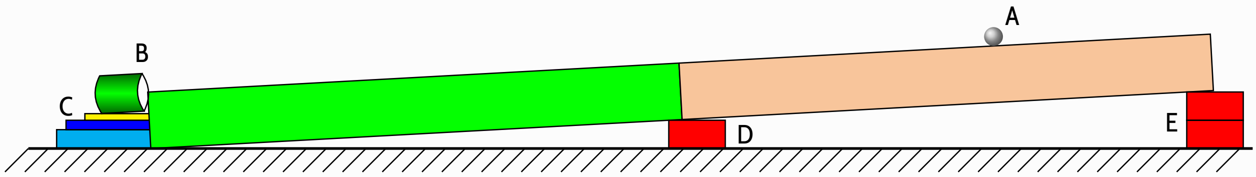


Figure 3.4: (A) Small ball. (B) Empty peanut can that acted as a warning device to stop the stopwatch. (C) Books. (D) y (E) Cleats intended to wedge the planes to give them the right inclination.

We can agree with Koyré about Galileo's Platonism, but to deny him ingenuity and experimental skill and, above all, to *ignore the stimulating power of experiment* (however crude and inexact it may be) in a curious and acute mind is, in my opinion, Koyré's defect and that of all historians of science who have held the same position. I think they have contributed to the myth that experiments in mechanics are very difficult, not at all convincing, and that it is not worth taking the trouble to do them because... they will not come out because of frictional forces...!

I immediately proceeded to calculate the *theoretical acceleration* with which the marble must have rolled using the equation:

$$a_{\mathrm{CM}} = \frac{5}{7} g \sin \alpha,$$

siendo:

$$\sin \alpha = \frac{3.25}{181.5} = 0.0179$$

and resulting:

$$a_{\mathrm{CM}} = 12.53 \, \frac{\mathrm{cm}}{\mathrm{s}^2}$$

This result was quite in agreement with the experimental value obtained, which would range between 10.2 and $12.2\ \mathrm{cm/s^2}$. There is a *solution of continuity* (a bump) between the two planes and, moreover, it is utopian to claim that the two would have exactly the same inclination. Any curious person can verify, by calculation, that twelve hundredths of a sexagesimal degree of difference between the two slopes is sufficient to justify the observed disagreement between the first seven and the last four values of $\mathbf{K}$ in Table 3.1. This is what I call the stimulating power of experiment, crude and inexact as it is.

My father-in-law and collaborator made some helpful suggestions to me, along with the Aristotelian statement that '*the time of rolling will depend on the weight of the little sphere used*'. I did not bother to refute his assertion in the expectation that experience itself would take care of that. When we tried it with a little sphere *of iron* of 16 mm diameter he became very serious and muttered: '*If I don't see it I don't believe it... I would have liked not to have had to work as a carpenter all my life to devote myself to these things*'. I suppose I can regard this reaction as another example of what I have called 'the stimulating power of experiment'.

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Chapter 4

Parabolic trajectories

Original text from 2003

4.1 Background

On November 17, 1979, I had my home-made inclined plane set up in the laboratory when I had the idea of adding a horizontal plane to it. I wanted to test if it was possible to record, and relate to each other, some points of the parabolic trajectory that the little sphere describes when it leaves the edge of the horizontal plane after traveling through the inclined one.

In June 1977 a good student, Juan Falgueras Cano, together with others who, like himself, had just passed the Selectividad exam [1], asked me to contact the Physics laboratory. My own contacts at that time were very rudimentary and full of prejudices, like those of most of my middle-level teaching colleagues; prejudices that I will call "Koyrésian", although at that time I had not read anything by Koyré... I had read quite a lot *about* Galileo, but almost nothing of what Galileo *had written*. I proposed to the aforementioned group the realization of a practice described in the manual of a team (Torres Quevedo) related to parabolic trajectories. While I participated – at last successfully – in the I.N.B. attaché examinations held that summer, they worked on the subject on

[1] **Editor's Note:** A test to which young students in Spain are subjected to gain access to university

their own. One afternoon I paid them a visit and they presented me with the results. They were not bad at all, but to obtain them they had been obliged to repeat many impacts for each point (the accelerating plane consisted of two parallel cylindrical bars through which the sphere descended maintaining two points of contact; the waxed table of the laboratory served as a horizontal deflector plane (?); between the bars and the table there was a small jump... etc.), and the impacts obtained for the same point were very ungrouped. The results were not very encouraging, but rather fed the "Koyrésian" prejudices that almost all of us carry inscribed in our genes.

4.2 The horizontal register

But on the aforementioned date, encouraged by the good results obtained with the inclined plane, I remembered the work done by my students in the past and decided to repeat it and, if possible, improve it. To do so, I assembled the set-up shown in Figure 4.1 about horizontal register.

Boards B and C were nothing more than the removable shelves of one of the laboratory cabinets. Board C was glued to a long strip of white paper covered by another strip of carbon paper, so that when the little sphere impacted it left a circular mark. The major difficulty was to ensure (who could do it?) the perfect horizontality of the plane C in the successive positions 1, 2, etc. That is why we fixed in advance the ordinates y that with our means (bars, supports, tables etc.) we could obtain with relative comfort, while the abscissae x would be read on the register of marks on the paper. Thus we would have the

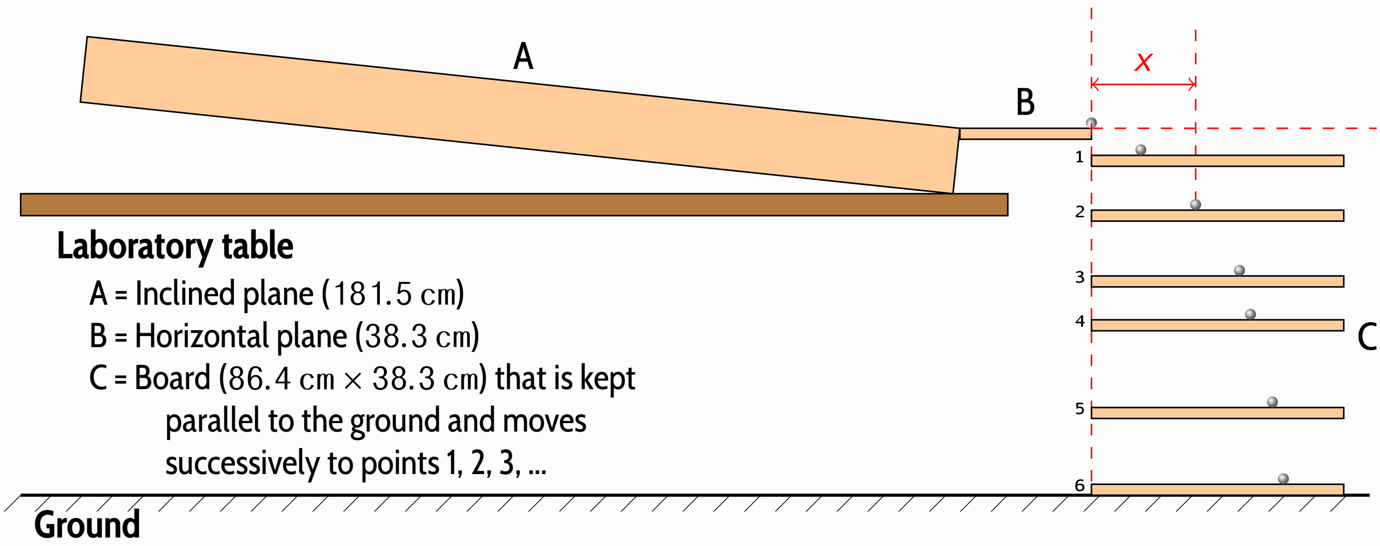


Figure 4.1: Registro Horizontal

coordinates of each point in a rectangular reference system with origin at the edge of the board B, where the coordinate axes x and y intersect.

The conditions under which this first record was made were:

a) The little sphere always travelled 180 cm, starting from rest, over the inclined plane.

b) Four impacts (which were highly clustered) were recorded for each position on board C. The mean value in each case was taken as representative of the abscissa.

The results are shown in Table 4.1.

As it is known, the physical interpretation of the constant $\mathbf{K} = \mathbf{y}/\mathbf{x}^2$ is:

$$K = \frac{g}{2v^2}$$

where v is the instantaneous speed of the small ball as it emerges horizontally from the edge of the horizontal board and g is the gravitational acceleration.

x cm	y cm	y/x^2
15.0	9.4	0.0417
28.7	33.0	0.0400
33.8	45.0	0.0394
38.3	57.5	0.0392
46.2	83.8	0.0392
49.6	95.8	0.0389

Table 4.1: Record with 180

From there we can calculate the value of v, which turns out to be:

$$v = 112.0\,\text{cm/s},$$

if we take

$$K = 0.039\,\text{cm}^{-1}$$

y

$$g = 980.0\,\text{cm/s}^2$$

But there is another way to find out the speed $\underline{v}$ of the small ball at the point where the inclined plane ends and the horizontal plane begins.

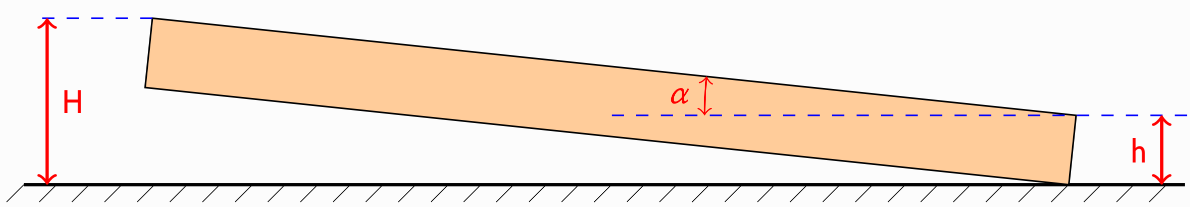


Figure 4.2: $H = 31.4\,\text{cm}$ $h = 20.9\,\text{cm}$ $H - h = 10.5\,\text{cm}$

$$\sin\alpha = \frac{10.5}{181.5} = 0.0578$$

$$a_{\text{CM}} = \frac{5}{7} g \sin\alpha = 40.5\,\text{cm/s}^2$$

$$\underline{v} = \sqrt{2ax} = 10.7\,\text{cm/s}$$

This is recorded in the report I wrote that day. I do not remember the reason that moved me to make the calculation of the acceleration by this procedure, instead of doing it by measuring the time taken to travel the 180.0 cm of inclined plane, data that I possibly took, but that does not appear in the record. Perhaps I wanted to obtain the "theoretical" value of the acceleration to evaluate the percentage of speed lost. Such percentage turns out to be 7.3 % I assumed that this loss was due to several reasonable reasons, such as loss of the vertical component of the velocity due to collision on the board (calculable and not very significant), energy dissipation, both in the inclined plane and in the horizontal plane (of doubtful horizontality), as well as in the continuity solution (inevitable bump) between both planes.

4.3 The stimulating power of the experiment

With all this empirical data in my possession I set out to calculate what would happen if I repeated the experience for various paths of the sphere (135 cm, 90 cm, 45 cm) on the inclined plane. The calculations of the corresponding $\underline{v}$ and v, taking into account the percentage of lost rapidity is easy. The results are given in Table 4.2.

Needless to say, I immediately launched into experimental testing. The results are shown in the Tables 4.3.

x cm	v cm/s	$\underline{v}$ cm/s
135.0	105.0	97.2
90.0	85.7	79.4
45.0	60.6	56.1

Table 4.2: Sphere over planes of 135 cm, 90 cm and 45 cm

Record with 135			Record with 90			Record with 45		
x	y	y/x^2	x	y	y/x^2	x	y	y/x^2
12.9	9.4	0.056	10.7	9.4	0.082	7.5	9.4	0.167
24.7	33.0	0.054	20.2	33.0	0.081	14.1	33.0	0.166
29.4	45.0	0.052	23.8	45.0	0.079	16.8	45.0	0.159
33.0	57.5	0.052	26.7	57.5	0.080	18.9	57.5	0.161
39.7	83.8	0.053	32.6	83.8	0.079	23.0	83.8	0.158
42.7	95.8	0.052	34.9	95.8	0.078	24.6	95.8	0.158

Table 4.3: Experimental data

$$\underline{v} = 96.1\,\text{cm/s} \qquad \underline{v} = 78.4\,\text{cm/s} \qquad \underline{v} = 55.16\,\text{cm/s}$$

Compare these experimental values of $\underline{v}$ with those calculated in Table 4.2.

I took advantage of this experience to interest my COU [2] students of that year. I took groups of volunteers to the laboratory in the afternoons and divided them into four teams. I explained the experimental technique and team (1) took the points of the 180 cm log. Then team (2) did the same for the 135 log. In the meantime team (1) interpreted their data and I challenged them to *predict* the results that team (2) would obtain. The interest of each group to check the degree of accuracy of their predictions animated the atmosphere, and the questions (Does the sphere take the same time to travel through all

[2] **Editor's Note:** COU stands for "Curso de Orientación Universitaria"

the parabolas? Can we calculate it? Can we measure it? Why do the marks left by the sphere increase in diameter as we advance in the record?) came up naturally and I encouraged them to look for the answers and to devise experimental techniques to check them. A real delight for them and, of course, for me.

4.4 The vertical register

On November 19, Adolfo Cruz Lobo, my Seminar partner at that time, suggested to me the idea of making vertical records. In fact, that was my initial intention, as it appeared in the manual of the Torres Quevedo team. When it came down to it, however, it seemed more feasible to keep a 86.4 cm board horizontal than vertical, and I decided to go for horizontal. Since I was beginning to stop being ”Koyrésian”, and the empiricism fever had taken hold of me, I jumped to the task at once. On a long strip of paper I made a ladder with divisions spaced two by two centimetres, glued it to one of the boards and laid it horizontally on the floor to serve as an abscissa axis. Another vertical board with the corresponding register and carbon paper would be placed on top of it. The aspect of the assembly is as shown in Figure 4.3 (Vertical register), and the conditions were the same as described above. In this case the abscissae were fixed on the graduated scale while the ordinates would be recorded on the mark register covered by the carbon paper. In this way we would have the coordinates of the marks with respect to the same reference system as before.

The results are shown in Table 4.4:

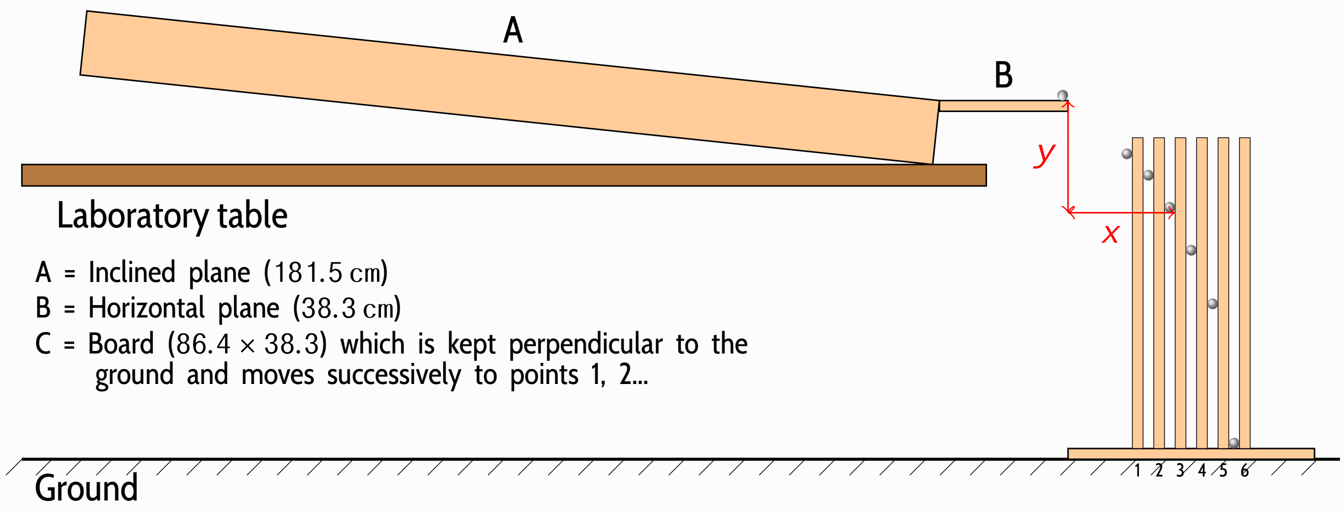


Figure 4.3: Vertical register

x cm	y cm	y/x^2
20.0	14.5	0.0362
22.0	17.5	0.0361
24.0	20.9	0.0363
26.0	24.5	0.0362
28.0	28.3	0.0361
30.0	33.4	0.0371
32.0	38.5	0.0376
34.0	42.8	0.0370
36.0	48.5	0.0374
38.0	53.6	0.0371
42.0	63.7	0.0361
44.0	70.4	0.0364
46.0	77.4	0.0366

Table 4.4: Record with 180 cm

When compared with Table 4.1 we find reason to be puzzled:

a) The constant does not have the same value:
Here it comes out $K = 0.037\,\text{cm}^{-1}$ compared to that obtained in the horizontal register $K = 0.039\,\text{cm}^{-1}$.

b) It is very flattering, but puzzling, that in this case the K values are more clustered, and not in decreasing progression, as in the horizontal log.

c) The output velocity turns out to be worth `115.0 cm/s` instead of the `112.0` that came out with the horizontal log data.

However, there seems to be something suspicious in such perfection. A few hours later, almost without consciously thinking about it, the solution to the enigma revealed itself.

4.5 The importance of the centre of mass

In both the *horizontal* and *vertical* recordings, *circular black spots or marks* are obtained on the white paper. The *coordinates of the centre* of each of these marks *tell us* about the *coordinates of the centre of mass of the sphere* at each impact.

What we are interested in are *the coordinates* (x, y) *of the* CM in the reference frame with origin located at the CM of the sphere at the instant the sphere exits the edge of the board (red axes in the Figure 4.4), but what we obtain in both registers (horizontal and vertical) are the coordinates (x, y) *centres of the marks* in the reference frame with origin at the edge of the board (blue axes in the figure).

In the *horizontal log* the values of both pairs of coordinates (red and blue) coincide, but not so in the *vertical log*, where a correction must be made to take

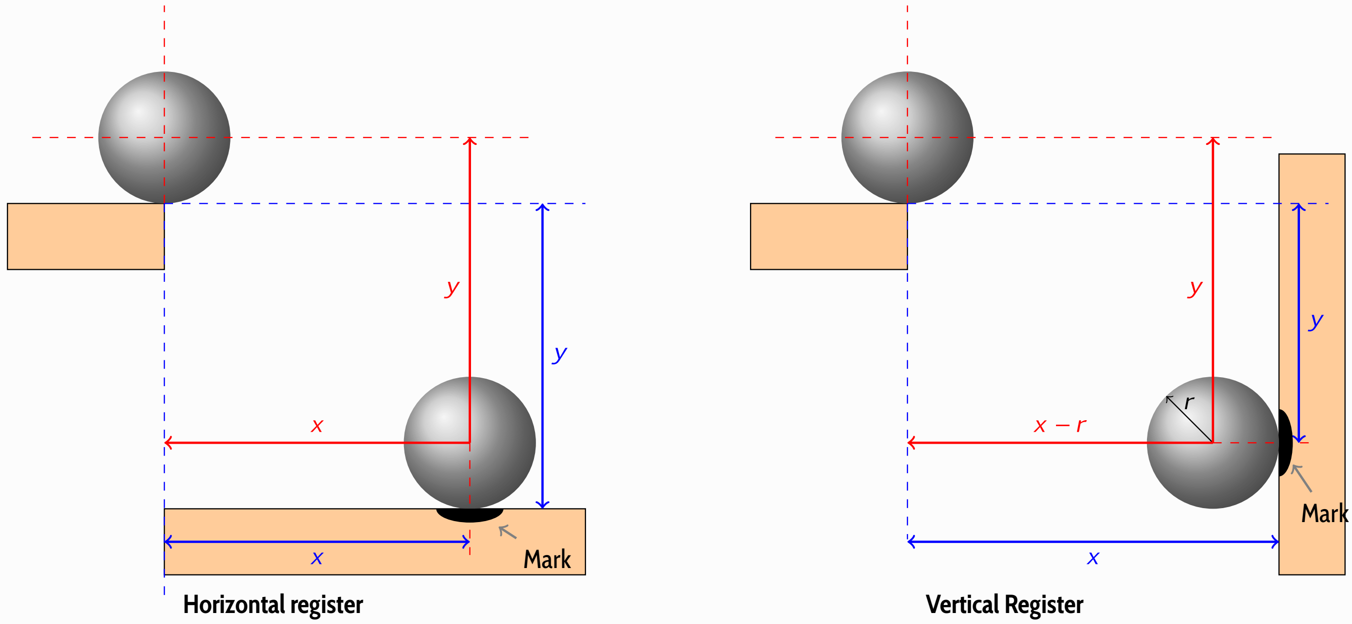


Figure 4.4: Difference in horizontal and vertical register markings

into account the value of the radius r of the sphere itself. I suppose Figure 4.4 is more eloquent than any meticulous clarification.

The small ball used had a radius $r = 0.8\,\mathrm{cm}$ Once the relevant correction has been made the result can be seen in Table 4.5: The agreement with the Table 4.1 is perfect, as it could not be otherwise: In both cases the coordinates of the centre of mass of the small ball with respect to the correct reference frame are already being considered. As we go down in both Tables 4.1 and 4.5, it is observed that the value of the constant tends to stabilize at $0.038\,\mathrm{cm}^{-1}$, after starting with values of $0.041\ \mathrm{cm}^{-1}$. The stabilization and reliability of the value of K as one moves down both tables is due to the fact that the *relative error* of the measurements decreases in the same direction, an observation and practical teaching of extraordinary interest to students.

The importance of the centre of mass of a system stands out naturally in this experience. Experience that I took advantage of to confront my students with the enigma and spur them on in search of the solution.

$x - r$ cm	$y + r$ cm	$(y + r)/(x - r)^2$
19.2	15.3	0.0415
21.2	18.3	0.0407
23.2	21.7	0.0403
25.2	25.3	0.0398
27.2	29.1	0.0393
29.2	34.2	0.0401
31.2	39.3	0.0404
33.2	43.6	0.0396
35.2	49.3	0.0398
37.2	54.4	0.0393
41.2	64.5	0.0379
43.2	71.2	0.0381
45.2	78.2	0.0383

Table 4.5: Vertical register

4.6 Complete prototype

During the summer of 1980, the prototype underwent several improvements, such as:

a) At the suggestion and initiative of Mercedes Marfil, a COU student during that course, the horizontal plane was replaced by a thick wooden ruler B of 50.5 cm, covered by a *glass plate* C. It was intended to obtain a *lively edge* at the exit of the sphere, as well as to *attenuate friction*, and it seemed to us that the use of glass would improve both circumstances with respect to the wood that had been used until then.

b) To place two graduated rulers (F), attached by means of a jack (D) to the *horizontal* ruler, which would serve as guides to determine the x of the centre of mass of the small ball. With them the correction of the radius could be made automatically.

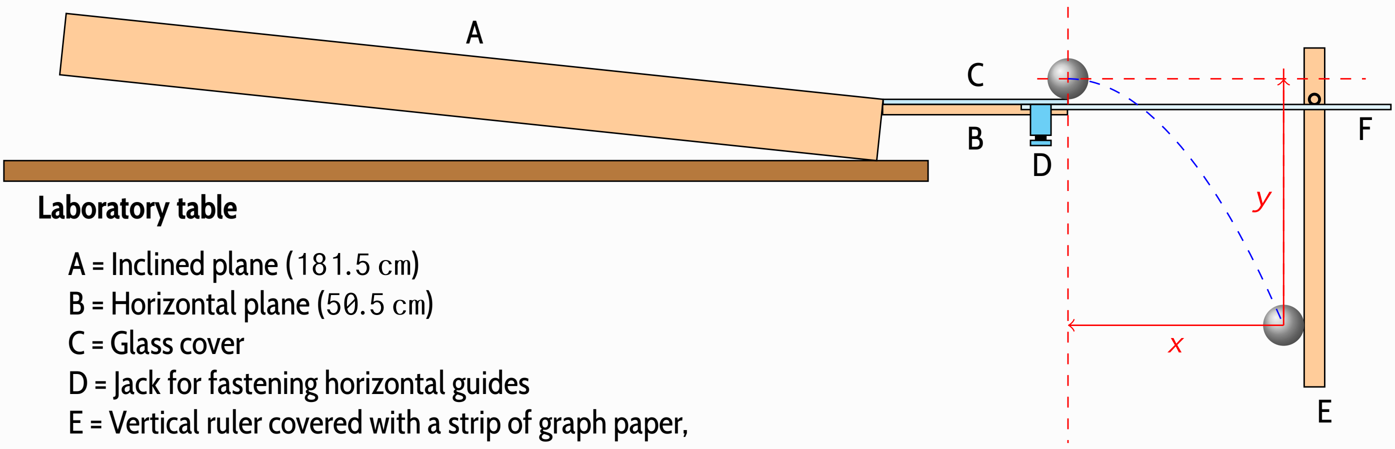
c) On these guides would slide another thick *vertical* ruler (E), crossed by a horizontal support screw, which would serve to locate the (Y) of the centre of mass of the small ball.

d) The use of a strip of *graph paper*, covered with another strip of carbon paper, on the vertical ruler to record the marks corresponding to each impact.

e) Record the *zero impact* by placing the ruler carrying the record (E) exactly on the edge of the glass (C). From the centre of the mark thus obtained the ordinates Y of the remaining points would be measured.

f) Record only one *impact for each point*, varying the abscissa from $x = 5.0\,\mathrm{cm}$ onwards from centimetre to centimetre. The marks for $x < 5.0\,\mathrm{cm}$ are so close together that they form practically a single spot.

The prototype was as shown in Figure4.5: A record obtained with this setup is reproduced in Figure 4.6 and Table 4.6. The sphere has travelled 150 cm. on the inclined plane accelerating from the rest. Then it has travelled 50.5 cm on the horizontal rule and has gone to impact on the vertical carrying the register The zero impact is observed at the head of the record. The other impacts have been numbered for easy location and to measure their ordinate
The impact marks all have the same diameter, 4 mm, since the horizontal component of the quantity of motion is conserved throughout all parabolic flights. The horizontal component of the velocity can easily be estimated as 127.4 cm/s. The zigzagging of the marks, which we have reproduced as faithfully as possible, is due to the obvious fact that the direction of the exit velocity is not



A = Inclined plane (181.5 cm)
B = Horizontal plane (50.5 cm)
C = Glass cover
D = Jack for fastening horizontal guides
E = Vertical ruler covered with a strip of graph paper, which in turn is covered with a strip of carbon paper
F = Horizontal guides on which it is possible to slide E

Figure 4.5: Prototype for vertical registration

always the same. This direction oscillates within a narrow range due to the fact that the channel improvised on the glass with the two parallel taut fishing lines is 5 mm. wide. Note that the zigzagging becomes more pronounced as we move down the register.

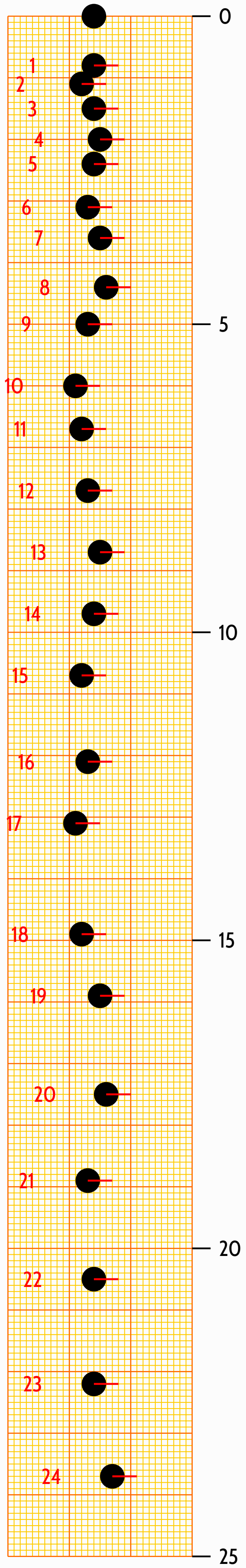


Figure 4.6

#	x cm	y cm	y/x^2
1	5.0	0.8	0.0320
2	6.0	1.1	0.0305
3	7.0	1.5	0.0306
4	8.0	2.0	0.0312
5	9.0	2.4	0.0296
6	10.0	3.1	0.0310
7	11.0	3.6	0.0297
8	12.0	4.4	0.0305
9	13.0	5.0	0.0296
10	14.0	6.0	0.0306
11	15.0	6.7	0.0298
12	16.0	7.7	0.0300
13	17.0	8.7	0.0301
14	18.0	9.7	0.0299
15	19.0	10.7	0.0296
16	20.0	12.1	0.0302
17	21.0	13.1	0.0297
18	22.0	14.9	0.0307
19	23.0	15.9	0.0300
20	24.0	17.5	0.0304
21	25.0	18.9	0.0302
22	26.0	20.5	0.0303
23	27.0	22.2	0.0304
24	28.0	23.7	0.0302

Table 4.6: This page reproduces a record obtained on January 17, 1994 with the device described in the previous page.

4.7 The final model

In 1998 Antonio Molina Fernández, my Seminar colleague, built a one-piece aluminium model, which has the advantages of not having any continuity solution between the inclined and horizontal planes and of being easily transportable to the classrooms. It has the disadvantage that the angle of inclination is fixed ($4°53'$), which is more than compensated by the advantages mentioned above. I have called it the "final model", whose perspective can be seen below, because it seems to me that it can no longer be improved.

It is easy to calculate the instantaneous speed of the small ball CM at the instant when it passes from the inclined plane to the horizontal plane: $v = 116.6\,\text{cm/s}$.

The record obtained in the experience of November 17, 1999, is reproduced on page 103. The result is unbeatable. From the value of the constant $K = 0.0380\,\text{cm}^{-1}$ it can be deduced that the instantaneous speed of the CM of the small ball in the instant in which this one leaves the horizontal plane is worth $113.4\,\text{cm/s}$. As expected, the small ball, no matter how perfect it is and how horizontal the plane is arranged, experiences a deceleration mainly due to rolling friction.

The ruler carrying the vertical register is, in this case, made of aluminium and is supported on the horizontal guides by two parallelepipeds that prevent oscillation and ensure verticality. This is an undeniable advantage with respect to the *prototype*, in which the screw that crosses the wooden rule is not so effective in assuring both conditions.
The fact that the horizontal guides are solid with the horizontal plane is also an advantage over the *prototype*, in which the horizontal plastic rulers had to

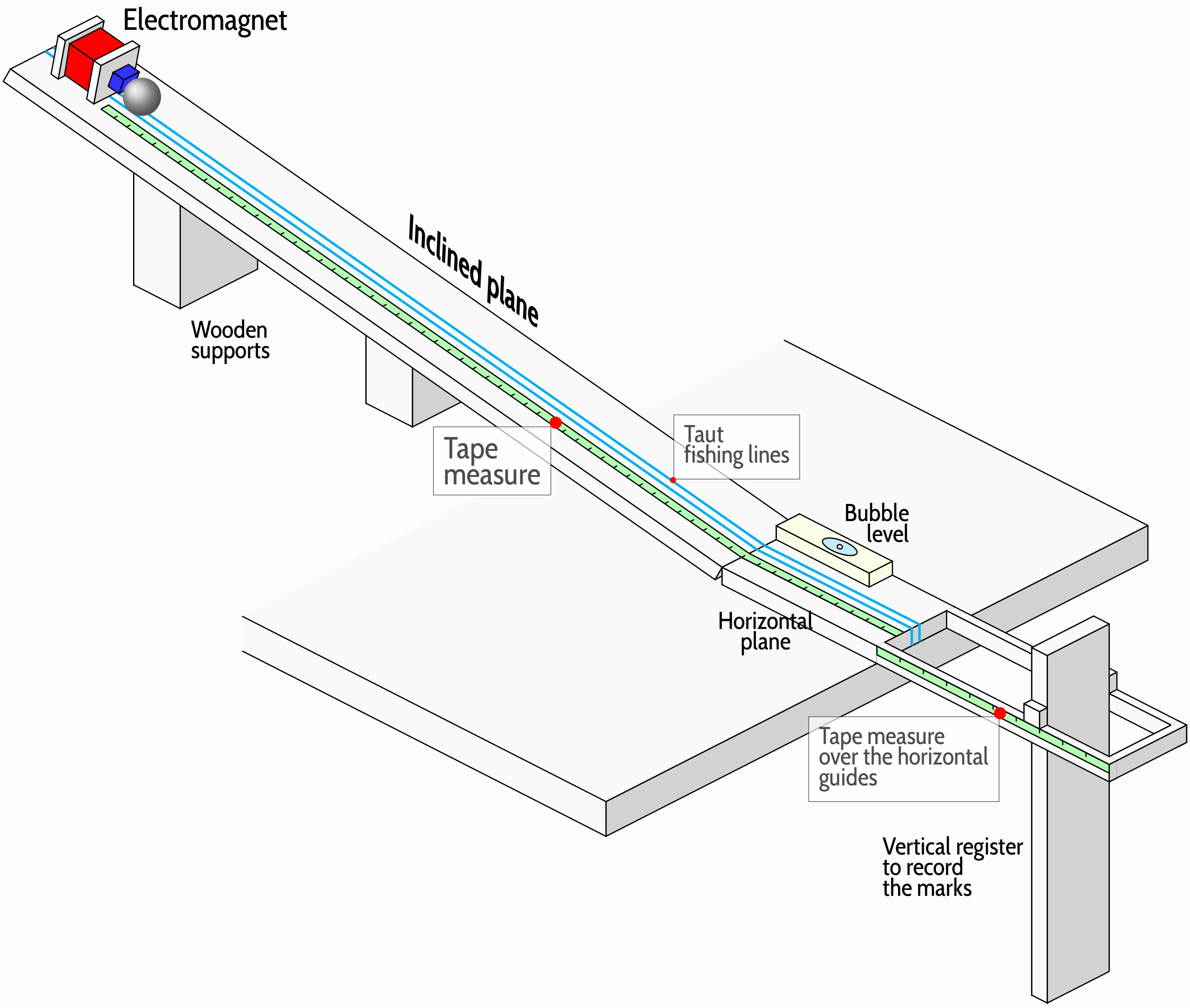


Figure 4.7: – Length of inclined plane = 120.0 cm
– Travel time = 2.06 s
– Acceleration of CM of the sphere = 56.6 cm/s^2

be fastened by means of a not entirely secure jack. The small ball used in this experiment is the same as in the previous record. The impact marks are, in this case, 2 mm in diameter for two reasons: The horizontal component of the velocity is smaller (113.4 cm/s vs. 127.4 cm/s), and, above all, because the carrier ruler is made of aluminium instead of wood.

The zigzagging of the marks along the vertical, which we have faithfully reproduced, has the same cause as in the previous recording.

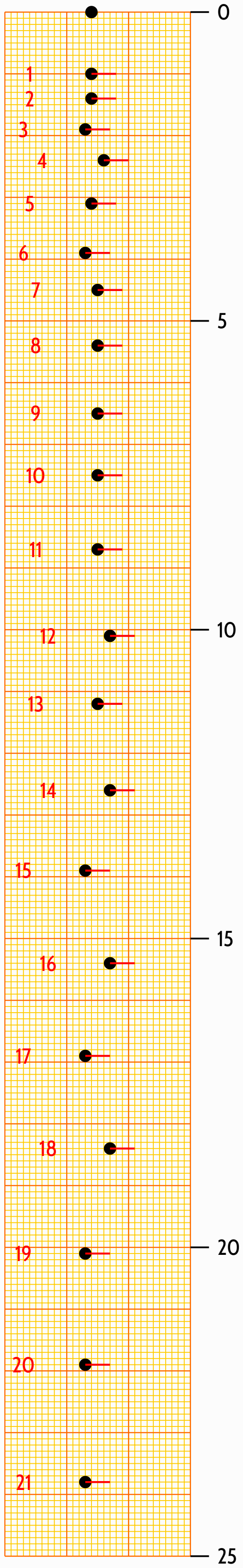


Figure 4.8

#	x cm	y cm	y/x^2
1	5.0	1.0	0.0400
2	6.0	1.4	0.0389
3	7.0	1.9	0.0388
4	8.0	2.4	0.0375
5	9.0	3.1	0.0383
6	10.0	3.9	0.0390
7	11.0	4.5	0.0372
8	12.0	5.4	0.0375
9	13.0	6.5	0.0385
10	14.0	7.5	0.0383
11	15.0	8.7	0.0387
12	16.0	10.1	0.0395
13	17.0	11.2	0.0387
14	18.0	12.6	0.0389
15	19.0	13.9	0.0385
16	20.0	15.4	0.0385
17	21.0	16.9	0.0382
18	22.0	18.4	0.0380
19	23.0	20.1	0.0380
20	24.0	21.9	0.0380
21	25.0	23.8	0.0381

Table 4.8: Vertical register carrier rule

Chapter 5

The folio 116^{v}

Original 2003 text revised in 2005

5.1 Description

In November 1985 I read an article by Pierre Thuillier entitled ‘Galileo and experimentation’ (Thuillier, 1983). Through it I learned that Stillman Drake (Drake, 1973) had published another, in 1973, about the reverse (^{v}) of folio 116 of volume 72 of Galileo’s unpublished manuscripts preserved in the National Library of Florence (folio 116^{v}). On the back of the folio there is a graph and some annotations by Galileo, which are reproduced below:

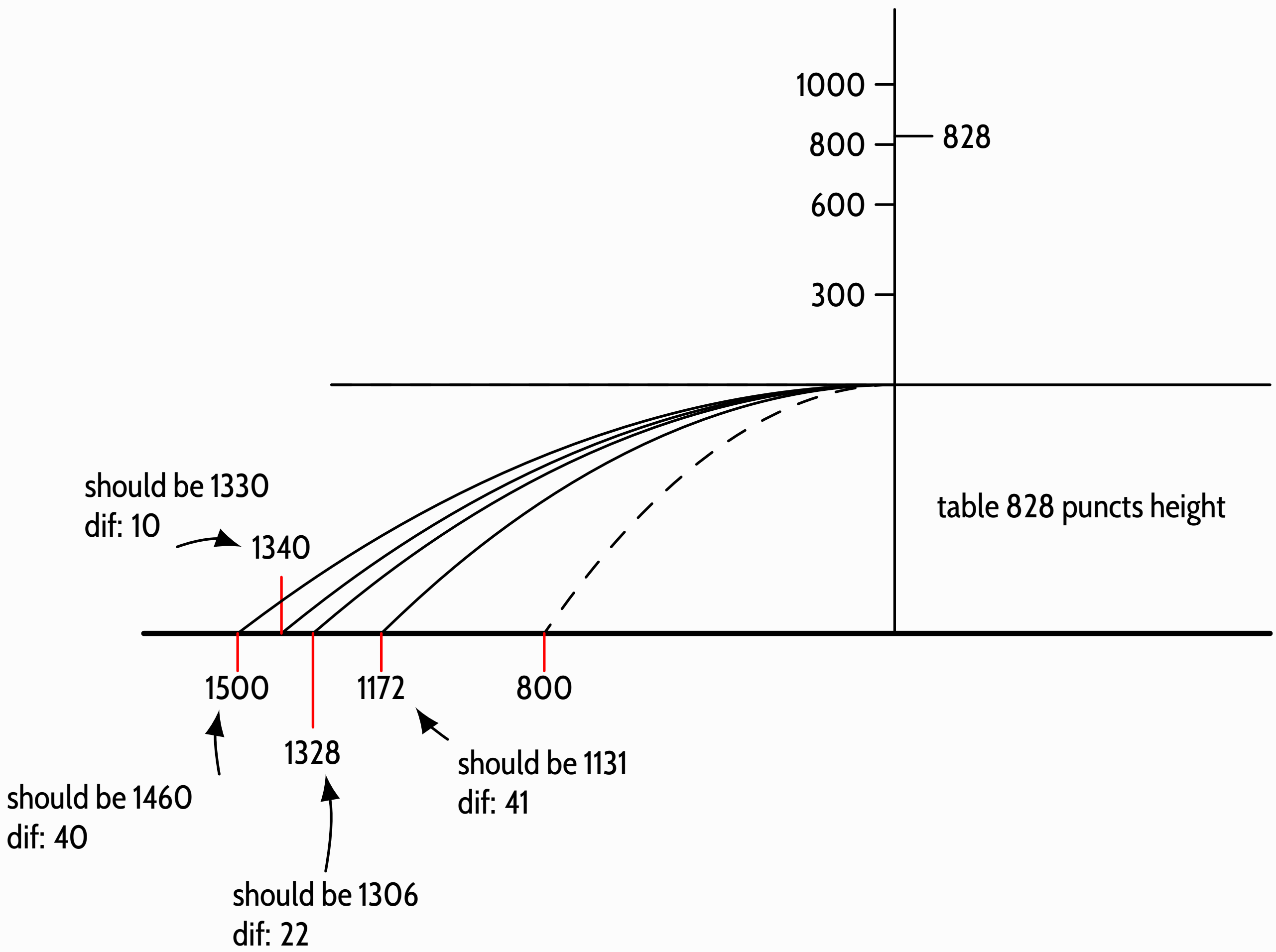


Figure 5.1: Reproduction of the folio 116^v

5.2 Stillman Drake's interpretation

Stillman Drake considers that the curves represent parabolic trajectories described by a ball, which leaves horizontally to the left along the edge of a table, after having been accelerated from rest along an inclined plane provided with a horizontal deflector at its lower end. The notations next to the vertical axis would correspond to the heights h, measured from the table surface, of the points on the inclined plane from which the ball has been allowed to depart in each case. The notations next to the horizontal axis (possibly the ground) would correspond to the distances d, *measured* from the foot of the table, at which the ball would have landed after each of its parabolic flights. The notations preceded by the phrase 'should be' would be the distances d *calculated* by Galileo according to some hypothesis that he would try to submit to empirical verification.

What Galileo would intend by such an experiment would be, according to Drake, to test whether the *velocity* of the sphere, as it leaves the edge of the deflector, remains *constant* as the *horizontal component* of that which will animate the ball throughout each parabolic flight.

According to Drake, Galileo should have already identified, around 1609, the *rolling of a sphere on an inclined plane* and *the free fall of any body in the air* as motions of constant acceleration. This enabled him to deduce that the square of the speed of the sphere as it leaves the edge of the deflector must be directly proportional to h. From that instant, if his hypothesis were correct, the distances d should be directly proportional to that speed and, therefore, the square of d would be directly proportional to h. That is, expressed mathematically:

$$d^2 = \underline{K}\, h \tag{5.1}$$

To determine in advance, *by calculation*, the value of $\underline{K}$ would require, on Galileo's part, knowledge of the value of the acceleration in free fall g and the dynamics of the joint rotation-translation motion that animates the ball along the inclined plane, both knowledge, obviously, beyond his reach. Thus, according to Drake, Galileo used the pair of experimental values ($h = 300\,\mathrm{p}$ and $d = 800\,\mathrm{p}$) – the only one on the graph that is not accompanied by the phrase 'should be' – to obtain *one* value of $\underline{K}$:

$$\underline{K} = \frac{800^2}{300} = 2\,133\,\text{p}$$

from which he calculated the others ('should be') to compare them with the supposedly experimental ones. It is significant that Galileo noted the difference in each case, apart from the fact that the relevant calculations appear in the same folio.

(The unit of length, the *dot punctus* used by Galileo, we can express it in centimetres since, according to Thuillier, 180 p equals 169 mm)[1]

5.3 The physical meaning of the constant $\underline{K}$

In Thuillier's article there is no development of any physical-mathematical argument in favour of this interpretation. I do not know if Stillman Drake makes any in his original article, since I have not been able to get hold of it, and I would very much like to consult it.

I suppose that anyone interested in this subject may have thought of the arguments I am going to present below, and I do not rule out the possibility that this may have been the case. But what is certain is that they have been appearing to me as I have gone deeper into the subject, and I believe – that is why

[1] **Editor's Note:** On measurements in the time of Galileo, including the *punctus*, see (Caffarelli, 2009). On Galileo's laboratory in Florence see (Teichmann, 2015)

I am going to present them – that such arguments favour the interpretation that Stillman Drake offers us of the chart in the folio 116v.

To interpret *physically* the meaning of $\underline{K}$ we will start from a couple of assumptions:

a) That the rolling of the sphere along its *entire* length is *pure*, that is, that there is no slippage of the point of contact between the sphere and its material support.

b) Since we lack data about the *shape* and *dimensions* of the *deflector*, we will assume that it is *an ideal agent that will not influence the value of the velocity of the centre of mass* (CM) *of the sphere, limiting itself to modify its original spatial orientation – the one imposed by the inclination of the plane – until it becomes horizontal*

Therefore, by applying our present knowledge about the *value of g and the simultaneous rotation/translation motion of the small ball in its pure frictionless rolling*, we will come to the following conclusions:

The acceleration of the sphere's CM along the inclined plane will be given by:

$$a_{\mathrm{CM}} = \frac{5}{7} g \sin \alpha$$

being α the **angle of the plane with the horizontal**.

On the other hand the instantaneous speed of the sphere CM when reaching the deflector will be [2]:

$$v^2 = \frac{2\,a\,h}{\sin \alpha}$$

[2] **Editor's Note:** In these equations H is the free flight height or table height and h the slope of the starting point in the roll, the height from which the ball starts rolling on the inclined plane plank. These can be best seen in Figure 6.2

h is the **difference in level from the starting point**.

The time of flight of the small ball (the same in all parabolas) will be given by:

$$t^2 = \frac{2H}{g}$$

H is the table height.

Since the distance d travelled horizontally in each flight *must be able to be calculated* by:

$$d = v\,t$$

according to the hypothesis which, according to Drake, *Galileo intended to prove by his experience*, we have everything ready to reach:

$$d^2 = \frac{20}{7} H\,h \tag{5.2}$$

from which it follows that the *physical interpretation* of the constant $\underline{K}$ in the Equqation 5.1– we will call it now K – we will see why- is as follows:

> *'Twenty-sevenths of the* H *value of the vertical free fall of the sphere's* CM'

that is:

$$K = \frac{20}{7} H \tag{5.3}$$

In the case of the folio 116^{v} its numeric value '*should be*':

$$K = \frac{20 \times 828}{7} = 2\,366\,\mathrm{p}$$

Since we have made the *deduction and calculation* of K without taking into account the *dissipation of energy* that occurs in the process we have to conclude that the *experimental* value $\underline{K} < K$, found and used by Galileo *in his calculations*, takes into account – *ignoring it himself of course* – the unavoidable presence of such dissipation. In other words: **The values consigned by Galileo are experimental without any doubt**[3]. The graph in the folio reflects an experience not only "imagined" – in the Koyrésian sense of the word – but also "carried out" with his own hands by Galileo himself.

5.4 Evaluation of dissipated energy

We will now show that *the percentage* χ of *the initial energy dissipated* in the processes described in the folio 116^{v} will be given by:

$$\chi = 100\left(1 - \frac{\underline{K}}{K}\right) \tag{5.4}$$

Let's see: Taking the surface of the table as a reference, the *mechanical energy available at the beginning of each roll* is given by the equation:

$$E_p = mgh$$

[3] **Editor's Note:** Editor's boldface

and the one that will finally be *conserved as kinetic at the beginning of the parabolic flight* will be given by:

$$E_{c_T} = \frac{7}{10} m v^2$$

which is the same as *must be conserved* as kinetic – derived from v (*the horizontal component of the velocity during flight*) – when the sphere touches the ground. We can overlook the energy dissipated by friction with the air during the short flight.

The quotient:

$$\frac{E_{c_T}}{E_p} = \frac{7 m v^2}{10 mgh}$$

represents the *so much for one of energy lost*, and it is easily reduced to[4]:

$$\frac{E_{c_T}}{E_p} = \frac{d^2}{h} \times \frac{7}{20H} = \frac{d^2/h}{20H/7h}$$

or

$$\frac{E_{c_T}}{E_p} = \frac{\underline{K}}{K}$$

From here to the equation 5.4 is immediate.

Table 5.1 shows the percentages of energy dissipated in each of the five processes recorded by Galileo in the folio 116^{v}. It is significant enough, in favour of Drake's thesis, that all the quotients $\underline{K}/K$ turn out to be less than unity.

Galileo could not have *invented* such realistic data, from which current analysis can deduce and calculate the percentage of energy dissipated in each of

[4] **Editor's Note:** v^2 as a function of the horizontal distance traveled in free flight is taken from A.13 and we divide by mgh which is the starting K. A more detailed deduction has been added in the Appendix A.

h p	d p	$\underline{K}$	$\underline{K}/K$	χ % dissipated
300	800	2 133	0.901	9.8
600	1 172	2 289	0.967	3.2
800	1 328	2 204	0.932	6.8
828	1 340	2 168	0.916	8.3
1 000	1 500	2 250	0.951	4.9

Table 5.1: Dissipated energies in Galileo's experiment of the folio 116v

the rolling of the sphere.[5]

[5] **Editor's Note:** Bolded by the editor

5.5 The inclination of the plane

According to Thuillier, in 1975 Stillman Drake repeated the experiment suggested in the folio 116v and published a new paper (Drake and MacLachlan, 1975) in collaboration with James McLachlan. He sought with it to improve on the demonstration made in the previous one. Of this new article Thuillier says [6]:

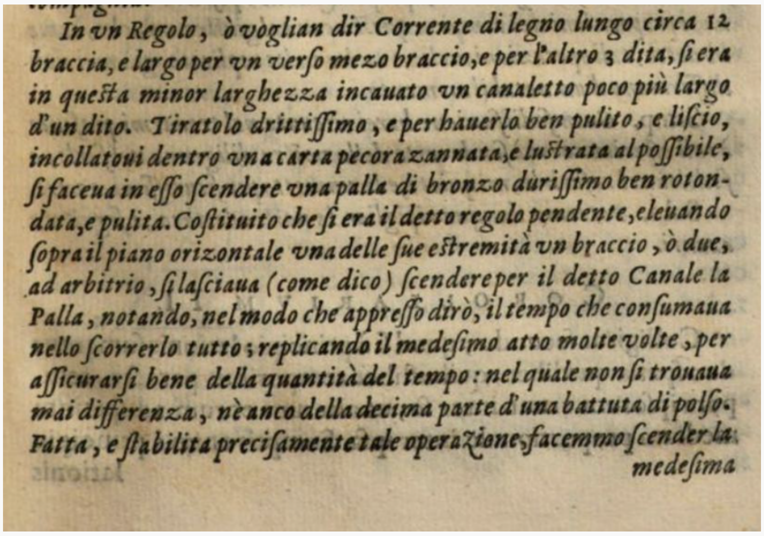
> ‘When we first analysed Galileo’s data in 1972, one of us (Drake) believed the inclined plane Galileo used for the experiment recorded in f. 116 was probably tilted at an angle of 64 degrees to the table, the steepest angle that could be accounted for. Now, however, we believe that, for that experiment as well as for the continuation of the work we shall discuss below, Galileo employed a plane at an angle of only 30 degrees to the table. A plane at that angle is easy to set up with considerable precision, and it also lends itself to easy computation.’

With respect to the content of this paragraph, the following should be noted:

The inclination of the plane is unknown, since Galileo does not record it in the folio 116v. But, moreover, *any inclination is valid in principle*, provided that the plane is sufficiently *long* to allow the experimenter to reach a *maximum level* of 1 000 p on the table, which is precisely *the greatest* of all those tested by Galileo in the experiment described. I do not know the reason why Drake and McLachlan began to test with an inclination of 64° and the criterion followed to stop and *give for good* that of 30°.

[6] **Editor’s Note:** The original article is quoted here (Drake and MacLachlan, 1975)

I copy below an excerpt from the above-mentioned and much-discussed passage from the '*Discorsi*' (Galilei, 1981) where Galileo himself describes the plane *he claims to have used in his alleged experiments:* [7]


In vn Regolo, ò voglian dir Corrente di legno lungo circa 12 braccia, e largo per vn verſo mezo braccio, e per l'altro 3 dita, ſi era in queſta minor larghezza incauato vn canaletto poco più largo d'un dito. Tiratolo drittiſſimo, e per hauerlo ben pulito, e liſcio, incollatoui dentro vna carta pecora zannata, e luſtrata al poſſibile, ſi faceua in eſſo ſcendere vna palla di bronzo duriſſimo ben rotondata, e pulita. Coſtituito che ſi era il detto regolo pendente, eleuando ſopra il piano orizontale vna delle ſue eſtremità vn braccio, ò due, ad arbitrio, ſi laſciaua (come dico) ſcendere per il detto Canale la Palla, notando, nel modo che appreſſo dirò, il tempo che conſumaua nello ſcorrerlo tutto; replicando il medeſimo atto molte volte, per aſſicurarſi bene della quantità del tempo: nel quale non ſi trouaua mai differenza, nè anco della decima parte d'una battuta di polſo. Fatta, e ſtabilita preciſamente tale operazione, facemmo ſcender la
medeſima

A piece of wooden moulding or scantling, about 12 cubits long, half a cubit wide, and three finger-breadths thick, was taken; on its edge was cut a channel a little more than one finger in breadth; having made this groove very straight, smooth, and polished, and having lined it with parchment, also as smooth and polished as possible, we rolled along it a hard, smooth, and very round bronze ball.
Having placed this board in a sloping position, by lifting one end some one or two cubits above the other, we rolled the ball, as I was just saying, along the channel, noting, in a manner presently to be described, the time required to make the descent.

If we give credit to the author of this description, the inclination of its plane must have oscillated between 4.8° and 9.6° – '*as it would seem*' – and the ball should maintain *one single contact zone* with the channel, because, if not,

[7] **Editor's Note:** Quoted from (Galilei, 2017) original page scanned by Google (Galilei, 1638)

it would make no sense '*to place a parchment paper polished to the maximum inside of it*'

In Figure 5.2 appears faithfully represented (to scale) the plane described by Galileo endowed with *precisely of an inclination of two elbows*. Arranged in this way Galileo would have the plane *justly* necessary to take the data cited in the folio 116^v. Why not give credit to the descriptions that the author himself makes?

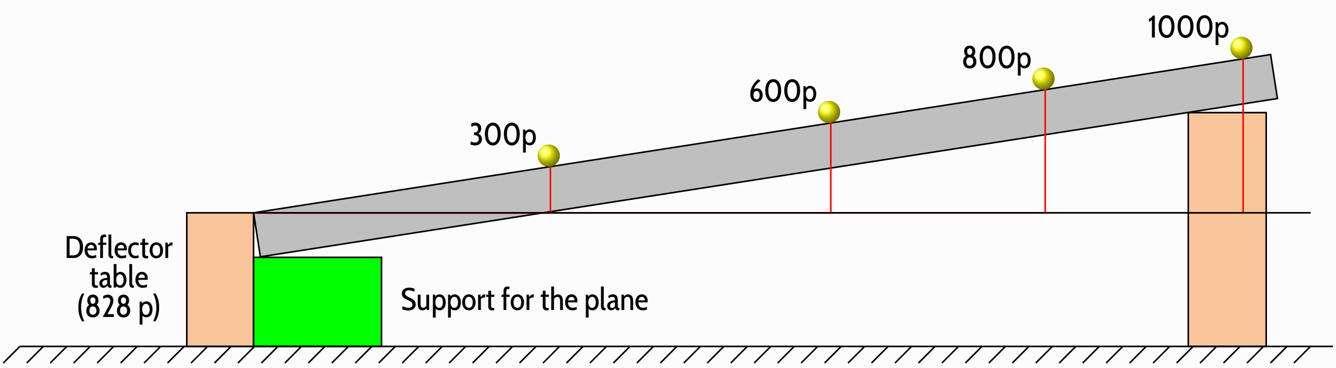


Figure 5.2: Faithful reproduction of the folio 116^v experiment. The *Florentine cubit* , probably used by Galileo, says Alexandre Koyré (Koyré, 1973) in page 295, is equal to 50.8 cm [a]

[a] **Editor's Note:** *La coudée florentine, utilisée sans doute par Galilée, contient 20 pouces, c'est-à-dire 1 pied et 8 pouces, et le pied florentin est égal eu pied romain, qui est égal à 29,57 cm.*
The Florentine cubit, probably used by Galileo, contains 20 inches, i.e. 1 foot and 8 inches, and the Florentine foot is equal to the Roman foot, which is equal to 29.57 cm.

5.6 A 1979 experience

The analysis of the data from the experience I carried out between November 17th and 20th, 1979, could perhaps help us to support what we have just stated.

The *horizontal records* obtained in this experiment appear in Chapter 4 (p. 87) dedicated to 'The parabolic trajectories'. If such data are consulted there we find an unintentional replica of the folio. And I qualify it as '*unintentional*' because Thuillier's article would still take six years to reach my hands, although Stillman Drake's – which I still do not know – had been published six years earlier.

It should be noted that the deflector used in these experiments was 38.3 cm long, so we can consider it responsible for part of the dissipated energy. In addition, the accelerating inclined plane formed an angle of $3.32°$ with the horizontal.

The heights h were not *measured* originally, but they have been calculated from the data given in the cited chapter.

The constant K is:

$$K = \frac{20 \times 95.8}{7} = 273.7 \text{ cm}$$

The $\underline{K}$ values, obtained from d and h, range from 236.6 to 232.7 cm, so the percentages of energy dissipated are between $14\,\%$ and $15\,\%$. If we discount the one we can attribute to the long horizontal deflector the results do not differ too much from the one we have calculated for the folio 116^v experience. Another factor to be taken into account is that the angle α in this experience

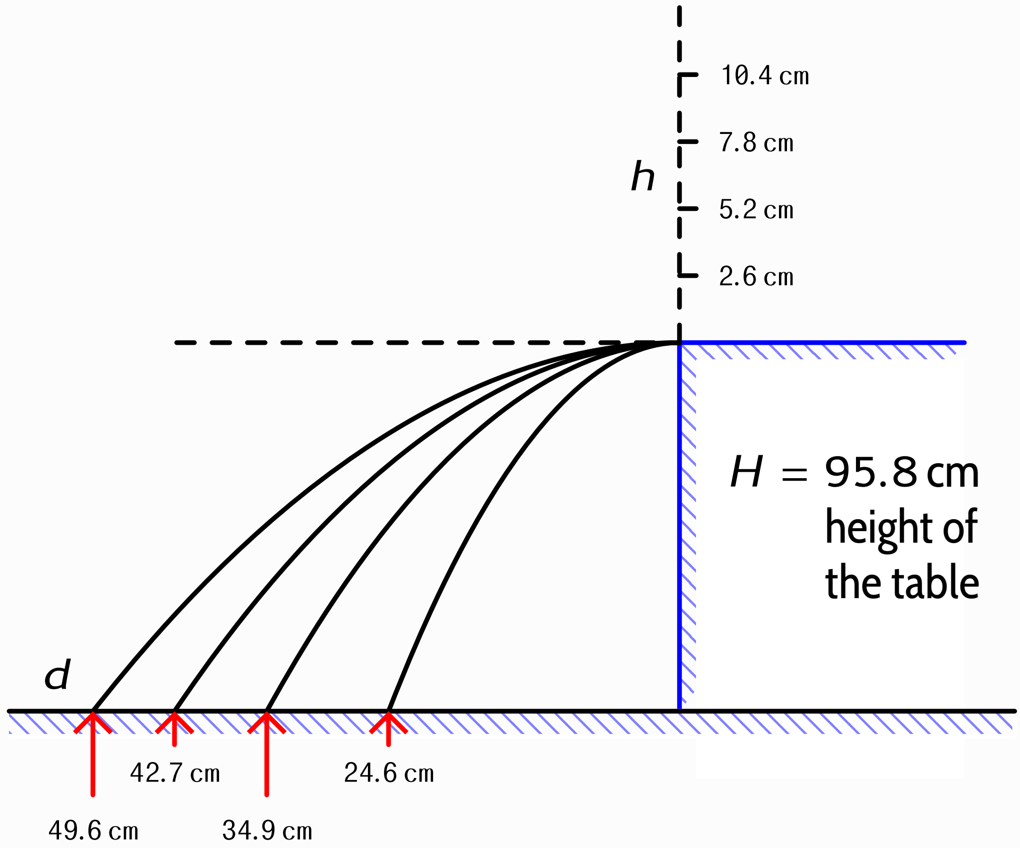


Figure 5.3: Horizontal records made by Galileo

is $3.32°$, and not $9.6°$ which is the one we attribute to Galileo's, and we have already shown in Chapter 2 (p. 48) that between the *angle of inclination* α and the *energy yield* χ there is a quantifiable relation.

5.7 To conclude

Thuillier's article came to my hands in 1985 through Juan Falgueras, leader of the group of students that during the summer of 1977 worked on the problem of parabolic trajectories. Juan Falgueras, who was my best student in those years, ended up graduating in Physics and currently works as a university professor. Reading the article moved me at the time because of the parallelism between Galileo's experience, as interpreted by Drake, and the stimulating graded work that I designed for my groups of students during the 1979/80 academic year, work that I describe briefly in Chapter 4 (p. 87) entitled 'Parabolic Trajectories'

At the end of 2003, when I was writing precisely Chapter 4, Thuillier's article came back to my memory and I went deeper into its contents and found some of the arguments that I present in this one. But it has been during the last few days (I write these lines on March 7, 2005) that I have reached, in my opinion, to the bottom of the question. It is like the culmination of a curious 'puzzle' in which the pieces have been arranging themselves over the years.

My sympathy for Galileo began in 1957, when I was a 6th grade student and our Religion teacher defended in class the Church's position in the Galileo case using the arguments of Bertrand L. Conway, CSP. (Conway, 1957) Such sympathy has only increased over the years. I was therefore very much struck by the enthusiasm put forth by Stillman Drake in writing his biography (Galilei, 1981) (pp. 160–161) and in examining these unpublished folios for evidence of Galileo's side as an experimenter, so discredited by Alexandre Koyré.

So I also had great fun, having just read Thuillier's article in 1985, reworking James McLachlan's ethyl experience – who in turn reworked the one described by Galileo (Koyré, 1973) (p. 252) – willingly sacrificing in it a bottle of red wine...

> *'Having taken a glass globe which had a mouth of about the same diameter as a straw, I filled it with water and turned it mouth downwards; nevertheless, the water, although quite heavy and prone to descend, and the air, which is very light and disposed to rise through the water, refused, the one to descend and the other to ascend through the opening, but both remained stubborn and defiant. On the other hand, as soon as I apply to this opening a glass of red wine, which is almost inappreciably lighter than water, red streaks are immediately observed to ascend slowly through the water while the water with equal slowness descends through the wine without mixing, until finally the globe is completely filled with wine and the water has all gone down into the vessel below....'*

Salviati is perplexed by this account, so Alexandre Koyré (Koyré, 1973) (p. 252) takes the opportunity to launch into an attack by saying:

> *'I confess that I share Salviati's perplexity. It is indeed difficult to propose an explanation of the surprising experiment he has just referred to. All the more so because if it were done again as he describes it, we would see the wine go up into the glass vessel (filled with water) and the water go down into the glass (filled with wine); but we would not see the water and the wine purely and simply replace each other: we would see a mixture take place.'*

Koyré wonders:

> *'What is there to conclude: must we admit that the (red) wines of the 17th century possessed qualities that today's wines no longer possess (...)?'*

Or may we suppose that Galileo, who no doubt had never put water in his wine – wine was for him 'the reincarnation of sunlight' – never made this experiment, but, having heard of it, reconstituted it in his imagination, admitting as something indubitable the essential and total incompatibility of water and wine? For my part I believe that the latter supposition is the good one.

What is certain is that the experiences – both McLachlan's and my own – *ratify the veracity of Galileo's account*, thus proving that Koyré, in his inordinate eagerness to make Galileo a pure Platonist, did not take the trouble to verify the veracity of Galileo's account, did not take the trouble to verify the account himself but, with excessive glee, came to the conclusion that Galileo had done nothing more than to '*present us with experiments that he had merely imagined...*'

Perhaps from the success I obtained in remaking the ethyl experience comes the question I ask myself above: '*Why not credit the author's own descriptions?*' That, no doubt, is what has led me to check whether the *description* and the *inclination* of the plane outlined by Galileo in the '*Discorsi*' are consistent with the data contained in the chart on folio 116^{v}.

The Koyrésian assertion that Galileo's discoveries (fall of objects, parabolic trajectories) have a Platonic root is not incompatible with the fact that he carried out the proving experiments he describes. The main difficulty (lack of means to accurately measure small time intervals) is ingeniously circumvented in the experiment that is condensed in the folio 116^{v}. On the other hand there is the 'heuristic' function – as Thuillier says – of the experiment to '*suggest more or less directly a new theoretical idea*'. I modestly attest to the efficacy of this 'heuristic function of the experiment' to correctly assimilate theoretical ideas even if they are old.

Finally, Thuillier's article cites Pierre Costabel (Bonelli and Shea, 1975) as the author of an argument, published in 1975, *against the experimental character of the folio* 116^{v}. He does not develop it either because '*is*' – he says – '*enough complicated to detail it here*'. Of course I would also love to know that argument of Pierre Costabel.

5.8 Appendix
October 30, 2008

An article by Alexander Hahn (Hahn, 2002) discusses a revision by Stillman Drake, circa 1985, of his first interpretation of the folio 116^{v} in an attempt to improve on the results obtained in the first one. It is not very clear to me what Drake intended by it. To understand it I would have to collect and translate several of the articles cited by Hanh in his, a task that may be ideal for a retiree like the one who writes this.

The revision consisted of considering that the ball used by Galileo measured 20 p in diameter and the plane channel 8.5 p in width, so that the ball would roll while maintaining two points of contact with the plane, cases which I myself have dealt with experimentally with my students. I would try thereby to come closer, by means of a reconstruction *purely mathematical* to Galileo's experimental values.

This new attempt confirms me in the suspicion that nobody has thought of the dissipation of energy, *or that they have considered the influence of this factor negligible beforehand*, assigning to the *frictional force* the task *static and exclusive* of avoiding the sliding and assuring the rotation synchronized with the translation of the ball.

Even without knowing in depth the reasons that impelled S. Drake to make this revision I do not see it in agreement with the description of the plane that Galileo makes in the '*Discorsi*'. Where does Drake get the data of the diameter of the ball? Why would Galileo have to place a '*parchment paper polished to the maximum inside (of the channel)*' if the ball was not going to touch the bottom but to roll leaning on the edges?

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Chapter 6

Folios 114^{v} and 81^{r}

6.1 Introduction

The day I start writing this chapter – January 6, 2007 – is almost two months since my initiation as an Internet user. I have located on the Internet – but I have not been able to access them – the articles that Stillman Drake (alone or in collaboration with J.McLachlan), R.H.Naylor and D.Hill dedicated in their day to these unpublished Galileo's folios. On the other hand, I have located full-size electronic reproductions of the aforementioned folios (Galilei, 1981), as well as two articles – one in Spanish (Álvarez and Posadas, 2003) and one in English (Hahn, 2002) – with abundant information on the contents of the aforementioned folios, which are inaccessible to me for the time being.

Although I was aware of the existence of the folio 114^{v} – a reproduction of it appears in Thuillier's article already cited – the graphic that appears in the folio *anverso* of folio 81^{r} – ^{r} for *recto* or ^{v} for *anverso* – has constituted for me a complete novelty. The initial intention of my Internet search was to satisfy my curiosity as to why Drake and McLachlan approached the reconstruction of the folio 116^{v} experience by choosing for the plane an inclination of 30°, after starting the test with an angle of 64°, but all the material accumulated during these days have fired my curiosity and imagination beyond this modest limit, so I have proposed to elaborate my own opinion about the folios 114^{v} and 81^{r}.

After a long time spent translating from Alexander Hanh's article (Hahn, 2002) – my knowledge of English is very rudimentary – I have come to the conclusion that the researchers involved in this adventure used kinematic and dynamic – and even purely mathematical – arguments to elaborate their own reconstructions, but that they *ignored or overlooked the energetic aspect*. It seems as if for them there were no energy dissipation in rolling, or at least no allusion is made to this subject in Professor Hanh's extensive summary. He himself – a mathematician – obtains in the case of the folio 116^{v} a relation between the height of the table H, those of departure of the ball h and the horizontal range of the flights d identical to that obtained by me in the previous chapter, but he does not rely for it on energetic arguments but on the kinematic and dynamic ones already mentioned. He writes it as:

$$d = 2\sqrt{\frac{5}{7}H}\sqrt{h}$$

and clarifies that with it the values of d would be obtained in some ideal conditions, among which one counts '*a friction force that makes the ball rotate without sliding and without causing additional impediment*'. He apparently does not realize the interesting meaning of the *theoretical constant:*

$$K = \frac{20H}{7} \tag{6.1}$$

that together with his *approximate empirical values:*

$$\underline{K} = \frac{d^2}{h} \tag{6.2}$$

being H, d and h *the data provided by Galileo himself*, allow to calculate the percentage of the energy *forcibly* dissipated, as I have already shown in the previous chapter.

However, he comments that '*alternatively this equation can be established using the law of conservation of energy*' and refers us to an article (Hahn, 2002) (p. 397–400) by W. R. Shea and N. S. Wolf – physicists apparently – who polemicised very early with Drake about the reconstruction of the experience of the folio 116^{v}. I have located this article, but I have not been able to access it either, so I do not know if these authors allude to the dissipative effect.

The article in Spanish (Álvarez and Posadas, 2003) is signed by J. L. Álvarez G. and Y. Posadas V., professors at the Universidad Nacional Autónoma de México. It contains tables with the numerical results of the reconstructions carried out by the aforementioned historians, as well as the ball exit heights h and the angles $alpha$ they gave to their inclined planes.

As for the dissipative effect of the frictional forces on the rolling, neither is alluded to in this text, the authors limiting themselves to making them responsible for the coupled motion of rotation and translation of the ball, commenting that this '*was not a problem for Galileo to be successful...*'. '*Although Galileo was not aware of the influence of these factors, his investigations of an experimental nature resulted in a very small margin of error. At this point we cannot decide whether it was luck or the intuition of a visionary*'

6.2 My starting assumptions

In previous chapters I have quoted at length Galileo's description of his plane, of how he raised one end of it '*to the height of one or two cubits, as it seemed*'[1], and shown that an inclination of two cubits is sufficient – *if the whole length of the plane is taken up* – to reach the maximum h of 1 000 p recorded in folio 116^v. Why would I have to raise it to 64° or 30° as Drake and McLachlan claim? I have the intuition, quite logical on the other hand, that Galileo *obtained his experimental results using the simplest means*. I do not believe that he employed – in the experiments suggested in these folios – angles greater than the 9.5° that can be obtained by elevating at most two cubits (101.6 cm) one of the ends of his twelve-cubit plane (609.6 cm).

On the other hand I am convinced that the *data* recorded in these controversial folios are experimental and not the product of calculations. That is very clear in folio 116^v, in which the *calculated ('should be')* data and the *experimental* values appear well differentiated and compared with each other, but the same is not true in the other two folios.

I will use the equations 6.1 and 6.2 in the discussions that follow to estimate the values of h possibly used by Galileo. The confidence I place in his word – along with those estimated values of h – will allow me to assign values to the angles of inclination α *inexorably*. That is: I will establish an *forced relationship* between both parameters (h, α) by applying the strictest logic to Galileo's own account. Then I will reconstruct theoretically the experiences of the folios 114^v and 81^r, *and only the results obtained can serve us to judge about the plausibility of the story I propose*.

[1] **Editor's Note:** A *cubit* is an ancient unit of length based on the distance from the tip of a person's middle finger to the elbow

Another of my intuitions dictates to me that Galileo was aware of what he was aiming at in designing the experiences of the folios 81^r and 114^v, and that he designed and carried them out to perfection in that of the folio 116^v. In the only experience with the inclined plane that Galileo described in detail (Galilei, 1981) (p. 299) he confirmed the law relating the paths s to the squares of the times t invested in the rolling starting from rest. He then assumed that this law could be extended to the free fall of any body, also starting from rest. A little later he came to the conclusion that in the records of parabolic flights it must be fulfilled that:

$$d^2 \propto h \tag{6.3}$$

or, in other words:

$$v^2 \propto h \tag{6.4}$$

is something that – involving also the principle of inertia and superposition of motions – can reasonably be expected from Galileo's genius. The relation 6.3 he puts it *explicitly* to the test in folio 116^v, being also *implicitly* in 114^v, and the equation 6.4 – where v is the speed of the ball at the instant it reaches the lower edge of the plane – is, to my mind, at the origin of the experience described in folio 81^r.

Drake's view, shared by other scholars, that folios 114^v and 81^r correspond to *failed experiments* because (Hahn, 2002) '*without analytical geometry (Galileo) lacked the (necessary) mathematical tool to obtain success, that being the reason why he never mentions these investigations*', seems to me unfortunate. For my part I am in full agreement with the opinion expressed by Naylor who says thus (ibid.) (pp. 105–134): '*The view that Galileo would roll spheres on inclined planes, compile a list of observations, and then realize his inability to in-*

terpret such information, certainly strikes me as uncharacteristic of the character'.

Anyone who has read the previous chapters will already be aware of my familiarity with the inclined plane and related energy dissipation. All that work I believe qualifies me to venture a plausible interpretation of folios 114^{v} and 81^{r}.

6.3 Folio 114^{v}

Rather, it is a fragment of a folio containing a diagram and some numbers, which we copy in Figure 6.1. The rest of the folio is filled with calculations that shed no light on the enigmatic diagram.

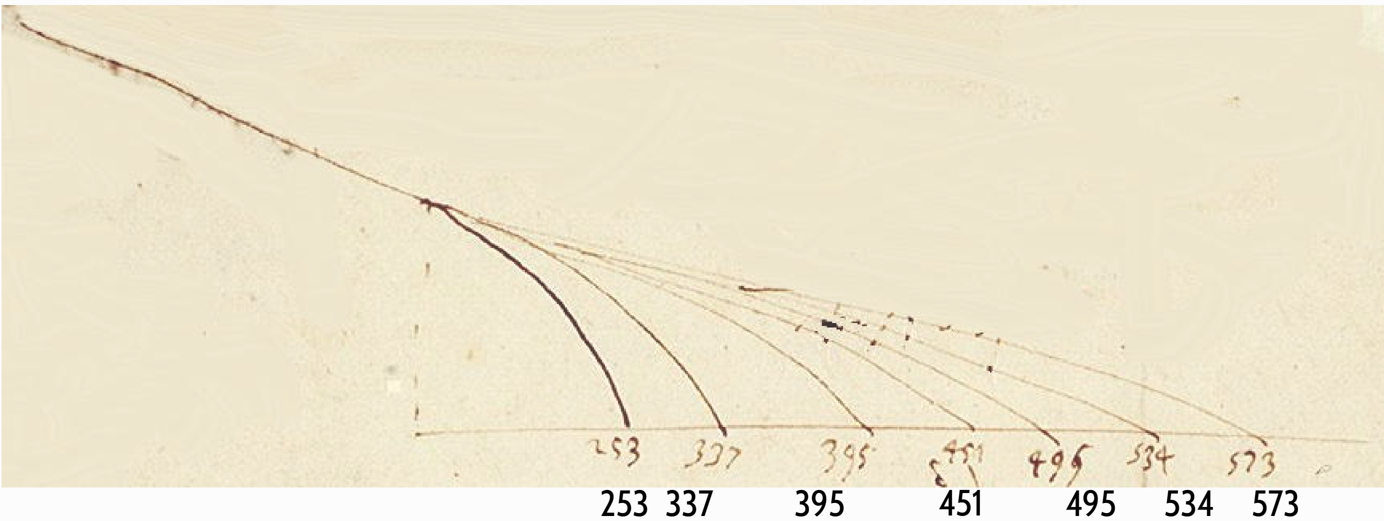

Figure 6.1: Transcription of folio 114^{v}

All the historians cited above agree that we are dealing with a series of parabolic trajectories *obtained without a horizontal deflector*, the numbers at the foot of each parabola representing the respective horizontal ranges d. Galileo does not report the height above the ground H at which the lower edge of the inclined plane is located, nor the heights h from which the ball starts to fly until it touches the ground, nor the angle α that the inclined plane forms with the horizontal....

The mathematical tool needed to tackle this problem of *oblique projection* is detailed below and to capture it well I have drawn Figure 6.2:

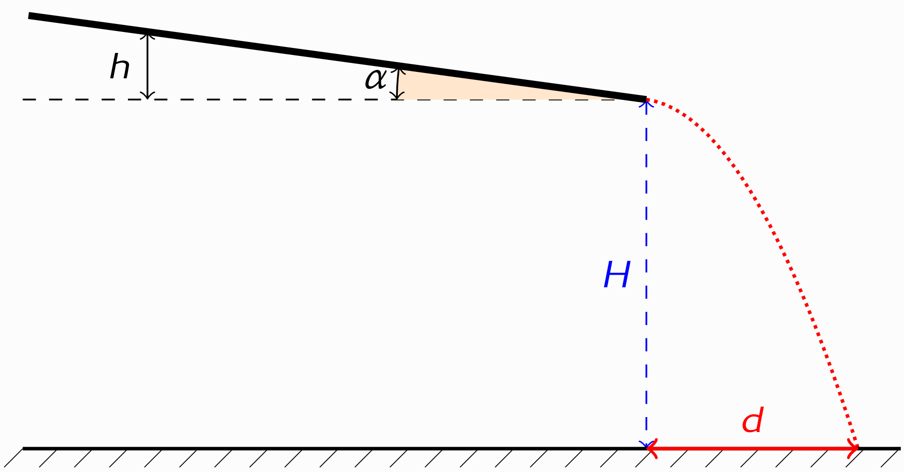

Figure 6.2: Data for the *oblique projection* problem

The equation:

$$v = \sqrt{\frac{10\,gh}{7}} \tag{6.5}$$

where g is the acceleration in free fall, allows us to calculate the speed of the CM of the sphere as it emerges from the lower edge of the inclined plane. *It is obtained by an energy balance on the assumption that the energy is conserved entirely as kinetic*. The equations:

$$d = vt\cos\alpha \tag{6.6}$$

and

$$H = vt\sin\alpha + \frac{1}{2}gt^2 \tag{6.7}$$

relate the horizontal ranges d and distance H to the time of flight t and the angle α formed by the plane with the horizontal. These are simply applications to this case of the parametric equations of the parabola. Combining these equations together we obtain:

$$d = -\frac{5}{7}h\ \sin(2\alpha) + \sqrt{\left[\frac{5}{7}h\ \sin(2\alpha)\right]^2 + \frac{20}{7}hH\ \cos^2\alpha} \tag{6.8}$$

In short: If we want to *ideally reproduce* the d values that appear at the bottom of each of the supposed parabolas in the folio 114^v, we will have to choose terns of *appropriate values* for h, H and α. What criteria should we follow to make such a choice?

Drake used an angle of 26° in the reconstruction he did alone. In a later one, with McLachlan, he increased it to 30°. In both cases I do not know the criteria used to make such choices. Nor do I know why Hill used an angle of 12.5° to make his. I have found no justification for fixing the series of heights h nor for the choice of H in either case. All of them obtain values for d close to those recorded by Galileo... One thing seems clear, and that is that the problem admits *infinite solutions, as infinite are the combinations we can make by assigning values to the three variables handled*.

To work out my own theoretical reconstruction I begin by assuming that $H =$

828 p, what Galileo calls '*height of the table*' in the folio 116v. This assumption follows from two previous ones, namely, that according to the numbering of the folios the experience of the folio 114v must have been *somewhat earlier* in time than that of the folio 116v and that Galileo would have already installed his plane, but still without the attachment of the horizontal deflector.

This assumption justifies that, *in principle*, we assign to the theoretical constant K the same value that corresponded to it in the folio 116v, that is: $K =$ 2 366 p. Now we can venture to the *estimative* calculation of the series of values h *that Galileo could have chosen* to let the ball split, since it must be fulfilled in any case that such h *must be greater* than those obtained by means of the quotients d^2/K being d *the values of the series experimentally recorded* by Galileo in the folio 114v, and K *the theoretical constant* which we have already assumed.

In the experience of the folio 116v Galileo tested h *explicitly* with integer values of h. This authorizes us to round the estimated values by d^2/K up to integers, taking into account the unavoidable energy dissipation that will accompany each rolling. The result of all this is given in Table 6.1: We can estim-

Galileo d	Estimated values d^2/K	Values of h chosen
253	27.0	30.00
337	48.0	50.00
395	65.9	70.00
451	85.9	90.00
495	103.5	110.00
534	120.5	130.00
573	138.7	150.00

Table 6.1: Estimated values of h for folio 116v

ate $\alpha by dividing$h = 150 p by the 6 500 p that the plane measures:

$$\sin\alpha = \frac{150}{6\,500} = 0.02307 \quad \text{which corresponds to:} \quad \alpha = 1.32^\circ$$

We now have everything we need to obtain the *ideal values* of the horizontal displacements d from the equation 6.8. See Table 6.2.

The values thus calculated are *higher than the experimental ones* recorded in the folio 114v, but the differences are more than justified by the unavoidable dissipation of energy along the rolling. The theoretical constant K and the

Calculated values $H = 828$; $\alpha = 1.32^\circ$		Galileo 114v	Differences
h	d	d	Δ
30	265	253	12
50	342	337	5
70	404	395	9
90	458	451	7
110	506	495	11
130	549	534	15
150	589	573	16

Table 6.2: Estimated values and those taken by Galileo

formula for calculating the percentage of energy dissipated in rolling are valid strictly under the conditions (horizontal projection of the ball) under which they were deduced. If I have dared to use them here to make an *estimative* calculation of the values of h it has been on the basis of my conviction – we shall see why – that Galileo must have made use of very small values for the angle α. See that the projection of the sphere in the experience recorded in the folio 114v turns out to be *almost horizontal*.

I venture now to make an estimate of the percentages of energy dissipated. See Table 6.3. They are somewhat lower, but of the same order, than those obtained at the time for the folio 116^v experience. It should be remembered that in the experience we are now analyzing there is no horizontal deflector, which means a saving in the energy dissipated. I am of the opinion, therefore,

h	$K = \underline{d}^2/h$	$\underline{K} = d^2/h$	$\chi = 100\,(1 - \underline{K}/K)$
30	2.341	2.133	8.9
50	2.339	2.271	2.9
70	2.334	2.229	4.5
90	2.329	2.260	3.0
110	2.326	2.227	4.3
130	2.322	2.193	5.6
150	2.319	2.189	5.6

Table 6.3: Galileo's verification of $d^2 \propto h$ in folio 114^v

that Galileo *did not need analytic geometry* to interpret the experience of the folio *114*v.

To test his previous deduction $d^2 \propto h$ *he needed to drive the sphere measurably* h *and for the outgoing velocity to be nearly horizontal* $\alpha \to 0$.

To do this, *disposing of the total length of his plane*, he was able to perform as many as seven trials spaced twenty points apart, for h. In the quotients of the third column of Table 6.3, the numerators d are *measurements made* by Galileo himself, and the denominators h *measurements projected and made* also by himself according to a plan which is revealed to have been consciously drawn. In my opinion this is the most economical solution that can be given to the enigma posed by the folio 114^v.

6.4 Folio 81^r

It is an almost blank sheet of paper on which there are three curves – Figure 6.3 – as well as a Latin phrase, the translation of which we offer in the same figure, and two groups of numbers that are obviously related to the graph.

All scholars of the subject agree that the three curves represent 'horizontal' records, consisting of intercepting the flight of the ball by means of a board, parallel to the ground, placed at different *vertical distances* (ordinates) from the lower edge of the inclined plane, to record on it the *horizontal distances* (abscissae) of some points of the *presumed* parabolic trajectories described in each flight. There is a similarity with the folio 114^v, except that in that one only one point was recorded for each trajectory and the absence of a horizontal deflector was suggested, which is not done here.

In their article (Álvarez and Posadas, 2003) (pp. 62–74), Professors Alvarez and Posadas write: '*About 1600, the Marquis Guidobaldo del Monte suggested to Galileo an experiment capable of revealing the shape of the trajectory followed by objects as they fall after rolling through an inclined plane*'. They further add that: '*In 1603, Galileo attempts to repeat Guidobaldo's experiment*'. They then describe the usual set-up for carrying out the experiment, and *they suppose* that the one in the folio 81^r is aimed at demonstrating that these trajectories are parabolic.

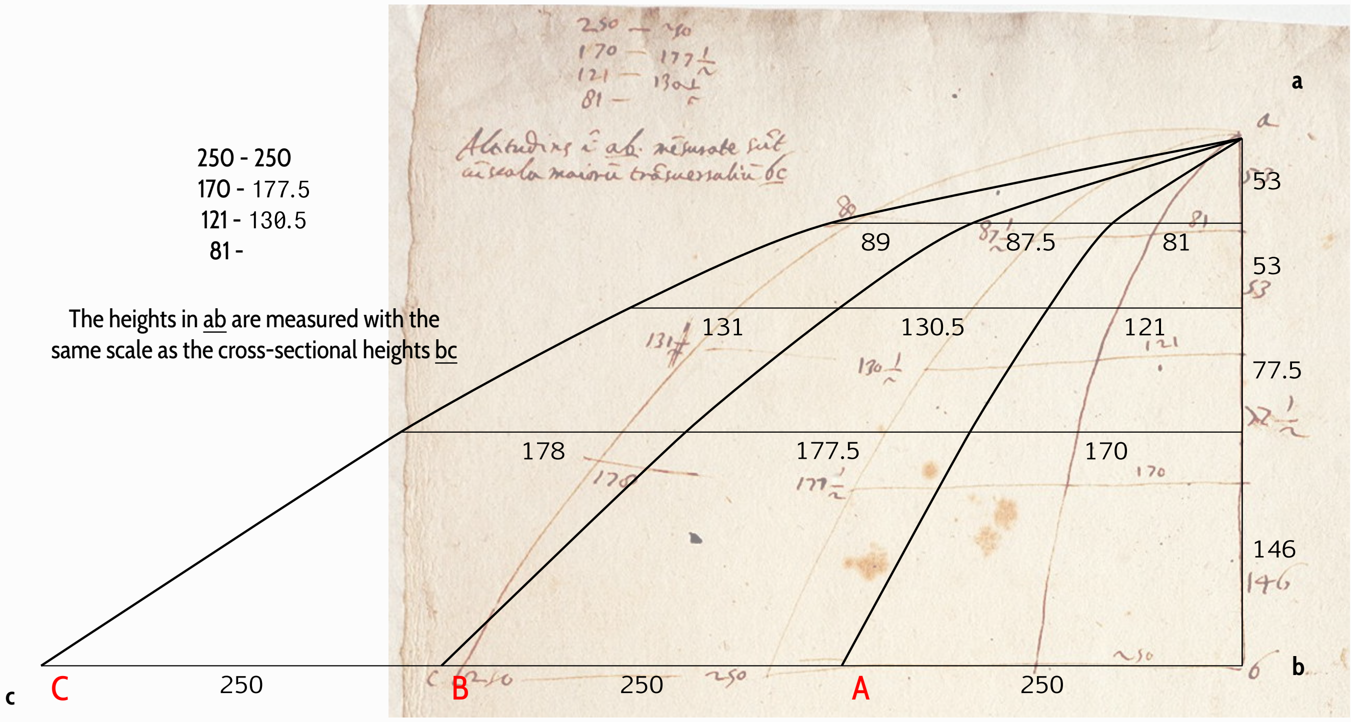

Figure 6.3: Transcription of the folio 81r

6.5 My opinion on folio 81r

I began by assuming that the ball was projected obliquely, but with a small and fixed angle, since no suggestion appears in the figure and in view of the preceding one sitting in the folio 114v. I set out to calculate the *approximate theoretical value* of the constant K considering the distance (a, b) as the value $H = 329.5$ p, resulting to be $K = 941.43$ p.

I took the largest values corresponding to d in the three parabolas to *estimate* those of the heights h from which Galileo should have let the sphere depart in each case. Convinced that Galileo would have chosen the values of h according to some sequence I set out to find out by dividing each estimated h value by the smallest of them all h_A, finding something *not at all surprising*, as recorded in the fourth column of Table 6.4: But this *obviousness* immediately made me suspect that *Galileo was trying to test the validity of the relation*

Galileo 81r	d	$h \geq d^2/K$	h/h_A
Curve (A)	250	66.389	1.0
Curve (B)	500	265.555	4.0
Curve (C)	750	597.495	9.0

Table 6.4: Verification of $d^2 \propto h$ in the folio 81r

between the exit height h *and the speed of the ball* v *when it reaches the bottom edge of the plane, that is, to test whether it holds that*:

$$v^2 \propto h$$

or, in other words, if:

$$v \propto \sqrt{h}$$

and would have chosen for this purpose heights related to each other such that the velocities at the foot of the plane were v, $2v$ *and* $3v$, *which would translate into horizontal ranges* d, $2d$ *and* $3d$ *related in the same way*.

But this would imply that *the time of flight should be the same for all parabolas, i.e., the ball should be projected horizontally...* However, since the quotients y/x^2 *do not remain constant* for each parabola, as would be required in such a case, *they must be, without doubt, oblique projections...* (It is not possible to describe the excitement of suddenly coming face to face with all this seemingly contradictory information.)

Once I calmed down, I *supposed* that Galileo must have started from the heights $h_A = 70\,\text{p}$, $h_B = 280\,\text{p}$ and $h_C = 630\,\text{p}$ *because of the predilection that I attribute to him towards the integers...*

For my part I justified this choice as appropriate to attempt a *theoretical re-*

construction, since it is already evident that part of the initial potential energy will be dissipated in the rolling. But which projection angle to choose?

I thought that if Galileo used the same plane as the one he used in the experiments in the folios 114^{v} and 116^{v}, *applying the criterion of taking advantage of all its length*, we would have that:

$$\sin \alpha = \frac{630}{6\,500} = 0.0969$$

that is:

$$\alpha = 5.56^{\circ}$$

I calculated the values of d, taking the angle of $5.56\,^{\circ}$, and found that those for curve A, *but that those for curves B and C* are nonsensical. I then thought about whether the length of the plane would allow him to perform at least *two trials*, ie:

$$\sin \alpha = \frac{280}{6\,500} = 0.0431$$

so that:

$$\alpha = 2.47^{\circ}$$

By testing with this new angle, I obtained for $\underline{d}$ the values shown in Table 6.5:

The first three points of each curve conform fairly well to what might be expected: The calculated values are higher than the experimental ones, as was to be expected, the differences being between 11 and $22\ \mathrm{p}$, always in favour of the calculated ones, since they have been so on the assumption that no energy is dissipated in rolling. Even the fourth point of curve A meets all the requirements, except that the difference is too small with respect to the previous three.

Height h_e	Theoretical value $\underline{d}$	Galileo d_e	Differences Δ
Curve A $h = 70\,\text{p}; \alpha = 2.47°$			
53.0	98.6	81	17.6
106.0	141.2	121	20.1
183.5	187.1	170	17.1
329.5	252.1	250	2.1
Curve B $h = 280\,\text{p}; \alpha = 2.47°$			
53.0	189.2	168.5	20.7
106.0	274.2	251.5	22.7
183.5	365.9	347.5	18.4
329.5	496.0	500.0	−4.0
Curve C $h = 630\,\text{p}; \alpha = 2.47°$			
53.0	272.2	257.5	14.7
106.0	399.3	382.5	16.8
183.5	536.7	525.5	11.2
329.5	731.6	750.0	−18.4

Table 6.5: Differences in theoretical and Galileo curves in folio 114^v and folio 116^v

As for the fourth point of the other two curves, the differences are negative. This detail, disconcerting in its absurdity in relation to the other data, made me think that of the three notes made by Galileo in the lower transversal only the first – the 250 p corresponding to curve A – *corresponds to an experimental data necessary to design the test*, the other two being *anticipated expressions of the results he expected to obtain if the hypothesis he was submitting to verification turned out to be correct.*

But such a hypothesis cannot be endorsed with complete accuracy by an experience in which the ball is projected obliquely, as it appears to be in this case. Galileo was perhaps perfectly aware of this, but, in my opinion, *he was obliged to look for a procedure that would allow him to reconcile two conflicting needs: to propel the ball in a measurable way* h *and to achieve this without distorting* *the horizontality too much*. The idea of attaching a horizontal deflector *to make the horizontality condition independent of the need to measure* h had not yet occurred to him. It is possible that the idea of the deflector arose immediately after the folio 114^v experience.

Naturally Galileo was ignorant of everything concerning the inevitable dissipation of energy, but this circumstance, *which would affect with some uniformity all his results*, would not prevent him from an approximate confirmation of his hypothesis. *What I find very strange is that the historians and mathematicians who have dealt with this matter during the last thirty years have not involved the dissipation of energy in their reconstructions*.

Another secondary enigma is how Galileo would manage to record the points relative to the curve C, whose coordinates are shown in the diagram, *but are not recorded in the list in the upper left corner of the folio*. I suppose that Galileo would initially design the experiment for the heights h of 70 and 280 p which were accessible to him with his twelve-cubit plane. He must have thought later that '*one swallow does not make a summer*', and it is possible that he decided to hunt a second swallow using the twelve elbows plane as a deflector, without altering the angle of 2.47° achieved, attaching to it at the top another plane *with a higher inclination* that would allow him to reach the necessary 630 p without resorting to an exaggerated length.

If the interpretation that I offer is admitted, the *empty* of the reverse and of the lower half of this folio, *empty* which have been considered by Alexander Hanh as '*proof of Galileo's inability to interpret the curves obtained*', cease to be strange. But – in my opinion – *Galileo did not have to interpret anything*. Perhaps he expected to *check* only whether the experimental results conformed to the expectations created by the conditions he himself had imposed *and empirically bounded with the first test*, that of curve A. And the truth is that the fit is quite good and must have satisfied its author. *This is not, in my opinion, a failed or abandoned experiment, but a success that reveals Galileo's sagacity and skill as an experimenter*.

As the numbering of the folios suggests, the experience embodied in this one must have been prior to those of the other two. The widespread opinion among historians that Galileo was trying to prove with the folio 81^r the suggestion of Guidobaldo del Monte about '*the shape of the trajectory followed by falling bodies after being propelled*', seems to me to be very strange and far-fetched. If that was Galileo's intention he might well have focused on *one single curve and obtained more points from it* instead of recording only *four points from three different curves*.

I mean to suggest by this that the experience of the folio 81^r must have been aimed rather at verifying the hypothesis $v^2 \propto h$ than at proving something *which would turn out to be mathematically true* if his experiments were crowned with success.

6.6 Conclusions

I believe I have offered a consistent interpretation of the experiences contained in folios 81^r and 114^v based on:

a) Galileo's description of his plane

b) The purpose that I attribute to it to take advantage of its full length in each trial

c) The interpretation offered by Stillman Drake of the folio 116^v

d) My own interpretation of the theoretical meaning of the *K* constant

e) My own experimental work on this issue over the years

These pieces from such different backgrounds seem to fit together reasonably well.

The fact that part of the initial gravitational potential energy must be dissipated in the rolling – which seems not to have been taken into account so far – seems to me fundamental to interpret these sheets. An *purely mathematical* interpretation – such as the one made by A. Hanh's interpretation of the data recorded in folio 81^r – rather overshadows than clarifies the intention pursued by Galileo with the design of this experiment.

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Chapter 7

An illustrated story

March 16-19, 2009

Yesterday, March 16, I woke up at half past seven in the morning, as has been usual for me for some time now. With nothing better to do, I thought I would dedicate a few hours to exercise with the drawing program, which has been very neglected during the last few months.

I also decided to tackle a long-delayed project: to make 1/5 scale reproductions of the trajectories drawn by Galileo in the folio 81^r and of those that would have been obtained in the absence of friction, according to the theory I exposed in Chapter 6 (p. 124). After a while I had finished my work, which I now set out in Figure 7.1: As I had time ahead of me, I decided to draw the plane used by Galileo, according to his own description, arranging it with the angle of $2.47°$ that – according to my theoretical reconstruction – Galileo must have given it to obtain the curves that appear in the folio 81^r. I chose the scale 1/50 and I amused myself drawing also to the same scale a human figure of $1.70\,\mathrm{m}$ of stature that should represent Galileo himself.

Why, I asked myself, *not add a reproduction at the foot of the plan at the same scale as the folio 81^r?*

I did... And I got Figure 7.2, which gave me a curious surprise:

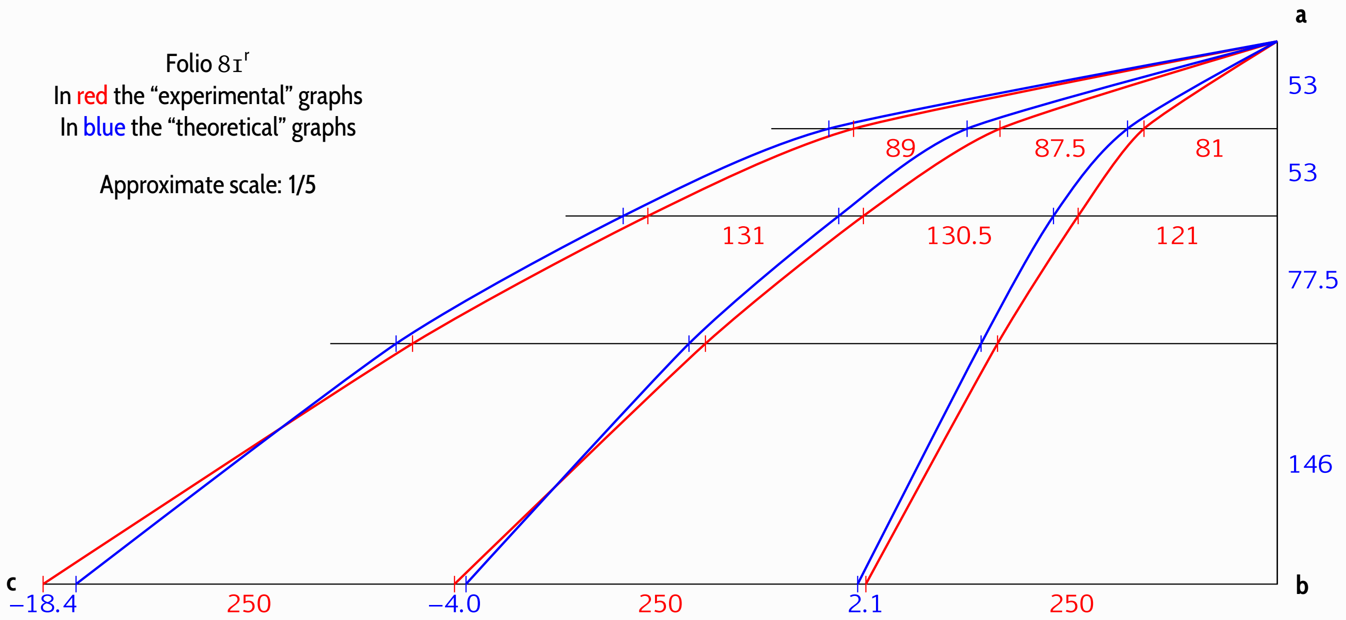

Figure 7.1: Reproduction of folio 81ʳ trajectories

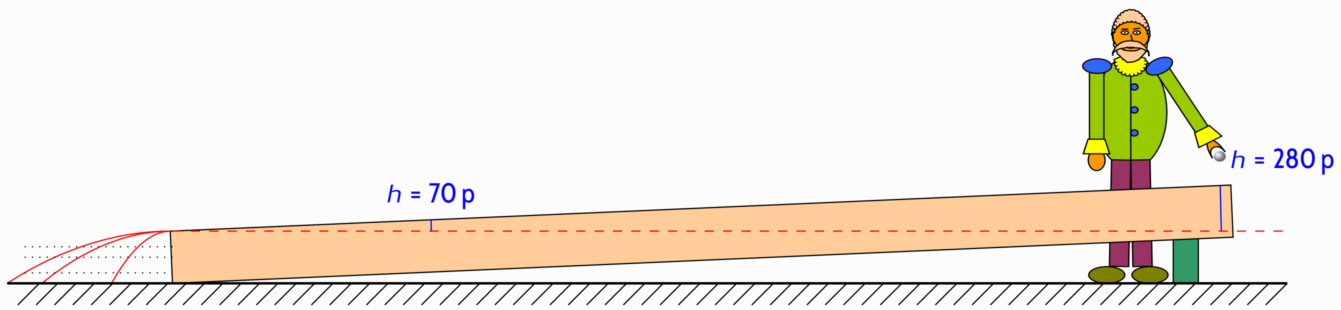

Figure 7.2: Galileo taking the data of the first two parabolas from the folio 81ʳ

> *My God* – I thought – *when Galileo made this experience he most likely had the plane on the ground!*

I remembered that Galileo assigns to his plane '*half a cubit in width more or less*'. I calculated that half a cubit is 272 p as opposed to the 329.5 pmeasured by the vertical axis that appears in the folio 81ʳ. But Galileo himself qualifies his statement with a '*more or less*'. We must bear in mind that this description of the plan was written thirty years after some events that must have taken place between 1603 and 1604, when Galileo was 39 years old and a stout fellow capable of handling – alone or with the help of another – a plank of more than fifty kilos of mass...

It is possible that before taking the data of the folio 81^r^, Galileo had been carrying out – with his plane of little inclination resting on the ground – the experiments that he describes in the '*Discorsi*' to justify *empirically* the relation $s \propto t^2$.

(In an article – which I have already discussed – T. B. Settle describes his own reconstruction of these experiments – the only ones concerning the inclined plane described in some detail in the '*Discorsi*' – showing that it is possible to measure times with a margin of error of less than '*one tenth of a pulse*' with means analogous to those used in his day by Galileo).

By adding a little imagination to the matter we may suppose that after performing this famous and much discussed experiment – and without changing the inclination of the plane – Galileo could go on to check whether it was true that $v \propto h$ – a consequence of the law he had just discovered extended to free fall combined with the principle of inertia and that of superposition of motions – as I have already developed in Chapter 6 (p. 124). *He would take the first experimental datum on the ground* ($id = 250$ p for $h = 70$ p), *and possibly also the other datum* ($d = 500$ p for $h = 280$ p), which would confirm his suspicion. *Then he would resort to thick books in which to support a paper to register the six remaining points of the two more closed parabolas, which, well looked at, also come to confirm his suspicion within admissible limits of error*.

To obtain the data corresponding to the third parabola – the most open – Galileo should have had a plane of about 14 m long to maintain the same angle of inclination of $2.47°$. To obviate this inconvenience he could well have placed a short auxiliary plane coupled to the first one, thus being able to reach the height $h = 630$ p while keeping the first plane as a *non-horizontal* deflector $\alpha = 2.47°$ as shown in Figure 7.3:

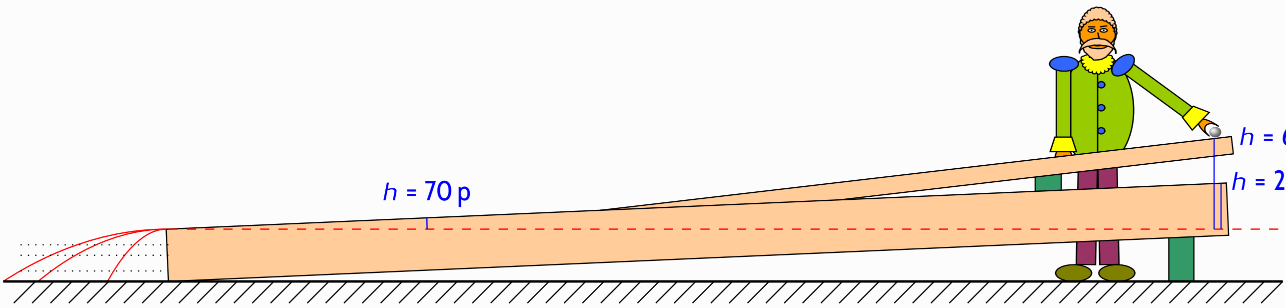

Figure 7.3: How could Galileo have taken the data corresponding to the third parabola of the folio 81^r

> *A factor that has contributed to my confusion over the years is that, being the originals of the three folios* 81^r, 114^v *and* 116^v *of analogous dimensions, the parabolas of the folio* 81^r *appear greatly enlarged in relation to those of the other two. I am ashamed to admit that it was not until I reduced the three to the same 1/50 scale to make the preceding drawings that I realized the new possibilities of interpretation that they offer me.*

I have shown in Chapter 6 (p. 124) that by arranging that same plane with an inclination of $\alpha = 1.32°$ and placing the lower end at a height of 828 p, the parabolas appearing in the folio 114^v can be accounted for as illustrated in Figure 7.4.

Galileo intends to check experimentally whether d^2/h remains constant with a certain approximation – another consequence of the extension of his newly discovered law to free fall combined with the principle of inertia – intuiting that the closer the plane is to the horizontal, the better it should turn out. The 828 p of free fall have not been chosen at random: *this is the height of the*

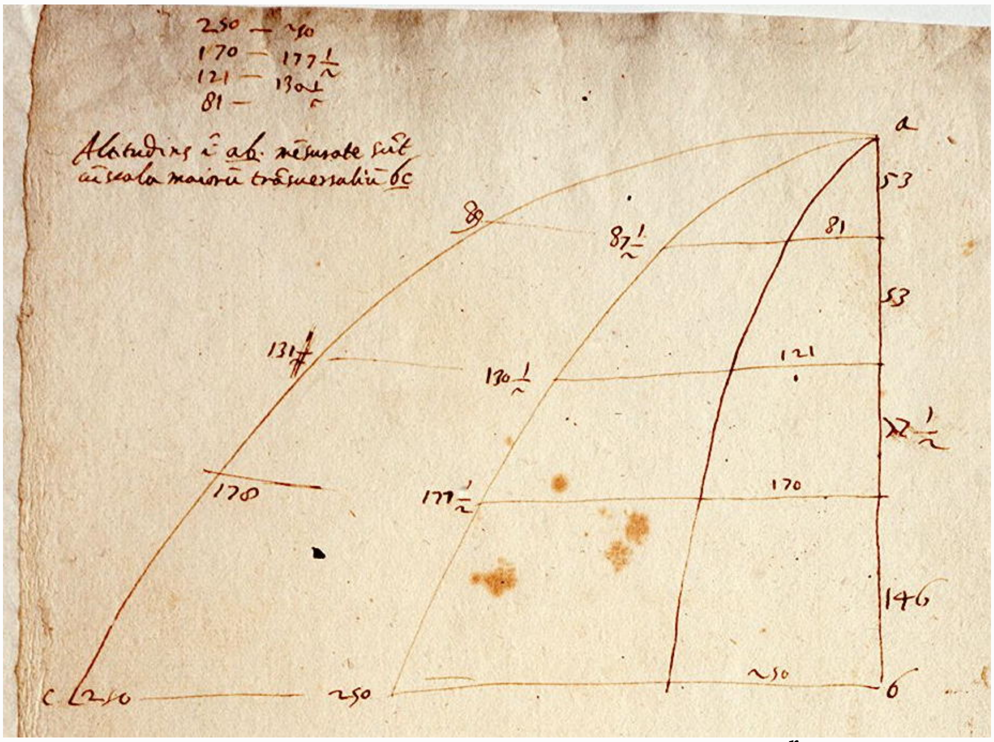

Figure 7.4: Fragment of the folio 81^{r}

table which is to serve as the horizontal deflector in the following experiment contained in the folio 116^{v}.

There is a clear link between the explanations I offer for the folios 81^{r} and 114^{v}: *The plane used is the same in both cases and its entire length is exploited, which inexorably determines the values of* α *in both cases. On the other hand the folios* 114^{v} *and* 116^{v} *are linked to each other, moreover, by the height of the deflecting table.*

Of course, perfection is achieved by Galileo with the experience reflected in the folio 116^{v}. When he adds the *horizontal deflector*, things begin to work better, *because in this series of experiments the time spent in traversing each of the parabolas is the same with total independence of* h. But it is not known which angle Galileo chose to tilt the plane. Again, following his own description, I calculate that it must have been an angle of $9.6°$, which allows *using the same plane along its entire length* and Galileo to place the ball at the highest

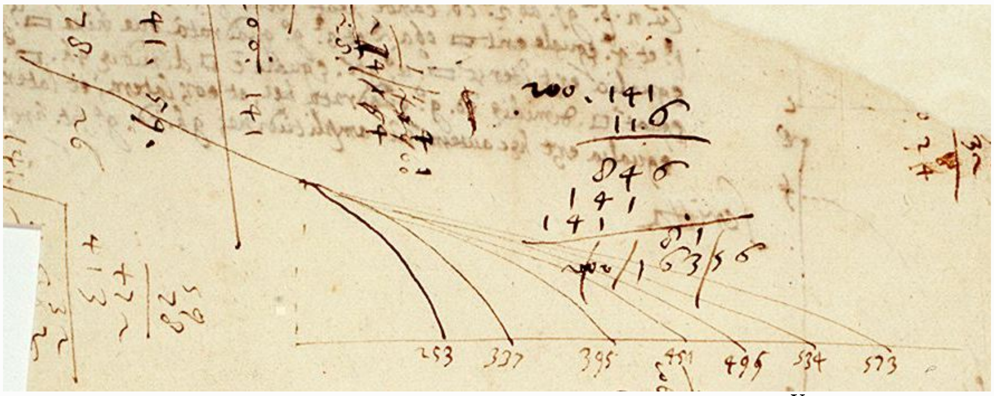

Figure 7.5: Fragment of the folio 114^{v}

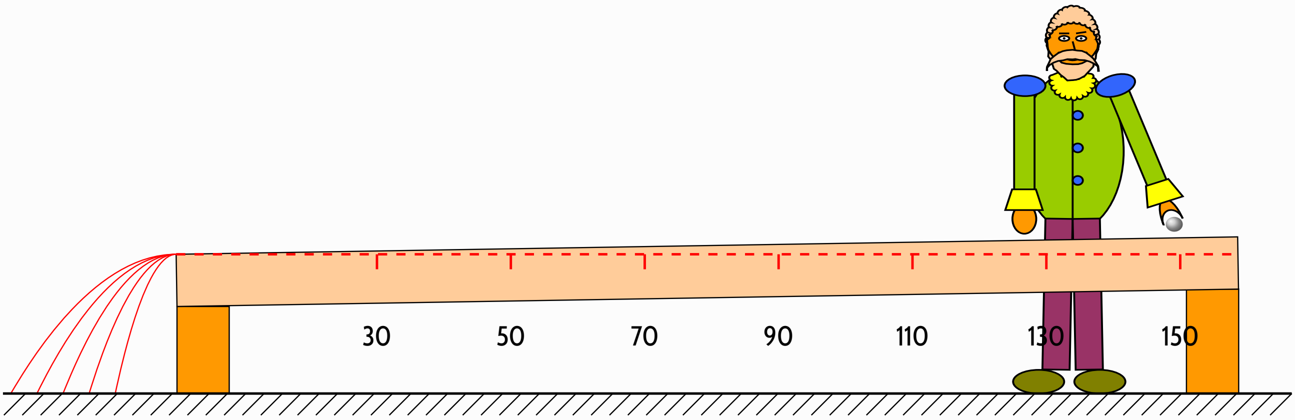

Figure 7.6: Galileo taking the data from the folio 114^{v}

possible point, as shown in Figure 7.5:

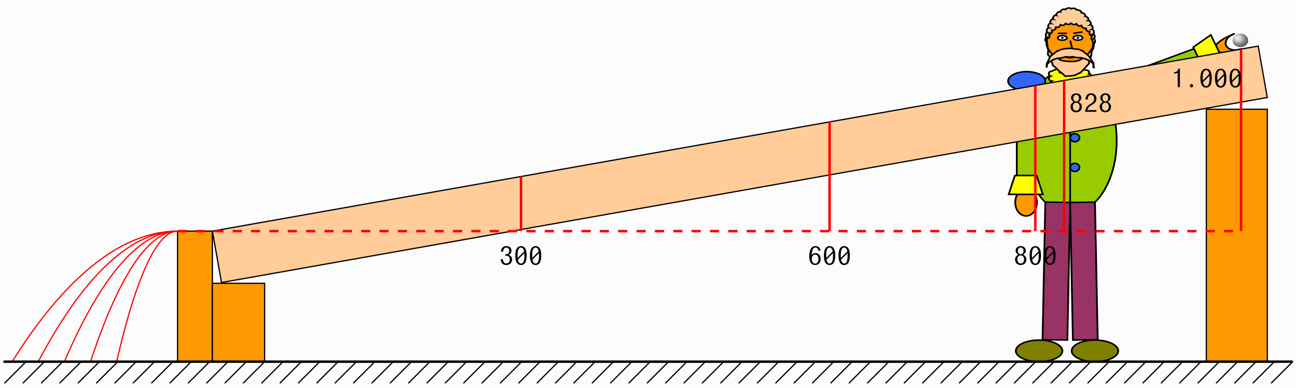

Figure 7.7: Galileo taking the data from the folio 116[v]

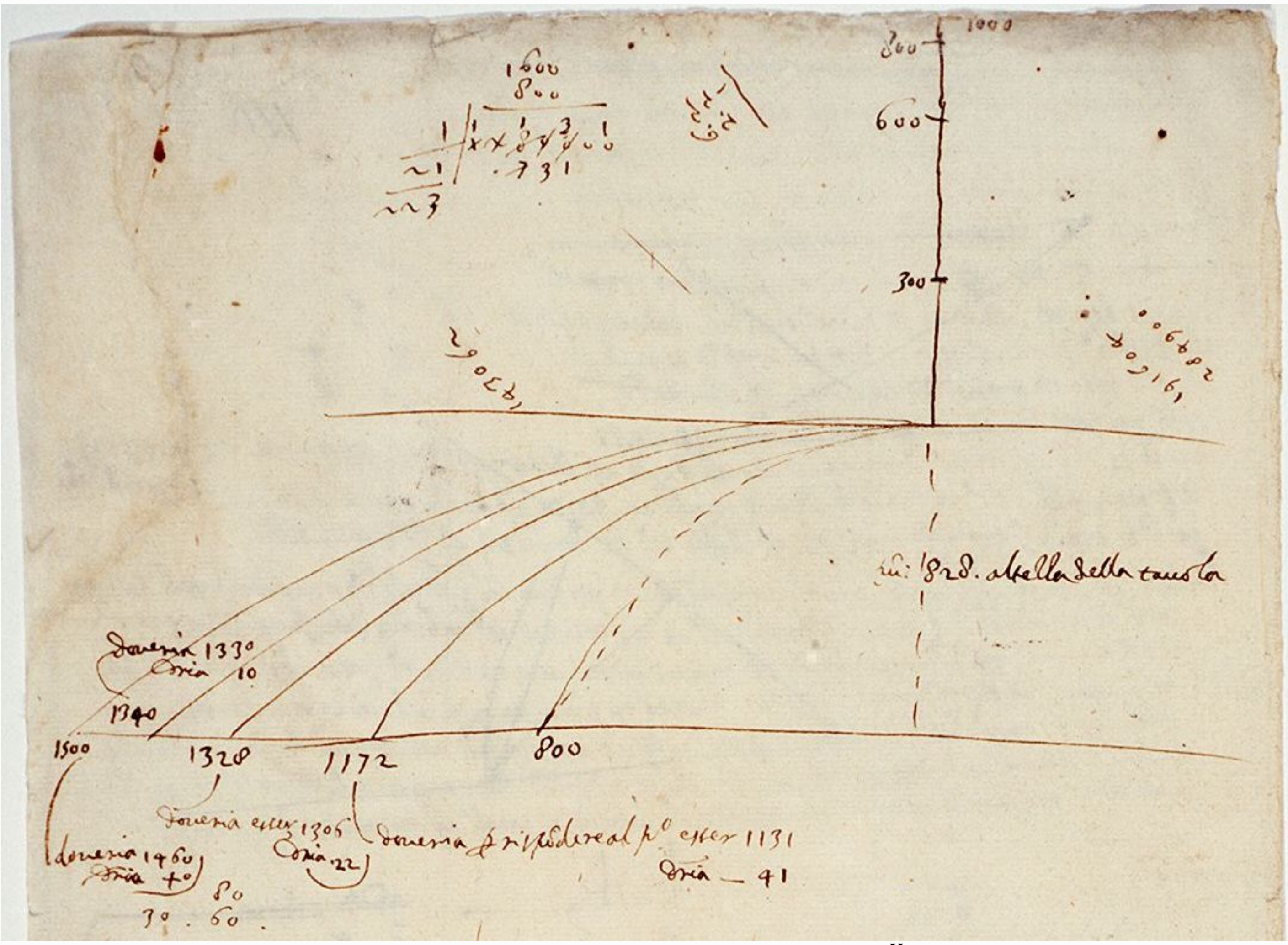

Figure 7.8: Fragment of folio 116[v]

Chapter 8

Latest comments and experiences

8.1 23rd$_{(4)}$ of April 2009

The suspicion stated at the beginning of the preceding chapter about the genesis of the folio 81^{r} has prompted me to deepen the study I began in Chapter 6 (p. 124).

To facilitate the work I have passed to the International System of Units the experimental data recorded at 'points' (p) by Galileo. These data – drop height H and distance travelled horizontally d – are related by the equations 6.6 and 6.7 that allow us to calculate the velocity v_{CM} of the centre of mass of the rolling sphere at the instant when it emerges from the edge of the plane to describe each of the parabolas. Combining these equations yields the following equations:

$$t = \sqrt{\frac{2\,(H - d\,\tan\alpha)}{g}} \tag{8.1}$$

y

$$v_{CM} = \frac{d}{t\,\cos\alpha} \tag{8.2}$$

The results obtained for v_{CM} would allow us to judge whether the value $2.47°$, which I attributed at the time to the angle α, is convincing.

On the other hand the equation 6.5 allows the calculation of the velocity $\underline{v}_{CM}$ that should present at that same instant the centre of mass of the rolling ball in the ideal case of absence of friction. The percentage of energy dissipated in each rolling can be obtained by 1.11.

This percentage can also provide clues to judge the goodness of the interpretation we propose.

8.2 Was Galileo very careful in measuring lengths?

It seems so, since he shows himself able to appreciate up to the 'half a point' in four of the annotations that appear in the folio 81^r, which seems incredible if we take into account that the point is equivalent to 0.94 mm. But my own experience teaches me that I can appreciate up to 'half a millimetre' when locating the centre of the circular track left by a ball on impact, so let's give Galileo a margin of confidence.

But what reference did Galileo use to *measure* the abscissae noted on the aforementioned sheet? It seems to be a vertical axis, but in view of Figures 7.2 and 7.3 of Chapter 7 (pp. 144 and 146) we may wonder whether such an axis would not form an angle of $2.47°$ to the right of the vertical.

In my opinion Galileo designed this experiment to test an idea that I have already stated at the time. We can now imagine that by letting the ball roll from the 70 p of height *annotated* that it came to impact on the ground at 250 p counted from... where? It is possible that he took as reference the point where the inclined plane rested on the ground, with which he would be committing an error *by excess* of about 14 p when writing down the horizontal reach of that impact– *All the abscissae that appear in the folio* 81^r *would be affected by easy-to-calculate errors if we accept this assumption*.

I have already expressed elsewhere my opinion that the abscissa of *that particular point* is *experimental*, but that *those of the other two points recorded on the same horizontal* would not be, rather, it would express Galileo's expectations if the idea he was testing turned out to be correct. All the other recorded points, except those two, would be *experimental* and suitable to proceed to the calculation of v_{CM} by application of the equations 8.1 and 8.2 after the appropriate corrections in d. (See Figure 8.1) Table 8.1 shows the results obtained

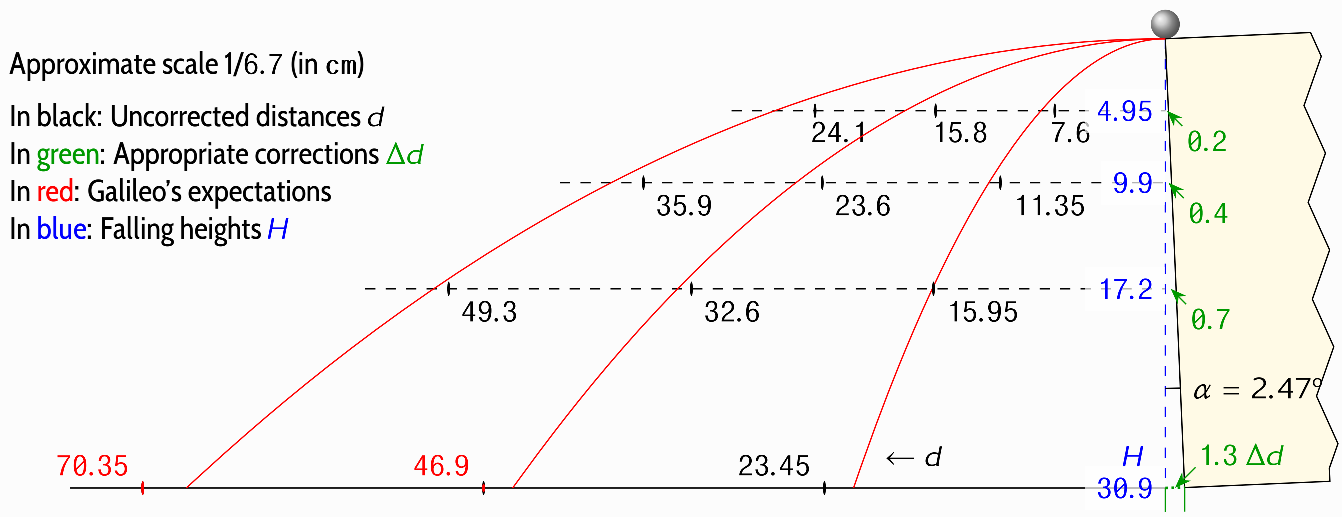

Figure 8.1: Accuracy of Galileo data

by applying these equations to the most closed parabola. The velocity $\underline{v}_{CM}$ in the absence of friction would be:

$$\underline{v}_{CM} = \sqrt{\frac{10 \times 980 \times 6.57}{7}} = 95.7\,\text{cm/s} \quad (70\ \text{p equivalen a } 6.57\ \text{cm})$$

H cm	d cm	v_{CM} cm/s	χ %
4.95	7.4	76.2	37
9.9	10.95	78.9	32
17.2	15.25	82.9	25
30.9	22.15	89.7	12

Table 8.1: Equations of the parabolas in the folio 81r

Ideally for our purposes, the columns headed by v_{CM} and χ would have shown constant values. In any case, the last values, marked in blue, are the most reliable since the starting data are those affected by a minimum relative error. The increasing progression (76.2 to 89.7), recorded in the velocity column, would be much more pronounced (78.3 to 95.8) if we had used the d *uncorrected* values. On the other hand, the value (95.8) is *precisely* that which results from the calculation of $\underline{v}_{CM}$ *in the absence of friction*, which makes it absurd since, as we already know, friction is inescapable. *These considerations show that the corrections made in* d *are appropriate*.

For the intermediate parabola it results:

$$\underline{v}_{CM} = 191.8\,\text{cm}$$

Table 8.2 shows the results obtained for this parabola.

The values marked in red should be discarded for two reasons:

a) The horizontal distance (45.6 cm) is not an experimental value.

H cm	d cm	v_{CM} cm/s	χ %
4.95	15.6	167.1	24
9.9	23.2	172.3	19
17.2	31.9	177.6	15
30.9	45.6	187.8	4

Table 8.2: Values for the intermediate parabola

b) The speed obtained (187.8 cm/s) is very close to the ideal speed without friction.

Therefore, the most reliable values are those marked in blue, since they have been calculated from corrected experimental data and present a percentage of dissipated energy of the same order as the most reliable result of the previous parabola.

For the most open parabola we will have:

$$\underline{v}_{CM} = 291.6\,\text{cm/s}$$

Table 8.3 shows the results obtained for it. The results marked in red should

H cm	d cm	v_{CM} cm/s	χ %
4.95	23.9	267.2	16
9.9	35.5	272.1	13
17.2	48.6	277.1	10
30.9	69.05	289.5	1.4

Table 8.3: Values for the most open parabola

be discarded for the two reasons already stated.

The one marked in blue is the best, but the other two are also quite good.

This is not surprising, since the starting data in this parabola are affected by minimum values of relative error. In all three tables the results marked in blue have values for χ between 10 and 15 %. We will return to this fact later. The analysis I have just made of these parabolas confirms that:

a) The first two were obtained with a plane of twelve elbows, perhaps resting on the ground, and inclined at an angle of 2.47° above the horizontal, as planned.

b) To obtain the third parabola Galileo had to make use of an auxiliary plane.

c) The experiment was designed by Galileo to confirm empirically a brilliant hypothesis, and it was not necessary to make any mathematical interpretation of it.

d) The experience was a resounding success, confirming the initial hypothesis.

8.3 Reproduction of the experience outlined on folio 81^r

25 April 2009

Yesterday I could not resist the temptation of dusting off my old inclined plane – the one described in Chapter 4 (p. 87) – to attempt a reproduction of Galileo's experience in the folio 81^r. I endowed my plane with an inclination of 2.37°, an angle very close to that which I suppose Galileo used, and calculated that by marking on it distances s of 20, 80, and 180 cm, starting from its lower end, I would obtain the necessary heights h. Namely:

$$h_1 = 0.827\,\text{cm} \qquad\qquad \text{for} \quad S = 20\,\text{cm}$$
$$h_2 = 3.308\,\text{cm} \qquad (h_1 \times 2^2) \qquad \text{for} \quad S = 80\,\text{cm}$$
$$h_3 = 7.443\,\text{cm} \qquad (h_1 \times 3^2) \qquad \text{for} \quad S = 180\,\text{cm}$$

I set up a $37 \times 24 \times 1.6$ cm board – whose horizontality I checked at each change of position with a bubble level – to record the impacts on a sheet of white paper covered by another sheet of carbon paper. I gradually raised the board using thick books as a support, thus obtaining eighteen pairs of co-ordinates of as many points corresponding to three parabolas analogous to those supposedly obtained by Galileo. The output velocities *without friction* obtained by calculation turn out to be:

$$\underline{v}_1 = 34.0\,\text{cm/s} \qquad \underline{v}_2 = 68.0\,\text{cm/s} \qquad \underline{v}_1 = 102.0\,\text{cm/s}$$

Table 8.4 shows the results obtained, using the d *corrected* values, in the analysis of the most closed parabola: The results obtained in the same way in the

H cm	d cm	v_{CM} cm/s	χ %
2.6	2.20	30.7	18.5
6.2	3.44	30.6	19.0
9.7	4.40	31.6	13.6
13.1	5.06	31.2	15.8
16.7	5.72	31.2	15.8
20.3	6.36	31.5	14.2

Table 8.4: Corrections to Galileo data

analysis of the intermediate and outer parabolas are shown in Tables 8.5 and

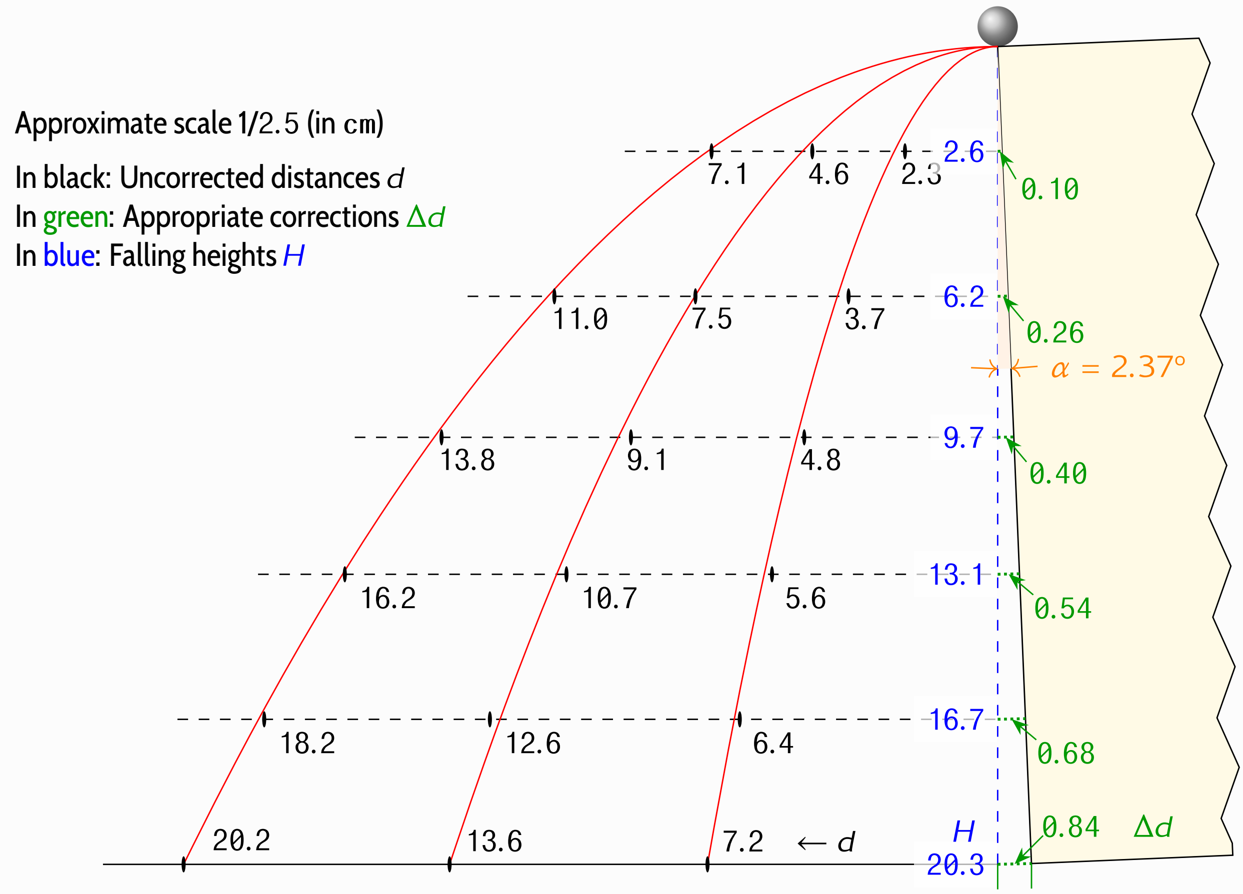

Figure 8.2: Corrections to folio 81[r] data

8.6:

H cm	d cm	v_{CM} cm/s	χ %
2.6	4.50	64.2	10.9
6.2	7.24	66.0	5.8
9.7	8.70	63.1	13.9
13.1	10.16	63.2	13.6
16.7	11.91	65.5	7.2
20.3	12.76	63.6	12.5

Table 8.5: Corrections to intermediate parabola

H cm	d cm	v_{CM} cm/s	χ %
2.6	7.00	101.9	0.4
6.2	10.74	99.1	5.6
9.7	13.40	98.1	7.5
13.1	15.66	98.2	7.3
16.7	17.51	97.0	9.6
20.3	19.36	97.1	9.4

Table 8.6: Corrections to external parabola

It is curious that in this experiment the *most reliable* percentages of energy dissipated range from 9.4 to 14.2 %, analogous to the most reliable percentages also calculated by me on the data in the folio 81^r, which range from 10 to 15 %. It is very clear, as I have already discussed in Chapter 3 (p. 60), *that the small ball never rolls twice exactly the same*, so the *percentage of energy dissipated turns out to be somewhat random*, but in section 2.5 of Chapter 2 (p. 55) the possibility that – superimposed to this randomness – there is a relation between the percentage and the angle of inclination α of the plane is pointed out. (Of course the result marked in red in Table 8.6 is clearly erroneous).

8.4 Reproduction of the experience outlined on folio 116^v

26 April 2009

In Chapter 5 (p. 104) I imagined that Galileo arranged his twelve-cubit plane with an inclination of 9.6° to achieve a maximum height of 1 000 p. Table 5.1 in the same chapter gives the percentages of energy dissipated, which range from a minimum of 3.2 to a maximum of 9.8 %. I have devoted today to reproduce with my plane the experience of the folio 116^v in order to obtain empirical data and to decide, in passing, whether it is reasonable to admit that there is any relation between the slope α and the percentage of energy dissipated χ. The limitations imposed by the furniture of my office and the lack of suitable material for the assembly have not allowed me to provide my plane with an inclination of 9.6°, so that I have had to be satisfied with 7.75°. The heights h have not been *measured* but *calculated* from measured lengths (30; 60; 90; 120 and 150 cm) over the total length of the plane. I have repeated ten times the launch for each h and used for the calculation of the percentage

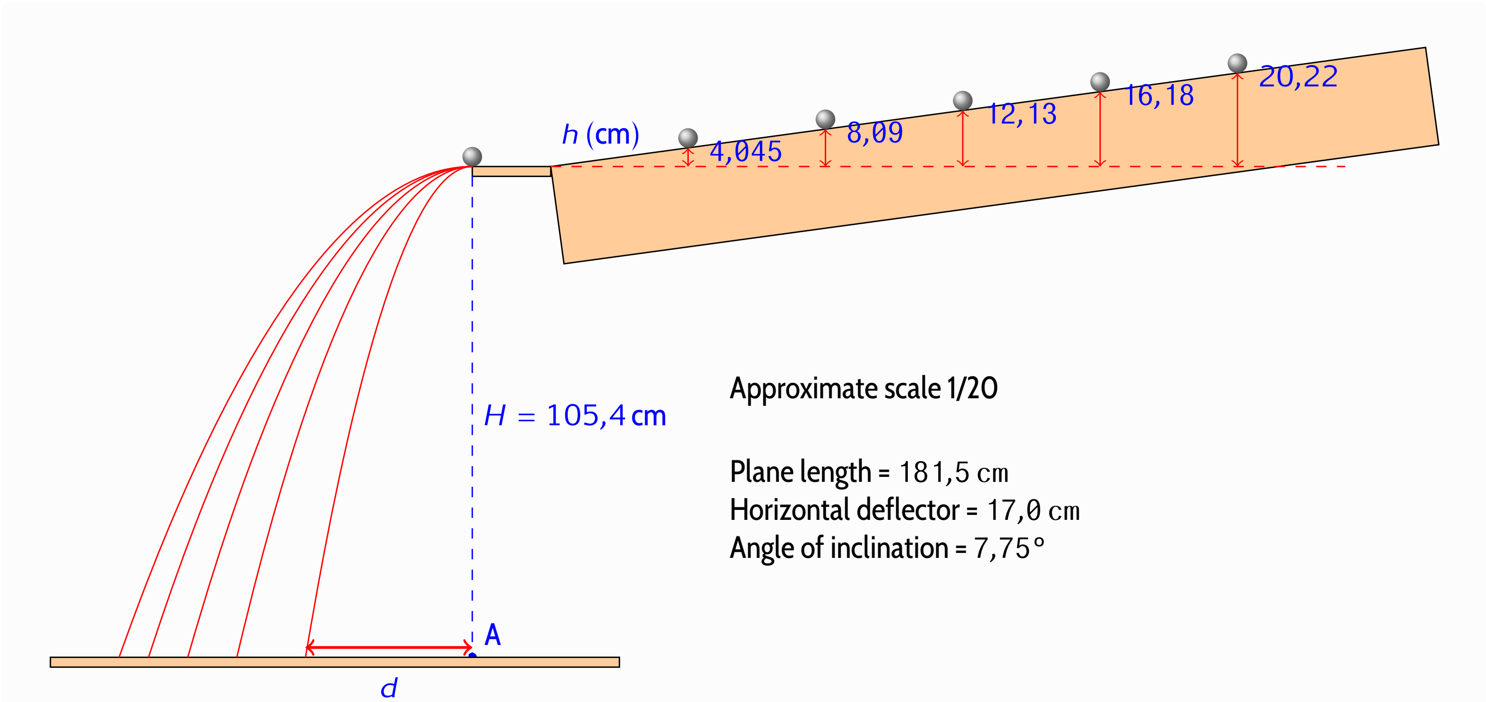

Figure 8.3: Reproduction of the folio 116ᵛ experience

of the dissipated energy the *mean value* d_m of the ten readings.

Let's remember – see Chapter 5 (p. 104) – that:

$$\chi = 100\left(1 - \frac{\underline{K}}{K}\right)$$

being

$$\underline{K} = \frac{d^2}{h} \qquad \text{and} \qquad K = \frac{20\,H}{7}$$

In our case $K = 301.1$ cm, as can be seen. Table 8.7 summarizes the result of this experience: When compared with the results given in Table ref Table 5.1 in Chapter 5 (p. 112) the parallelism is striking, right down to the detail that χ is maximum for the minimum h.

In the reproduction of the experience outlined in folio 81ʳ I merely took *a single value* of the coordinates of the points of each parabola and used them in the relevant calculations. In the reproduction of the experience relative to

#	h cm	d_m cm	$\underline{K} = d_m^2/h$ cm/s	χ %
1	4.045	33.3	274.1	9.0
2	8.09	47.6	280.0	7.0
3	12.13	58.8	285.0	5.3
4	16.18	68.5	290.0	3.7
5	20.22	75.8	284.1	5.6

Table 8.7: Folio 116^v experience considering energy loss

the folio 116^v, as I have already said, I have taken *ten times* the coordinates of each point and I have used in the calculations the mean values d_m.

It may not be superfluous to note the observations I have made in this connection:

a) In the impact record, the circular tracks left by the small ball are 3 mm in diameter. I have marked the *centre* of each mark by a dot – practised with a fine-tipped red pen – to measure the distances d to point A, marked on the record by a plumb bob suspended from the edge of the horizontal deflector. (See Figure 8.3).

b) Of course the ten traces corresponding to each h appear scattered over a small area that increases as h grows. For example, the footprints corresponding to $h = 4.045$ cm are concentrated in a circle of radius 0.75 cm. This radius increases progressively until it reaches 1.5 cm for the traces corresponding to $h = 20.22$ cm. This *dispersion* confirms that *the ball does not roll twice exactly the same* despite the fact that we try to reproduce the initial conditions as much as possible.

c) I have noted the values d *maximum* and *minimum* for each h and proceeded to calculate the values corresponding to the percentage χ of dissipated energy. This is shown in Table 8.8 and Table 8.9.

#	h cm	d_{min} cm	$\underline{K} = d_{min}^2/h$ cm/s	χ %
1	4.045	32.7	264.7	12.1
2	8.09	47.2	275.4	8.5
3	12.13	57.8	275.4	8.5
4	16.18	67.7	283.3	5.9
5	20.22	74.5	274.5	8.8

Table 8.8: Minimum dissipated energy calculations

#	h cm	d_{max} cm	$\underline{K} = d_{max}^2/h$ cm/s	χ %
1	4.045	33.6	279.1	7.3
2	8.09	48.2	287.2	4.6
3	12.13	59.8	294.8	2.1
4	16.18	69.0	294.2	2.3
5	20.22	77.1	294.0	2.4

Table 8.9: Maximum energy calculations

It is found, then, that for an angle of $7.75°$ the percentage of energy dissipated can vary *randomly* between a minimum *minimum* of one 2 percent and a maximum *maximum* of one 12 % and that this percentage χ *decreases* progressively as *increases* h.

To be able to decide *experimentally* on the dependence of χ versus α we would need more data, although we already have those relating to the angle of $2.37°$ which, together with those we have just obtained, seem to confirm our suspicion.

8.5 Reproduction of the experience outlined on folio 114v

The interpretation of this folio, presented in Chapter 6 (page 124), fits well with those of the other two, except in the detail that the percentages of energy dissipated seem to me to be too low in relation to the angle of inclination 1.32° that I must attribute to the plane. To reproduce this experience as far as possible I gave my plane an inclination of 1.59°, so that the lengths measured on it (30; 60; 90; 120 and 150 cm) now correspond to the heights shown in Figure 8.4. I took ten measurements of d for each h and used the mean value d_m to make the calculations using the equations outlined in section 8.1 (p. 150), which are appropriate in this case where there is no horizontal deflector. The results are summarized in Table 8.10. It is observed, as in the previous case, that χ decreases as h increases, varying between 18 and 14 %. Doing a similar study to the previous one, which I do not detail for brevity, using the d_{max} and d_{min} it is found that the bounds of this variation are further extended between

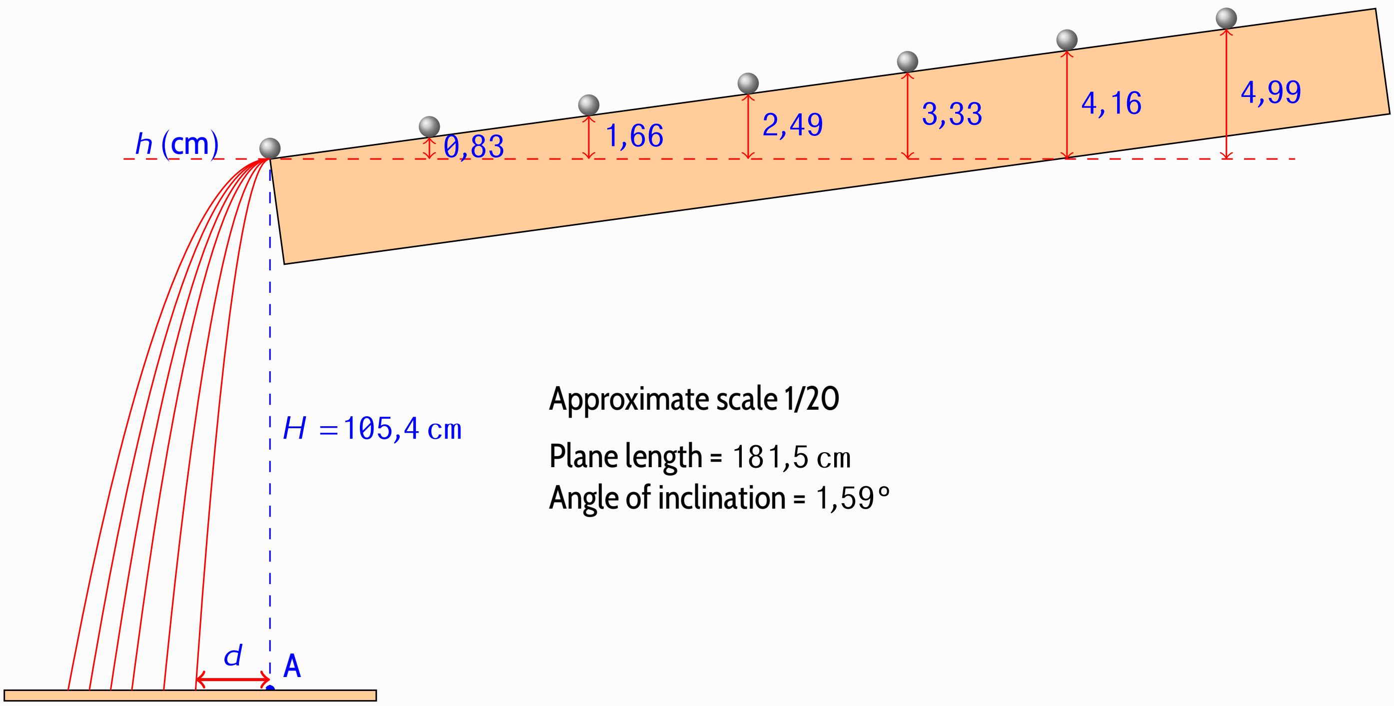

Figure 8.4: Reproduction of the folio 114v experience

#	H cm	h cm	d_m cm	v cm/s	$\underline{v}$ cm/s	χ %
1	105.4	0.83	14.3	30.9	34.1	17.9
2	"	1.66	20.4	44.1	48.2	16.3
3	"	2.49	25.3	54.7	59.0	14.0
4	"	3.33	29.0	62.8	68.3	15.4
5	"	4.16	32.8	71.0	76.3	13.4
6	"	4.99	35.8	77.6	83.6	13.8

Table 8.10: Variation of dissipated energy with h

a 20 and a 12 %.

These results confirm *experimentally*, in general lines, our suspicion that between the percentage of dissipated energy χ and the angle of inclination α there exists relation, as it was already demonstrated in the penultimate section (2.5) of the Chapter 2 (p. 55).

As I have already pointed out the explanation of folio 114^v suffers from a defect:

> *The percentages of dissipated energy calculated in Chapter 6 (p. 124) are too low to be compatible with the small angle of inclination that I attribute to the plane in the analysis that I make there.*

But we have already seen that a slight error in the measurement of d has a decisive influence on the values of χ. That is why it occurs to me to think whether Galileo overlooked, in this case, another *tiniest detail*, as I supposed happened to him when taking the measurements recorded in the folio 81^r:

If we look at Figure 8.4 and assume that Galileo used as a reference to measure

d the base of the piece of furniture on which we imagine the plane rests, it is easy to show that he would be making a *systematic error* of $0.71\,\mathrm{cm}$ per *excess* when measuring d. I have calculated that this simple error would raise the value of χ to a $15\,\%$ in the most closed parabola, and between a $7\,\%$ and a $8.5\,\%$ in the others.

Chapter 9

EPILOGUE

15 September 2009

The empirical equation:

$$a_{CM} = A\,(H - B)$$

and the theoretical:

$$\lambda = \sec\alpha\left(\sin\alpha - \frac{7a}{5g}\right)$$

can give this one:

$$\lambda = \sec\alpha\,(C\,H + D)$$

I have not seen them reflected so far in any text, so I would dare to consider them my property without this meaning that others could not have found them before, after or simultaneously. Nor have I found anything similar to my lucubrations on how to calculate the *percentage of the energy dissipated in the non-slip rolling of a sphere*. That is why in the introduction to the Chapter 1 (pág. 30) I express my suspicion that this subject must be of very little practical interest.

Historians of science – from Alexandre Koyré to Stillman Drake to Thomas B. Settle – fulfil their mission by discovering new documents, analysing them, criticizing them, and even trying to reproduce the experiments suggested in them. On the other hand, professional physicists cannot devote themselves to these tasks because other more serious investigations demand their ingenuity and effort.

I – who am not a professional physicist but a chemist fond of physics – have dedicated many years of my life to teach both sciences at secondary school level, and I have been fortunate to stumble at the end of my professional life with a small historical enigma that has kept me excited and entertained during these last years.

But the resolution I offer of this little historical enigma is likely to be of some interest. In the first place, it may contribute to a better *understanding* of the *mental process* that may have led Galileo to the discovery of his kinematic laws, a process that must not have been purely platonic, whatever Alexandre Koyré may think.

Secondly, there is its possible *didactic* interest: The role of *friction forces in pure rolling* is treated in most of the texts I have consulted in a very superficial, if not frankly far-fetched, manner. The same is true of the physical concept of *work of (sic) a force*.

The *experimental* part of my work was interrupted in June 2002, following my retirement. Since I love to write and had written some meticulous class and lab journals during the last twenty-three years of active life, I decided to start writing a series of articles about my teaching experience, in case they might be useful to anyone.

To my surprise I turned out to be the first to benefit, because I found that I *could* and *should* delve more deeply into the *theoretical part of rolling*, so that sections 1.4 and 1.6 of Chapter 1 (pp. 39 and 45) and 2.5 (p. 55) of the second are of very recent writing, as well as the exhaustive analysis of the data contained in section 2.6 (p. 56) of this same Chapter 2, which at the time were taken with all care with no other interest than pure pleasure and recreation.

It was then when I decided to go deeper into the interpretation of Galileo's enigmatic folios by applying my own ideas about the *percentage of dissipated energy*. Having completed this analysis in the first half of Chapter 8, I recovered my plan – which I had left in my old workplace – to submit to experimental verification what I had just found in that first half. With all this my retired distraction has stretched to the end of April of the current year with great satisfaction on my part.

Juan Luis Alcántara López

Appendix A.

On the rolling friction of a ball

Assume a sphere falling, starting from rest, **without any friction**, down an inclined plane an angle α, traversing a surface of length s from a height h.

A.1 Speed

The acceleration is due to forces (gravity, and plane reaction) that remain constant all the way, so the acceleration is constant and therefore:

$$v_{CM} = a_{CM}\, t \tag{A.1}$$

but this being a uniformly accelerated motion

$$s = \frac{1}{2} a_{CM} t^2 \quad \Rightarrow \quad t^2 = 2s/a_{CM}$$

which gives us the velocity as a function of acceleration and space traveled A.1

$$v_{CM}^2 = (a_{CM}\, t)^2 = a_{CM}^2 \times 2\, s/a_{CM} = 2\, a_{CM}\, s \tag{A.2}$$

but since we know the height and angle

$$\sin\alpha = \frac{h}{s} \quad \Rightarrow \quad s = \frac{h}{\sin\alpha} \tag{A.3}$$

is

$$v_{\text{CM}}^2 = (a_{\text{CM}}\, t)^2 = a_{\text{CM}}^2 \times 2\, s / a_{\text{CM}} = 2\, a_{\text{CM}} \frac{h}{\sin\alpha} \tag{A.4}$$

The acceleration, if it were a particle without a bearing, would correspond only to the component of gravity parallel to the plane $mg\sin\alpha$, and would be $g\sin\alpha$. But the bearing delays this acceleration by $1 \to \frac{5}{7}$, about one 30 % regardless of the radius r since the decrease in radius decreases on the one hand the moment of inertia I_{CM} and on the other hand increases the rotational velocity ω, both with the same factor r^2.

In **??** the following expression was obtained

$$a_{\text{CM}} = \frac{5}{7} g \sin\alpha$$

which corresponds to the acceleration due to gravity (without friction: $l\lambda = 0$) and taking into account the inertia and moment of inertia of the sphere.

We can obtain this expression that comes from the torques of the forces that produce the rotation of the sphere, on the one hand the non-vertical component of the weight, due to the inclination:

$$\mathbf{r} \times \mathbf{w} = r\, mg \sin\alpha \tag{A.5}$$

and, on the other hand, the force due to friction that we will better call coupling to the plane, since it is not a slip and does not produce any work: the point where it occurs does not move and the distance of application of the force is zero, the point A. This torque would be (see Figure 1.6):

$$\mathbf{r} \times \mathbf{w} + \mathbf{q} \times \mathbf{F} = I_{\mathrm{A}}\, \boldsymbol{\gamma}$$

that is:

$$\mathbf{r} \times \mathbf{w} = I_{\mathrm{A}}\, \boldsymbol{\gamma}$$

and since it is a rotation around, not the CM but the point A, the moment of inertia is not that of a sphere.

$$I_{\mathrm{CM}} = \frac{2}{5} m r^2 \tag{A.6}$$

but the one indicated in 1.3: $I_{\mathrm{A}} = 7/5\, m r^2$. On the other hand, the angular acceleration is

$$\gamma = \frac{a_{\mathrm{CM}}}{r} \tag{A.7}$$

with what is left

$$r m g \sin\alpha = \frac{7}{5} m r^2 \gamma = \frac{7}{5} m r^2 \frac{a_{\mathrm{CM}}}{r} = \frac{7}{5} m\, a_{\mathrm{CM}}\, r \tag{A.8}$$

and from here the obtained in **??**

$$a_{\mathrm{CM}} = \frac{5}{7} g \sin\alpha \tag{A.9}$$

Joining A.2 with A.3 and the expression obtained for a_{CM} in A.9:

$$v_{CM}^2 = 2\,\frac{5}{7}\,g \sin\alpha\,\frac{h}{\sin\alpha} = \frac{10}{7}\,gh$$

The theoretical final velocity of the center of mass is

$$v_{CM}^2 = \frac{10}{7}\,gh \tag{A.10}$$

Velocity of the center of mass of the sphere, independent of the radius and mass of the sphere and, therefore, independent of its material.

A.2 Horizontal distance in free flight

In free flight we have a rotating sphere with a constant rotational velocity that does not affect its free fall and with the center of mass with a horizontal velocity v_{CM} that is also constant since there are no horizontal forces. This sphere starts to free fall from a height H, and since the vertical component is not affected by the horizontal component, it will make that H vertical path in t seconds:

$$H = \frac{1}{2}\,g\,t^2 \tag{A.11}$$

from where the flight time will be

$$t^2 = \frac{2H}{g} \quad \Rightarrow \quad t = \sqrt{\frac{2H}{g}} \tag{A.12}$$

What will be the horizontal travel d it achieves in this flight time? Since the horizontal velocity is constant v_{CM}:

$$d = v_{CM}\, t = v_{CM} \sqrt{\frac{2H}{g}} \quad \Rightarrow \quad d^2 = v_{CM}^2 \frac{2H}{g} \tag{A.13}$$

A.3 Total kinetic energy

The final total kinetic energy E_{c_T} is the sum of the translational kinetic energy of the center of mass CM (E_{CM}) and the rotational kinetic energy (E_ω):

$$E_{c_T} = E_{CM} + E_\omega$$

and here E_ω is the kinetic energy of rotation around the CM.

$$E_\omega = \frac{1}{2} I \omega^2 \tag{A.14}$$

The moment of inertia I here refers not to that of rotation around the contact point A but directly around the CM, which is A.6, so that

$$E_{c_T} = E_{CM} + E_\omega = \frac{1}{2} m v_{CM}^2 + \frac{1}{2} I \omega^2 = \frac{1}{2} m v_{CM}^2 + \frac{1}{2} (\frac{2}{5} m r^2)\, \omega^2$$

and as

$$v_{CM} = \omega r \quad \Rightarrow \quad \omega = \frac{v_{CM}}{r}$$

is

$$E_{c_T} = \frac{1}{2} m v_{\text{CM}}^2 + \frac{1}{2}\frac{2}{5} m r^2 \frac{v_{\text{CM}}^2}{r^2} = \frac{1}{2} m v_{\text{CM}}^2 + \frac{1}{5} m v_{\text{CM}}^2$$

giving

$$E_{c_T} = \frac{7}{10} m v_{\text{CM}}^2 \tag{A.15}$$

A.4 Measurement of friction on the inclined plane from the length reached on the free flight

The total theoretical kinetic energy of the sphere when it reaches the end of its path to begin horizontal flight is given by the equation A.15. In this motion it will maintain the same rotational kinetic energy E_ω and, since deflection by the deflecting plane changes its velocity to a velocity with only horizontal component, it will start it with a kinetic energy given by the final velocity after falling on the inclined plane E_{CM}.

The horizontal distance d traveled since the beginning of the free fall with initial horizontal velocity v_{CM} must be deduced from the mechanics of motion under a constant force, that of the weight, and we will assume negligible, the friction of the air.

This initial velocity will allow us to measure the real loss of energy due to rolling friction in the fall on the inclined plane. For this, we measure the horizontal distance d traveled in the free fall H and from the equation A.13 we obtain the v_{CM} that the sphere had at the end of the fall on the inclined plane:

$$v_{CM}^2 = d^2 \frac{g}{2H} \tag{A.16}$$

Therefore, if the ball reaches a horizontal distance d, the equation A.16 gives us its horizontal velocity at height H. This horizontal velocity corresponds to a total kinetic E_{c_T}:

$$E_{c_T} = \frac{7}{10} m v_{CM}^2 = \frac{7}{10} m d^2 \frac{g}{2H} = d^2 mg \frac{7}{20H} \tag{A.17}$$

If we divide the total kinetic energy E_{c_T} with which the ball has arrived at the table by the initial potential energy at the beginning of its journey from rest, $E_p = mgh$, h being the height of the inclined plane, we have

$$\frac{E_{c_T}}{E_p} = d^2 \cancel{mg} \frac{7}{20H} \Big/ \cancel{mg}h = \frac{d^2/h}{20H/7} = \frac{\underline{K}}{K} \tag{A.18}$$

where we have directly substituted the values of K and TK from the equations 6.2 and 6.1.

Recall that the author defines in **??** the constants K as the simple coefficient between the total kinetic energy E_{c_T} and the square of the velocity of the center of mass v_{CM}^2 at the arrival at the base of the inclined plane.

The definition of the constant $\underline{K}$ is that of the coefficient between the horizontal distance traveled in free flight, d, and the table height: 5.1.

Appendix B.

On the rolling friction of a ball

A simple experimental study of rolling friction and rolling energy loss in a ball on a wooden groove can be found in (Minkin and Sikes, 2018). These authors start from the practical assumption that rolling friction is constant and independent of the material, only depending on the normal force exerted by the ball on the surface, as with static and dynamic sliding friction. The equation from which they start is therefore:

$$F_r = \mu N$$

These authors perform a simple experiment in which they wait for a ball oscillating on a wooden trough and also on a plastic trough to stop. For this purpose they use short but concave curved boards so that the final motion of the ball resembles that of a pendulum. In any case, after a series of oscillations that resemble a damped pendulum, the ball will eventually stop. The main reason, disregarding other factors of much lesser influence, why the ball will stop is because of its friction with the grooved surface. Thus, if we assume that the force that stops the ball is independent of the velocity, all its kinetic energy will be converted into heat at first by the work of the frictional force.

The force normal to the plane can be approximated by the weight. The error will be less than 30 % since the surface had an inclination of less than 15°, and it would be

$$N = mg\cos(0.15/0.57) = 0.97mg$$

With all that we can assume a horizontal path to stop and a total work:

$$W = -\mu mg\, s$$

being s the total distance travelled to stop. The calculations of these authors lead them to the following values

$$\mu_{\text{wood}} = 2.0 \times 10^{-3}$$

y

$$\mu_{\text{plastic}} = 0.75 \times 10^{-3}$$

B.1 Easy evaluation of the coefficient of rolling friction

A simple experiment is schematized in Figure B.1. Assuming that the loss of mechanical energy ($\Delta E = E_{\mathbf{B}} - E_{\mathbf{A}}$) is consumed only by friction: E_r, and taking into account that the rolling friction is produced by a constant force, as these studies claim, we would have:

$$E_{\mathbf{A}} = E_{\mathbf{B}} + F_r\, s_r; \qquad \Delta E = F_r\, s_r \tag{B.1}$$

Being s_r the distance *real* travelled by the ball since it starts from the end **A** with $v_0 = 0$ until it reaches the other end **B** with $v_f = 0$ at a somewhat lower height in Δh due to the lost energy.

For simplicity, we are taking half a period since the motion has constant period although with decreasing linear amplitude due to the constancy of F_r. The energy decrement, given that the ini-

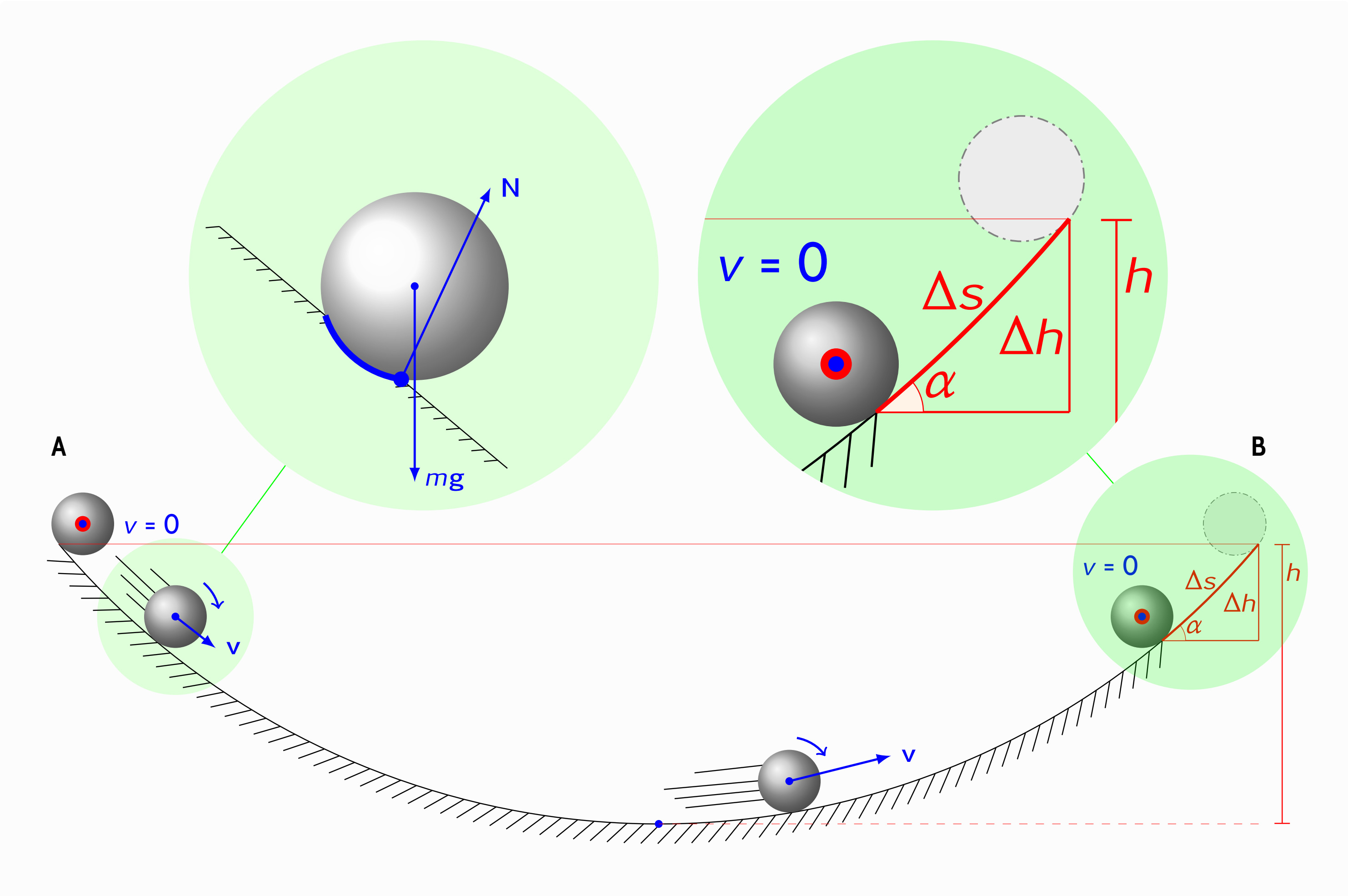

Figure B.1: Frictional energy loss

tial total energy, potential only, is $E_i = mgh$ and the final energy, potential only, is $E_f = mg\,(h - \Delta h)$:

$$\Delta E = mg\,\Delta h = F_r\,s_r$$

being s_r the space travelled in that oscillation.

$$F_r\,s_r = F_r\,(s - \Delta s) = mg\,\Delta h$$

or,

$$F_r = \frac{mg\,\Delta h}{s - \Delta s}$$

Considering that F_r is constant along the path and proportional to the normal force that we can approximate directly with the weight since the angle of inclination of the track is small, it would be:

$$\mu mg = \frac{mg\,\Delta h}{s - \Delta s}$$

and since Δh is more difficult to measure than Δs we use $\Delta h = \sin\alpha\,\Delta s$ so,using the well-known approximation, $\sin\alpha = h/(s/2)$, we are left with:

$$\mu = \frac{\Delta s\,\sin\alpha}{s - \Delta s} = \frac{\sin\alpha}{\frac{s}{\Delta s} - 1} \qquad \text{(B.2)}$$

References

Minkin, Leonid and Daniel Sikes (Jan. 2018). 'Coefficient of rolling friction - Lab experiment'. In: *American Journal of Physics* 86.1, pp. 77–78. ISSN: 0002-9505.