Galilean Principles of “Local” Motion

In the absence of air resistance, bodies descending from rest

1. **In vertical descent acquire equal increments of speed in equal increments of time.**

2. **Acquire the same speed in descending from the same height regardless of their weight or shape.**

3. **Acquire the same speed in falling from a given height whether falling vertically or along an inclined plane.**

4. **Acquire a speed in descending from any given height which is just sufficient to raise them to that height.**

What experimental evidence did Galileo and those in the decade following him provide in support of each of these principles; and how telling was that evidence in showing whether each holds merely to high approximation or exactly?

Galilean Principles and Evidence for Them

Descent in the absence of air resistance the same for all bodies, regardless of weight, shape, and density

1. **Qualitative experiments falsifying Aristotle**
2. **Qualitative experiments in different media**
3. **Qualitative experiments on paired pendulums**

Descent in the absence of air resistance is a uniformly accelerated motion, so that $\Delta v \propto \Delta t$ and $\Delta s \propto (\Delta t)^2$

1. **Galileo's experiments on shallow inclined planes**
2. **Riccioli's experiments on direct vertical fall**
3. **(Challenged somewhat by Mersenne's experiments)**

The same speed is acquired in descent from rest from the same height in the absence of air resistance, whether the descent be directly vertical or along an inclined plane of any angle

1. **Comparison of Galileo's results for different angles of inclined planes: $\Delta v \propto \sin \alpha$**

The speed acquired in descent from any given height in the absence of air resistance is just sufficient to raise the body back to that height

1. **Qualitative experiments with pendulums**

Problems with Experiments in Mechanics

1. Elapsed times were short (e.g. < 5 sec) making any result highly sensitive to small errors in time measurements

2. There was no direct way to measure velocities, yet many central claims concerned them

3. Experiments needed at least to control for, if not minimize, resistance effects, yet the only ways acknowledged for doing this were to employ very heavy bodies and keep speeds low

Upshot: ***Discrepancies between theory and experimental results were ambiguous: (1) insufficient control of "external" effects, (2) measurement error; (3) inadequate theory; but then so too ambiguous was the absence of discrepancies.***

Galileo's Approach

- **Develop a mathematical theory from hypotheses that appear reasonable and mathematically tractable**

- **Derive some "striking" predictions within that theory, like the 1, 3, 5, ... pattern, among others**

  Predictions that are *prima facie* counterintuitive

  Predictions that involve qualitative contrasts

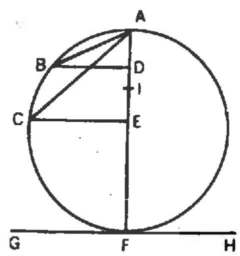
- **Design experiments to test the striking predictions, hoping at the very least that the results do not clearly falsify them (limited effects of air resistance notwithstanding)**

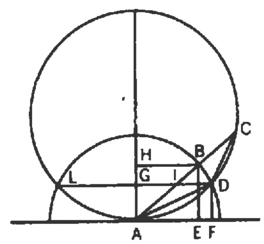
"One finds in this subject a kind of demonstration which does not carry with it so high a degree of certainty as that employed in geometry; and which differs distinctly from the method employed by geometers in that they prove their propositions by well-established and incontrovertible principles, while here principles are tested by the inferences which are derivable from them. The nature of the subject permits of no other treatment. It is possible, however, in this way to establish a probability which is little short of certainty. This is the case when the consequences of the assumed principles are in perfect accord with the observed phenomena, and especially when these verifications are numerous; but above all when one employs the hypothesis to predict new phenomena and finds his expectations realized."

Christiaan Huygens, 1690

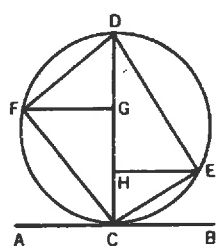
A Further Example of a Striking Result

Prop. VI. ***If, from the highest or lowest point of a vertical circle, any inclined planes whatever are drawn to its circumference, the times of descent along these planes will be equal.***





Corol. I. ***From this it is deduced that the times of descent from all chords drawn from the terminals C and D are equal to one another.***



Corol. II. ***It is also deduced that if from the same points there descend a vertical and an inclined plane, over which descents are made in equal times, they are [inscribable] in a semicircle of which the diameter is vertical.***

The Structure of "Day 3"

Propositions I – III: ***fundamental results***

Propositions IV – IX: ***comparisons involving inclined planes***

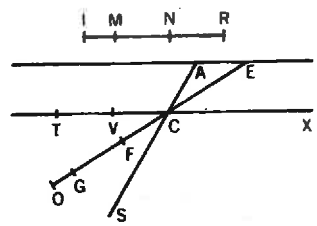
Propositions X – XXVI: ***initial speeds and diverting motion from one plane to another, culminating in the Scholium to Prop.*** **XXIII**

Propositions XXVII –XXXI: ***minimum time trajectories***

Propositions XXXII – XXXVII: ***time comparisons along different paths, culminating in the Scholium to Prop.*** **XXXVI**

21 theorems, 16 problems, where latter require solving for an unknown quantity (geometric magnitude), given other quantities, using purely geometrical methods (compass and rules)

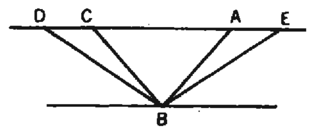
Scholium to Prop. XXIII



And we may then deduce that if, in the above diagram, after descent through the inclined plane AC, there is diversion along a horizontal line such as CT, the space through which the moveable will next be moved, in a time equal to that of descent through AC, would be exactly double the space AC....

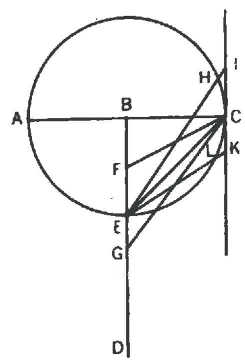
It may also be noted that whatever degree of speed is found in the moveable, this is by its nature indelibly impressed on it when external causes of acceleration or retardation are removed, which occurs only on the horizontal plane; for on declining planes there is a cause of more acceleration, and on rising planes, of retardation. From this it likewise follows that motion in the horizontal is also eternal, since if it is indeed equable it is not weakened or remitted, much less removed.

Scholium to Prop. XXIII



From this we may therefore reasonably assert that if descent is made through some inclined plane, after which there follows reflection through some rising plane, the moveable ascends, by the impetus received, all the way to the same altitude or height from the horizontal. Thus if the descent is along AB, the moveable is carried along the diverted plane BC to the horizontal ACD; and not only if the inclinations of the planes are equal, but also if they are unequal, as is plane BD. For it was assumed earlier that the degrees of speed acquired over unequally inclined planes are equal whenever the planes are of the same height above the horizontal. But if the same inclination exists for planes EB and BD, descent through EB suffices to impel the moveable along plane BD all the way to D, as such an impulse is made on account of the received impetus of speed at point B; and there is the same impetus at B whether the moveable descends through AB or through EB. It follows that the moveable is pushed out likewise along BD after descent along AB or along EB.

Proposition 30



Drop the vertical BD from a point B in the horizontal line AC, in which take any point C; and in the vertical, take a distance BE equal to the distance BC, drawing CE, I say that of all inclined planes from point C to the vertical [BD], CE is that along which descent will be made to the vertical [BD] in the shortest time of all.

I.e. let α be the angle the plane makes with the horizon – e.g. angle BCF. Let d be the distance along the horizontal from the apex of the inclined plane to the vertical, and s the length of the inclined plane. Then

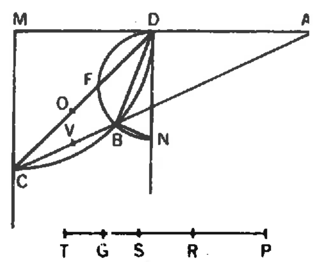
$$s=1/2\ (a\cdot\sin\alpha)\ t^2 \quad \text{and} \quad d=s\cdot\cos\alpha$$

so that

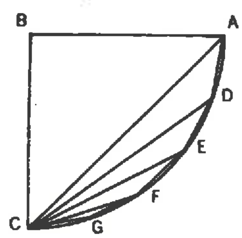
$$d = 1/2\ (a\cdot\sin\alpha\cdot\cos\alpha)t^2$$

Given d, therefore t is least when the expression in parenthesis is a maximum, which is when α is 45 degrees.

Proposition 36 and Scholium



Let the circumference CBD be no more than one quadrant of the verticle circle with its lowest point at C, to which is raised the plane CD; and let two planes be deflected from the ends D and C to some point B taken on the circumference; I say that the time of descent through both the planes DB and BC is briefer than the time of descent through DC alone.



From the things demonstrated, it appears that one can deduce that the swiftest movement of all from one terminus to the other is not through the shortest line of all, which is the straightest line AC, but through the circular arc.... Therefore descent is made in still shorter time through the five AD-DE-EF-FG-GC than through the four AD-DE-EF-FC. Hence motion between two selected points, A and C, is finished more quickly, the more closely we approach the circumference through inscribed polygons.

What has been explained for the quadrant happens also in arcs less than the quadrant.

Sagredo's Assessment of Day 3

It appears to me that we may grant that our Academician was not boasting when, at the beginning of this treatise, he credited himself with bringing to us a new science concerning a most ancient subject. When I see with what ease and clarity, from a single simple postulate, he deduces the demonstrations of so many propositions, I marvel not a little that this kind of material was left untouched by Archimedes, Apollonius, and Euclid, and so many other illustrious mathematicians and philosophers; especially seeing that many and thick volumes have been written on motion.

What exactly has the mathematical theory of "natural" motion in Day Three accomplished?

"Predictive" power: so much

"Explanatory" power: from so little

"Question-answering" power

On the Motion of Projectiles

We have considered properties existing in equable motion, and those in naturally accelerated motion over inclined planes of whatever slope. In the studies on which I now enter, I shall try to present certain leading essentials [*symptomata*] , and to establish them by firm demonstrations, bearing on a moveable [*Mobili*] when its motion is compounded from two movements; that is, when it is moved equably and is also naturally accelerated. Of this kind appear to be those which we speak of as projections, the origin of which I lay down as follows.

I mentally conceive of some moveable projected on a horizontal plane, all impediments being put aside. Now it is evident from what has been said elsewhere at greater length that equable motion on this plane would be perpetual if the plane were of infinite extent; but if we assume it to be ended, and [situated] on high, the moveable (which I conceive of as being endowed with heaviness [*gravitate*]), driven to the end of this plane and going on further, adds on to its previous equable and indelible motion that downward tendency [*propensionem*] which it has from its own heaviness. Thus there emerges a certain motion, compounded from equable horizontal and from naturally accelerated downward [motion], which I call "projection." We shall demonstrate some of its *accidentia*, of which the first is this:

> ***Proposition 1*, *Theorem 1*. When a projectile is carried in motion compounded from equable horizontal and from naturally accelerated downward [motions], it describes a semiparabolic line in its movement.**

remo veduto questa parte che resta intorno al Moto de i Proietti; che sarà, se così gli piace, nel seguente giorno.

Salu. *Non mancherò d'esser con lei.*

Finisce la terza Giornata.

GIORNATA QUARTA.

Salu. *A Ttempo arriva ancora il S. Simplicio, però senza interpor quiete venghiamo al Moto, & ecco il Testo del nostro Autore.*

DE MOTV PROIECTORVM.

Quæ in Motu æquabili contingunt accidentia, itemque in Motu naturaliter accelerato super quascunque planorum inclinationes, supra consideravimus. In hac, quam modo aggredior, contemplatione, præcipua quædam symptomata, eaque scitu digna in medium afferre conabor, eademque firmis demonstrationibus stabilire, quæ Mobili accidunt dum motu ex duplici latione composito, æquabili nempe, & naturaliter accelerata, movetur: hujusmodi autem videtur esse Motus ille, quem de Projectis dicimus: cujus generationem talem constituo.

Mobile quoddam super planum horizontale projectum mente concipio omni secluso impedimento: jam constat ex his quæ fusius alibi dicta sunt illius motum æquabilem, & perpetuum super ipso plano futurum esse, si planum in infinitum extendatur: si vero terminatum, & in sublimi positum intelligamus, mobile, quod gravitate præditum concipio, ad plani terminum delatum, ulterius progrediens, æquabili, atque indelebili priori lationi superaddet illam, quam à

pro-

propria gravitate habet deorſum propenſionem, indeque motus quidam emerget compoſitus ex ęquabili horizontali, & ex deorſum naturaliter accelerato: quem Projectionem voco. Cujus accidentia nonnulla demonſtrabimus; quorum primum ſit.

THEOR. I. PROPOS. I.

Projectum dum fertur motu compoſito ex horizontali æquabili, & ex naturaliter accelerato deorſum, lineam ſemiparabolicam deſcribit in ſua latione.

Sagr. *E' forza S. Salu. in gratia di me, & anco credo io del S. Simpl. far qui vn poco di pauſa; auuenga che io non mi ſon tanto inoltrato nella Geometria ch' io habbia fatto ſtudio in Apollonio, ſenon in quanto sò ch'ei tratta di queſte Parabole e dell' altre ſezzioni coniche, ſenza la cognizione delle quali, e delle lor paſſioni, non credo che intenderſi poſſano le dimoſtrazioni di altre propoſizioni à quelle aderenti. E perche già nella bella prima propoſizione ci vien propoſto dall' Autore douerſi dimoſtrare la linea deſcritta dal Projetto eſſer Parabolica, mi vò imaginando, che, non douendoſi trattar d'altro che di tali linee, ſia aſſolutamente neceſſario hauere vna perfetta intelligenza, ſe non di tutte le paſſioni di tali Figure dimoſtrate da Apollonio, almeno di quelle, che per la preſente ſcienza ſon neceſſarie.*

Salu. *V. S. ſi humilia molto, volendoſi far nuouo di quelle cognizioni, le quali non è gran tempo che ammeſſe come ben ſapute: allora dico che nel trattato delle Reſiſtenze hauemmo biſogno della notizia di certa propoſizione d'Apollonio, ſopra la quale ella non moſſe difficoltà.*

Sagr. *Può eſſere ò che io la ſapeſſi per ventura, ò che io la ſuppoñeſſe per vna volta, tanto che ella mi biſognò in tutto quel trattato: mà qui doue mi imagino d'hauere à ſentir tutte le dimoſtrazioni circa tali linee, non biſogna, come ſi dice, beuer groſſo, buttando via il tempo e la fatica.*

Gg 3 Simp.

Sagredo: It cannot be denied that the reasoning is novel, ingenious and conclusive, being argued _ex supposition_; that is, by assuming that the transverse motion is kept always equable, and that the natural downward [motion] likewise maintains its tenor of always accelerating according to the squared ratio of the times; also that such motions, or their speeds, in mixing together do not alter, disturb, or impede one another. In this way, the line of the projectile, continuing its motion, will not degenerate into some other kind [of curve]. But this seems to me impossible; for the axis of our parabola is vertical, just as we assume the natural motion of heavy bodies to be, and it goes to the end of the center of the earth. Yet the parabolic line goes ever widening from its axis, so that no projectile would ever end at the center [of the earth], or if it did, as it seems it must, then the path of the projectile would become transformed into some other line, quite different from the parabolic.

Simplicio: To these difficulties I add some more. One is that we assume the [initial] plane to be horizontal, which would be neither rising nor falling, and to be a straight line – as if every part of such a line could be at the same distance from the center, which is not true. For as we move away from its midpoint towards its extremities, this [line] departs ever farther from the center [of the earth], and hence it is always rising. One consequence of this is that it is impossible that the motion is perpetuated, or even remains equable through any distance; rather it would be always growing weaker. Besides, in my opinion, it is impossible to remove the impediment of the medium so that this will not destroy the equability of the transverse motion and the rule of acceleration for falling heavy things. All these difficulties make it highly improbable that anything demonstrated from such fickle assumptions can ever be verified in actual experiments.

***Salviati*: All the difficulties and objections you advance are so well founded that I deem it impossible to remove them. For my part, I grant them all, as I believe our Author would also concede them. I admit that the conclusions demonstrated so in the abstract are altered in the concrete, and are so falsified that horizontal [motion] is not equable; nor does natural acceleration occur [exactly] in the ratio assumed; nor is the line of the projectile parabolic, and so on. But on the other hand, I ask you not to reject in our Author what other very great men have assumed, despite its falsity. The authority of Archimedes alone should satisfy everyone....**

Here I add that we may say that Archimedes and others imagined themselves, in their theorizing, to be situated at infinite distance from the center. In that case their said assumptions would not be false, and hence their conclusions were drawn with absolute proof. Then if we wish later to put to use, for a finite distance [from the center], these conclusions proved by supposing immense remoteness [therefrom] we must remove from the demonstrated truth whatever is significant in [the fact that] our distance from the center is not really infinite, though it is such that it can be called immense in comparison with the devices employed by us.... And these shots coming to end on the surface of the terrestrial globe may alter in shape only insensibly, whereas that shape is conceded to be enormously transformed in going to end at the center....

Also that motion in the horizontal plane, all obstacles being removed, ought to be equable and perpetual; but it will be altered by the air, and finally stopped.

***Salviati*: Next, a more considerable disturbance arises from the impediment of the medium; by reason of its multiple varieties, this is incapable of being subjected to firm rules, understood, and made into science. Considering merely the impediment that the air makes to the motions in question here, it will be found to disturb them all in an infinitude of ways, according to the infinitely many ways that the shapes of moveables vary, and their heaviness, and their speeds. As to speed, the greater this is, the greater will be the opposition made to it by the air, which will also impede bodies the more, the less heavy they are....**

No firm science can be given of such events [*accidenti*] of heaviness, speeds, and shape, which are variable in infinitely many ways. Here to deal with such matters scientifically, it is necessary to abstract away from them. We must find and demonstrate conclusions abstracted from the impediments, in order to make use of them in practice under those limitations that experience will teach us.... Indeed, in projectiles that we find practicable, which are those of heavy material and spherical shape, ... the deviations from exact parabolic paths will be quite insensible.

$r = 1\ cm$

$m = 10gm$

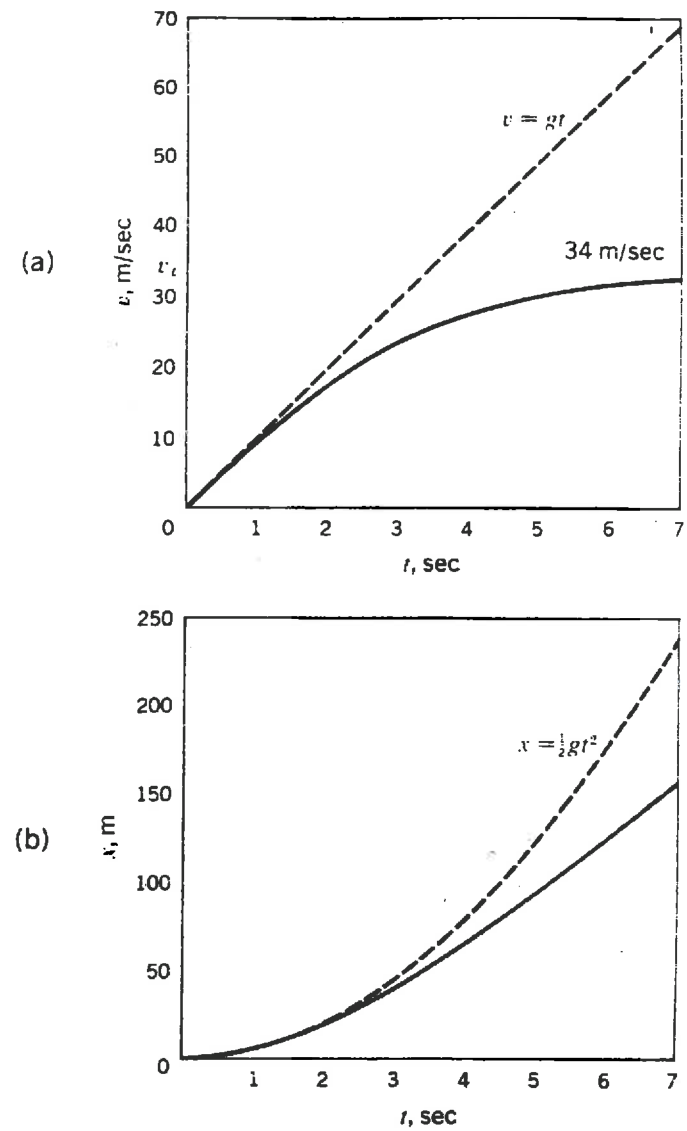


Fig. 7-22 (a) Comparison of idealized (resistanceless) and actual dependence of speed on time for a falling pebble of radius 1 cm.
(b) Idealized and actual distances fallen by such a pebble.

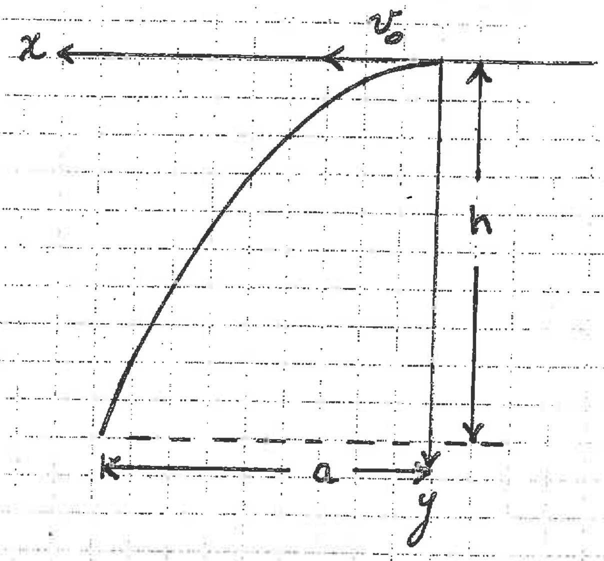
$$resistance\ decel\ of\ spheres \propto \frac{velocity^2}{radius \times density}$$

from French, Newtonian Mechanics, p.217

Our Way

$$x = v_0 t \qquad y = \tfrac{1}{2} g t^2$$

$$y = (g/2v_0^2)\, x^2$$



$$\frac{dy}{dx} = (g/v_0^2)\, x = \tan\theta$$

$$v = \sqrt{v_0^2 + 2gy}$$

at impact, $y = h$, $x = a$

$$h = (g/2v_0^2)\, a^2$$

$$\tan\theta_{imp} = \cancel{h/a} = (g/\cancel{2}v_0^2)\, a$$

$$v_{imp} = \sqrt{v_0^2 + 2gh}$$

$$= \sqrt{v_0^2 + (g^2/v_0^2)\, a^2}$$

$$= v_0 \sqrt{1 + (g^2/v_0^4)\, a^2}$$

$$v_{imp} = v_0 \sqrt{1 + \tan^2\theta_{imp}} = v_0 / \cos\theta_{imp}$$

***Sagredo*: The theory of compounding these different impetuses and of the quantity of impetus that results from such mixing is so new to me as to leave no little confusion in my mind. I speak not of the mixing of two equable movements, one along the horizontal line and the other along the vertical, even though unequal to one another; for as to this, I quite understand that a motion results which is equal in the square to both components of it. But I am confused by the mixture of equable horizontal and naturally accelerated vertical [motion].**

***Salviati*: We can reason definitively about movements and their speeds or impetuses (whether these are equable or naturally accelerated) only if we first determine some standard [*misura*] that we can use to measure such speeds, as also some measure of time. As to the measure of time, we already have universal agreement on hours, minutes, seconds, etc.; and just as the measure of time is for us that one in common use, accepted by everybody, so it is necessary to assign some measure for speeds to be commonly understood and accepted by all; that is, one that will be the same for everyone.**

As explained previously, the Author deemed suitable for such a purpose the speed of naturally falling heavy bodies, of which the growing speeds keep the same tenor everywhere in the world. ... To determine and represent this unique impetus and speed, our Author has found no better means than to make use of the impetus acquired by the moveable in a naturally accelerated motion. Any acquired momentum, turned to equable motion, retains its limited speed precisely, and it is such that in another time equal to that of the descent, it will pass through exactly twice the distance of the height from which fall took place.

Prerequisites for *height* of fall from rest to serve as a *proxy* for purposes of representing and measuring *velocity squared*:

1. **Speed acquired in descent from rest is proportional to the time of descent – i.e. descent involves uniformly accelerated motion.**

2. **The same speed is acquired in descent from rest from a given height regardless of the path of descent – i.e. pathwise independence of speed acquired.**

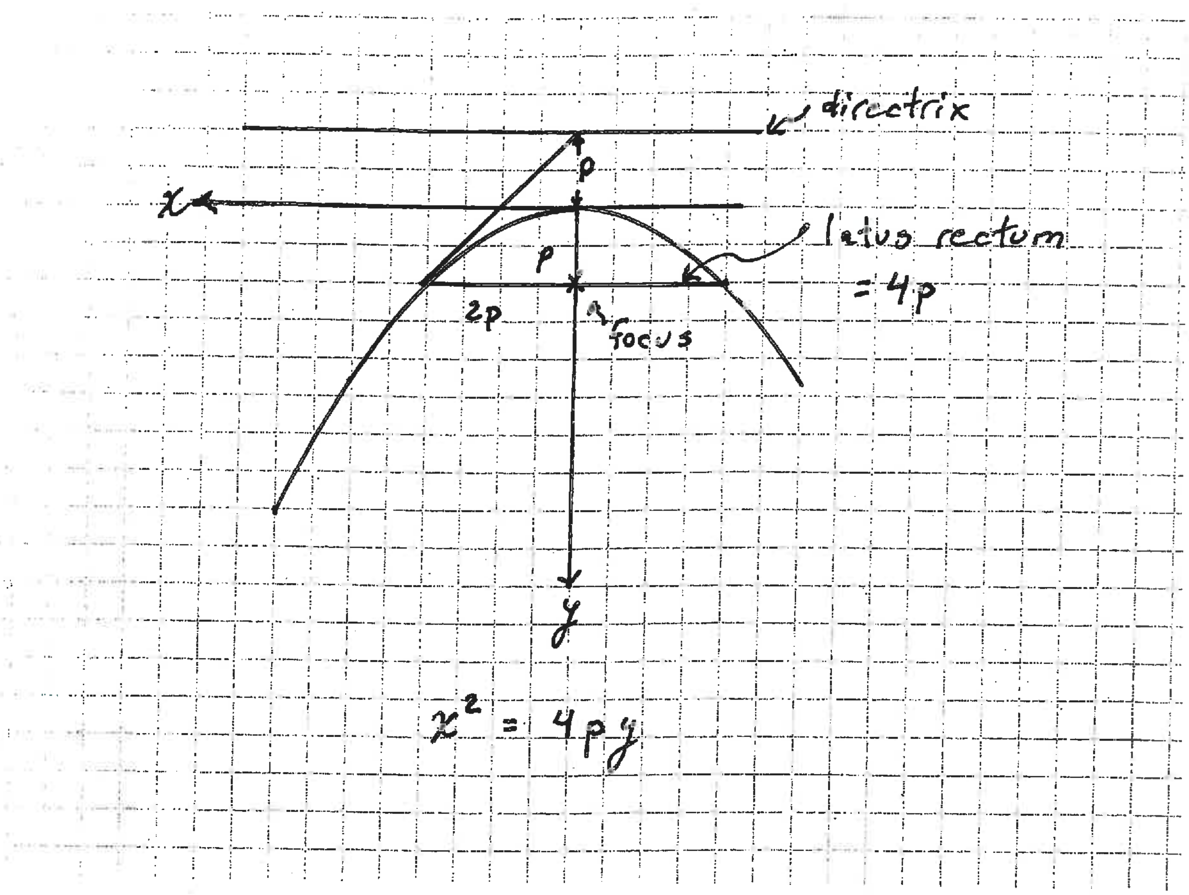
3. **All bodies acquire the same speed in descent from any given height regardless of their weight, shape, composition, etc. – i.e. the only variable that makes a difference to the speed acquired is the height of descent.**

In order for *height* to serve as a uniform, universal measure of *velocity squared*, and not just a local measure:

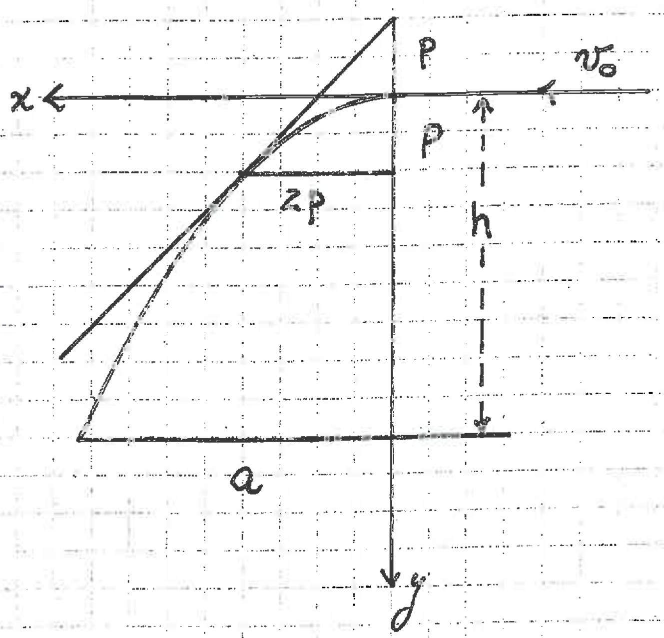
4. **The increments in speed acquired in equal times in direct vertical fall are the same everywhere – e.g., the distance of fall in the first second is the same everywhere on earth.**

The Parabola

The locus of points equidistant from a point (the *focus*) and a straight line (the *directrix*). Like the circle, up to similarity there is but one parabola, with scaling factor *p* = half the distance between the directrix and focus.



Galileo's Parabola



p: sublimity

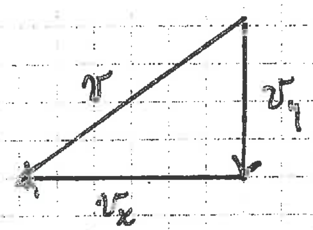
h: altitude

a: amplitude

$$x = v_0 t \qquad y = \tfrac{1}{2} g t^2 \qquad v_0 = \sqrt{2gp}$$

so that $y = x^2/4p = (g/2v_0^2)\,x^2$

$$\tan\theta = dy/dx = x/2p$$



$$v = \sqrt{2gp + 2gy}$$

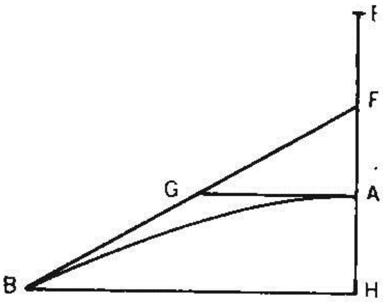
At impact, $y = h$, $x = a$

so that $h = a^2/4p$ and $a = 2\sqrt{hp}$

$$\tan\theta_{imp} = a/2p = \sqrt{h/p}$$

$$v_{imp}/v_0 = 1/\cos\theta_{imp} = \sqrt{1 + h/p}$$

Proposition 5, Problem 2. **In the axis of a given parabola extended [upward], to find a high point from which a falling body describes this same parabola [when deflected horizontally at its vertex].**



Let there be a parabola AB whose amplitude is HB and whose axis is HE. We seek the sublimity from which a falling body, being turned horizontally with the impetus acquired at A, describes the said parabola. Draw of horizontal AG parallel to BH, and putting AF equal to AH, draw the straight line FB tangent to the parabola at B, which intersects the horizontal line AG at G. Take AE, the third proportional to FA and AG; I say that E is the high point sought, from which a body falling from rest at E, and turned into the horizontal with the impetus acquired at A, where there supervenes the impetus of fall to H [as if] from rest at A, will describe the parabola AB….

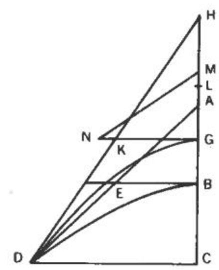
Corollary. **From this it follows that one half the base, or amplitude, of a semiparabola (which is one-quarter the amplitude of the whole parabola) is a mean proportional between its altitude and the sublimity from which a falling [body] would describe it.**

That is, in modern form, the specific parabola is given by:

$$y = \frac{1}{4p}x^2$$

$$\frac{a}{2} = \sqrt{hp}$$

Proposition 7, Theorem 4. **In projectiles by which semiparabolas of the same amplitude are described, less impetus is required for the describing of one whose amplitude is double its altitude than any other.**



Let semiparabola BD be one whose amplitude CD is double its altitude CB; and in the axis extended upward, take BA equal to the altitude BC. Draw AD, which will be tangent to the semiparabola at D and will intersect the horizontal BE at E, while BE will be equal to BC (or BA). It follows that this [curve] will be described by a projectile whose equable horizontal impetus is that of fall to C from rest at B. From this it is evident that the impetus compounded from these and impinging on point D is as the diagonal AE, equal in square to both [CD and DB]….

Corollary. **From this it is clear that in reverse [direction] through the semiparabola DB, the projectile from point D requires less impetus than through any other having greater or smaller elevation than semiparabola BD, which [elevation] is according to the tangent AD and contains one-half a right angle with the horizontal. Hence it follows that if projections are made with the same impetus from point D, but according to different elevations, the maximum projection, or amplitude of semiparabola (or whole parabola) will be that corresponding to the elevation of half a right angle. The others, made according to larger or small angles, will be shorter [in range].**

That is, because:

$$\tan \theta_D = \frac{a}{2p}$$

***Sagredo*. The force of necessary demonstrations is full of marvel and delight; and such are mathematical [demonstrations] alone. I already knew, by trusting to the accounts of mny bombardiers, that the maximum of all ranges of shots, for artillery pieces or mortars – that is, that shot which takes the ball farthest – is the one made at elevation of half a right angle, which they call "at the sixth point of the [Tartaglia's gunner's] square." But to understand the reason for this phenomenon infinitely surpasses the simple idea obtained from the statements of others, or even from experience many times repeated.**

***Saviati*. You say well. The knowledge of one single effect acquired through its causes opens the mind to the understanding and certainty of other effects without need of recourse to experiments. That is exactly what happens in the present instance ; for having gained by demonstrative reasoning the certainty that the maximum of all ranges of shots is that of elevation at half a right angle, the Author demonstrates to us something that has perhaps not been observed through experiment; and this is that of the other shots, those are equal [in range] to one another whose elevations exceed or fall short of half a right angle by equal angles.**

That is, because:

$$\tan \theta_D = \frac{a}{2p}$$

[TABLE 1]

Amplitudes of semiparabolas described with the same initial speed.

Angle of Elevation		Angle of Elevation
45°	10000	
46	9994	44°
47	9976	43
48	9945	42
49	9902	41
50	9848	40
51	9782	39
52	9704	38
53	9612	37
54	9511	36
55	9396	35
56	9272	34
57	9136	33
58	8989	32
59	8829	31
60	8659	30
61	8481	29
62	8290	28
63	8090	27
64	7880	26
65	7660	25
66	7431	24
67	7191	23
68	6944	22
69	6692	21
70	6428	20
71	6157	19
72	5878	18
73	5592	17
74	5300	16
75	5000	15
76	4694	14
77	4383	13
78	4067	12
79	3746	11
80	3420	10
81	3090	9
82	2756	8
83	2419	7
84	2079	6
85	1736	5
86	1391	4
87	1044	3
88	698	2
89	349	1

[TABLE 2]

Altitudes of semiparabolas described with the same initial speed.

Angle of Elevation		Angle of Elevation	
1°	3	46°	5173
2	13	47	5346
3	28	48	5523
4	50	49	5698
5	76	50	5868
6	108	51	6038
7	150	52	6207
8	194	53	6379
9	245	54	6546
10	302	55	6710
11	365	56	6873
12	432	57	7033
13	506	58	7190
14	585	59	7348
15	670	60	7502
16	760	61	7649
17	855	62	7796
18	955	63	7939
19	1060	64	8078
20	1170	65	8214
21	1285	66	8346
22	1402	67	8474
23	1527	68	8597
24	1685	69	8715
25	1786	70	8830
26	1922	71	8940
27	2061	72	9045
28	2204	73	9144
29	2351	74	9240
30	2499	75	9330
31	2653	76	9415
32	2810	77	9493
33	2967	78	9567
34	3128	79	9636
35	3289	80	9698
36	3456	81	9755
37	3621	82	9806
38	3793	83	9851
39	3962	84	9890
40	4132	85	9924
41	4302	86	9951
42	4477	87	9972
43	4654	88	9987
44	4827	89	9998
45	5000	90	10000

[TABLE 3]

Giving the altitudes and sublimities of parabolas of constant amplitude, namely 10000, computed for each degree of elevation.

Angle of Elevation	Altitude	Sublimity	Angle of Elevation	Altitude	Sublimity
1°	87	286533	46°	5177	4828
2	175	142450	47	5363	4662
3	262	95802	48	5553	4502
4	349	71531	49	5752	4345
5	437	57142	50	5959	4196
6	525	47573	51	6174	4048
7	614	40716	52	6399	3906
8	702	35587	53	6635	3765
9	792	31565	54	6882	3632
10	881	28367	55	7141	3500
11	972	25720	56	7413	3372
12	1063	23518	57	7699	3247
13	1154	21701	58	8002	3123
14	1246	20056	59	8332	3004
15	1339	18663	60	8600	2887
16	1434	17405	61	9020	2771
17	1529	16355	62	9403	2658
18	1624	15389	63	9813	2547
19	1722	14522	64	10251	2438
20	1820	13736	65	10722	2331
21	1919	13024	66	11230	2226
22	2020	12376	67	11779	2122
23	2123	11778	68	12375	2020
24	2226	11230	69	13025	1919
25	2332	10722	70	13237	1819
26	2439	10253	71	14521	1721
27	2547	9814	72	15388	1624
28	2658	9404	73	16354	1528
29	2772	9020	74	17437	1433
30	2887	8659	75	18660	1339
31	3008	8336	76	20054	1246
32	3124	8001	77	21657	1154
33	3247	7699	78	23523	1062
34	3373	7413	79	25723	972
35	3501	7141	80	28356	881
36	3633	6882	81	31569	792
37	3768	6635	82	35577	702
38	3906	6395	83	40222	613
39	4049	6174	84	47572	525
40	4196	5959	85	57150	437
41	4346	5752	86	71503	349
42	4502	5553	87	95405	262
43	4662	5362	88	143181	174
44	4828	5177	89	286499	87
45	5000	5000	90	infinity	[zero]

Calibrating Galileo's Tables

1. **For a given initial velocity (i.e. charge, cannonball, and cannon), measure the range for a 45° angle:** *actual-range$_{45}$*

2. **For all other angles, multiply the value in the amplitude vs. angle table divided by 10000 by** *actual-range$_{45}$*

i.e.

$$predicted\ range_\theta = \frac{theoretical\ \mathbf{range}_\theta}{theoretical\ range_{45}} \times actual\ range_{45}$$

where

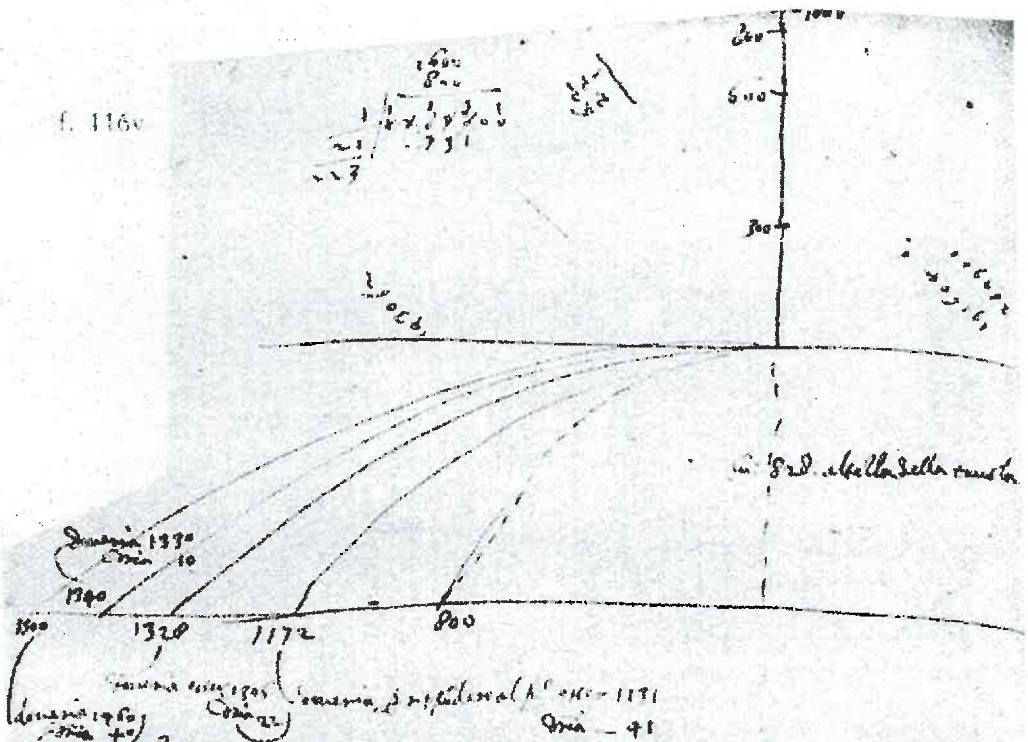
$$actual\ range_{45} = theoretical\ range_{45} - resist\ loss_{45}$$

Therefore, to the extent that *resist-loss$_\theta$* **is proportional to** *theoretical-range$_\theta$*, **so that the fraction of the theoretical range that is lost to air resistance is always proportional to the range that would occur in the absence of air resistance,** *predicted-range$_\theta$* **will match** *actual-range$_\theta$*

In other words, Galileo's tables, as formulated in terms of ratios and then "calibrated," can yield more accurate predictions than if they had been formulated in terms of calculated ranges in the absence of air resistance – a standard engineering technique that serves to compensate for, and hence suppress, intractable sources of discrepancy

Question: ***Suppose that the predicted ranges had agreed with observation to a reasonably high degree – i.e. suppose the tables had "worked" in practice; to what extent would that have provided evidence for Galileo's theory of projectile motion in the absence of air resistance?***

f. 116v



Galileo's "Ski-Jump" Experiment
Folio 116v

Fourth Day, Prop. 5, Corol.: Hence it follows that half the base, or amplitude, of the semi-parabola is a mean proportional between its altitude and the sublimity from which a falling body will describe this parabola.

i.e. $a = 2\sqrt{(hp)}$

sublimity p	altitude h	mean proportional	theoretical a	Galileo's measured a	% difference
300	828	498	996	800	19.7
600	828	705	1410	1172	16.9
800	828	814	1628	1328	18.4
828	**828**	**828**	**1656**	**1340**	**19.1**
1000	828	910	1820	1500	17.6

With p from rolling sphere in a groove of width = 4/9 diameter of sphere, % difference should be 18.3.

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

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

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

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

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

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

This state of affairs calls for a sober re-examination of these folios. What aspects of his insights about motion did Galileo put to the test? How precise were his experiments? What conclusions can legitimately and compellingly be drawn from Galileo's record

of them? Is there indeed convincing evidence that they were successful? The answer to these questions is the purpose of the discussion that follows. The focus will be on the folios themselves (rather than the reconstruction of the experiments) and on related aspects of the *Discorsi*. The folios 116v, 81r, and 114v and all the information on them are reproduced below. The originals can be studied at either of the websites listed above. The organization of the calculations on 116v and 114v into rectangular "frames" follows the practice of the websites.

Archive for History of Exact Sciences 56 (2002), 339–361

3. The experiment of folio 116v

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

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

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

$$800, \ 1172, \ 1328, \ 1340, \text{ and } 1500 \tag{a}$$

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

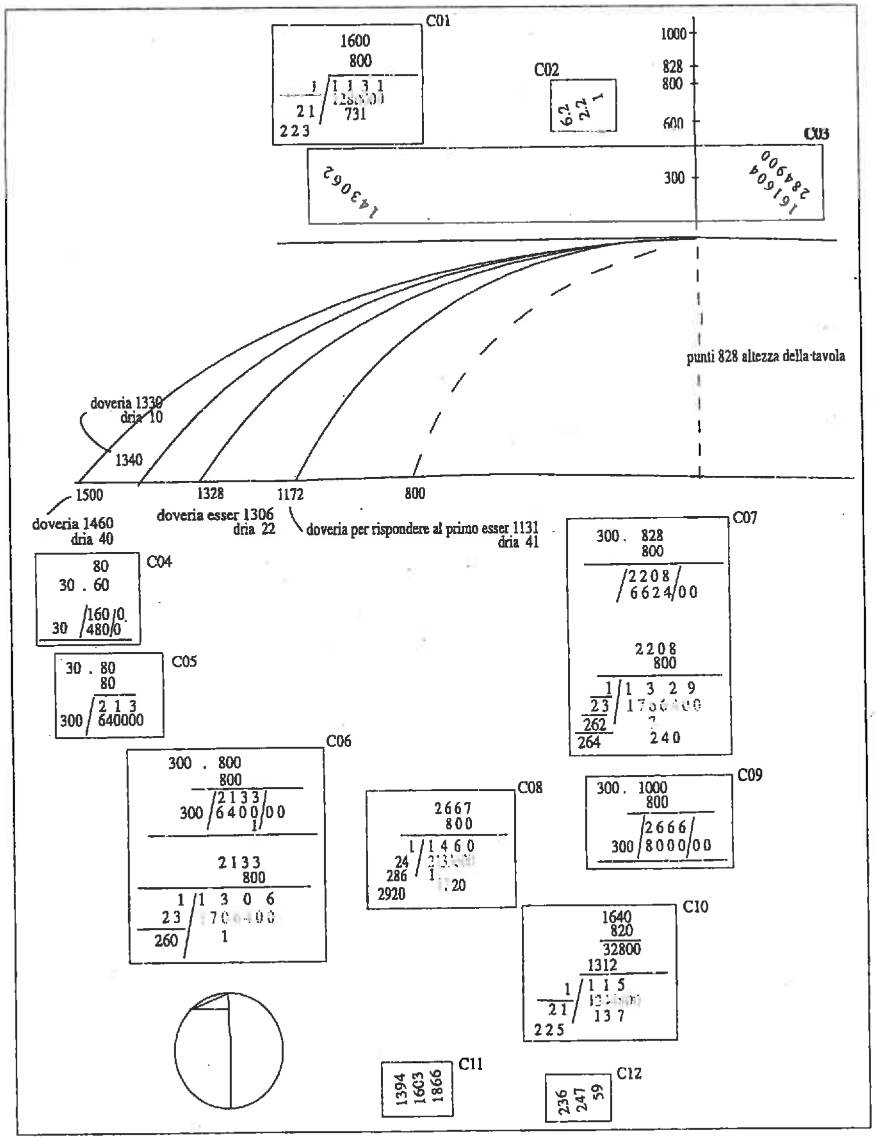
3A. *Understanding the folio*

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

h = the height of the ball above the table, and
d = the distance from the ball to the bottom of the inclined plane.

Now release the ball and let

t = the time it takes for the ball to descend to the bottom of the plane,
v = the speed of the ball at time t. This is also the speed of the ball at the beginning of its fall from the table. Finally, let
R = the distance from the point of impact of the ball to the point on the floor precisely below the starting point of the ball's flight.



Folio 116v (size of original: 306 by 207 mm)

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

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

as well as the square law

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

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

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

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

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

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

$$\frac{R^2}{R_0^2} = \frac{h}{h_0} \,. \qquad \text{(v)}$$

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

$$R = \frac{800}{\sqrt{300}}\sqrt{h} \,. \qquad \text{(vi)}$$

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

$$—,\ 1131,\ 1306,\ 1330,\ \text{and } 1460\ . \qquad \text{(b)}$$

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

Galileo records these numbers on the folio with the phrase *doveria esser* (or simply *doveria*) meaning "ought to be." He also includes his calculations. For example, the calculation for $h = 600$ is carried out in frame C01. Galileo first computes

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

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

We turn next to the question of the precision of the experiment of folio 116v. We will test the accuracy of the experimental values (a) against the predictions of elementary mechanics. (Galileo's theory can't be used because it depends on his experiment.) We will only outline these matters here. The details are available in many texts, for example, in Chap. 9.3 of the basic calculus text [42].[2] Note that the analysis that follows goes far beyond what Galileo was familiar with.

3B. *The underlying mathematics*

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

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

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

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

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

and therefore,

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

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

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

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

$$R = 2\sqrt{\frac{5}{7}828}\sqrt{h} \ . \tag{vii}$$

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

$$842, \ 1191, \ 1376, \ 1400, \ \text{and } 1538 \tag{c}$$

for the corresponding distances R.

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

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

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

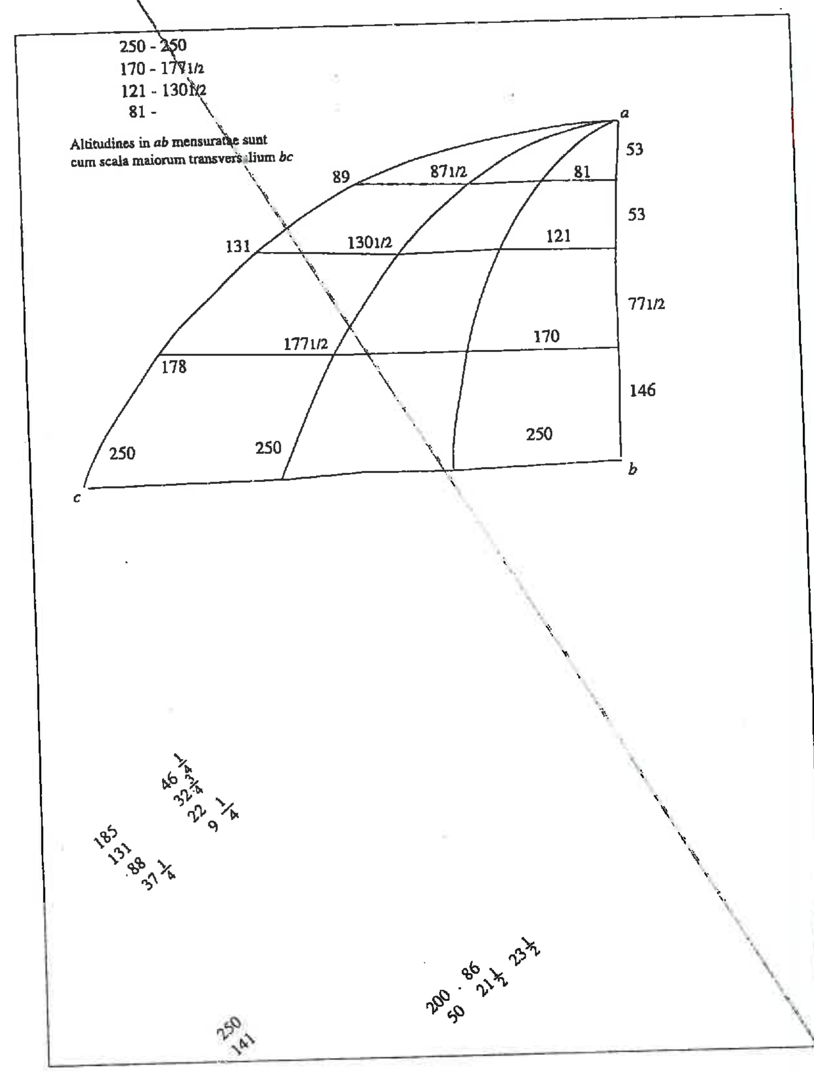
This equation also applies to a groove with a cross-section in the shape of an isosceles triangle, if w is taken to be the distance between the two points of contact of the ball with the groove. Let $y_0 = 828$ punti be the height of the table. Because $\frac{d^2}{d^2-w^2} > 1$, the value of equation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

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

4. The experiment of folio 81r

There is a consensus among historians – see [18], [35], and [37, Chap. 8] – that folio 81r focusses its attention on the trajectories of balls that are propelled obliquely into

space after having descended down an inclined plane placed on a table. In important contrast to folio 116v, the balls drop directly from the inclined plane and there is no horizontal deflection. Each of the three curves on the folio corresponds to a certain fixed angle of inclination of the plane and fixed starting height of the ball. In repeated trials Galileo intercepts the flight of the ball with horizontal planes placed at different heights and marks the points of impact. Evidently, he starts by placing the intercepting plane at a distance of $53 + 53 + 77\frac{1}{2} + 146 = 329\frac{1}{2}$ punti below the plane of the table and "calibrates" the three trajectories so that the the points of impact are at the respective horizontal distances of 250, $250+250 = 500$, and $250+250+250 = 750$ punti from the



Folio 81r (size of original: 304 by 205 mm)

Eight Galilean Principles

In the absence of a resisting medium:

1. **Direct vertical fall is a uniformly accelerated motion: $s \propto t^2$.**

2. **The same speed is acquired from any given height whether in direct fall or along an inclined plane.**

3. **The speed acquired in descent from any height is exactly sufficient to raise the object back to that height.**

4. **Speed acquired from any given height is the same for all objects, regardless of their weight (or shape).**

5. **In the absence of impediments, motion along the horizontal remains uniform (at least over distances small in comparison with the radius of the Earth).**

6. **The two components of motion of a body moving uniformly in parallel with the horizontal and simultaneously falling vertically remain independent of one another.**

7. **A body projected horizontally describes, to high approximation, a semi-parabola (at least over distances small in comparison with the radius of the Earth), with its dimensions dictated by the height from which the body's initial horizontal speed would be acquired naturally; and, by symmetry, a body that is projected at an acute angle upwards describes a corresponding full parabola, with its dimensions dictated by the initial velocity and angle of projection.**

8. **The distance of fall from rest in the first second (i.e. *g*/2) is the same at every location around the Earth.**

Questions Raised by Galileo's Theory of Local Motion

1. **Do the four fundamental principles of fall in the absence of a resisting medium – uniform acceleration, irrelevance of weight, shape, etc., pathwise independence, and return to height in ascent – hold exactly or only approximately, and if the latter, in the mean or otherwise?**

2. **Given that parabolic projection does not hold exactly, even in the absence of a resisting medium:**

   a. **What is the true trajectory of a projectile near the surface of the earth?**

   b. **What would the continuing trajectory be if a body were to continue to the center of the earth without impediment or resistance?**

   c. **What is the trajectory in the presence of air resistance?**

3. **Does a body really gain the same increments in velocity in equal increments of time in vertical fall in the absence of air resistance everywhere on the surface of the earth?**

4. **How far above the surface of the earth and below it does the distance of vertical fall in the first second in the absence of air resistance remain what it is at the surface?**

Because I too, among so many others, have had the idea to submit to the judgment of the public what I think not only about the location and motion of this light but also about its substance and its origin and because I believe I have found an opinion which contains no obvious contradictions and which, therefore, might be true, it has been necessary for me, so that I might be sure of myself, to go ahead slowly and await the return of this star in the east after its separation from the Sun, and to observe again very diligently what changes it might have undergone both in its location as well as in its visible brightness and the quality of its light; and continuing my speculations about this marvel, ***I have finally come to believe that I could know something more than what ends in mere conjecture.*** And because this fantasy of mine draws out, or rather puts forth, most weighty consequences and conclusions, I have resolved to change my lessons in one part of the discourse, which I am now elaborating in regard to this material. (X, pp. 134-135, italics added)

Galileo (1605) on the Supernova of 1604

Beyond Mere Conjecture: Alternative Conceptions

1. **A "science" seeking detailed agreement, within observational accuracy, between all of its predictions and the results of experiments in which "external" confounding effects have been suitably controlled.**

   Discrepancies: a source of continuing evidence

2. **A "science" seeking agreement between its more striking (mathematical) consequences and the results of select experiments centered on unusual, distinctive phenomena to a degree sufficient for it not to be clearly falsified.**

   Discrepancies: to be explained away

3. **A "science" seeking, once it is suitably calibrated, sufficiently good agreement with empirical phenomena of interest for it to serve practical purposes – in prediction primarily, but also in explanation.**

   Discrepancies: to be "swept under the rug"

Experiment in *Two New Sciences*

Galilean Innovations

1. Because of the need to eliminate or at least to control for real-world complexities, mere observation and intervention in nature are not enough; experiments need to be *designed* and *developed*, leading to their often involving highly contrived situations that never occur in nature.

2. Theory can and should play a large role in the design of experiments, first in singling out situations in which the results can be most telling, second in supplying enhanced means for indirect measurement, and third in providing justification for background assumptions.

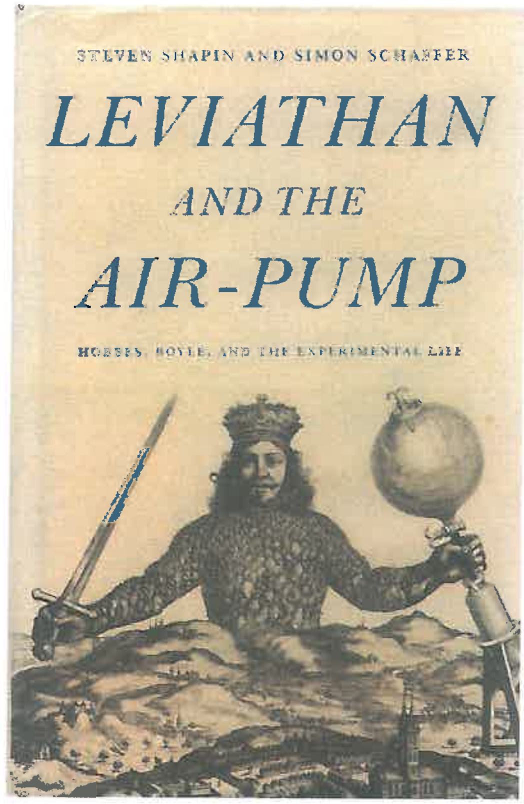
Uses of Experiment

1. To falsify opposing (e.g. Aristotelian) theoretical claims

2. To justify initial conceptual assumptions (generally by means of qualitative "cross-roads" experiments)

3. To confirm theory via successful salient predictions (or more modestly, via failure to falsify)

4. (*Mersenne and Riccioli*: to measure the constants of a theory, especially constants of proportionality)



"Establishing matters of fact did require immense amounts of labour. Here we endeavor to recover this labour for our historiographical purposes: ***to show the inadequacy of the [historiographical] method which regards experimentally produced matters of fact as self-evident and self-explanatory.***" [p. 225, italics added]