Dialogues Concerning
TWO NEW SCIENCES

GALILEO GALILEI

TRANSLATED BY
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"La Dynamique est la science des forces accélératrices or retardatrices, et des mouvemens variés qu'elles doivent produire. Cette science est due entièrement aux modernes, et Galilée est celui qui en a jeté les premiers fondemens." Lagrange *Mec. Anal.* I. 221.

TRANSLATORS' PREFACE

FOR more than a century English speaking students have been placed in the anomalous position of hearing Galileo constantly referred to as the founder of modern physical science, without having any chance to read, in their own language, what Galileo himself has to say. Archimedes has been made available by Heath; Huygens' *Light* has been turned into English by Thompson, while Motte has put the *Principia* of Newton back into the language in which it was conceived. To render the Physics of Galileo also accessible to English and American students is the purpose of the following translation.

The last of the great creators of the Renaissance was not a prophet without honor in his own time; for it was only one group of his country-men that failed to appreciate him. Even during his life time, his *Mechanics* had been rendered into French by one of the leading physicists of the world, Mersenne.

Within twenty-five years after the death of Galileo, his *Dialogues on Astronomy*, and those on *Two New Sciences*, had been done into English by Thomas Salusbury and were worthily printed in two handsome quarto volumes. The *Two New Sciences*, which contains practically all that Galileo has to say on the subject of physics, issued from the English press in 1665.

It is supposed that most of the copies were destroyed in the great London fire which occurred in the year following. We are not aware of any copy in America: even that belonging to the British Museum is an imperfect one.

Again in 1730 the *Two New Sciences* was done into English by Thomas Weston; but this book, now nearly two centuries old, is scarce and expensive. Moreover, the literalness with which this translation was made renders many passages either ambiguous or unintelligible to the modern reader. Other than these two, no English version has been made.

Quite recently an eminent Italian scholar, after spending thirty of the best years of his life upon the subject, has brought to completion the great National Edition of the Works of Galileo. We refer to the twenty superb volumes in which Professor Antonio Favaro of Padua has given a definitive presentation of the labors of the man who created the modern science of physics.

The following rendition includes neither *Le Mechaniche* of Galileo nor his paper *De Motu Accelerato*, since the former of these contains little but the Statics which was current before the time of Galileo, and the latter is essentially included in the Dialogue of the Third Day. Dynamics was the one subject to which under various forms, such as Ballistics, Acoustics, Astronomy, he consistently and persistently devoted his whole life. Into the one volume here translated he seems to have gathered, during his last years, practically all that is of value either to the engineer or the physicist. The historian, the philosopher, and the astronomer will find the other volumes replete with interesting material.

It is hardly necessary to add that we have strictly followed the text of the National Edition—essentially the Elzevir edition of 1638. All comments and annotations have been omitted save here and there a foot-note intended to economize the reader's time. To each of these footnotes has been attached the signature [*Trans.*] in order to preserve the original as nearly intact as possible.

Much of the value of any historical document lies in the language employed, and this is doubly true when one attempts to trace the rise and growth of any set of concepts such as those employed in modern physics. We have therefore made this translation as literal as is consistent with clearness and modernity. In cases where there is any important deviation from this rule, and in the case of many technical terms where there is no deviation from it, we have given the original Italian or Latin phrase in italics enclosed in square brackets. The intention here is to illustrate the great variety of terms employed by the early physicists to describe a single definite idea, and conversely, to illustrate the numerous senses in which, then as now, a single word is used. For the few explanatory English words which are placed in square brackets without italics, the translators alone are responsible. The paging of the National Edition is indicated in square brackets inserted along the median line of the page.

The imperfections of the following pages would have been many more but for the aid of three of our colleagues. Professor D. R. Curtiss was kind enough to assist in the translation of those pages which discuss the nature of Infinity: Professor O. H. Basquin gave valuable help in the rendition of the chapter on Strength of Materials; and Professor O. F. Long cleared up the meaning of a number of Latin phrases.

To Professor A. Favaro of the University of Padua the translators share, with every reader, a feeling of sincere obligation for his Introduction.

H. C.
A. DE S.

EVANSTON, ILLINOIS,
15 *February*, 1914.

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INTRODUCTION

WRITING to his faithful friend Elia Diodati, Galileo speaks of the "New Sciences" which he had in mind to print as being "superior to everything else of mine hitherto published"; elsewhere he says "they contain results which I consider the most important of all my studies"; and this opinion which he expressed concerning his own work has been confirmed by posterity: the "New Sciences" are, indeed, the masterpiece of Galileo who at the time when he made the above remarks had spent upon them more than thirty laborious years.

One who wishes to trace the history of this remarkable work will find that the great philosopher laid its foundations during the eighteen best years of his life—those which he spent at Padua. As we learn from his last scholar, Vincenzio Viviani, the numerous results at which Galileo had arrived while in this city, awakened intense admiration in the friends who had witnessed various experiments by means of which he was accustomed to investigate interesting questions in physics. Fra Paolo Sarpi exclaimed: To give us the Science of Motion, God and Nature have joined hands and created the intellect of Galileo. And when the "New Sciences" came from the press one of his foremost pupils, Paolo Aproino, wrote that the volume contained much which he had "already heard from his own lips" during student days at Padua.

Limiting ourselves to only the more important documents which might be cited in support of our statement, it will suffice to mention the letter, written to Guidobaldo del Monte on the 29th of November, 1602, concerning the descent of heavy bodies along the arcs of circles and the chords subtended by them; that to Sarpi, dated 16th of October, 1604, dealing with the free fall of heavy bodies; the letter to Antonio de' Medici on the 11th of February, 1609, in which he states that he has "completed all the theorems and demonstrations pertaining to forces and resistances of beams of various lengths, thicknesses and shapes, proving that they are weaker at the middle than near the ends, that they can carry a greater load when that load is distributed throughout the length of the beam than when concentrated at one point, demonstrating also what shape should be given to a beam in order that it may have the same bending strength at every point," and that he was now engaged "upon some questions dealing with the motion of projectiles"; and finally in the letter to Belisario Vinta, dated 7th of May, 1610, concerning his return from Padua to Florence, he enumerates various pieces of work which were still to be completed, mentioning explicitly three books on an entirely new science dealing with the theory of motion. Although at various times after the return to his native state he devoted considerable thought to the work which, even at that date, he had in mind as is shown by certain fragments which clearly belong to different periods of his life and which have, for the first time, been published in the National Edition; and although these studies were always uppermost in his thought it does not appear that he gave himself seriously to them until after the publication of the *Dialogue* and the completion of that trial which was rightly described as the disgrace of the century. In fact as late as October, 1630, he barely mentions to Aggiunti his discoveries in the theory of motion, and only two years later, in a letter to Marsili concerning the motion of projectiles, he hints at a book nearly ready for publication in which he will treat also of this subject; and only a year after this he writes to Arrighetti that he has in hand a treatise on the resistance of solids.

But the work was given definite form by Galileo during his enforced residence at Siena: in these five months spent quietly with the Archbishop he himself writes that he has completed "a treatise on a new branch of mechanics full of interesting and useful ideas"; so that a few months later he was able to send word to Micanzio that the "work was ready"; as soon as his friends learned of this, they urged its publication. It was, however, no easy matter to print the work of a man already condemned by the Holy Office: and since Galileo could not hope to print it either in Florence or in Rome, he turned to the faithful Micanzio asking him to find out whether this would be possible in Venice, from whence he had received offers to print the *Dialogue on the Principal Systems*, as soon as the news had reached there that he was encountering difficulties. At first everything went smoothly; so that Galileo commenced sending to Micanzio some of the manuscript which was received by the latter with an enthusiasm in which he was second to none of the warmest admirers of the great philosopher. But when Micanzio consulted the Inquisitor, he received the answer that there was an express order prohibiting the printing or reprinting of any work of Galileo, either in Venice or in any other place, *nullo excepto.*

As soon as Galileo received this discouraging news he began to look with more favor upon offers which had come to him from Germany where his friend, and perhaps also his scholar, Giovanni Battista Pieroni, was in the service of the Emperor, as military engineer; consequently Galileo gave to Prince Mattia de' Medici who was just leaving for Germany the first two Dialogues to be handed to Pieroni who was undecided whether to publish them at Vienna or Prague or at some place in Moravia; in the meantime, however, he had obtained permission to print both at Vienna and at Olmütz. But Galileo recognized danger at every point within reach of the long arm of the Court of Rome; hence, availing himself of the opportunity offered by the arrival of Louis Elzevir in Italy in 1636, also of the friendship between the latter and Micanzio, not to mention a visit at Arcetri, he decided to abandon all other plans and entrust to the Dutch publisher the printing of his new work the manuscript of which, although not complete, Elzevir took with him on his return home.

In the course of the year 1637, the printing was finished, and at the beginning of the following year there was lacking only the index, the title-page and the dedication. This last had, through the good offices of Diodati, been offered to the Count of Noailles, a former scholar of Galileo at Padua, and since 1634 ambassador of France at Rome, a man who did much to alleviate the distressing consequences of the celebrated trial; and the offer was gratefully accepted. The phrasing of the dedication deserves brief comment. Since Galileo was aware, on the one hand, of the prohibition against the printing of his works and since, on the other hand, he did not wish to irritate the Court of Rome from whose hands he was always hoping for complete freedom, he pretends in the dedicatory letter (where, probably through excess of caution, he gives only main outlines) that he had nothing to do with the printing of his book, asserting that he will never again publish any of his researches, and will at most distribute here and there a manuscript copy. He even expresses great surprise that his new Dialogues have fallen into the hands of the Elzevirs and were soon to be published; so that, having been asked to write a dedication, he could think of no man more worthy who could also on this occasion defend him against his enemies.

As to the title which reads: *Discourses and Mathematical Demonstrations concerning Two New Sciences pertaining to Mechanics and Local Motions*, this only is known, namely, that the title is not the one which Galileo had devised and suggested; in fact he protested against the publishers taking the liberty of changing it and substituting "a low and common title for the noble and dignified one carried upon the title-page."

In reprinting this work in the National Edition, I have followed the Leyden text of 1638 faithfully but not slavishly, because I wished to utilize the large amount of manuscript material which has come down to us, for the purpose of correcting a considerable number of errors in this first edition, and also for the sake of inserting certain additions desired by the author himself. In the Leyden Edition, the four Dialogues are followed by an "*Appendix containing some theorems and their proofs, dealing with centers of gravity of solid bodies, written by the same Author at an earlier date*," which has no immediate connection with the subjects treated in the Dialogues; these theorems were found by Galileo, as he tells us, "at the age of twenty-two and after two years study of geometry" and were here inserted only to save them from oblivion.

But it was not the intention of Galileo that the *Dialogues on the New Sciences* should contain only the four Days and the above-mentioned appendix which constitute the Leyden Edition; while, on the one hand, the Elzevirs were hastening the printing and striving to complete it at the earliest possible date, Galileo, on the other hand, kept on speaking of another Day, besides the four, thus embarrassing and perplexing the printers. From the correspondence which went on between author and publisher, it appears that this Fifth Day was to have treated "of the force of percussion and the use of the catenary"; but as the typographical work approached completion, the printer became anxious for the book to issue from the press without further delay; and thus it came to pass that the *Discorsi e Dimostrazioni* appeared containing only the four Days and the Appendix, in spite of the fact that in April, 1638, Galileo had plunged more deeply than ever "into the profound question of percussion" and "had almost reached a complete solution."

The "New Sciences" now appear in an edition following the text which I, after the most careful and devoted study, determined upon for the National Edition. It appears also in that language in which, above all others, I have desired to see it. In this translation, the last and ripest work of the great philosopher makes its first appearance in the New World: if toward this important result I may hope to have contributed in some measure I shall feel amply rewarded for having given to this field of research the best years of my life.

ANTONIO FAVARO.

UNIVERSITY OF PADUA,
27th of October, 1913.

FIRST DAY

INTERLOCUTORS: SALVIATI, SAGREDO AND SIMPLICIO

SALV. The constant activity which you Venetians display in your famous arsenal suggests to the studious mind a large field for investigation, especially that part of the work which involves mechanics; for in this department all types of instruments and machines are constantly being constructed by many artisans, among whom there must be some who, partly by inherited experience and partly by their own observations, have become highly expert and clever in explanation.

SAGR. You are quite right. Indeed, I myself, being curious by nature, frequently visit this place for the mere pleasure of observing the work of those who, on account of their superiority over other artisans, we call "first rank men." Conference with them has often helped me in the investigation of certain effects including not only those which are striking, but also those which are recondite and almost incredible. At times also I have been put to confusion and driven to despair of ever explaining something for which I could not account, but which my senses told me to be true. And notwithstanding the fact that what the old man told us a little while ago is proverbial and commonly accepted, yet it seemed to me altogether false, like many another saying which is current among the ignorant; for I think they introduce these expressions in order to give the appearance of knowing something about matters which they do not understand.

Salv.

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SALV. You refer, perhaps, to that last remark of his when we asked the reason why they employed stocks, scaffolding and bracing of larger dimensions for launching a big vessel than they do for a small one; and he answered that they did this in order to avoid the danger of the ship parting under its own heavy weight [*vasta mole*], a danger to which small boats are not subject?

SAGR. Yes, that is what I mean; and I refer especially to his last assertion which I have always regarded as a false, though current, opinion; namely, that in speaking of these and other similar machines one cannot argue from the small to the large, because many devices which succeed on a small scale do not work on a large scale. Now, since mechanics has its foundation in geometry, where mere size cuts no figure, I do not see that the properties of circles, triangles, cylinders, cones and other solid figures will change with their size. If, therefore, a large machine be constructed in such a way that its parts bear to one another the same ratio as in a smaller one, and if the smaller is sufficiently strong for the purpose for which it was designed, I do not see why the larger also should not be able to withstand any severe and destructive tests to which it may be subjected.

SALV. The common opinion is here absolutely wrong. Indeed, it is so far wrong that precisely the opposite is true, namely, that many machines can be constructed even more perfectly on a large scale than on a small; thus, for instance, a clock which indicates and strikes the hour can be made more accurate on a large scale than on a small. There are some intelligent people who maintain this same opinion, but on more reasonable grounds, when they cut loose from geometry and argue that the better performance of the large machine is owing to the imperfections and variations of the material. Here I trust you will not charge

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me with arrogance if I say that imperfections in the material, even those which are great enough to invalidate the clearest mathematical proof, are not sufficient to explain the deviations observed between machines in the concrete and in the abstract. Yet I shall say it and will affirm that, even if the imperfections did

did not exist and matter were absolutely perfect, unalterable and free from all accidental variations, still the mere fact that it is matter makes the larger machine, built of the same material and in the same proportion as the smaller, correspond with exactness to the smaller in every respect except that it will not be so strong or so resistant against violent treatment; the larger the machine, the greater its weakness. Since I assume matter to be unchangeable and always the same, it is clear that we are no less able to treat this constant and invariable property in a rigid manner than if it belonged to simple and pure mathematics. Therefore, Sagredo, you would do well to change the opinion which you, and perhaps also many other students of mechanics, have entertained concerning the ability of machines and structures to resist external disturbances, thinking that when they are built of the same material and maintain the same ratio between parts, they are able equally, or rather proportionally, to resist or yield to such external disturbances and blows. For we can demonstrate by geometry that the large machine is not proportionately stronger than the small. Finally, we may say that, for every machine and structure, whether artificial or natural, there is set a necessary limit beyond which neither art nor nature can pass; it is here understood, of course, that the material is the same and the proportion preserved.

Sagr. My brain already reels. My mind, like a cloud momentarily illuminated by a lightning-flash, is for an instant filled with an unusual light, which now beckons to me and which now suddenly mingles and obscures strange, crude ideas. From what you have said it appears to me impossible to build two similar structures of the same material, but of different sizes and have them proportionately strong; and if this were so, it would

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not be possible to find two single poles made of the same wood which shall be alike in strength and resistance but unlike in size.

Salv. So it is, Sagredo. And to make sure that we understand each other, I say that if we take a wooden rod of a certain length and size, fitted, say, into a wall at right angles, i. e., parallel

parallel to the horizon, it may be reduced to such a length that it will just support itself; so that if a hair's breadth be added to its length it will break under its own weight and will be the only rod of the kind in the world.* Thus if, for instance, its length be a hundred times its breadth, you will not be able to find another rod whose length is also a hundred times its breadth and which, like the former, is just able to sustain its own weight and no more: all the larger ones will break while all the shorter ones will be strong enough to support something more than their own weight. And this which I have said about the ability to support itself must be understood to apply also to other tests; so that if a piece of scantling [*corrente*] will carry the weight of ten similar to itself, a beam [*trave*] having the same proportions will not be able to support ten similar beams.

Please observe, gentlemen, how facts which at first seem improbable will, even on scant explanation, drop the cloak which has hidden them and stand forth in naked and simple beauty. Who does not know that a horse falling from a height of three or four cubits will break his bones, while a dog falling from the same height or a cat from a height of eight or ten cubits will suffer no injury? Equally harmless would be the fall of a grasshopper from a tower or the fall of an ant from the distance of the moon. Do not children fall with impunity from heights which would cost their elders a broken leg or perhaps a fractured skull? And just as smaller animals are proportionately stronger and more robust than the larger, so also smaller plants are able to stand up better than larger. I am certain you both know that an oak two hundred cubits [*braccia*] high would not be able to sustain its own branches if they were distributed as in a tree of ordinary size; and that nature cannot produce a horse as large as twenty ordinary horses or a giant ten times taller than an

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ordinary man unless by miracle or by greatly altering the proportions of his limbs and especially of his bones, which would have to be considerably enlarged over the ordinary. Likewise the current belief that, in the case of artificial machines the very

* The author here apparently means that the solution is unique. [*Trans.*]

large and the small are equally feasible and lasting is a manifest error. Thus, for example, a small obelisk or column or other solid figure can certainly be laid down or set up without danger of breaking, while the very large ones will go to pieces under the slightest provocation, and that purely on account of their own weight. And here I must relate a circumstance which is worthy of your attention as indeed are all events which happen contrary to expectation, especially when a precautionary measure turns out to be a cause of disaster. A large marble column was laid out so that its two ends rested each upon a piece of beam; a little later it occurred to a mechanic that, in order to be doubly sure of its not breaking in the middle by its own weight, it would be wise to lay a third support midway; this seemed to all an excellent idea; but the sequel showed that it was quite the opposite, for not many months passed before the column was found cracked and broken exactly above the new middle support.

SIMP. A very remarkable and thoroughly unexpected accident, especially if caused by placing that new support in the middle.

SALV. Surely this is the explanation, and the moment the cause is known our surprise vanishes; for when the two pieces of the column were placed on level ground it was observed that one of the end beams had, after a long while, become decayed and sunken, but that the middle one remained hard and strong, thus causing one half of the column to project in the air without any support. Under these circumstances the body therefore behaved differently from what it would have done if supported only upon the first beams; because no matter how much they might have sunken the column would have gone with them. This is an accident which could not possibly have happened to a small column, even though made of the same stone and having a length corresponding to its thickness, i. e., preserving the ratio between thickness and length found in the large pillar.

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SAGR. I am quite convinced of the facts of the case, but I do not understand why the strength and resistance are not multiplied in the same proportion as the material; and I am the more puzzled

puzzled because, on the contrary, I have noticed in other cases that the strength and resistance against breaking increase in a larger ratio than the amount of material. Thus, for instance, if two nails be driven into a wall, the one which is twice as big as the other will support not only twice as much weight as the other, but three or four times as much.

SALV. Indeed you will not be far wrong if you say eight times as much; nor does this phenomenon contradict the other even though in appearance they seem so different.

SAGR. Will you not then, Salviati, remove these difficulties and clear away these obscurities if possible: for I imagine that this problem of resistance opens up a field of beautiful and useful ideas; and if you are pleased to make this the subject of to-day's discourse you will place Simplicio and me under many obligations.

SALV. I am at your service if only I can call to mind what I learned from our Academician * who had thought much upon this subject and according to his custom had demonstrated everything by geometrical methods so that one might fairly call this a new science. For, although some of his conclusions had been reached by others, first of all by Aristotle, these are not the most beautiful and, what is more important, they had not been proven in a rigid manner from fundamental principles. Now, since I wish to convince you by demonstrative reasoning rather than to persuade you by mere probabilities, I shall suppose that you are familiar with present-day mechanics so far as it is needed in our discussion. First of all it is necessary to consider what happens when a piece of wood or any other solid which coheres firmly is broken; for this is the fundamental fact, involving the first and simple principle which we must take for granted as well known.

To grasp this more clearly, imagine a cylinder or prism, AB, made of wood or other solid coherent material. Fasten the upper end, A, so that the cylinder hangs vertically. To the lower end, B, attach the weight C. It is clear that however great they may be, the tenacity and coherence [*tenacità e*

* I. e. Galileo: The author frequently refers to himself under this name. [*Trans.*]

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coerenza] between the parts of this solid, so long as they are not infinite, can be overcome by the pull of the weight C, a weight which can be increased indefinitely until finally the solid breaks like a rope. And as in the case of the rope whose strength we know to be derived from a multitude of hemp threads which compose it, so in the case of the wood, we observe its fibres and filaments run lengthwise and render it much stronger than a hemp rope of the same thickness. But in the case of a stone or metallic cylinder where the coherence seems to be still greater the cement which holds the parts together must be something other than filaments and fibres; and yet even this can be broken by a strong pull.

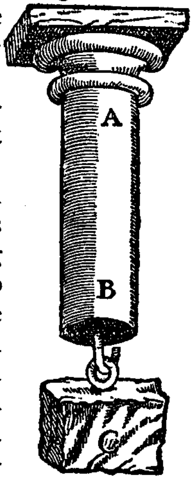

Fig. 1

SIMP. If this matter be as you say I can well understand that the fibres of the wood, being as long as the piece of wood itself, render it strong and resistant against large forces tending to break it. But how can one make a rope one hundred cubits long out of hempen fibres which are not more than two or three cubits long, and still give it so much strength? Besides, I should be glad to hear your opinion as to the manner in which the parts of metal, stone, and other materials not showing a filamentous structure are put together; for, if I mistake not, they exhibit even greater tenacity.

SALV. To solve the problems which you raise it will be necessary to make a digression into subjects which have little bearing upon our present purpose.

SAGR. But if, by digressions, we can reach new truth, what harm is there in making one now, so that we may not lose this knowledge, remembering that such an opportunity, once omitted, may not return; remembering also that we are not tied down to a fixed and brief method but that we meet solely for our own entertainment? Indeed, who knows but that we may thus

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frequently

frequently discover something more interesting and beautiful than the solution originally sought? I beg of you, therefore, to grant the request of Simplicio, which is also mine; for I am no less curious and desirous than he to learn what is the binding material which holds together the parts of solids so that they can scarcely be separated. This information is also needed to understand the coherence of the parts of fibres themselves of which some solids are built up.

SALV. I am at your service, since you desire it. The first question is, How are fibres, each not more than two or three cubits in length, so tightly bound together in the case of a rope one hundred cubits long that great force [*violenza*] is required to break it?

Now tell me, Simplicio, can you not hold a hempen fibre so tightly between your fingers that I, pulling by the other end, would break it before drawing it away from you? Certainly you can. And now when the fibres of hemp are held not only at the ends, but are grasped by the surrounding medium throughout their entire length is it not manifestly more difficult to tear them loose from what holds them than to break them? But in the case of the rope the very act of twisting causes the threads to bind one another in such a way that when the rope is stretched with a great force the fibres break rather than separate from each other.

At the point where a rope parts the fibres are, as everyone knows, very short, nothing like a cubit long, as they would be if the parting of the rope occurred, not by the breaking of the filaments, but by their slipping one over the other.

SAGR. In confirmation of this it may be remarked that ropes sometimes break not by a lengthwise pull but by excessive twisting. This, it seems to me, is a conclusive argument because the threads bind one another so tightly that the compressing fibres do not permit those which are compressed to lengthen the spirals even that little bit by which it is necessary for them to lengthen in order to surround the rope which, on twisting, grows shorter and thicker.

SALV. You are quite right. Now see how one fact suggests another

another. The thread held between the fingers does not yield [57] to one who wishes to draw it away even when pulled with considerable force, but resists because it is held back by a double compression, seeing that the upper finger presses against the lower as hard as the lower against the upper. Now, if we could retain only one of these pressures there is no doubt that only half the original resistance would remain; but since we are not able, by lifting, say, the upper finger, to remove one of these pressures without also removing the other, it becomes necessary to preserve one of them by means of a new device which causes the thread to press itself against the finger or against some other solid body upon which it rests; and thus it is brought about that the very force which pulls it in order to snatch it away compresses it more and more as the pull increases. This is accomplished by wrapping the thread around the solid in the manner of a spiral; and will be better understood by means of a figure. Let AB and CD be two cylinders between which is stretched the thread EF: and for the sake of greater clearness we will imagine it to be a small cord. If these two cylinders be pressed strongly together, the cord EF, when drawn by the end F, will undoubtedly stand a considerable pull before it slips between the two compressing solids. But if we remove one of these cylinders the cord, though remaining in contact with the other, will not thereby be prevented from slipping freely. On the other hand, if one holds the cord loosely against the top of the cylinder A, winds it in the spiral form AFLOTR, and then pulls it by the end R, it is evident that the cord will begin to bind the cylinder; the greater the number of spirals the more tightly will the cord be pressed against the cylinder by any given pull. Thus as the number of turns increases, the line of contact

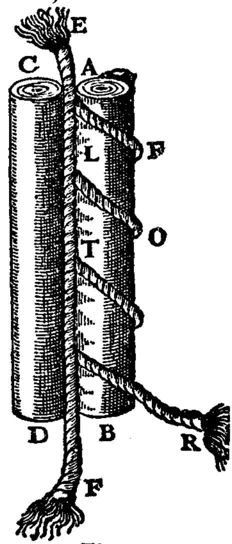

Fig. 2

contact becomes longer and in consequence more resistant; so that the cord slips and yields to the tractive force with increasing difficulty.

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Is it not clear that this is precisely the kind of resistance which one meets in the case of a thick hemp rope where the fibres form thousands and thousands of similar spirals? And, indeed, the binding effect of these turns is so great that a few short rushes woven together into a few interlacing spirals form one of the strongest of ropes which I believe they call pack rope [*susta*].

SAGR. What you say has cleared up two points which I did not previously understand. One fact is how two, or at most three, turns of a rope around the axle of a windlass cannot only hold it fast, but can also prevent it from slipping when pulled by the immense force of the weight [*forza del peso*] which it sustains; and moreover how, by turning the windlass, this same axle, by mere friction of the rope around it, can wind up and lift huge stones while a mere boy is able to handle the slack of the rope. The other fact has to do with a simple but clever device, invented by a young kinsman of mine, for the purpose of descending from a window by means of a rope without lacerating the palms of his hands, as had happened to him shortly before and greatly to his discomfort. A small sketch will make this clear. He took a wooden cylinder, AB, about as thick as a walking stick and about one span long: on this he cut a spiral channel of about one turn and a half, and large enough to just receive the rope which he wished to use. Having introduced the rope at the end A and led it out again at the end B, he enclosed both the cylinder and the rope in a case of wood or tin, hinged along the side so that it could be easily opened and closed. After he had fastened the rope to a firm support above, he could, on grasping and squeezing the case with both hands, hang by his arms. The pressure on the rope, lying between the case and the cylinder, was such that he could, at will, either grasp the case more

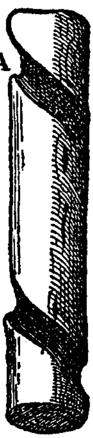

Fig. 3

more tightly and hold himself from slipping, or slacken his hold and descend as slowly as he wished.

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SALV. A truly ingenious device! I feel, however, that for a complete explanation other considerations might well enter; yet I must not now digress upon this particular topic since you are waiting to hear what I think about the breaking strength of other materials which, unlike ropes and most woods, do not show a filamentous structure. The coherence of these bodies is, in my estimation, produced by other causes which may be grouped under two heads. One is that much-talked-of repugnance which nature exhibits towards a vacuum; but this horror of a vacuum not being sufficient, it is necessary to introduce another cause in the form of a gluey or viscous substance which binds firmly together the component parts of the body.

First I shall speak of the vacuum, demonstrating by definite experiment the quality and quantity of its force [*virtù*]. If you take two highly polished and smooth plates of marble, metal, or glass and place them face to face, one will slide over the other with the greatest ease, showing conclusively that there is nothing of a viscous nature between them. But when you attempt to separate them and keep them at a constant distance apart, you find the plates exhibit such a repugnance to separation that the upper one will carry the lower one with it and keep it lifted indefinitely, even when the latter is big and heavy.

This experiment shows the aversion of nature for empty space, even during the brief moment required for the outside air to rush in and fill up the region between the two plates. It is also observed that if two plates are not thoroughly polished, their contact is imperfect so that when you attempt to separate them slowly the only resistance offered is that of weight; if, however, the pull be sudden, then the lower plate rises, but quickly falls back, having followed the upper plate only for that very short interval of time required for the expansion of the small amount of air remaining between the plates, in consequence of their not fitting, and for the entrance of the surrounding air. This resistance which is exhibited between the two plates

plates is doubtless likewise present between the parts of a solid, and enters, at least in part, as a concomitant cause of their coherence.

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SAGR. Allow me to interrupt you for a moment, please; for I want to speak of something which just occurs to me, namely, when I see how the lower plate follows the upper one and how rapidly it is lifted, I feel sure that, contrary to the opinion of many philosophers, including perhaps even Aristotle himself, motion in a vacuum is not instantaneous. If this were so the two plates mentioned above would separate without any resistance whatever, seeing that the same instant of time would suffice for their separation and for the surrounding medium to rush in and fill the vacuum between them. The fact that the lower plate follows the upper one allows us to infer, not only that motion in a vacuum is not instantaneous, but also that, between the two plates, a vacuum really exists, at least for a very short time, sufficient to allow the surrounding medium to rush in and fill the vacuum; for if there were no vacuum there would be no need of any motion in the medium. One must admit then that a vacuum is sometimes produced by violent motion [*violenza*] or contrary to the laws of nature, (although in my opinion nothing occurs contrary to nature except the impossible, and that never occurs).

But here another difficulty arises. While experiment convinces me of the correctness of this conclusion, my mind is not entirely satisfied as to the cause to which this effect is to be attributed. For the separation of the plates precedes the formation of the vacuum which is produced as a consequence of this separation; and since it appears to me that, in the order of nature, the cause must precede the effect, even though it appears to follow in point of time, and since every positive effect must have a positive cause, I do not see how the adhesion of two plates and their resistance to separation—actual facts—can be referred to a vacuum as cause when this vacuum is yet to follow. According to the infallible maxim of the Philosopher, the non-existent can produce no effect.

Simp.

SIMP. Seeing that you accept this axiom of Aristotle, I hardly think you will reject another excellent and reliable maxim of his, namely, Nature undertakes only that which happens without resistance; and in this saying, it appears to me, you will find the solution of your difficulty. Since nature abhors a vacuum, she prevents that from which a vacuum would follow as a necessary consequence. Thus it happens that nature prevents the separation of the two plates.

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SAGR. Now admitting that what Simplicio says is an adequate solution of my difficulty, it seems to me, if I may be allowed to resume my former argument, that this very resistance to a vacuum ought to be sufficient to hold together the parts either of stone or of metal or the parts of any other solid which is knit together more strongly and which is more resistant to separation. If for one effect there be only one cause, or if, more being assigned, they can be reduced to one, then why is not this vacuum which really exists a sufficient cause for all kinds of resistance?

SALV. I do not wish just now to enter this discussion as to whether the vacuum alone is sufficient to hold together the separate parts of a solid body; but I assure you that the vacuum which acts as a sufficient cause in the case of the two plates is not alone sufficient to bind together the parts of a solid cylinder of marble or metal which, when pulled violently, separates and divides. And now if I find a method of distinguishing this well known resistance, depending upon the vacuum, from every other kind which might increase the coherence, and if I show you that the aforesaid resistance alone is not nearly sufficient for such an effect, will you not grant that we are bound to introduce another cause? Help him, Simplicio, since he does not know what reply to make.

SIMP. Surely, Sagredo's hesitation must be owing to another reason, for there can be no doubt concerning a conclusion which is at once so clear and logical.

SAGR. You have guessed rightly, Simplicio. I was wondering whether, if a million of gold each year from Spain were not sufficient to pay the army, it might not be necessary to make

make provision other than small coin for the pay of the soldiers.*

But go ahead, Salviati; assume that I admit your conclusion and show us your method of separating the action of the vacuum from other causes; and by measuring it show us how it is not sufficient to produce the effect in question.

SALV. Your good angel assist you. I will tell you how to separate the force of the vacuum from the others, and afterwards how to measure it. For this purpose let us consider a continuous substance whose parts lack all resistance to separation except that derived from a vacuum, such as is the case with water, a fact fully demonstrated by our Academician in one of his treatises. Whenever a cylinder of water is subjected to a pull and

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offers a resistance to the separation of its parts this can be attributed to no other cause than the resistance of the vacuum. In order to try such an experiment I have invented a device which I can better explain by means of a sketch than by mere words. Let CABD represent the cross section of a cylinder either of metal or, preferably, of glass, hollow inside and accurately turned. Into this is introduced a perfectly fitting cylinder of wood, represented in cross section by EGHF, and capable of up-and-down motion. Through the middle of this cylinder is bored a hole to receive an iron wire, carrying a hook at the end K, while the upper end of the wire, I, is provided with a conical head. The wooden cylinder is countersunk at the top so as to receive, with a perfect fit, the conical head I of the wire, IK, when pulled down by the end K.

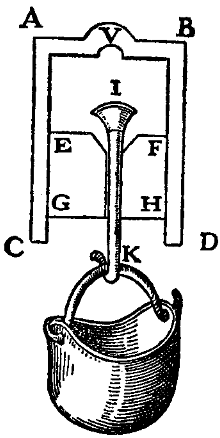

Fig. 4

Now insert the wooden cylinder EH in the hollow cylinder AD, so as not to touch the upper end of the latter but to leave free a space of two or three finger-breadths; this space is to be filled

* The bearing of this remark becomes clear on reading what Salviati says on p. 18 below. [*Trans.*]

with water by holding the vessel with the mouth CD upwards, pushing down on the stopper EH, and at the same time keeping the conical head of the wire, I, away from the hollow portion of the wooden cylinder. The air is thus allowed to escape alongside the iron wire (which does not make a close fit) as soon as one presses down on the wooden stopper. The air having been allowed to escape and the iron wire having been drawn back so that it fits snugly against the conical depression in the wood, invert the vessel, bringing it mouth downwards, and hang on the hook K a vessel which can be filled with sand or any heavy material in quantity sufficient to finally separate the upper surface of the stopper, EF, from the lower surface of the water to which it was attached only by the resistance of the vacuum. Next weigh the stopper and wire together with the attached vessel and its contents; we shall then have the force of the vacuum [*forza del vacuo*]. If one attaches to a cylinder of marble

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or glass a weight which, together with the weight of the marble or glass itself, is just equal to the sum of the weights before mentioned, and if breaking occurs we shall then be justified in saying that the vacuum alone holds the parts of the marble and glass together; but if this weight does not suffice and if breaking occurs only after adding, say, four times this weight, we shall then be compelled to say that the vacuum furnishes only one fifth of the total resistance [*resistenza*].

Simp. No one can doubt the cleverness of the device; yet it presents many difficulties which make me doubt its reliability. For who will assure us that the air does not creep in between the glass and stopper even if it is well packed with tow or other yielding material? I question also whether oiling with wax or turpentine will suffice to make the cone, I, fit snugly on its seat. Besides, may not the parts of the water expand and dilate? Why may not the air or exhalations or some other more subtile substances penetrate the pores of the wood, or even of the glass itself?

Salv. With great skill indeed has Simplicio laid before us the difficulties; and he has even partly suggested how to prevent the air

air from penetrating the wood or passing between the wood and the glass. But now let me point out that, as our experience increases, we shall learn whether or not these alleged difficulties really exist. For if, as is the case with air, water is by nature expansible, although only under severe treatment, we shall see the stopper descend; and if we put a small excavation in the upper part of the glass vessel, such as indicated by V, then the air or any other tenuous and gaseous substance, which might penetrate the pores of glass or wood, would pass through the water and collect in this receptacle V. But if these things do not happen we may rest assured that our experiment has been performed with proper caution; and we shall discover that water does not dilate and that glass does not allow any material, however tenuous, to penetrate it.

SAGR. Thanks to this discussion, I have learned the cause of a certain effect which I have long wondered at and despaired of understanding. I once saw a cistern which had been provided with a pump under the mistaken impression that the water might thus be drawn with less effort or in greater quantity than by means of the ordinary bucket. The stock of the pump car-

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ried its sucker and valve in the upper part so that the water was lifted by attraction and not by a push as is the case with pumps in which the sucker is placed lower down. This pump worked perfectly so long as the water in the cistern stood above a certain level; but below this level the pump failed to work. When I first noticed this phenomenon I thought the machine was out of order; but the workman whom I called in to repair it told me the defect was not in the pump but in the water which had fallen too low to be raised through such a height; and he added that it was not possible, either by a pump or by any other machine working on the principle of attraction, to lift water a hair's breadth above eighteen cubits; whether the pump be large or small this is the extreme limit of the lift. Up to this time I had been so thoughtless that, although I knew a rope, or rod of wood, or of iron, if sufficiently long, would break by its own weight when held by the upper end, it never occurred to me that

that the same thing would happen, only much more easily, to a column of water. And really is not that thing which is attracted in the pump a column of water attached at the upper end and stretched more and more until finally a point is reached where it breaks, like a rope, on account of its excessive weight?

SALV. That is precisely the way it works; this fixed elevation of eighteen cubits is true for any quantity of water whatever, be the pump large or small or even as fine as a straw. We may therefore say that, on weighing the water contained in a tube eighteen cubits long, no matter what the diameter, we shall obtain the value of the resistance of the vacuum in a cylinder of any solid material having a bore of this same diameter. And having gone so far, let us see how easy it is to find to what length cylinders of metal, stone, wood, glass, etc., of any diameter can be elongated without breaking by their own weight.

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Take for instance a copper wire of any length and thickness; fix the upper end and to the other end attach a greater and greater load until finally the wire breaks; let the maximum load be, say, fifty pounds. Then it is clear that if fifty pounds of copper, in addition to the weight of the wire itself which may be, say, $^1/_8$ ounce, is drawn out into wire of this same size we shall have the greatest length of this kind of wire which can sustain its own weight. Suppose the wire which breaks to be one cubit in length and $^1/_8$ ounce in weight; then since it supports 50 lbs. in addition to its own weight, i. e., 4800 eighths-of-an-ounce, it follows that all copper wires, independent of size, can sustain themselves up to a length of 4801 cubits and no more. Since then a copper rod can sustain its own weight up to a length of 4801 cubits it follows that that part of the breaking strength [*resistenza*] which depends upon the vacuum, comparing it with the remaining factors of resistance, is equal to the weight of a rod of water, eighteen cubits long and as thick as the copper rod. If, for example, copper is nine times as heavy as water, the breaking strength [*resistenza allo strapparsi*] of any copper rod, in so far as it depends upon the vacuum, is equal to the weight of two cubits of this same rod. By a similar method one can find

find the maximum length of wire or rod of any material which will just sustain its own weight, and can at the same time discover the part which the vacuum plays in its breaking strength.

SAGR. It still remains for you to tell us upon what depends the resistance to breaking, other than that of the vacuum; what is the gluey or viscous substance which cements together the parts of the solid? For I cannot imagine a glue that will not burn up in a highly heated furnace in two or three months, or certainly within ten or a hundred. For if gold, silver and glass are kept for a long while in the molten state and are removed from the furnace, their parts, on cooling, immediately reunite and bind themselves together as before. Not only so, but whatever difficulty arises with respect to the cementation of the parts of the glass arises also with regard to the parts of the glue; in other words, what is that which holds these parts together so firmly?

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SALV. A little while ago, I expressed the hope that your good angel might assist you. I now find myself in the same straits. Experiment leaves no doubt that the reason why two plates cannot be separated, except with violent effort, is that they are held together by the resistance of the vacuum; and the same can be said of two large pieces of a marble or bronze column. This being so, I do not see why this same cause may not explain the coherence of smaller parts and indeed of the very smallest particles of these materials. Now, since each effect must have one true and sufficient cause and since I find no other cement, am I not justified in trying to discover whether the vacuum is not a sufficient cause?

SIMP. But seeing that you have already proved that the resistance which the large vacuum offers to the separation of two large parts of a solid is really very small in comparison with that cohesive force which binds together the most minute parts, why do you hesitate to regard this latter as something very different from the former?

SALV. Sagredo has already [p. 13 above] answered this question when he remarked that each individual soldier was being paid

paid from coin collected by a general tax of pennies and farthings, while even a million of gold would not suffice to pay the entire army. And who knows but that there may be other extremely minute vacua which affect the smallest particles so that that which binds together the contiguous parts is throughout of the same mintage? Let me tell you something which has just occurred to me and which I do not offer as an absolute fact, but rather as a passing thought, still immature and calling for more careful consideration. You may take of it what you like; and judge the rest as you see fit. Sometimes when I have observed how fire winds its way in between the most minute particles of this or that metal and, even though these are solidly cemented together, tears them apart and separates them, and when I have observed that, on removing the fire, these particles reunite with the same tenacity as at first, without any loss of quantity in the case of gold and with little loss in the case of other metals, even though these parts have been separated for a long while, I have thought that the explanation might lie in the fact that the extremely fine particles of fire, penetrating the slender pores of the metal (too small to admit even the finest particles of air or of many other fluids), would fill the small intervening vacua and would set free these small particles from the attraction which these same vacua exert upon them and which prevents their separation. Thus the particles are able to

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move freely so that the mass [*massa*] becomes fluid and remains so as long as the particles of fire remain inside; but if they depart and leave the former vacua then the original attraction [*attrazzione*] returns and the parts are again cemented together.

In reply to the question raised by Simplicio, one may say that although each particular vacuum is exceedingly minute and therefore easily overcome, yet their number is so extraordinarily great that their combined resistance is, so to speak, multipled almost without limit. The nature and the amount of force [*forza*] which results [*risulta*] from adding together an immense number of small forces [*debolissimi momenti*] is clearly illustrated by the fact that a weight of millions of pounds, suspended by great cables, is overcome and lifted, when the south wind carries innumerable atoms of water, suspended in thin mist, which moving through the air penetrate between the fibres of the tense ropes in spite of the tremendous force of the hanging weight. When these particles enter the narrow pores they swell the ropes, thereby shorten them, and perforce lift the heavy mass [*mole*].

SAGR. There can be no doubt that any resistance, so long as it is not infinite, may be overcome by a multitude of minute forces. Thus a vast number of ants might carry ashore a ship laden with grain. And since experience shows us daily that one ant can easily carry one grain, it is clear that the number of grains in the ship is not infinite, but falls below a certain limit. If you take another number four or six times as great, and if you set to work a corresponding number of ants they will carry the grain ashore and the boat also. It is true that this will call for a prodigious number of ants, but in my opinion this is precisely the case with the vacua which bind together the least particles of a metal.

SALV. But even if this demanded an infinite number would you still think it impossible?

SAGR. Not if the mass [*mole*] of metal were infinite; otherwise. . . .

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SALV. Otherwise what? Now since we have arrived at paradoxes let us see if we cannot prove that within a finite extent it is possible to discover an infinite number of vacua. At the same time we shall at least reach a solution of the most remarkable of all that list of problems which Aristotle himself calls wonderful; I refer to his *Questions in Mechanics*. This solution may be no less clear and conclusive than that which he himself gives and quite different also from that so cleverly expounded by the most learned Monsignor di Guevara.*

First it is necessary to consider a proposition, not treated by others, but upon which depends the solution of the problem and from which, if I mistake not, we shall derive other new and remarkable facts. For the sake of clearness let us draw an

* Bishop of Teano; b. 1561, d.1641. [*Trans.*]

accurate figure. About G as a center describe an equiangular and equilateral polygon of any number of sides, say the hexagon ABCDEF. Similar to this and concentric with it, describe another smaller one which we shall call HIKLMN. Prolong the

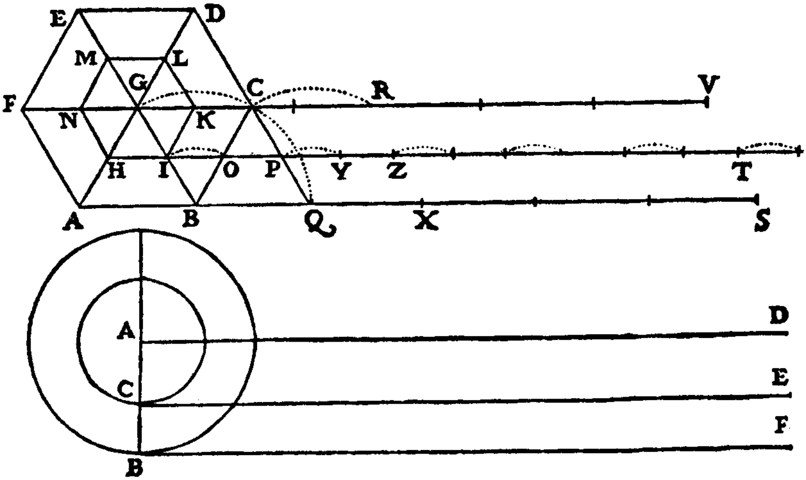

Fig. 5

side AB, of the larger hexagon, indefinitely toward S; in like manner prolong the corresponding side HI of the smaller hexagon, in the same direction, so that the line HT is parallel to AS; and through the center draw the line GV parallel to the other two. This done, imagine the larger polygon to roll upon

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the line AS, carrying with it the smaller polygon. It is evident that, if the point B, the end of the side AB, remains fixed at the beginning of the rotation, the point A will rise and the point C will fall describing the arc CQ until the side BC coincides with the line BQ, equal to BC. But during this rotation the point I, on the smaller polygon, will rise above the line IT because IB is oblique to AS; and it will not again return to the line IT until the point C shall have reached the position Q. The point I, having described the arc IO above the line HT, will reach the position

O at

O at the same time the side IK assumes the position OP; but in the meantime the center G has traversed a path above GV and does not return to it until it has completed the arc GC. This step having been taken, the larger polygon has been brought to rest with its side BC coinciding with the line BQ while the side IK of the smaller polygon has been made to coincide with the line OP, having passed over the portion IO without touching it; also the center G will have reached the position C after having traversed all its course above the parallel line GV. And finally the entire figure will assume a position similar to the first, so that if we continue the rotation and come to the next step, the side DC of the larger polygon will coincide with the portion QX and the side KL of the smaller polygon, having first skipped the arc PY, will fall on YZ, while the center still keeping above the line GV will return to it at R after having jumped the interval CR. At the end of one complete rotation the larger polygon will have traced upon the line AS, without break, six lines together equal to its perimeter; the lesser polygon will likewise have imprinted six lines equal to its perimeter, but separated by the interposition of five arcs, whose chords represent the parts of HT not touched by the polygon: the center G never reaches the line GV except at six points. From this it is clear that the space traversed by the smaller polygon is almost equal to that traversed by the larger, that is, the line HT approximates the line AS, differing from it only by the length of one chord of one of these arcs, provided we understand the line HT to include the five skipped arcs.

Now this exposition which I have given in the case of these hexagons must be understood to be applicable to all other polygons, whatever the number of sides, provided only they are

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similar, concentric, and rigidly connected, so that when the greater one rotates the lesser will also turn however small it may be. You must also understand that the lines described by these two are nearly equal provided we include in the space traversed by the smaller one the intervals which are not touched by any part of the perimeter of this smaller polygon.

Let

Let a large polygon of, say, one thousand sides make one complete rotation and thus lay off a line equal to its perimeter; at the same time the small one will pass over an approximately equal distance, made up of a thousand small portions each equal to one of its sides, but interrupted by a thousand spaces which, in contrast with the portions that coincide with the sides of the polygon, we may call empty. So far the matter is free from difficulty or doubt.

But now suppose that about any center, say A, we describe two concentric and rigidly connected circles; and suppose that from the points C and B, on their radii, there are drawn the tangents CE and BF and that through the center A the line AD is drawn parallel to them, then if the large circle makes one complete rotation along the line BF, equal not only to its circumference but also to the other two lines CE and AD, tell me what the smaller circle will do and also what the center will do. As to the center it will certainly traverse and touch the entire line AD while the circumference of the smaller circle will have measured off by its points of contact the entire line CE, just as was done by the above mentioned polygons. The only difference is that the line HT was not at every point in contact with the perimeter of the smaller polygon, but there were left untouched as many vacant spaces as there were spaces coinciding with the sides. But here in the case of the circles the circumference of the smaller one never leaves the line CE, so that no part of the latter is left untouched, nor is there ever a time when some point on the circle is not in contact with the straight line. How now can the smaller circle traverse a length greater than its circumference unless it go by jumps?

SAGR. It seems to me that one may say that just as the center of the circle, by itself, carried along the line AD is constantly in contact with it, although it is only a single point, so the points on the circumference of the smaller circle, carried along by the motion of the larger circle, would slide over some small parts of the line CE.

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SALV. There are two reasons why this cannot happen. First because

because there is no ground for thinking that one point of contact, such as that at C, rather than another, should slip over certain portions of the line CE. But if such slidings along CE did occur they would be infinite in number since the points of contact (being mere points) are infinite in number: an infinite number of finite slips will however make an infinitely long line, while as a matter of fact the line CE is finite. The other reason is that as the greater circle, in its rotation, changes its point of contact continuously the lesser circle must do the same because B is the only point from which a straight line can be drawn to A and pass through C. Accordingly the small circle must change its point of contact whenever the large one changes: no point of the small circle touches the straight line CE in more than one point. Not only so, but even in the rotation of the polygons there was no point on the perimeter of the smaller which coincided with more than one point on the line traversed by that perimeter; this is at once clear when you remember that the line IK is parallel to BC and that therefore IK will remain above IP until BC coincides with BQ, and that IK will not lie upon IP except at the very instant when BC occupies the position BQ; at this instant the entire line IK coincides with OP and immediately afterwards rises above it.

SAGR. This is a very intricate matter. I see no solution. Pray explain it to us.

SALV. Let us return to the consideration of the above mentioned polygons whose behavior we already understand. Now in the case of polygons with 100000 sides, the line traversed by the perimeter of the greater, i. e., the line laid down by its 100000 sides one after another, is equal to the line traced out by the 100000 sides of the smaller, provided we include the 100000 vacant spaces interspersed. So in the case of the circles, polygons having an infinitude of sides, the line traversed by the continuously distributed [*continuamente disposti*] infinitude of sides is in the greater circle equal to the line laid down by the infinitude of sides in the smaller circle but with the exception that these latter alternate with empty spaces; and since the sides are not finite in number, but infinite, so also are the intervening

vening empty spaces not finite but infinite. The line traversed by the larger circle consists then of an infinite number of points which completely fill it; while that which is traced by the smaller circle consists of an infinite number of points which leave empty spaces and only partly fill the line. And here I wish you to observe that after dividing and resolving a line into a finite number of parts, that is, into a number which can be counted, it

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is not possible to arrange them again into a greater length than that which they occupied when they formed a *continuum* [*continuate*] and were connected without the interposition of as many empty spaces. But if we consider the line resolved into an infinite number of infinitely small and indivisible parts, we shall be able to conceive the line extended indefinitely by the interposition, not of a finite, but of an infinite number of infinitely small indivisible empty spaces.

Now this which has been said concerning simple lines must be understood to hold also in the case of surfaces and solid bodies, it being assumed that they are made up of an infinite, not a finite, number of atoms. Such a body once divided into a finite number of parts it is impossible to reassemble them so as to occupy more space than before unless we interpose a finite number of empty spaces, that is to say, spaces free from the substance of which the solid is made. But if we imagine the body, by some extreme and final analysis, resolved into its primary elements, infinite in number, then we shall be able to think of them as indefinitely extended in space, not by the interposition of a finite, but of an infinite number of empty spaces. Thus one can easily imagine a small ball of gold expanded into a very large space without the introduction of a finite number of empty spaces, always provided the gold is made up of an infinite number of indivisible parts.

SIMP. It seems to me that you are travelling along toward those vacua advocated by a certain ancient philosopher.

SALV. But you have failed to add, "who denied Divine Providence," an inapt remark made on a similar occasion by a certain antagonist of our Academician.

Simp.

SIMP. I noticed, and not without indignation, the rancor of this ill-natured opponent; further references to these affairs I omit, not only as a matter of good form, but also because I know how unpleasant they are to the good tempered and well ordered mind of one so religious and pious, so orthodox and God-fearing as you.

But to return to our subject, your previous discourse leaves with me many difficulties which I am unable to solve. First among these is that, if the circumferences of the two circles are equal to the two straight lines, CE and BF, the latter considered as a *continuum*, the former as interrupted with an infinity of empty points, I do not see how it is possible to say that the line AD described by the center, and made up of an infinity of points, is equal to this center which is a single point. Besides, this building up of lines out of points, divisibles out of indivisibles, and finites out of infinites, offers me an obstacle difficult to avoid; and the necessity of introducing a vacuum, so conclusively refuted by Aristotle, presents the same difficulty.

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SALV. These difficulties are real; and they are not the only ones. But let us remember that we are dealing with infinities and indivisibles, both of which transcend our finite understanding, the former on account of their magnitude, the latter because of their smallness. In spite of this, men cannot refrain from discussing them, even though it must be done in a roundabout way.

Therefore I also should like to take the liberty to present some of my ideas which, though not necessarily convincing, would, on account of their novelty, at least, prove somewhat startling. But such a diversion might perhaps carry us too far away from the subject under discussion and might therefore appear to you inopportune and not very pleasing.

SAGR. Pray let us enjoy the advantages and privileges which come from conversation between friends, especially upon subjects freely chosen and not forced upon us, a matter vastly different from dealing with dead books which give rise to many doubts but remove none. Share with us, therefore, the thoughts which

which our discussion has suggested to you; for since we are free from urgent business there will be abundant time to pursue the topics already mentioned; and in particular the objections raised by Simplicio ought not in any wise to be neglected.

SALV. Granted, since you so desire. The first question was, How can a single point be equal to a line? Since I cannot do more at present I shall attempt to remove, or at least diminish, one improbability by introducing a similar or a greater one, just as sometimes a wonder is diminished by a miracle.*

And this I shall do by showing you two equal surfaces, together with two equal solids located upon these same surfaces as bases, all four of which diminish continuously and uniformly in such a way that their remainders always preserve equality among themselves, and finally both the surfaces and the solids terminate their previous constant equality by degenerating, the one solid and the one surface into a very long line, the other solid and the other surface into a single point; that is, the latter to one point, the former to an infinite number of points.

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SAGR. This proposition appears to me wonderful, indeed; but let us hear the explanation and demonstration.

SALV. Since the proof is purely geometrical we shall need a figure. Let AFB be a semicircle with center at C; about it describe the rectangle ADEB and from the center draw the straight lines CD and CE to the points D and E. Imagine the radius CF to be drawn perpendicular to either of the lines AB or DE, and the entire figure to rotate about this radius as an axis. It is clear that the rectangle ADEB will thus describe a cylinder, the semicircle AFB a hemisphere, and the triangle CDE, a cone. Next let us remove the hemisphere but leave the cone and the rest of the cylinder, which, on account of its shape, we will call a "bowl." First we shall prove that the bowl and the cone are equal; then we shall show that a plane drawn parallel to the circle which forms the base of the bowl and which has the line DE for diameter and F for a center—a plane whose trace is GN—cuts the bowl in the points G, I, O, N, and the cone in the points H, L, so that the part of the cone indicated by CHL is always equal to

* *Cf.* p. 30 below. [*Trans.*]

the part of the bowl whose profile is represented by the triangles GAI and BON. Besides this we shall prove that the base of the cone, i. e., the circle whose diameter is HL, is equal to the circular surface which forms the base of this portion of the bowl, or as one might say, equal to a ribbon whose width is GI. (Note by the way the nature of mathematical definitions which consist merely in the imposition of names or, if you prefer, abbreviations of speech established and introduced in order to avoid the tedious drudgery which you and I now experience simply because we have not agreed to call this surface a "circular band" and that sharp solid portion of the bowl a "round razor.") Now call them by

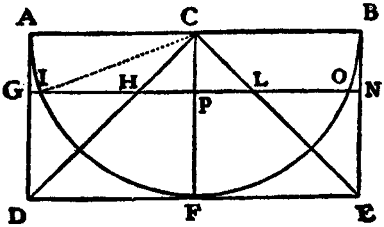

Fig. 6

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what name you please, it suffices to understand that the plane, drawn at any height whatever, so long as it is parallel to the base, i. e., to the circle whose diameter is DE, always cuts the two solids so that the portion CHL of the cone is equal to the upper portion of the bowl; likewise the two areas which are the bases of these solids, namely the band and the circle HL, are also equal. Here we have the miracle mentioned above; as the cutting plane approaches the line AB the portions of the solids cut off are always equal, so also the areas of their bases. And as the cutting plane comes near the top, the two solids (always equal) as well as their bases (areas which are also equal) finally vanish, one pair of them degenerating into the circumference of a circle, the other into a single point, namely, the upper edge of the bowl and the apex of the cone. Now, since as these solids diminish equality is maintained between them up to the very last, we are justified in saying that, at the extreme and final end of this diminution, they are still equal and that one is not infinitely greater than the other. It appears therefore that we may equate the circumference of a large circle to a single point. And this which is true of the solids is true also of the surfaces which form

form their bases; for these also preserve equality between themselves throughout their diminution and in the end vanish, the one into the circumference of a circle, the other into a single point. Shall we not then call them equal seeing that they are the last traces and remnants of equal magnitudes? Note also that, even if these vessels were large enough to contain immense celestial hemispheres, both their upper edges and the apexes of the cones therein contained would always remain equal and would vanish, the former into circles having the dimensions of the largest celestial orbits, the latter into single points. Hence in conformity with the preceding we may say that all circumferences of circles, however different, are equal to each other, and are each equal to a single point.

SAGR. This presentation strikes me as so clever and novel that, even if I were able, I would not be willing to oppose it; for to deface so beautiful a structure by a blunt pedantic attack would be nothing short of sinful. But for our complete satisfac-

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tion pray give us this geometrical proof that there is always equality between these solids and between their bases; for it cannot, I think, fail to be very ingenious, seeing how subtle is the philosophical argument based upon this result.

SALV. The demonstration is both short and easy. Referring to the preceding figure, since IPC is a right angle the square of the radius IC is equal to the sum of the squares on the two sides IP, PC; but the radius IC is equal to AC and also to GP, while CP is equal to PH. Hence the square of the line GP is equal to the sum of the squares of IP and PH, or multiplying through by 4, we have the square of the diameter GN equal to the sum of the squares on IO and HL. And, since the areas of circles are to each other as the squares of their diameters, it follows that the area of the circle whose diameter is GN is equal to the sum of the areas of circles having diameters IO and HL, so that if we remove the common area of the circle having IO for diameter the remaining area of the circle GN will be equal to the area of the circle whose diameter is HL. So much for the first part. As for the other part, we leave its demonstration for the present, partly because

because those who wish to follow it will find it in the twelfth proposition of the second book of *De centro gravitatis solidorum* by the Archimedes of our age, Luca Valerio,* who made use of it for a different object, and partly because, for our purpose, it suffices to have seen that the above-mentioned surfaces are always equal and that, as they keep on diminishing uniformly, they degenerate, the one into a single point, the other into the circumference of a circle larger than any assignable; in this fact lies our miracle.†

Sagr. The demonstration is ingenious and the inferences drawn from it are remarkable. And now let us hear something concerning the other difficulty raised by Simplicio, if you have anything special to say, which, however, seems to me hardly possible, since the matter has already been so thoroughly discussed.

Salv. But I do have something special to say, and will first of all repeat what I said a little while ago, namely, that infinity and indivisibility are in their very nature incomprehensible to us; imagine then what they are when combined. Yet if

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we wish to build up a line out of indivisible points, we must take an infinite number of them, and are, therefore, bound to understand both the infinite and the indivisible at the same time. Many ideas have passed through my mind concerning this subject, some of which, possibly the more important, I may not be able to recall on the spur of the moment; but in the course of our discussion it may happen that I shall awaken in you, and especially in Simplicio, objections and difficulties which in turn will bring to memory that which, without such stimulus, would have lain dormant in my mind. Allow me therefore the customary liberty of introducing some of our human fancies, for indeed we may so call them in comparison with supernatural truth which furnishes the one true and safe recourse for decision in our discussions and which is an infallible guide in the dark and dubious paths of thought.

* Distinguished Italian mathematician; born at Ferrara about 1552; admitted to the Accademia dei Lincei 1612; died 1618. [*Trans.*]

† *Cf.* p. 27 above. [*Trans.*]

One of the main objections urged against this building up of continuous quantities out of indivisible quantities [*continuo d' indivisibili*] is that the addition of one indivisible to another cannot produce a divisible, for if this were so it would render the indivisible divisible. Thus if two indivisibles, say two points, can be united to form a quantity, say a divisible line, then an even more divisible line might be formed by the union of three, five, seven, or any other odd number of points. Since however these lines can be cut into two equal parts, it becomes possible to cut the indivisible which lies exactly in the middle of the line. In answer to this and other objections of the same type we reply that a divisible magnitude cannot be constructed out of two or ten or a hundred or a thousand indivisibles, but requires an infinite number of them.

SIMP. Here a difficulty presents itself which appears to me insoluble. Since it is clear that we may have one line greater than another, each containing an infinite number of points, we are forced to admit that, within one and the same class, we may have something greater than infinity, because the infinity of points in the long line is greater than the infinity of points in the short line. This assigning to an infinite quantity a value greater than infinity is quite beyond my comprehension.

SALV. This is one of the difficulties which arise when we attempt, with our finite minds, to discuss the infinite, assigning to it those properties which we give to the finite and limited; but

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this I think is wrong, for we cannot speak of infinite quantities as being the one greater or less than or equal to another. To prove this I have in mind an argument which, for the sake of clearness, I shall put in the form of questions to Simplicio who raised this difficulty.

I take it for granted that you know which of the numbers are squares and which are not.

SIMP. I am quite aware that a squared number is one which results from the multiplication of another number by itself; thus 4, 9, etc., are squared numbers which come from multiplying 2, 3, etc., by themselves.

Salv.

SALV. Very well; and you also know that just as the products are called squares so the factors are called sides or roots; while on the other hand those numbers which do not consist of two equal factors are not squares. Therefore if I assert that all numbers, including both squares and non-squares, are more than the squares alone, I shall speak the truth, shall I not?

SIMP. Most certainly.

SALV. If I should ask further how many squares there are one might reply truly that there are as many as the corresponding number of roots, since every square has its own root and every root its own square, while no square has more than one root and no root more than one square.

SIMP. Precisely so.

SALV. But if I inquire how many roots there are, it cannot be denied that there are as many as there are numbers because every number is a root of some square. This being granted we must say that there are as many squares as there are numbers because they are just as numerous as their roots, and all the numbers are roots. Yet at the outset we said there are many more numbers than squares, since the larger portion of them are not squares. Not only so, but the proportionate number of squares diminishes as we pass to larger numbers. Thus up to 100 we have 10 squares, that is, the squares constitute 1/10 part of all the numbers; up to 10000, we find only 1/100

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part to be squares; and up to a million only 1/1000 part; on the other hand in an infinite number, if one could conceive of such a thing, he would be forced to admit that there are as many squares as there are numbers all taken together.

SAGR. What then must one conclude under these circumstances?

SALV. So far as I see we can only infer that the totality of all numbers is infinite, that the number of squares is infinite, and that the number of their roots is infinite; neither is the number of squares less than the totality of all numbers, nor the latter greater than the former; and finally the attributes "equal," "greater," and "less," are not applicable to infinite, but

but only to finite, quantities. When therefore Simplicio introduces several lines of different lengths and asks me how it is possible that the longer ones do not contain more points than the shorter, I answer him that one line does not contain more or less or just as many points as another, but that each line contains an infinite number. Or if I had replied to him that the points in one line were equal in number to the squares; in another, greater than the totality of numbers; and in the little one, as many as the number of cubes, might I not, indeed, have satisfied him by thus placing more points in one line than in another and yet maintaining an infinite number in each? So much for the first difficulty.

SAGR. Pray stop a moment and let me add to what has already been said an idea which just occurs to me. If the preceding be true, it seems to me impossible to say either that one infinite number is greater than another or even that it is greater than a finite number, because if the infinite number were greater than, say, a million it would follow that on passing from the million to higher and higher numbers we would be approaching the infinite; but this is not so; on the contrary, the larger the number to which we pass, the more we recede from [this property of] infinity, because the greater the numbers the fewer [relatively] are the squares contained in them; but the squares in infinity cannot be less than the totality of all the numbers, as we have just agreed; hence the approach to greater and greater numbers means a departure from infinity.*

SALV. And thus from your ingenious argument we are led to

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conclude that the attributes "larger," "smaller," and "equal" have no place either in comparing infinite quantities with each other or in comparing infinite with finite quantities.

I pass now to another consideration. Since lines and all continuous quantities are divisible into parts which are themselves divisible without end, I do not see how it is possible

* A certain confusion of thought appears to be introduced here through a failure to distinguish between the *number n* and the *class* of the first *n* numbers; and likewise from a failure to distinguish infinity as a number from infinity as the class of all numbers. [*Trans.*]

to avoid the conclusion that these lines are built up of an infinite number of indivisible quantities because a division and a subdivision which can be carried on indefinitely presupposes that the parts are infinite in number, otherwise the subdivision would reach an end; and if the parts are infinite in number, we must conclude that they are not finite in size, because an infinite number of finite quantities would give an infinite magnitude. And thus we have a continuous quantity built up of an infinite number of indivisibles.

SIMP. But if we can carry on indefinitely the division into finite parts what necessity is there then for the introduction of non-finite parts?

SALV. The very fact that one is able to continue, without end, the division into finite parts [*in parti quante*] makes it necessary to regard the quantity as composed of an infinite number of immeasurably small elements [*di infiniti non quanti*]. Now in order to settle this matter I shall ask you to tell me whether, in your opinion, a *continuum* is made up of a finite or of an infinite number of finite parts [*parti quante*].

SIMP. My answer is that their number is both infinite and finite; potentially infinite but actually finite [*infinite, in potenza; e finite, in atto*]; that is to say, potentially infinite before division and actually finite after division; because parts cannot be said to exist in a body which is not yet divided or at least marked out; if this is not done we say that they exist potentially.

SALV. So that a line which is, for instance, twenty spans long is not said to contain actually twenty lines each one span in length except after division into twenty equal parts; before division it is said to contain them only potentially. Suppose the facts are as you say; tell me then whether, when the division is once made, the size of the original quantity is thereby increased, diminished, or unaffected.

SIMP. It neither increases nor diminishes.

SALV. That is my opinion also. Therefore the finite parts [*parti quante*] in a *continuum*, whether actually or potentially present, do not make the quantity either larger or smaller; but it is perfectly clear that, if the number of finite parts actually contained

contained in the whole is infinite in number, they will make the magnitude infinite. Hence the number of finite parts, although existing only potentially, cannot be infinite unless the magnitude containing them be infinite; and conversely if the magnitude is
[81]
finite it cannot contain an infinite number of finite parts either actually or potentially.

SAGR. How then is it possible to divide a *continuum* without limit into parts which are themselves always capable of subdivision?

SALV. This distinction of yours between actual and potential appears to render easy by one method what would be impossible by another. But I shall endeavor to reconcile these matters in another way; and as to the query whether the finite parts of a limited *continuum* [*continuo terminato*] are finite or infinite in number I will, contrary to the opinion of Simplicio, answer that they are neither finite nor infinite.

SIMP. This answer would never have occurred to me since I did not think that there existed any intermediate step between the finite and the infinite, so that the classification or distinction which assumes that a thing must be either finite or infinite is faulty and defective.

SALV. So it seems to me. And if we consider discrete quantities I think there is, between finite and infinite quantities, a third intermediate term which corresponds to every assigned number; so that if asked, as in the present case, whether the finite parts of a *continuum* are finite or infinite in number the best reply is that they are neither finite nor infinite but correspond to every assigned number. In order that this may be possible, it is necessary that those parts should not be included within a limited number, for in that case they would not correspond to a number which is greater; nor can they be infinite in number since no assigned number is infinite; and thus at the pleasure of the questioner we may, to any given line, assign a hundred finite parts, a thousand, a hundred thousand, or indeed any number we may please so long as it be not infinite. I grant, therefore, to the philosophers, that the *continuum* contains as many

many finite parts as they please and I concede also that it contains them, either actually or potentially, as they may like; but I must add that just as a line ten fathoms [*canne*] in length contains ten lines each of one fathom and forty lines each of one cubit [*braccia*] and eighty lines each of half a cubit, etc., so it contains an infinite number of points; call them actual or potential, as you like, for as to this detail, Simplicio, I defer to your opinion and to your judgment.

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SIMP. I cannot help admiring your discussion; but I fear that this parallelism between the points and the finite parts contained in a line will not prove satisfactory, and that you will not find it so easy to divide a given line into an infinite number of points as the philosophers do to cut it into ten fathoms or forty cubits; not only so, but such a division is quite impossible to realize in practice, so that this will be one of those potentialities which cannot be reduced to actuality.

SALV. The fact that something can be done only with effort or diligence or with great expenditure of time does not render it impossible; for I think that you yourself could not easily divide a line into a thousand parts, and much less if the number of parts were 937 or any other large prime number. But if I were to accomplish this division which you deem impossible as readily as another person would divide the line into forty parts would you then be more willing, in our discussion, to concede the possibility of such a division?

SIMP. In general I enjoy greatly your method; and replying to your query, I answer that it would be more than sufficient if it prove not more difficult to resolve a line into points than to divide it into a thousand parts.

SALV. I will now say something which may perhaps astonish you; it refers to the possibility of dividing a line into its infinitely small elements by following the same order which one employs in dividing the same line into forty, sixty, or a hundred parts, that is, by dividing it into two, four, etc. He who thinks that, by following this method, he can reach an infinite number of points is greatly mistaken; for if this process were followed to eternity

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eternity there would still remain finite parts which were undivided.

Indeed by such a method one is very far from reaching the goal of indivisibility; on the contrary he recedes from it and while he thinks that, by continuing this division and by multiplying the multitude of parts, he will approach infinity, he is, in my opinion, getting farther and farther away from it. My reason is this. In the preceding discussion we concluded that, in an infinite number, it is necessary that the squares and cubes should be as numerous as the totality of the natural numbers [*tutti i numeri*], because both of these are as numerous as their roots which constitute the totality of the natural numbers. Next we saw that the larger the numbers taken the more sparsely distributed were the squares, and still more sparsely the cubes; therefore it is clear that the larger the numbers to which we pass the farther we recede from the infinite number; hence it follows

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that, since this process carries us farther and farther from the end sought, if on turning back we shall find that any number can be said to be infinite, it must be unity. Here indeed are satisfied all those conditions which are requisite for an infinite number; I mean that unity contains in itself as many squares as there are cubes and natural numbers [*tutti i numeri*].

SIMP. I do not quite grasp the meaning of this.

SALV. There is no difficulty in the matter because unity is at once a square, a cube, a square of a square and all the other powers [*dignità*]; nor is there any essential peculiarity in squares or cubes which does not belong to unity; as, for example, the property of two square numbers that they have between them a mean proportional; take any square number you please as the first term and unity for the other, then you will always find a number which is a mean proportional. Consider the two square numbers, 9 and 4; then 3 is the mean proportional between 9 and 1; while 2 is a mean proportional between 4 and 1; between 9 and 4 we have 6 as a mean proportional. A property of cubes is that they must have between them two mean proportional numbers; take 8 and 27; between them lie 12 and 18; while between

SECOND DAY

SAGR. While Simplicio and I were awaiting your arrival we were trying to recall that last consideration which you advanced as a principle and basis for the results you intended to obtain; this consideration dealt with the resistance which all solids offer to fracture and depended upon a certain cement which held the parts glued together so that they would yield and separate only under considerable pull [*potente attrazzione*]. Later we tried to find the explanation of this coherence, seeking it mainly in the vacuum; this was the occasion of our many digressions which occupied the entire day and led us far afield from the original question which, as I have already stated, was the consideration of the resistance [*resistenza*] that solids offer to fracture.

SALV. I remember it all very well. Resuming the thread of our discourse, whatever the nature of this resistance which solids offer to large tractive forces [*violenta attrazzione*] there can at least be no doubt of its existence; and though this resistance is very great in the case of a direct pull, it is found, as a rule, to be less in the case of bending forces [*nel violentargli per traverso*]. Thus, for example, a rod of steel or of glass will sustain a longitudinal pull of a thousand pounds while a weight of fifty pounds would be quite sufficient to break it if the rod were fastened at right angles into a vertical wall. It is this second type of resistance which we must consider, seeking to discover in what

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proportion

proportion it is found in prisms and cylinders of the same material, whether alike or unlike in shape, length, and thickness. In this discussion I shall take for granted the well-known mechanical principle which has been shown to govern the behavior of a bar, which we call a lever, namely, that the force bears to the resistance the inverse ratio of the distances which separate the fulcrum from the force and resistance respectively.

SIMP. This was demonstrated first of all by Aristotle, in his *Mechanics*.

SALV. Yes, I am willing to concede him priority in point of time; but as regards rigor of demonstration the first place must be given to Archimedes, since upon a single proposition proved in his book on *Equilibrium* * depends not only the law of the lever but also those of most other mechanical devices.

SAGR. Since now this principle is fundamental to all the demonstrations which you propose to set forth would it not be advisable to give us a complete and thorough proof of this proposition unless possibly it would take too much time?

SALV. Yes, that would be quite proper, but it is better I think to approach our subject in a manner somewhat different from that employed by Archimedes, namely, by first assuming merely that equal weights placed in a balance of equal arms will produce equilibrium—a principle also assumed by Archimedes—and then proving that it is no less true that unequal weights produce equilibrium when the arms of the steelyard have lengths inversely proportional to the weights suspended from them; in other words, it amounts to the same thing whether one places equal weights at equal distances or unequal weights at distances which bear to each other the inverse ratio of the weights.

In order to make this matter clear imagine a prism or solid cylinder, AB, suspended at each end to the rod [*linea*] HI, and supported by two threads HA and IB; it is evident that if I attach a thread, C, at the middle point of the balance beam HI, the entire prism AB will, according to the principle assumed, hang in equilibrium since one-half its weight lies on one side, and the other half on the other side, of the point of suspension C. Now

* *Works of Archimedes*. Trans. by T. L. Heath, pp. 189–220. [*Trans.*]

suppose the prism to be divided into unequal parts by a plane
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through the line D, and let the part DA be the larger and DB the smaller: this division having been made, imagine a thread ED, attached at the point E and supporting the parts AD and DB, in order that these parts may remain in the same position relative to line HI: and since the relative position of the prism and the beam HI remains unchanged, there can be no doubt but that the prism will maintain its former state of equilibrium.

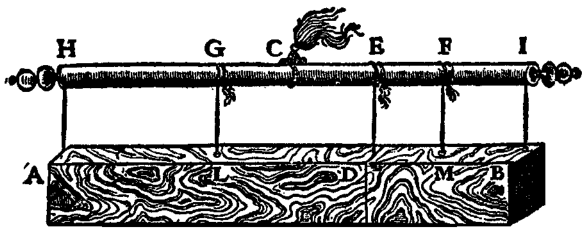

Fig. 14

But circumstances would remain the same if that part of the prism which is now held up, at the ends, by the threads AH and DE were supported at the middle by a single thread GL; and likewise the other part DB would not change position if held by a thread FM placed at its middle point. Suppose now the threads HA, ED, and IB to be removed, leaving only the two GL and FM, then the same equilibrium will be maintained so long as the suspension is at C. Now let us consider that we have here two heavy bodies AD and DB hung at the ends G and F, of a balance beam GF in equilibrium about the point C, so that the line CG is the distance from C to the point of suspension of the heavy body AD, while CF is the distance at which the other heavy body, DB, is supported. It remains now only to show that these distances bear to each other the inverse ratio of the weights themselves, that is, the distance GC is to the distance CF as the prism DB is to the prism DA—a proposition which we shall prove as follows: Since the line GE is the half of EH, and since EF is the half of EI, the whole length GF will be half

half of the entire line HI, and therefore equal to CI: if now we subtract the common part CF the remainder GC will be equal to the remainder FI, that is, to FE, and if to each of these we add CE we shall have GE equal to CF: hence GE:EF=FC:CG. But GE and EF bear the same ratio to each other as do their doubles HE and EI, that is, the same ratio as the prism AD to DB. Therefore, by equating ratios we have, *convertendo*, the distance GC is to the distance CF as the weight BD is to the weight DA, which is what I desired to prove.

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If what precedes is clear, you will not hesitate, I think, to admit that the two prisms AD and DB are in equilibrium about the point C since one-half of the whole body AB lies on the right of the suspension C and the other half on the left; in other words, this arrangement is equivalent to two equal weights disposed at equal distances. I do not see how any one can doubt, if the two prisms AD and DB were transformed into cubes, spheres, or into any other figure whatever and if G and F were retained as points of suspension, that they would remain in equilibrium about the point C, for it is only too evident that change of figure does not produce change of weight so long as the mass [*quantità di materia*] does not vary. From this we may derive the general conclusion that any two heavy bodies are in equilibrium at distances which are inversely proportional to their weights.

This principle established, I desire, before passing to any other subject, to call your attention to the fact that these forces, resistances, moments, figures, etc., may be considered either in the abstract, dissociated from matter, or in the concrete, associated with matter. Hence the properties which belong to figures that are merely geometrical and non-material must be modified when we fill these figures with matter and therefore give them weight. Take, for example, the lever BA which, resting upon the support E, is used to lift a heavy stone D. The principle just demonstrated makes it clear that a force applied at the extremity B will just suffice to equilibrate the resistance offered by the heavy body D provided this force [*momento*] bears to the force [*momento*] at D the same ratio as the distance

distance AC bears to the distance CB; and this is true so long as we consider only the moments of the single force at B and of the resistance at D, treating the lever as an immaterial body devoid of weight. But if we take into account the weight of the lever itself—an instrument which may be made either of wood or of iron—it is manifest that, when this weight has been added to the

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force at B, the ratio will be changed and must therefore be expressed in different terms. Hence before going further let

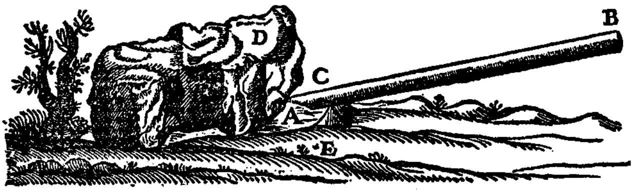

Fig. 15

us agree to distinguish between these two points of view; when we consider an instrument in the abstract, i. e., apart from the weight of its own material, we shall speak of "taking it in an absolute sense" [*prendere assolutamente*]; but if we fill one of these simple and absolute figures with matter and thus give it weight, we shall refer to such a material figure as a "moment" or "compound force" [*momento o forza composta*].

SAGR. I must break my resolution about not leading you off into a digression; for I cannot concentrate my attention upon what is to follow until a certain doubt is removed from my mind, namely, you seem to compare the force at B with the total weight of the stone D, a part of which—possibly the greater part—rests upon the horizontal plane: so that . . .

SALV. I understand perfectly: you need go no further. However please observe that I have not mentioned the total weight of the stone; I spoke only of its force [*momento*] at the point A, the extremity of the lever BA, which force is always less than the total weight of the stone, and varies with its shape and elevation.

SAGR. Good: but there occurs to me another question about which

which I am curious. For a complete understanding of this matter, I should like you to show me, if possible, how one can determine what part of the total weight is supported by the underlying plane and what part by the end A of the lever.

SALV. The explanation will not delay us long and I shall therefore have pleasure in granting your request. In the accompanying figure, let us understand that the weight having its center of gravity at A rests with the end B upon the horizontal plane and with the other end upon the lever CG. Let N be the fulcrum of a lever to which the force [*potenza*] is applied at G. Let fall the perpendiculars, AO and CF, from the center A and the end C. Then I say, the magnitude [*momento*] of the entire weight bears to the magnitude of the force [*momento della potenza*] at G a ratio compounded of the ratio between the two

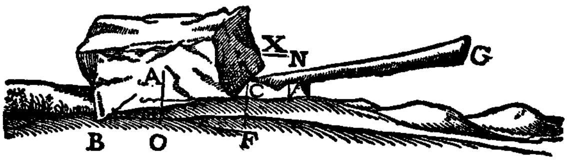

Fig. 16

distances GN and NC and the ratio between FB and BO. Lay off a distance X such that its ratio to NC is the same as that of BO to FB; then, since the total weight A is counterbalanced by the two forces at B and at C, it follows that the force at B is to that at C as the distance FO is to the distance OB. Hence,

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componendo, the sum of the forces at B and C, that is, the total weight A [*momento di tutto 'l peso A*], is to the force at C as the line FB is to the line BO, that is, as NC is to X: but the force [*momento della potenza*] applied at C is to the force applied at G as the distance GN is to the distance NC; hence it follows, *ex æquali in proportione perturbata*,* that the entire weight A is to the force applied at G as the distance GN is to X. But the ratio of GN to X is compounded of the ratio of GN to NC and of NC to X, that is, of FB to BO; hence the weight A bears to the

* For definition of *perturbata* see Todhunter's *Euclid*. Book V, Def. 20. [*Trans.*]

equilibrating force at G a ratio compounded of that of GN to NC and of FB to BO: which was to be proved.

Let us now return to our original subject; then, if what has hitherto been said is clear, it will be easily understood that,

Proposition I

A prism or solid cylinder of glass, steel, wood or other breakable material which is capable of sustaining a very heavy weight when applied longitudinally is, as previously remarked, easily broken by the transverse application of a weight which may be much smaller in proportion as the length of the cylinder exceeds its thickness.

Let us imagine a solid prism ABCD fastened into a wall at the end AB, and supporting a weight E at the other end; understand aiso that the wall is vertical and that the prism or cylinder is fastened at right angles to the wall. It is clear that, if the cylinder breaks, fracture will occur at the point B where the edge of the mortise acts as a fulcrum for the lever BC, to which the force is applied; the thickness of the solid BA is the other arm of the lever along which is located the resistance. This resistance opposes the separation of the part BD, lying outside the wall, from that portion lying inside. From the preceding, it follows that the magnitude [*momento*] of the force applied at C bears to the magnitude [*momento*] of the resistance, found in the thickness of the prism, i. e., in the attachment of the base BA to its contiguous parts, the same ratio which the length CB bears to half the length BA; if now we define absolute resistance to fracture

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as that offered to a longitudinal pull (in which case the stretching force acts in the same direction as that through which the body is moved), then it follows that the absolute resistance of the prism BD is to the breaking load placed at the end of the lever BC in the same ratio as the length BC is to the half of AB in the case of a prism, or the semidiameter in the case of a cylinder. This is our first proposition.* Observe that in what

* The one fundamental error which is implicitly introduced into this proposition and which is carried through the entire discussion of the

has here been said the weight of the solid BD itself has been left out of consideration, or rather, the prism has been assumed to be devoid of weight. But if the weight of the prism is to be taken account of in conjunction with the weight E, we must add to the weight E one half that of the prism BD: so that if, for example, the latter weighs two pounds and the weight E is ten pounds we must treat the weight E as if it were eleven pounds.

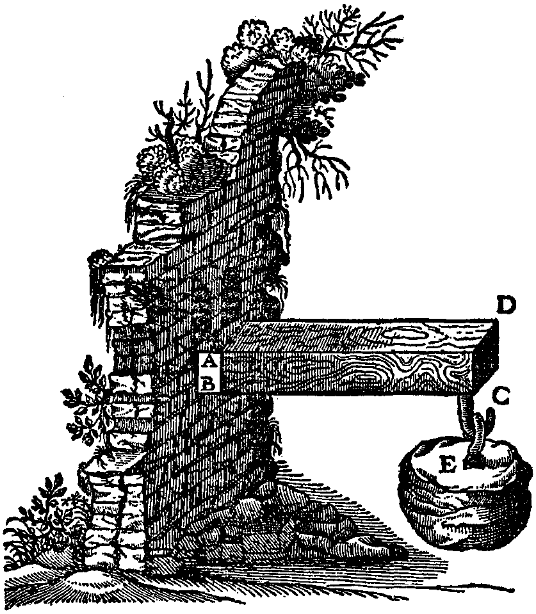

Fig. 17

SIMP. Why not twelve?

SALV. The weight E, my dear Simplicio, hanging at the extreme end C acts upon the lever BC with its full moment of ten pounds: so also would the solid BD if suspended at the same point exert its full moment of two pounds; but, as you know, this solid is uniformly distributed through-

Second Day consists in a failure to see that, in such a beam, there must be equilibrium between the forces of tension and compression over any cross-section. The correct point of view seems first to have been found by E. Mariotte in 1680 and by A. Parent in 1713. Fortunately this error does not vitiate the conclusions of the subsequent propositions which deal only with proportions—not actual strength—of beams. Following K. Pearson (Todhunter's *History of Elasticity*) one might say that Galileo's mistake lay in supposing the fibres of the strained beam to be inextensible. Or, confessing the anachronism, one might say that the error consisted in taking the lowest fibre of the beam as the neutral axis. [*Trans.*]

out its entire length, BC, so that the parts which lie near the end B are less effective than those more remote.

Accordingly if we strike a balance between the two, the weight of the entire prism may be considered as concentrated at its center of gravity which lies midway of the lever BC. But a weight hung at the extremity C exerts a moment twice as great as it would if suspended from the middle: therefore

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if we consider the moments of both as located at the end C we must add to the weight E one-half that of the prism.

SIMP. I understand perfectly; and moreover, if I mistake not, the force of the two weights BD and E, thus disposed, would exert the same moment as would the entire weight BD together with twice the weight E suspended at the middle of the lever BC.

SALV. Precisely so, and a fact worth remembering. Now we can readily understand

PROPOSITION II

How and in what proportion a rod, or rather a prism, whose width is greater than its thickness offers more resistance to fracture when the force is applied in the direction of its breadth than in the direction of its thickness.

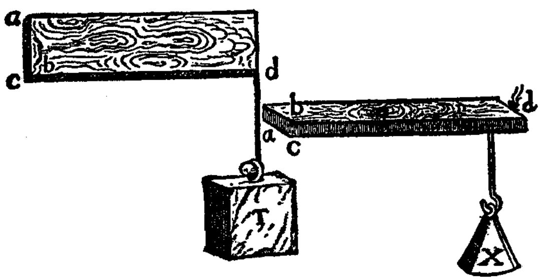

Fig. 18

For the sake of clearness, take a ruler *ad* whose width is *ac* and whose thickness, *cb*, is much less than its width. The question now is why will the ruler, if stood on edge, as in the first figure, withstand a great weight T, while, when laid flat, as in the second figure, it will not support the weight X which is less than T. The answer is evident when we remember that in the one case the

the fulcrum is at the line *bc*, and in the other case at *ca*, while the distance at which the force is applied is the same in both cases, namely, the length *bd:* but in the first case the distance of the resistance from the fulcrum—half the line *ca*—is greater than in the other case where it is only half of *bc*. Therefore the weight T is greater than X in the same ratio as half the width *ca* is greater than half the thickness *bc*, since the former acts as a lever arm for *ca*, and the latter for *cb*, against the same resistance, namely, the strength of all the fibres in the cross-section *ab*. We conclude, therefore, that any given ruler, or prism, whose width exceeds its thickness, will offer greater resistance to fracture when standing on edge than when lying flat, and this in the ratio of the width to the thickness.

Proposition III

Considering now the case of a prism or cylinder growing longer in a horizontal direction, we must find out in what ratio the moment of its own weight increases in comparison with its resistance to fracture. This moment I find increases in propor-
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tion to the square of the length. In order to prove this let AD be a prism or cylinder lying horizontal with its end A firmly fixed in a wall. Let the length of the prism be increased by the addition of the portion BE. It is clear that merely changing the length of the lever from AB to AC will, if we disregard its weight, increase the moment of the force [at the end] tending to produce fracture at A in the ratio of CA to BA. But, besides this, the weight of the solid portion BE, added to the weight of the solid AB increases the moment of the total weight in the ratio of the weight of the prism AE to that of the prism AB, which is the same as the ratio of the length AC to AB.

It follows, therefore, that, when the length and weight are simultaneously increased in any given proportion, the moment, which is the product of these two, is increased in a ratio which is the square of the preceding proportion. The conclusion is then that the bending moments due to the weight of prisms and cylinders which have the same thickness but different lengths, bear

bear to each other a ratio which is the square of the ratio of their lengths, or, what is the same thing, the ratio of the squares of their lengths.

We shall next show in what ratio the resistance to fracture

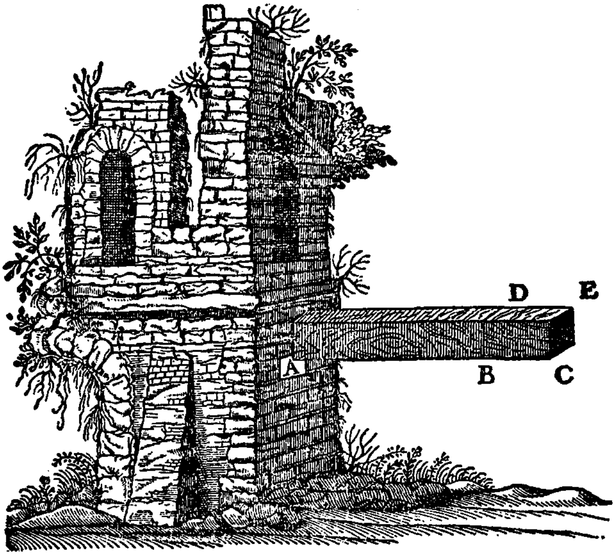

Fig. 19

[bending strength], in prisms and cylinders, increases with in- [160] crease of thickness while the length remains unchanged. Here I say that

PROPOSITION IV

In prisms and cylinders of equal length, but of unequal thicknesses, the resistance to fracture increases in the same ratio as the cube of the diameter of the thickness, i. e., of the base.

Let A and B be two cylinders of equal lengths DG, FH; let their bases be circular but unequal, having the diameters CD and EF. Then I say that the resistance to fracture offered by the cylinder B

B is to that offered by A as the cube of the diameter FE is to the cube of the diameter DC. For, if we consider the resistance to fracture by longitudinal pull as dependent upon the bases, i. e., upon the circles EF and DC, no one can doubt that the strength [*resistenza*] of the cylinder B is greater than that of A in the same proportion in which the area of the circle EF exceeds that of CD; because it is precisely in this ratio that the number of fibres binding the parts of the solid together in the one cylinder exceeds that in the other cylinder.

But in the case of a force acting transversely it must be remembered that we are employing two levers in which the forces are applied at distances DG, FH, and the fulcrums are located at the points D and F; but the resistances are applied at distances which are equal to the radii of the circles DC and EF, since the fibres distributed over these entire cross-sections act as if concentrated at the centers.

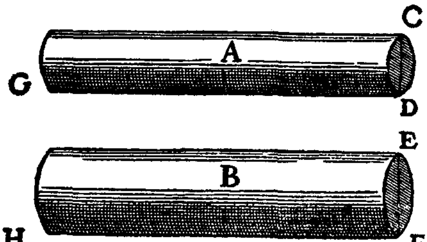

Fig. 20

Remembering this and remembering also that the arms, DG and FH, through which the forces G and H act are equal, we can understand that the resistance, located at the center of the base EF, acting against the force at H, is more effective [*maggiore*] than the resistance at the center of the base CD opposing the force G, in the ratio of the radius FE to the radius DC. Accordingly the resistance to fracture offered by the cylinder B is greater than that of the cylinder A in a ratio which is compounded of that of the area of the circles EF and DC and that of their radii, i. e., of their diameters; but the areas of circles are as the squares of their diameters. Therefore the ratio of the resistances, being the product of the two preceding ratios, is the same as that of the cubes of the diameters. This is what I set out to prove. Also since the volume of a cube

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varies as the third power of its edge we may say that the resistance

sistance [strength] of a cylinder whose length remains constant varies as the third power of its diameter.

From the preceding we are able to conclude that

COROLLARY

The resistance [strength] of a prism or cylinder of constant length varies in the sesquialteral ratio of its volume.

This is evident because the volume of a prism or cylinder of constant altitude varies directly as the area of its base, i. e., as the square of a side or diameter of this base; but, as just demonstrated, the resistance [strength] varies as the cube of this same side or diameter. Hence the resistance varies in the sesquialteral ratio of the volume—consequently also of the weight—of the solid itself.

SIMP. Before proceeding further I should like to have one of my difficulties removed. Up to this point you have not taken into consideration a certain other kind of resistance which, it appears to me, diminishes as the solid grows longer, and this is quite as true in the case of bending as in pulling; it is precisely thus that in the case of a rope we observe that a very long one is less able to support a large weight than a short one. Whence, I believe, a short rod of wood or iron will support a greater weight than if it were long, provided the force be always applied longitudinally and not transversely, and provided also that we take into account the weight of the rope itself which increases with its length.

SALV. I fear, Simplicio, if I correctly catch your meaning, that in this particular you are making the same mistake as many others; that is if you mean to say that a long rope, one of perhaps 40 cubits, cannot hold up so great a weight as a shorter length, say one or two cubits, of the same rope.

SIMP. That is what I meant, and as far as I see the proposition is highly probable.

SALV. On the contrary, I consider it not merely improbable but false; and I think I can easily convince you of your error. Let AB represent the rope, fastened at the upper end A: at the lower end attach a weight C whose force is just sufficient to break

break the rope. Now, Simplicio, point out the exact place where you think the break ought to occur.

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SIMP. Let us say D.

SALV. And why at D?

SIMP. Because at this point the rope is not strong enough to support, say, 100 pounds, made up of the portion of the rope DB and the stone C.

SALV. Accordingly whenever the rope is stretched [*violentata*] with the weight of 100 pounds at D it will break there.

SIMP. I think so.

SALV. But tell me, if instead of attaching the weight at the end of the rope, B, one fastens it at a point nearer D, say, at E: or if, instead of fixing the upper end of the rope at A, one fastens it at some point F, just above D, will not the rope, at the point D, be subject to the same pull of 100 pounds?

Fig. 21

SIMP. It would, provided you include with the stone C the portion of rope EB.

SALV. Let us therefore suppose that the rope is stretched at the point D with a weight of 100 pounds, then according to your own admission it will break; but FE is only a small portion of AB; how can you therefore maintain that the long rope is weaker than the short one? Give up then this erroneous view which you share with many very intelligent people, and let us proceed.

Now having demonstrated that, in the case of [uniformly loaded] prisms and cylinders of constant thickness, the moment of force tending to produce fracture [*momento sopra le proprie resistenze*] varies as the square of the length; and having likewise shown that, when the length is constant and the thickness varies, the resistance to fracture varies as the cube of the side, or diameter, of the base, let us pass to the investigation of the case of solids which simultaneously vary in both length and thickness. Here I observe that,

Proposition V

Prisms and cylinders which differ in both length and thickness offer resistances to fracture [i. e., can support at their ends loads] which are directly proportional to the cubes of the diameters of their bases and inversely proportional to their lengths.

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Let ABC and DEF be two such cylinders; then the resistance [bending strength] of the cylinder AC bears to the resistance of the cylinder DF a ratio which is the product of the cube of the diameter AB divided by the cube of the diameter DE, and of the length EF divided by the length BC. Make EG equal to BC: let H be a third proportional to the lines AB and DE; let I be a fourth proportional, [AB/DE = H/I]: and let I:S = EF:BC.

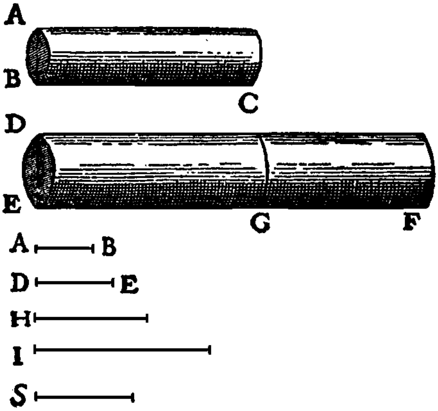

Fig. 22

Now since the resistance of the cylinder AC is to that of the cylinder DG as the cube of AB is to the cube of DE, that is, as the length AB is to the length I; and since the resistance of the cylinder DG is to that of the cylinder DF as the length FE is to EG, that is, as I is to S, it follows that the length AB is to S as the resistance of the cylinder AC is to that of the cylinder DF. But the line AB bears to S a ratio which is the product of AB/I and I/S. Hence the resistance [bending strength] of the cylinder AC bears to the resistance of the cylinder DF a ratio which is the product of AB/I (that is, AB^3/DE^3) and of I/S (that is, EF/BC): which is what I meant to prove.

This proposition having been demonstrated, let us next consider

consider the case of prisms and cylinders which are similar. Concerning these we shall show that,

Proposition VI

In the case of similar cylinders and prisms, the moments [stretching forces] which result from multiplying together their weight and length [i. e., from the moments produced by their own weight and length], which latter acts as a lever-arm, bear to each other a ratio which is the sesquialteral of the ratio between the resistances of their bases.

In order to prove this let us indicate the two similar cylinders by AB and CD: then the magnitude of the force [*momento*] in the cylinder AB, opposing the resistance of its base B, bears to the magnitude [*momento*] of the force at CD, opposing the resistance of its base D, a ratio which is the sesquialteral of the ratio

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between the resistance of the base B and the resistance of the base D. And since the solids AB and CD, are effective in opposing the resistances of their bases B and D, in proportion to their weights and to the mechanical advantages [*forze*] of their lever arms respectively, and since the advantage [*forza*] of the lever arm AB is equal to the advantage [*forza*] of the lever arm CD (this is true because in virtue of the similarity of the cylinders the length AB is to the radius of the base B as the length CD is to the radius of the base D), it follows that the total force [*momento*] of the cylinder AB is to the total force [*momento*] of the cylinder CD as the weight alone of the cylinder AB is to the weight alone of the cylinder CD, that is, as the volume of the cylinder AB [*l'istesso cilindro AB*] is to the volume CD [*all'istesso CD*]: but these are as the cubes of the diameters of their bases B and D; and the resistances of the bases, being to

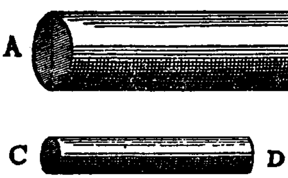

Fig. 23

to each other as their areas, are to each other consequently as the squares of their diameters. Therefore the forces [*momenti*] of the cylinders are to each other in the sesquialteral ratio of the resistance of their bases.*

SIMP. This proposition strikes me as both new and surprising: at first glance it is very different from anything which I myself should have guessed: for since these figures are similar in all other respects, I should have certainly thought that the forces [*momenti*] and the resistances of these cylinders would have borne to each other the same ratio.

SAGR. This is the proof of the proposition to which I referred, at the very beginning of our discussion, as one imperfectly understood by me.

SALV. For a while, Simplicio, I used to think, as you do, that the resistances of similar solids were similar; but a certain casual observation showed me that similar solids do not exhibit a strength which is proportional to their size, the larger ones being less fitted to undergo rough usage just as tall men are more apt than small children to be injured by a fall. And, as we remarked at the outset, a large beam or column falling from a

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given height will go to pieces when under the same circumstances a small scantling or small marble cylinder will not break. It was this observation which led me to the investigation of the fact which I am about to demonstrate to you: it is a very remarkable thing that, among the infinite variety of solids which are similar one to another, there are no two of which the forces [*momenti*], and the resistances of these solids are related in the same ratio.

SIMP. You remind me now of a passage in Aristotle's *Questions*

* The preceding paragraph beginning with Prop. VI is of more than usual interest as illustrating the confusion of terminology current in the time of Galileo. The translation given is literal except in the case of those words for which the Italian is supplied. The facts which Galileo has in mind are so evident that it is difficult to see how one can here interpret "*moment*" to mean the force "*opposing the resistance of its base*," unless "*the force of the lever arm AB*" be taken to mean "*the mechanical advantage of the lever made up of AB and the radius of the base B*"; and similarly for "*the force of the lever arm CD.*"

[*Trans.*]

in Mechanics in which he tries to explain why it is that a wooden beam becomes weaker and can be more easily bent as it grows longer, notwithstanding the fact that the shorter beam is thinner and the longer one thicker: and, if I remember correctly, he explains it in terms of the simple lever.

SALV. Very true: but, since this solution seemed to leave room for doubt, Bishop di Guevara,* whose truly learned commentaries have greatly enriched and illuminated this work, indulges in additional clever speculations with the hope of thus overcoming all difficulties; nevertheless even he is confused as regards this particular point, namely, whether, when the length and thickness of these solid figures increase in the same ratio, their strength and resistance to fracture, as well as to bending, remain constant. After much thought upon this subject, I have reached the following result. First I shall show that,

PROPOSITION VII

Among heavy prisms and cylinders of similar figure, there is one and only one which under the stress of its own weight lies just on the limit between breaking and not breaking: so that every larger one is unable to carry the load of its own weight and breaks; while every smaller one is able to withstand some additional force tending to break it.

Let AB be a heavy prism, the longest possible that will just sustain its own weight, so that if it be lengthened the least bit it will break. Then, I say, this prism is unique among all similar prisms—infinite in number—in occupying that boundary line between breaking and not breaking; so that every larger one

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will break under its own weight, and every smaller one will not break, but will be able to withstand some force in addition to its own weight.

Let the prism CE be similar to, but larger than, AB: then, I say, it will not remain intact but will break under its own weight. Lay off the portion CD, equal in length to AB. And, since, the resistance [bending strength] of CD is to that of AB as

* Bishop of Teano; b. 1561; d. 1641. [*Trans.*]

the cube of the thickness of CD is to the cube of the thickness of AB, that is, as the prism CE is to the similar prism AB, it follows that the weight of CE is the utmost load which a prism of the length CD can sustain; but the length of CE is greater; therefore the prism CE will break. Now take another prism FG which is smaller than AB. Let FH equal AB, then it can be shown in a similar manner that the resistance [bending strength] of FG is to that of AB as the prism FG is to the prism AB provided the distance AB that is FH, is equal to the distance FG; but AB is greater than FG, and therefore the moment of the prism FG applied at G is not sufficient to break the prism FG.

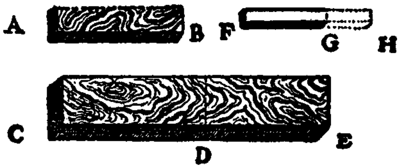

Fig. 24

SAGR. The demonstration is short and clear; while the proposition which, at first glance, appeared improbable is now seen to be both true and inevitable. In order therefore to bring this prism into that limiting condition which separates breaking from not breaking, it would be necessary to change the ratio between thickness and length either by increasing the thickness or by diminishing the length. An investigation of this limiting state will, I believe, demand equal ingenuity.

SALV. Nay, even more; for the question is more difficult; this I know because I spent no small amount of time in its discovery which I now wish to share with you.

PROPOSITION VIII

Given a cylinder or prism of the greatest length consistent with its not breaking under its own weight; and having given a greater length, to find the diameter of another cylinder or prism of this greater length which shall be the only and largest one capable of withstanding its own weight.

Let BC be the largest cylinder capable of sustaining its own weight; and let DE be a length greater than AC: the problem is to find the diameter of the cylinder which, having the length

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DE,

DE, shall be the largest one just able to withstand its own weight. Let I be a third proportional to the lengths DE and AC; let the diameter FD be to the diameter BA as DE is to I; draw the cylinder FE; then, among all cylinders having the same proportions, this is the largest and only one just capable of sustaining its own weight.

Let M be a third proportional to DE and I: also let O be a fourth proportional to DE, I, and M; lay off FG equal to AC. Now since the diameter FD is to the diameter AB as the length DE is to I, and since O is a fourth proportional to DE, I and M, it follows that $\overline{FD}^3:\overline{BA}^3=DE:O$. But the resistance [bending strength] of the cylinder DG is to the resistance of the cylinder BC as the cube of FD is to the cube of BA: hence the resistance of the cylinder DG is to that of cylinder BC as the length DE is to O. And since the moment of the cylinder BC is held in

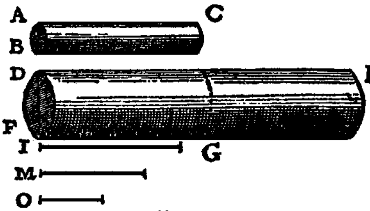

Fig. 25

equilibrium by [*e equale alla*] its resistance, we shall accomplish our end (which is to prove that the moment of the cylinder FE is equal to the resistance located at FD), if we show that the moment of the cylinder FE is to the moment of the cylinder BC as the resistance DF is to the resistance BA, that is, as the cube of FD is to the cube of BA, or as the length DE is to O. The moment of the cylinder FE is to the moment of the cylinder DG as the square of DE is to the square of AC, that is, as the length DE is to I; but the moment of the cylinder DG is to the moment of the cylinder BC, as the square of DF is to the square of BA, that is, as the square of DE is to the square of I, or as the square of I is to the square of M, or, as I is to O. Therefore by equating ratios, it results that the moment of the cylinder FE is to the moment of the cylinder BC as the length DE is to O, that is, as the cube of DF is to the cube of BA, or as the resistance of the base DF is to the resistance of the base BA; which was to be proven.

SAGR. This demonstration, Salviati, is rather long and difficult

cult to keep in mind from a single hearing. Will you not, therefore, be good enough to repeat it?

SALV. As you like; but I would suggest instead a more direct and a shorter proof: this will, however, necessitate a different figure.

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SAGR. The favor will be that much greater: nevertheless I hope you will oblige me by putting into written form the argument just given so that I may study it at my leisure.

SALV. I shall gladly do so. Let A denote a cylinder of diameter DC and the largest capable of sustaining its own weight: the problem is to determine a larger cylinder which shall be at once the maximum and the unique one capable of sustaining its own weight.

Let E be such a cylinder, similar to A, having the assigned length, and having a diameter KL. Let MN be a third proportional to the two lengths DC and KL: let MN also be the diameter of another cylinder, X, having the same length as E: then, I say, X is the cylinder sought. Now since the resistance of the base DC is to the resistance of the base KL as the square of DC is to the square of KL, that is, as the square of KL is to the square of MN, or, as the cylinder E is to the cylinder X, that is, as the moment E is to the moment X; and since also the resistance [bending strength] of the base KL is to the resistance of the base MN as the cube of KL is to the cube of MN, that is, as the cube of DC is to the cube of KL, or, as the cylinder A is to the cylinder E, that is, as the moment of A is to the moment of E; hence it follows, *ex æquali in proportione perturbata*,* that the moment of A is to the moment of X as the resistance of the base DC is to the resistance of the base MN; therefore moment and resistance are related to each other in prism X precisely as they are in prism A.

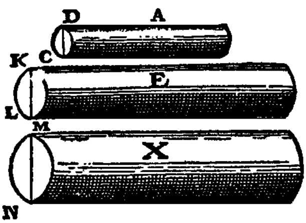

Fig. 26

* For definition of *perturbata* see Todhunter's *Euclid*, Book V, Def. 20. [*Trans.*]

Next Page

Let us now generalize the problem; then it will read as follows:

> Given a cylinder AC in which moment and resistance [bending strength] are related in any manner whatsoever; let DE be the length of another cylinder; then determine what its thickness must be in order that the relation between its moment and resistance shall be identical with that of the cylinder AC.

Using Fig. 25 in the same manner as above, we may say that, since the moment of the cylinder FE is to the moment of the portion DG as the square of ED is to the square of FG, that is, as the length DE is to I; and since the moment of the cylinder FG is to the moment of the cylinder AC as the square of FD is to the square of AB, or, as the square of ED is to the square of I, or, as the square of I is to the square of M, that is, as the length I is to O; it follows, *ex æquali*, that the moment of the

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cylinder FE is to the moment of the cylinder AC as the length DE is to O, that is, as the cube of DE is to the cube of I, or, as the cube of FD is to the cube of AB, that is, as the resistance of the base FD is to the resistance of the base AB; which was to be proven.

From what has already been demonstrated, you can plainly see the impossibility of increasing the size of structures to vast dimensions either in art or in nature; likewise the impossibility of building ships, palaces, or temples of enormous size in such a way that their oars, yards, beams, iron-bolts, and, in short, all their other parts will hold together; nor can nature produce trees of extraordinary size because the branches would break down under their own weight; so also it would be impossible to build up the bony structures of men, horses, or other animals so as to hold together and perform their normal functions if these animals were to be increased enormously in height; for this increase in height can be accomplished only by employing a material which is harder and stronger than usual, or by enlarging the size of the bones, thus changing their shape until the form and appearance of the animals suggest a monstrosity. This is perhaps

THIRD DAY

[190]

CHANGE OF POSITION. [*De Motu Locali*]

MY purpose is to set forth a very new science dealing with a very ancient subject. There is, in nature, perhaps nothing older than motion, concerning which the books written by philosophers are neither few nor small; nevertheless I have discovered by experiment some properties of it which are worth knowing and which have not hitherto been either observed or demonstrated. Some superficial observations have been made, as, for instance, that the free motion [*naturalem motum*] of a heavy falling body is continuously accelerated;* but to just what extent this acceleration occurs has not yet been announced; for so far as I know, no one has yet pointed out that 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.†

It has been observed that missiles and projectiles describe a curved path of some sort; however no one has pointed out the fact that this path is a parabola. But this and other facts, not few in number or less worth knowing, I have succeeded in proving; and what I consider more important, there have been opened up to this vast and most excellent science, of which my

* "Natural motion" of the author has here been translated into "free motion"—since this is the term used to-day to distinguish the "natural" from the "violent" motions of the Renaissance. [*Trans.*]

† A theorem demonstrated on p. 175 below. [*Trans.*]

work is merely the beginning, ways and means by which other minds more acute than mine will explore its remote corners.

This discussion is divided into three parts; the first part deals with motion which is steady or uniform; the second treats of motion as we find it accelerated in nature; the third deals with the so-called violent motions and with projectiles.

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UNIFORM MOTION

In dealing with steady or uniform motion, we need a single definition which I give as follows:

Definition

By steady or uniform motion, I mean one in which the distances traversed by the moving particle during any equal intervals of time, are themselves equal.

Caution

We must add to the old definition (which defined steady motion simply as one in which equal distances are traversed in equal times) the word "any," meaning by this, all equal intervals of time; for it may happen that the moving body will traverse equal distances during some equal intervals of time and yet the distances traversed during some small portion of these time-intervals may not be equal, even though the time-intervals be equal.

From the above definition, four axioms follow, namely:

Axiom I

In the case of one and the same uniform motion, the distance traversed during a longer interval of time is greater than the distance traversed during a shorter interval of time.

Axiom II

In the case of one and the same uniform motion, the time required to traverse a greater distance is longer than the time required for a less distance.

Axiom III

In one and the same interval of time, the distance traversed at a greater speed is larger than the distance traversed at a less speed.

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Axiom IV

The speed required to traverse a longer distance is greater than that required to traverse a shorter distance during the same time-interval.

Theorem I, Proposition I

If a moving particle, carried uniformly at a constant speed, traverses two distances the time-intervals required are to each other in the ratio of these distances.

Let a particle move uniformly with constant speed through two distances AB, BC, and let the time required to traverse AB be represented by DE; the time required to traverse BC, by EF;

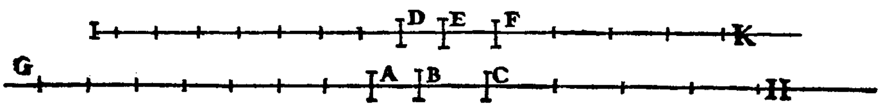

Fig. 40

then I say that the distance AB is to the distance BC as the time DE is to the time EF.

Let the distances and times be extended on both sides towards G, H and I, K; let AG be divided into any number whatever of spaces each equal to AB, and in like manner lay off in DI exactly the same number of time-intervals each equal to DE. Again lay off in CH any number whatever of distances each equal to BC; and in FK exactly the same number of time-intervals each equal to EF; then will the distance BG and the time EI be equal and arbitrary multiples of the distance BA and the time ED; and likewise the distance HB and the time KE are equal and arbitrary multiples of the distance CB and the time FE.

And since DE is the time required to traverse AB, the whole time

time EI will be required for the whole distance BG, and when the motion is uniform there will be in EI as many time-intervals each equal to DE as there are distances in BG each equal to BA; and likewise it follows that KE represents the time required to traverse HB.

Since, however, the motion is uniform, it follows that if the distance GB is equal to the distance BH, then must also the time IE be equal to the time EK; and if GB is greater than BH, then also IE will be greater than EK; and if less, less.* There [193] are then four quantities, the first AB, the second BC, the third DE, and the fourth EF; the time IE and the distance GB are arbitrary multiples of the first and the third, namely of the distance AB and the time DE.

But it has been proved that *both* of these latter quantities are either equal to, greater than, or less than the time EK and the space BH, which are arbitrary multiples of the second and the fourth. Therefore the first is to the second, namely the distance AB is to the distance BC, as the third is to the fourth, namely the time DE is to the time EF. Q. E. D.

Theorem II, Proposition II

If a moving particle traverses two distances in equal intervals of time, these distances will bear to each other the same ratio as the speeds. And conversely if the distances are as the speeds then the times are equal.

Referring to Fig. 40, let AB and BC represent the two distances traversed in equal time-intervals, the distance AB for instance with the velocity DE, and the distance BC with the velocity EF. Then, I say, the distance AB is to the distance BC as the velocity DE is to the velocity EF. For if equal multiples of both distances and speeds be taken, as above, namely, GB and IE of AB and DE respectively, and in like manner HB and KE of BC and EF, then one may infer, in the same manner as above, that the multiples GB and IE are either less than, equal

* The method here employed by Galileo is that of Euclid as set forth in the famous 5th Definition of the Fifth Book of his *Elements*, for which see *art. Geometry* Ency. Brit. 11th Ed. p. 683. [*Trans.*]

to, or greater than equal multiples of BH and EK. Hence the theorem is established.

Theorem III, Proposition III

In the case of unequal speeds, the time-intervals required to traverse a given space are to each other inversely as the speeds.

Let the larger of the two unequal speeds be indicated by A; the smaller, by B; and let the motion corresponding to both traverse the given space CD. Then I say the time required to traverse the distance CD at speed A is to the time required to traverse the same distance at speed B, as the speed B is to the speed A. For let CD be to CE as A is to B; then, from the preceding, it follows that the time required to complete the distance CD at speed A is the same as

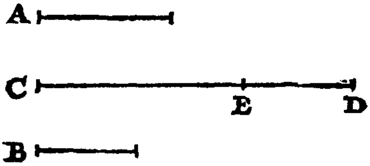

Fig. 41

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the time necessary to complete CE at speed B; but the time needed to traverse the distance CE at speed B is to the time required to traverse the distance CD at the same speed as CE is to CD; therefore the time in which CD is covered at speed A is to the time in which CD is covered at speed B as CE is to CD, that is, as speed B is to speed A. Q. E. D.

Theorem IV, Proposition IV

If two particles are carried with uniform motion, but each with a different speed, the distances covered by them during unequal intervals of time bear to each other the compound ratio of the speeds and time intervals.

Let the two particles which are carried with uniform motion be E and F and let the ratio of the speed of the body E be to that of the body F as A is to B; but let the ratio of the time consumed by the motion of E be to the time consumed by the motion of F as C is to D. Then, I say, that the distance covered by E, with speed A in time C, bears to the space traversed by F with speed B

B in time D a ratio which is the product of the ratio of the speed A to the speed B by the ratio of the time C to the time D. For if G is the distance traversed by E at speed A during the time-interval C, and if G is to I as the speed A is to the speed B; and if also the time-interval C is to the time-interval D as I is to L, then it follows that I is the distance traversed by F in the same time that G is traversed by E since G is to I in the same ratio as the speed A to the speed B. And since I is to L in the same ratio as the time-intervals C and D, if I is the distance traversed by F during the interval C, then L will be the distance traversed by F during the interval D at the speed B.

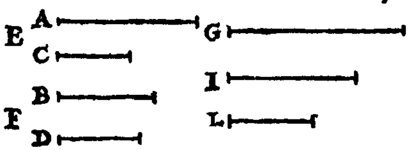

Fig. 42

But the ratio of G to L is the product of the ratios G to I and I to L, that is, of the ratios of the speed A to the speed B and of the time-interval C to the time-interval D. Q. E. D.

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Theorem V, Proposition V

If two particles are moved at a uniform rate, but with unequal speeds, through unequal distances, then the ratio of the time-intervals occupied will be the product of the ratio of the distances by the inverse ratio of the speeds.

Let the two moving particles be denoted by A and B, and let the speed of A be to the speed of B in the ratio of V to T; in like manner let the distances traversed be in the ratio of S to R; then I say that the ratio of the time-interval during which the motion of A occurs to the time-interval occupied by the motion of B is the product of the ratio of the speed T to the speed V by the ratio of the distance S to the distance R.

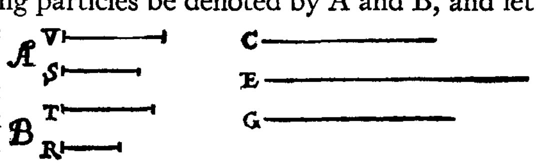

Fig. 43

Let C be the time-interval occupied by the motion of A, and let

let the time-interval C bear to a time-interval E the same ratio as the speed T to the speed V.

And since C is the time-interval during which A, with speed V, traverses the distance S and since T, the speed of B, is to the speed V, as the time-interval C is to the time-interval E, then E will be the time required by the particle B to traverse the distance S. If now we let the time-interval E be to the time-interval G as the distance S is to the distance R, then it follows that G is the time required by B to traverse the space R. Since the ratio of C to G is the product of the ratios C to E and E to G (while also the ratio of C to E is the inverse ratio of the speeds of A and B respectively, i. e., the ratio of T to V); and since the ratio of E to G is the same as that of the distances S and R respectively, the proposition is proved.

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Theorem VI, Proposition VI

> If two particles are carried at a uniform rate, the ratio of their speeds will be the product of the ratio of the distances traversed by the inverse ratio of the time-intervals occupied.

Let A and B be the two particles which move at a uniform rate; and let the respective distances traversed by them have the ratio of V to T, but let the time-intervals be as S to R. Then I say the speed of A will bear to the speed of B a ratio which is the product of the ratio of the distance V to the distance T and the time-interval R to the time-interval S.

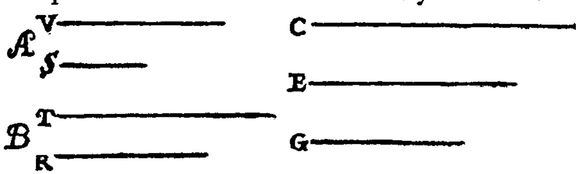

Fig. 44

Let C be the speed at which A traverses the distance V during the time-interval S; and let the speed C bear the same ratio to another speed E as V bears to T; then E will be the speed at which B traverses the distance T during the time-interval S. If now the speed E is to another speed G as the time-interval R is to the time-interval S, then G will be the speed at which the particle

particle B traverses the distance T during the time-interval R. Thus we have the speed C at which the particle A covers the distance V during the time S and also the speed G at which the particle B traverses the distance T during the time R. The ratio of C to G is the product of the ratio C to E and E to G; the ratio of C to E is by definition the same as the ratio of the distance V to distance T; and the ratio of E to G is the same as the ratio of R to S. Hence follows the proposition.

SALV. The preceding is what our Author has written concerning uniform motion. We pass now to a new and more discriminating consideration of naturally accelerated motion, such as that generally experienced by heavy falling bodies; following is the title and introduction.

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NATURALLY ACCELERATED MOTION

The properties belonging to uniform motion have been discussed in the preceding section; but accelerated motion remains to be considered.

And first of all it seems desirable to find and explain a definition best fitting natural phenomena. For anyone may invent an arbitrary type of motion and discuss its properties; thus, for instance, some have imagined helices and conchoids as described by certain motions which are not met with in nature, and have very commendably established the properties which these curves possess in virtue of their definitions; but we have decided to consider the phenomena of bodies falling with an acceleration such as actually occurs in nature and to make this definition of accelerated motion exhibit the essential features of observed accelerated motions. And this, at last, after repeated efforts we trust we have succeeded in doing. In this belief we are confirmed mainly by the consideration that experimental results are seen to agree with and exactly correspond with those properties which have been, one after another, demonstrated by us. Finally, in the investigation of naturally accelerated motion we were led, by hand as it were, in following the habit and custom of nature

nature herself, in all her various other processes, to employ only those means which are most common, simple and easy.

For I think no one believes that swimming or flying can be accomplished in a manner simpler or easier than that instinctively employed by fishes and birds.

When, therefore, I observe a stone initially at rest falling from an elevated position and continually acquiring new increments of speed, why should I not believe that such increases take place in a manner which is exceedingly simple and rather obvious to everybody? If now we examine the matter carefully we find no addition or increment more simple than that which repeats itself always in the same manner. This we readily understand when we consider the intimate relationship between time and motion; for just as uniformity of motion is defined by and conceived through equal times and equal spaces (thus we call a motion uniform when equal distances are traversed during equal time-intervals), so also we may, in a similar manner, through equal time-intervals, conceive additions of speed as taking place without complication; thus we may picture to our

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mind a motion as uniformly and continuously accelerated when, during any equal intervals of time whatever, equal increments of speed are given to it. Thus if any equal intervals of time whatever have elapsed, counting from the time at which the moving body left its position of rest and began to descend, the amount of speed acquired during the first two time-intervals will be double that acquired during the first time-interval alone; so the amount added during three of these time-intervals will be treble; and that in four, quadruple that of the first time-interval. To put the matter more clearly, if a body were to continue its motion with the same speed which it had acquired during the first time-interval and were to retain this same uniform speed, then its motion would be twice as slow as that which it would have if its velocity had been acquired during *two* time-intervals.

And thus, it seems, we shall not be far wrong if we put the increment of speed as proportional to the increment of time; hence

hence the definition of motion which we are about to discuss may be stated as follows: A motion is said to be uniformly accelerated, when starting from rest, it acquires, during equal time-intervals, equal increments of speed.

SAGR. Although I can offer no rational objection to this or indeed to any other definition, devised by any author whomsoever, since all definitions are arbitrary, I may nevertheless without offense be allowed to doubt whether such a definition as the above, established in an abstract manner, corresponds to and describes that kind of accelerated motion which we meet in nature in the case of freely falling bodies. And since the Author apparently maintains that the motion described in his definition is that of freely falling bodies, I would like to clear my mind of certain difficulties in order that I may later apply myself more earnestly to the propositions and their demonstrations.

SALV. It is well that you and Simplicio raise these difficulties. They are, I imagine, the same which occurred to me when I first saw this treatise, and which were removed either by discussion with the Author himself, or by turning the matter over in my own mind.

SAGR. When I think of a heavy body falling from rest, that is, starting with zero speed and gaining speed in proportion to the

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time from the beginning of the motion; such a motion as would, for instance, in eight beats of the pulse acquire eight degrees of speed; having at the end of the fourth beat acquired four degrees; at the end of the second, two; at the end of the first, one: and since time is divisible without limit, it follows from all these considerations that if the earlier speed of a body is less than its present speed in a constant ratio, then there is no degree of speed however small (or, one may say, no degree of slowness however great) with which we may not find this body travelling after starting from infinite slowness, i. e., from rest. So that if that speed which it had at the end of the fourth beat was such that, if kept uniform, the body would traverse two miles in an hour, and if keeping the speed which it had at the end of the second

second beat, it would traverse one mile an hour, we must infer that, as the instant of starting is more and more nearly approached, the body moves so slowly that, if it kept on moving at this rate, it would not traverse a mile in an hour, or in a day, or in a year or in a thousand years; indeed, it would not traverse a span in an even greater time; a phenomenon which baffles the imagination, while our senses show us that a heavy falling body suddenly acquires great speed.

SALV. This is one of the difficulties which I also at the beginning, experienced, but which I shortly afterwards removed; and the removal was effected by the very experiment which creates the difficulty for you. You say the experiment appears to show that immediately after a heavy body starts from rest it acquires a very considerable speed: and I say that the same experiment makes clear the fact that the initial motions of a falling body, no matter how heavy, are very slow and gentle. Place a heavy body upon a yielding material, and leave it there without any pressure except that owing to its own weight; it is clear that if one lifts this body a cubit or two and allows it to fall upon the same material, it will, with this impulse, exert a new and greater pressure than that caused by its mere weight; and this effect is brought about by the [weight of the] falling body together with the velocity acquired during the fall, an effect which will be greater and greater according to the height of the fall, that is according as the velocity of the falling body becomes greater. From the quality and intensity of the blow we are thus enabled to accurately estimate the speed of a falling body. But tell me, gentlemen, is it not true that if a block be allowed to fall upon a stake from a height of four cubits and drives it into the earth,

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say, four finger-breadths, that coming from a height of two cubits it will drive the stake a much less distance, and from the height of one cubit a still less distance; and finally if the block be lifted only one finger-breadth how much more will it accomplish than if merely laid on top of the stake without percussion? Certainly very little. If it be lifted only the thickness of a leaf, the effect will be altogether imperceptible. And since the

effect

effect of the blow depends upon the velocity of this striking body, can any one doubt the motion is very slow and the speed more than small whenever the effect [of the blow] is imperceptible? See now the power of truth; the same experiment which at first glance seemed to show one thing, when more carefully examined, assures us of the contrary.

But without depending upon the above experiment, which is doubtless very conclusive, it seems to me that it ought not to be difficult to establish such a fact by reasoning alone. Imagine a heavy stone held in the air at rest; the support is removed and the stone set free; then since it is heavier than the air it begins to fall, and not with uniform motion but slowly at the beginning and with a continuously accelerated motion. Now since velocity can be increased and diminished without limit, what reason is there to believe that such a moving body starting with infinite slowness, that is, from rest, immediately acquires a speed of ten degrees rather than one of four, or of two, or of one, or of a half, or of a hundredth; or, indeed, of any of the infinite number of small values [of speed]? Pray listen. I hardly think you will refuse to grant that the gain of speed of the stone falling from rest follows the same sequence as the diminution and loss of this same speed when, by some impelling force, the stone is thrown to its former elevation: but even if you do not grant this, I do not see how you can doubt that the ascending stone, diminishing in speed, must before coming to rest pass through every possible degree of slowness.

SIMP. But if the number of degrees of greater and greater slowness is limitless, they will never be all exhausted, therefore such an ascending heavy body will never reach rest, but will continue to move without limit always at a slower rate; but this is not the observed fact.

SALV. This would happen, Simplicio, if the moving body were to maintain its speed for any length of time at each degree of velocity; but it merely passes each point without delaying more than an instant: and since each time-interval however

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small may be divided into an infinite number of instants, these

will

will always be sufficient [in number] to correspond to the infinite degrees of diminished velocity.

That such a heavy rising body does not remain for any length of time at any given degree of velocity is evident from the following: because if, some time-interval having been assigned, the body moves with the same speed in the last as in the first instant of that time-interval, it could from this second degree of elevation be in like manner raised through an equal height, just as it was transferred from the first elevation to the second, and by the same reasoning would pass from the second to the third and would finally continue in uniform motion forever.

Sagr. From these considerations it appears to me that we may obtain a proper solution of the problem discussed by philosophers, namely, what causes the acceleration in the natural motion of heavy bodies? Since, as it seems to me, the force [*virtù*] impressed by the agent projecting the body upwards diminishes continuously, this force, so long as it was greater than the contrary force of gravitation, impelled the body upwards; when the two are in equilibrium the body ceases to rise and passes through the state of rest in which the impressed impetus [*impeto*] is not destroyed, but only its excess over the weight of the body has been consumed—the excess which caused the body to rise. Then as the diminution of the outside impetus [*impeto*] continues, and gravitation gains the upper hand, the fall begins, but slowly at first on account of the opposing impetus [*virtù impressa*], a large portion of which still remains in the body; but as this continues to diminish it also continues to be more and more overcome by gravity, hence the continuous acceleration of motion.

Simp. The idea is clever, yet more subtle than sound; for even if the argument were conclusive, it would explain only the case in which a natural motion is preceded by a violent motion, in which there still remains active a portion of the external force [*virtù esterna*]; but where there is no such remaining portion and the body starts from an antecedent state of rest, the cogency of the whole argument fails.

Sagr. I believe that you are mistaken and that this distinction

tion between cases which you make is superfluous or rather non-existent. But, tell me, cannot a projectile receive from the projector either a large or a small force [*virtù*] such as will throw it to a height of a hundred cubits, and even twenty or four or one?

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SIMP. Undoubtedly, yes.

SAGR. So therefore this impressed force [*virtù impressa*] may exceed the resistance of gravity so slightly as to raise it only a finger-breadth; and finally the force [*virtù*] of the projector may be just large enough to exactly balance the resistance of gravity so that the body is not lifted at all but merely sustained. When one holds a stone in his hand does he do anything but give it a force impelling [*virtù impellente*] it upwards equal to the power [*facoltà*] of gravity drawing it downwards? And do you not continuously impress this force [*virtù*] upon the stone as long as you hold it in the hand? Does it perhaps diminish with the time during which one holds the stone?

And what does it matter whether this support which prevents the stone from falling is furnished by one's hand or by a table or by a rope from which it hangs? Certainly nothing at all. You must conclude, therefore, Simplicio, that it makes no difference whatever whether the fall of the stone is preceded by a period of rest which is long, short, or instantaneous provided only the fall does not take place so long as the stone is acted upon by a force [*virtù*] opposed to its weight and sufficient to hold it at rest.

SALV. The present does not seem to be the proper time to investigate the cause of the acceleration of natural motion concerning which various opinions have been expressed by various philosophers, some explaining it by attraction to the center, others to repulsion between the very small parts of the body, while still others attribute it to a certain stress in the surrounding medium which closes in behind the falling body and drives it from one of its positions to another. Now, all these fantasies, and others too, ought to be examined; but it is not really worth while. At present it is the purpose of our Author merely to

investigate

investigate and to demonstrate some of the properties of accelerated motion (whatever the cause of this acceleration may be)—meaning thereby a motion, such that the momentum of its velocity [*i momenti della sua velocità*] goes on increasing after departure from rest, in simple proportionality to the time, which is the same as saying that in equal time-intervals the body receives equal increments of velocity; and if we find the properties [of accelerated motion] which will be demonstrated later are realized in freely falling and accelerated bodies, we may conclude that the assumed definition includes such a motion of falling bodies and that their speed [*accelerazione*] goes on increasing as the time and the duration of the motion.

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SAGR. So far as I see at present, the definition might have been put a little more clearly perhaps without changing the fundamental idea, namely, uniformly accelerated motion is such that its speed increases in proportion to the space traversed; so that, for example, the speed acquired by a body in falling four cubits would be double that acquired in falling two cubits and this latter speed would be double that acquired in the first cubit. Because there is no doubt but that a heavy body falling from the height of six cubits has, and strikes with, a momentum [*impeto*] double that it had at the end of three cubits, triple that which it had at the end of one.

SALV. It is very comforting to me to have had such a companion in error; and moreover let me tell you that your proposition seems so highly probable that our Author himself admitted, when I advanced this opinion to him, that he had for some time shared the same fallacy. But what most surprised me was to see two propositions so inherently probable that they commanded the assent of everyone to whom they were presented, proven in a few simple words to be not only false, but impossible.

SIMP. I am one of those who accept the proposition, and believe that a falling body acquires force [*vires*] in its descent, its velocity increasing in proportion to the space, and that the momentum [*momento*] of the falling body is doubled when it falls
from

from a doubled height; these propositions, it appears to me, ought to be conceded without hesitation or controversy.

SALV. And yet they are as false and impossible as that motion should be completed instantaneously; and here is a very clear demonstration of it. If the velocities are in proportion to the spaces traversed, or to be traversed, then these spaces are traversed in equal intervals of time; if, therefore, the velocity with which the falling body traverses a space of eight feet were double that with which it covered the first four feet (just as the one distance is double the other) then the time-intervals required for these passages would be equal. But for one and the same body to fall eight feet and four feet in the same time is possible only in the case of instantaneous [discontinuous] motion;

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but observation shows us that the motion of a falling body occupies time, and less of it in covering a distance of four feet than of eight feet; therefore it is not true that its velocity increases in proportion to the space.

The falsity of the other proposition may be shown with equal clearness. For if we consider a single striking body the difference of momentum in its blows can depend only upon difference of velocity; for if the striking body falling from a double height were to deliver a blow of double momentum, it would be necessary for this body to strike with a doubled velocity; but with this doubled speed it would traverse a doubled space in the same time-interval; observation however shows that the time required for fall from the greater height is longer.

SAGR. You present these recondite matters with too much evidence and ease; this great facility makes them less appreciated than they would be had they been presented in a more abstruse manner. For, in my opinion, people esteem more lightly that knowledge which they acquire with so little labor than that acquired through long and obscure discussion.

SALV. If those who demonstrate with brevity and clearness the fallacy of many popular beliefs were treated with contempt instead of gratitude the injury would be quite bearable; but on the other hand it is very unpleasant and annoying to see men,

who

who claim to be peers of anyone in a certain field of study, take for granted certain conclusions which later are quickly and easily shown by another to be false. I do not describe such a feeling as one of envy, which usually degenerates into hatred and anger against those who discover such fallacies; I would call it a strong desire to maintain old errors, rather than accept newly discovered truths. This desire at times induces them to unite against these truths, although at heart believing in them, merely for the purpose of lowering the esteem in which certain others are held by the unthinking crowd. Indeed, I have heard from our Academician many such fallacies held as true but easily refutable; some of these I have in mind.

SAGR. You must not withhold them from us, but, at the proper time, tell us about them even though an extra session be necessary. But now, continuing the thread of our talk, it would

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seem that up to the present we have established the definition of uniformly accelerated motion which is expressed as follows:

> A motion is said to be equally or uniformly accelerated when, starting from rest, its momentum (*celeritatis momenta*) receives equal increments in equal times.

SALV. This definition established, the Author makes a single assumption, namely,

> The speeds acquired by one and the same body moving down planes of different inclinations are equal when the heights of these planes are equal.

By the height of an inclined plane we mean the perpendicular let fall from the upper end of the plane upon the horizontal line drawn through the lower end of the same plane. Thus, to illustrate, let the line AB be horizontal, and let the planes CA and CD be inclined to it; then the Author calls the perpendicular CB the "height" of the planes CA and CD; he supposes that the speeds acquired by one and the same body, descending along the planes CA and CD to the terminal points A and D are equal since the heights of these planes are the same, CB; and also it must be understood that this speed is that which would be acquired by the same body falling from C to B.

Sagr.

SAGR. Your assumption appears to me so reasonable that it ought to be conceded without question, provided of course there are no chance or outside resistances, and that the planes are hard and smooth, and that the figure of the moving body is perfectly round, so that neither plane nor moving body is rough. All resistance and opposition having been removed, my reason tells me at once that a heavy and perfectly round ball descending along the lines CA, CD, CB would reach the terminal points A, D, B, with equal momenta [*impeti eguali*].

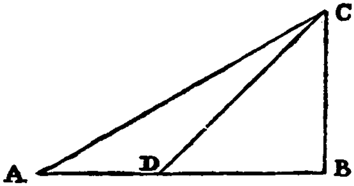

Fig. 45

SALV. Your words are very plausible; but I hope by experiment to increase the probability to an extent which shall be little short of a rigid demonstration.

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Imagine this page to represent a vertical wall, with a nail driven into it; and from the nail let there be suspended a lead bullet of one or two ounces by means of a fine vertical thread, AB, say from four to six feet long, on this wall draw a horizontal line DC, at right angles to the vertical thread AB, which hangs about two finger-breadths in front of the wall. Now bring the thread AB with the attached ball into the position AC and set it free; first it will be observed to descend along the arc CBD, to pass the point B, and to travel along the arc BD, till it almost reaches the horizontal CD, a slight shortage being caused by the resistance of the air and the string; from this we may rightly infer that the ball in its descent through the arc CB acquired a momentum [*impeto*] on reaching B, which was just sufficient to carry it through a similar arc BD to the same height. Having repeated this experiment many times, let us now drive a nail into the wall close to the perpendicular AB, say at E or F, so that it projects out some five or six finger-breadths in order that the thread, again carrying the bullet through the arc CB, may strike upon the nail E when the bullet reaches B, and thus compel it to traverse the arc BG, described about E as center. From this we we can see what can be done by the same momentum [*impeto*] which previously starting at the same point B carried the same body through the arc BD to the horizontal CD. Now, gentlemen, you will observe with pleasure that the ball swings to the point G in the horizontal, and you would see the same thing happen if the obstacle were placed at some lower point, say at F, about which the ball would describe the arc BI, the rise of the

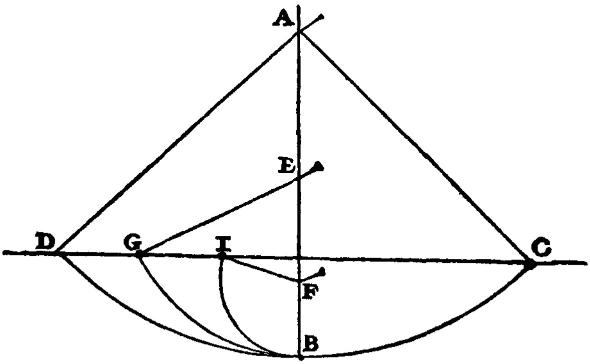

Fig. 46

ball always terminating exactly on the line CD. But when the nail is placed so low that the remainder of the thread below it will not reach to the height CD (which would happen if the nail were placed nearer B than to the intersection of AB with the
[207]
horizontal CD) then the thread leaps over the nail and twists itself about it.

This experiment leaves no room for doubt as to the truth of our supposition; for since the two arcs CB and DB are equal and similarly placed, the momentum [*momento*] acquired by the fall through the arc CB is the same as that gained by fall through the arc DB; but the momentum [*momento*] acquired at B, owing to fall through CB, is able to lift the same body [*mobile*] through the arc BD; therefore, the momentum acquired in the fall BD is equal to that which lifts the same body through the same arc from B to D; so, in general, every momentum acquired by fall through

through an arc is equal to that which can lift the same body through the same arc. But all these momenta [*momenti*] which cause a rise through the arcs BD, BG, and BI are equal, since they are produced by the same momentum, gained by fall through CB, as experiment shows. Therefore all the momenta gained by fall through the arcs DB, GB, IB are equal.

SAGR. The argument seems to me so conclusive and the experiment so well adapted to establish the hypothesis that we may, indeed, consider it as demonstrated.

SALV. I do not wish, Sagredo, that we trouble ourselves too much about this matter, since we are going to apply this principle mainly in motions which occur on plane surfaces, and not upon curved, along which acceleration varies in a manner greatly different from that which we have assumed for planes.

So that, although the above experiment shows us that the descent of the moving body through the arc CB confers upon it momentum [*momento*] just sufficient to carry it to the same height through any of the arcs BD, BG, BI, we are not able, by similar means, to show that the event would be identical in the case of a perfectly round ball descending along planes whose inclinations are respectively the same as the chords of these arcs. It seems likely, on the other hand, that, since these planes form angles at the point B, they will present an obstacle to the ball which has descended along the chord CB, and starts to rise along the chord BD, BG, BI.

In striking these planes some of its momentum [*impeto*] will be lost and it will not be able to rise to the height of the line CD; but this obstacle, which interferes with the experiment, once removed, it is clear that the momentum [*impeto*] (which gains

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in strength with descent) will be able to carry the body to the same height. Let us then, for the present, take this as a postulate, the absolute truth of which will be established when we find that the inferences from it correspond to and agree perfectly with experiment. The author having assumed this single principle passes next to the propositions which he clearly demonstrates; the first of these is as follows:

THEOREM I, PROPOSITION I

The time in which any space is traversed by a body starting from rest and uniformly accelerated is equal to the time in which that same space would be traversed by the same body moving at a uniform speed whose value is the mean of the highest speed and the speed just before acceleration began.

Let us represent by the line AB the time in which the space CD is traversed by a body which starts from rest at C and is uniformly accelerated; let the final and highest value of the speed gained during the interval AB be represented by the line EB drawn at right angles to AB; draw the line AE, then all lines drawn from equidistant points on AB and parallel to BE will represent the increasing values of the speed, beginning with the instant A. Let the point F bisect the line EB; draw FG parallel to BA, and GA parallel to FB, thus forming a parallelogram AGFB which will be equal in area to the triangle AEB, since the side GF bisects the side AE at the point I; for if the parallel lines in the triangle AEB are extended to GI, then the sum of all the parallels contained in the quadrilateral is equal to the sum of those contained in the triangle AEB; for those in the triangle IEF are equal to those contained in the triangle GIA, while those included in the trapezium AIFB are common. Since each and every instant of time in the time-interval AB has its corresponding point on the line AB, from which points parallels drawn in and limited by the triangle AEB represent the increasing values of the growing velocity, and since parallels contained within the rectangle represent the values of a speed which is not increasing, but constant, it appears, in like manner, that the momenta [*momenta*] assumed by the moving body may also be represented, in the case of the accelerated motion, by the increasing parallels of the triangle AEB,

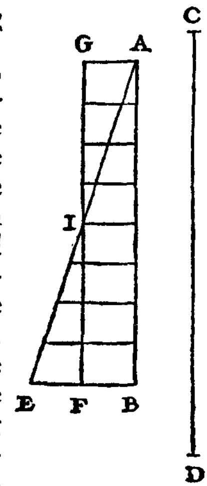

Fig. 47

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AEB, and, in the case of the uniform motion, by the parallels of the rectangle GB. For, what the momenta may lack in the first part of the accelerated motion (the deficiency of the momenta being represented by the parallels of the triangle AGI) is made up by the momenta represented by the parallels of the triangle IEF.

Hence it is clear that equal spaces will be traversed in equal times by two bodies, one of which, starting from rest, moves with a uniform acceleration, while the momentum of the other, moving with uniform speed, is one-half its maximum momentum under accelerated motion. Q. E. D.

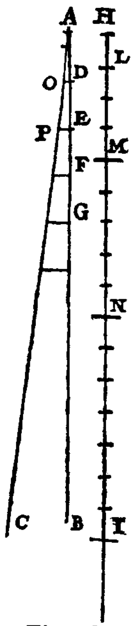

Fig. 48

Theorem II, Proposition II

The spaces described by a body falling from rest with a uniformly accelerated motion are to each other as the squares of the time-intervals employed in traversing these distances.

Let the time beginning with any instant A be represented by the straight line AB in which are taken any two time-intervals AD and AE. Let HI represent the distance through which the body, starting from rest at H, falls with uniform acceleration. If HL represents the space traversed during the time-interval AD, and HM that covered during the interval AE, then the space MH stands to the space LH in a ratio which is the square of the ratio of the time AE to the time AD; or we may say simply that the distances HM and HL are related as the squares of AE and AD.

Draw the line AC making any angle whatever with the line AB; and from the points D and E, draw the parallel lines DO and EP; of these two lines, DO represents the greatest velocity attained during the interval AD, while EP represents the maximum velocity acquired during the interval AE. But it has just been proved that so far as distances traversed are concerned

cerned it is precisely the same whether a body falls from rest with a uniform acceleration or whether it falls during an equal time-interval with a constant speed which is one-half the maximum speed attained during the accelerated motion. It follows therefore that the distances HM and HL are the same as would be traversed, during the time-intervals AE and AD, by uniform velocities equal to one-half those represented by DO and EP respectively. If, therefore, one can show that the distances HM and HL are in the same ratio as the squares of the time-intervals AE and AD, our proposition will be proven.

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But in the fourth proposition of the first book [p. 157 above] it has been shown that the spaces traversed by two particles in uniform motion bear to one another a ratio which is equal to the product of the ratio of the velocities by the ratio of the times. But in this case the ratio of the velocities is the same as the ratio of the time-intervals (for the ratio of AE to AD is the same as that of ½ EP to ½ DO or of EP to DO). Hence the ratio of the spaces traversed is the same as the squared ratio of the time-intervals. Q. E. D.

Evidently then the ratio of the distances is the square of the ratio of the final velocities, that is, of the lines EP and DO, since these are to each other as AE to AD.

COROLLARY I

Hence it is clear that if we take any equal intervals of time whatever, counting from the beginning of the motion, such as AD, DE, EF, FG, in which the spaces HL, LM, MN, NI are traversed, these spaces will bear to one another the same ratio as the series of odd numbers, 1, 3, 5, 7; for this is the ratio of the differences of the squares of the lines [which represent time], differences which exceed one another by equal amounts, this excess being equal to the smallest line [viz. the one representing a single time-interval]: or we may say [that this is the ratio] of the differences of the squares of the natural numbers beginning with unity.

While,

While, therefore, during equal intervals of time the velocities increase as the natural numbers, the increments in the distances traversed during these equal time-intervals are to one another as the odd numbers beginning with unity.

SAGR. Please suspend the discussion for a moment since there just occurs to me an idea which I want to illustrate by means of a diagram in order that it may be clearer both to you and to me.

Let the line AI represent the lapse of time measured from the initial instant A; through A draw the straight line AF making any angle whatever; join the terminal points I and F; divide the time AI in half at C; draw CB parallel to IF. Let us consider CB as the maximum value of the velocity which increases from zero at the beginning, in simple proportionality to the intercepts on the triangle ABC of lines drawn parallel to BC; or what is the same thing, let us suppose the velocity to increase in proportion to the time; then I admit without question, in view of the preceding argument, that the space described by a body falling in the aforesaid manner will be equal to the space traversed by the same body during the same length of time travelling with a uniform speed equal to EC, the half of BC. Further let us imagine that the

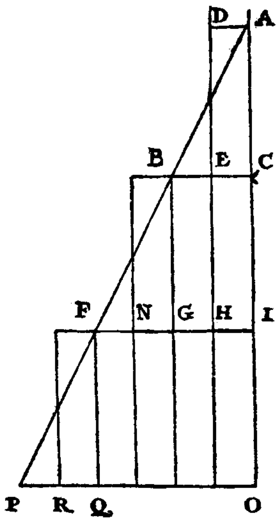

Fig. 49

[211]

body has fallen with accelerated motion so that, at the instant C, it has the velocity BC. It is clear that if the body continued to descend with the same speed BC, without acceleration, it would in the next time-interval CI traverse double the distance covered during the interval AC, with the uniform speed EC which is half of BC; but since the falling body acquires equal increments of speed during equal increments of time, it follows that the velocity BC, during the next time-interval

interval CI will be increased by an amount represented by the parallels of the triangle BFG which is equal to the triangle ABC. If, then, one adds to the velocity GI half of the velocity FG, the highest speed acquired by the accelerated motion and determined by the parallels of the triangle BFG, he will have the uniform velocity with which the same space would have been described in the time CI; and since this speed IN is three times as great as EC it follows that the space described during the interval CI is three times as great as that described during the interval AC. Let us imagine the motion extended over another equal time-interval IO, and the triangle extended to APO; it is then evident that if the motion continues during the interval IO, at the constant rate IF acquired by acceleration during the time AI, the space traversed during the interval IO will be four times that traversed during the first interval AC, because the speed IF is four times the speed EC. But if we enlarge our triangle so as to include FPQ which is equal to ABC, still assuming the acceleration to be constant, we shall add to the uniform speed an increment RQ, equal to EC; then the value of the equivalent uniform speed during the time-interval IO will be five times that during the first time-interval AC; therefore the space traversed will be quintuple that during the first interval AC. It is thus evident by simple computation that a moving body starting from rest and acquiring velocity at a rate proportional to the time, will, during equal intervals of time, traverse distances which are related to each other as the odd numbers beginning with unity, 1, 3, 5;* or considering the total space traversed, that covered

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in double time will be quadruple that covered during unit time; in triple time, the space is nine times as great as in unit time.

* As illustrating the greater elegance and brevity of modern analytical methods, one may obtain the result of Prop. II directly from the fundamental equation

$$s = {}^1/_2\, g\, (t^2_2 - t^2_1) = g/_2\, (t_2 + t_1)\, (t_2 - t_1)$$

where g is the acceleration of gravity and s, the space traversed between the instants t_1 and t_2. If now $t_2 - t_1 = 1$, say one second then $s = g/_2\, (t_2 + t_1)$ where t_2+t_1, must always be an odd number, seeing that it is the sum of two consecutive terms in the series of natural numbers. [*Trans.*]

And in general the spaces traversed are in the duplicate ratio of the times, i. e., in the ratio of the squares of the times.

SIMP. In truth, I find more pleasure in this simple and clear argument of Sagredo than in the Author's demonstration which to me appears rather obscure; so that I am convinced that matters are as described, once having accepted the definition of uniformly accelerated motion. But as to whether this acceleration is that which one meets in nature in the case of falling bodies, I am still doubtful; and it seems to me, not only for my own sake but also for all those who think as I do, that this would be the proper moment to introduce one of those experiments—and there are many of them, I understand—which illustrate in several ways the conclusions reached.

SALV. The request which you, as a man of science, make, is a very reasonable one; for this is the custom—and properly so—in those sciences where mathematical demonstrations are applied to natural phenomena, as is seen in the case of perspective, astronomy, mechanics, music, and others where the principles, once established by well-chosen experiments, become the foundations of the entire superstructure. I hope therefore it will not appear to be a waste of time if we discuss at considerable length this first and most fundamental question upon which hinge numerous consequences of which we have in this book only a small number, placed there by the Author, who has done so much to open a pathway hitherto closed to minds of speculative turn. So far as experiments go they have not been neglected by the Author; and often, in his company, I have attempted in the following manner to assure myself that the acceleration actually experienced by falling bodies is that above described.

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

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board

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.

Simp. I would like to have been present at these experiments; but feeling confidence in the care with which you performed them, and in the fidelity with which you relate them, I am satisfied and accept them as true and valid

Salv. Then we can proceed without discussion.

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COROLLARY II

Secondly, it follows that, starting from any initial point, if we take any two distances, traversed in any time-intervals whatsoever, these time-intervals bear to one another the same ratio as one of the distances to the mean proportional of the two distances.

For if we take two distances ST and SY measured from the initial point S, the mean proportional of which is SX, the time of fall through ST is to the time of fall through SY as ST is to SX; or one may say the time of fall through SY is to the time of fall through ST as SY is to SX. Now since it has been shown that the spaces traversed are in the same ratio as the squares of the times; and since, moreover, the ratio of the space SY to the space ST is the square of the ratio SY to SX, it follows that the ratio of the times of fall through SY and ST is the ratio of the respective distances SY and SX.

Fig. 50

SCHOLIUM

The above corollary has been proven for the case of vertical fall; but it holds also for planes inclined at any angle; for it is to be assumed that along these planes the velocity increases in the same ratio, that is, in proportion to the time, or, if you prefer, as the series of natural numbers.*

SALV. Here, Sagredo, I should like, if it be not too tedious to Simplicio, to interrupt for a moment the present discussion in order to make some additions on the basis of what has already been proved and of what mechanical principles we have already learned from our Academician. This addition I make for the better establishment on logical and experimental grounds, of the principle which we have above considered; and what is more important, for the purpose of deriving it geometrically, after first demonstrating a single lemma which is fundamental in the science of motion [*impeti*].

* The dialogue which intervenes between this Scholium and the following theorem was elaborated by Viviani, at the suggestion of Galileo. See *National Edition*, viii, 23. [*Trans.*]

Next Page

SAGR. If the advance which you propose to make is such as will confirm and fully establish these sciences of motion, I will gladly devote to it any length of time. Indeed, I shall not only

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be glad to have you proceed, but I beg of you at once to satisfy the curiosity which you have awakened in me concerning your proposition; and I think that Simplicio is of the same mind.

SIMP. Quite right.

SALV. Since then I have your permission, let us first of all consider this notable fact, that the momenta or speeds [*i momenti o le velocità*] of one and the same moving body vary with the inclination of the plane.

The speed reaches a maximum along a vertical direction, and for other directions diminishes as the plane diverges from the vertical. Therefore the impetus, ability, energy, [*l'impeto, il talento, l'energia*] or, one might say, the momentum [*il momento*] of descent of the moving body is diminished by the plane upon which it is supported and along which it rolls.

For the sake of greater clearness erect the line AB perpendicular to the horizontal AC; next draw AD, AE, AF, etc., at different inclinations to the horizontal. Then I say that all the momentum of the falling body is along the vertical and is a maximum when it falls in that direction; the momentum is less along DA and still less along EA, and even less yet along the more inclined plane FA. Finally on the horizontal plane the momentum vanishes altogether; the body finds itself in a condition of indifference as to motion or rest; has no inherent tendency to move in any direction, and offers no resistance to being set in motion. For just as a heavy body or system of bodies cannot of itself move upwards, or recede from the common center [*comun centro*] toward which all heavy things tend, so it is impossible for any body of its own accord to assume any motion other than one which carries it nearer to the aforesaid common center. Hence, along the horizontal, by which we understand a surface, every point of which is equidistant from this same common center, the body will have no momentum whatever.

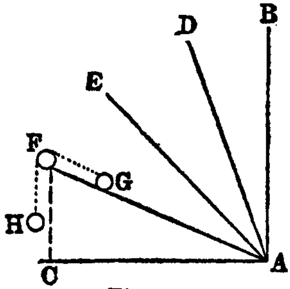

Fig. 51

This

FOURTH DAY

SALVIATI. Once more, Simplicio is here on time; so let us without delay take up the question of motion. The text of our Author is as follows:

THE MOTION OF PROJECTILES

In the preceding pages we have discussed the properties of uniform motion and of motion naturally accelerated along planes of all inclinations. I now propose to set forth those properties which belong to a body whose motion is compounded of two other motions, namely, one uniform and one naturally accelerated; these properties, well worth knowing, I propose to demonstrate in a rigid manner. This is the kind of motion seen in a moving projectile; its origin I conceive to be as follows:

Imagine any particle projected along a horizontal plane without friction; then we know, from what has been more fully explained in the preceding pages, that this particle will move along this same plane with a motion which is uniform and perpetual, provided the plane has no limits. But if the plane is limited and elevated, then the moving particle, which we imagine to be a heavy one, will on passing over the edge of the plane acquire, in addition to its previous uniform and perpetual motion, a downward propensity due to its own weight; so that the resulting motion which I call projection [*projectio*], is compounded of one which is uniform and horizontal and of another which is vertical and naturally accelerated. We now proceed to demonstrate

demonstrate some of its properties, the first of which is as follows:

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THEOREM I, PROPOSITION I

A projectile which is carried by a uniform horizontal motion compounded with a naturally accelerated vertical motion describes a path which is a semi-parabola.

SAGR. Here, Salviati, it will be necessary to stop a little while for my sake and, I believe, also for the benefit of Simplicio; for it so happens that I have not gone very far in my study of Apollonius and am merely aware of the fact that he treats of the parabola and other conic sections, without an understanding of which I hardly think one will be able to follow the proof of other propositions depending upon them. Since even in this first beautiful theorem the author finds it necessary to prove that the path of a projectile is a parabola, and since, as I imagine, we shall have to deal with only this kind of curves, it will be absolutely necessary to have a thorough acquaintance, if not with all the properties which Apollonius has demonstrated for these figures, at least with those which are needed for the present treatment.

SALV. You are quite too modest, pretending ignorance of facts which not long ago you acknowledged as well known—I mean at the time when we were discussing the strength of materials and needed to use a certain theorem of Apollonius which gave you no trouble.

SAGR. I may have chanced to know it or may possibly have assumed it, so long as needed, for that discussion; but now when we have to follow all these demonstrations about such curves we ought not, as they say, to swallow it whole, and thus waste time and energy.

SIMP. Now even though Sagredo is, as I believe, well equipped for all his needs, I do not understand even the elementary terms; for although our philosophers have treated the motion of projectiles, I do not recall their having described the path of a projectile except to state in a general way that it is always a curved

curved line, unless the projection be vertically upwards. But

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if the little Euclid which I have learned since our previous discussion does not enable me to understand the demonstrations which are to follow, then I shall be obliged to accept the theorems on faith without fully comprehending them.

SALV. On the contrary, I desire that you should understand them from the Author himself, who, when he allowed me to see this work of his, was good enough to prove for me two of the principal properties of the parabola because I did not happen to have at hand the books of Apollonius. These properties, which are the only ones we shall need in the present discussion, he proved in such a way that no prerequisite knowledge was required. These theorems are, indeed, given by Apollonius, but after many preceding ones, to follow which would take a long while. I wish to shorten our task by deriving the first property purely and simply from the mode of generation of the parabola and proving the second immediately from the first.

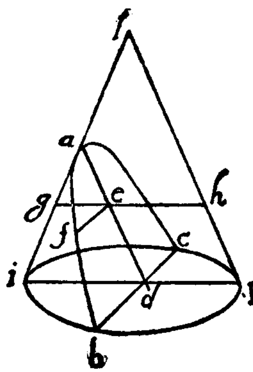

Fig. 106

Beginning now with the first, imagine a right cone, erected upon the circular base *ibkc* with apex at *l*. The section of this cone made by a plane drawn parallel to the side *lk* is the curve which is called a *parabola*. The base of this parabola *bc* cuts at right angles the diameter *ik* of the circle *ibkc*, and the axis *ad* is parallel to the side *lk;* now having taken any point *f* in the curve *bfa* draw the straight line *fe* parallel to *bd;* then, I say, the square of *bd* is to the square of *fe* in the same ratio as the axis *ad* is to the portion *ae*. Through the point *e* pass a plane parallel to the circle *ibkc*, producing in the cone a circular section whose diameter is the line *geh*. Since *bd* is at right angles to *ik* in the circle *ibk*, the square of *bd* is equal to the rectangle formed by *id* and *dk;* so also in the upper circle which passes through the points *gfh* the square of *fe* is equal to the rectangle formed by

ge

ge and *eh;* hence the square of *bd* is to the square of *fe* as the rectangle *id.dk* is to the rectangle *ge.eh.* And since the line *ed* is parallel to *hk,* the line *eh,* being parallel to *dk,* is equal to it; therefore the rectangle *id.dk* is to the rectangle *ge.eh* as *id* is to

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ge, that is, as *da* is to *ae;* whence also the rectangle *id.dk* is to the rectangle *ge.eh,* that is, the square of *bd* is to the square of *fe,* as the axis *da* is to the portion *ae.* Q. E. D.

The other proposition necessary for this discussion we demonstrate as follows. Let us draw a parabola whose axis *ca* is prolonged upwards to a point *d;* from any point *b* draw the line *bc* parallel to the base of the parabola; if now the point *d* is chosen so that *da* = *ca,* then, I say, the straight line drawn through the points *b* and *d* will be tangent to the parabola at *b.* For imagine, if possible, that this line cuts the parabola above or that its prolongation cuts it below, and through any point *g* in it draw the straight line *fge.* And since the square of *fe* is greater than the square of *ge,* the square of *fe* will bear a greater ratio to the square of *bc* than the square of *ge* to that of *bc;* and since, by the preceding proposition, the square of *fe* is to that of *bc* as the line *ea* is to *ca,* it follows that the line *ea* will bear to the line *ca* a greater ratio than the square of *ge* to that of *bc,* or, than the square of *ed* to that of *cd* (the sides of the triangles *deg* and *dcb* being proportional).

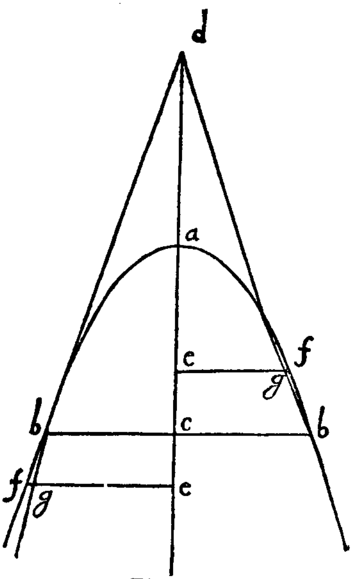

Fig. 107

But the line *ea* is to *ca,* or *da,* in the same ratio as four times the rectangle *ea.ad* is to four times the square of *ad,* or, what is the same, the square of *cd,* since this is four times the square of *ad;* hence four times the rectangle *ea.ad* bears to the square of *cd* a a greater ratio than the square of *ed* to the square of *cd;* but that would make four times the rectangle *ea.ad* greater than the square of *ed;* which is false, the fact being just the opposite, because the two portions *ea* and *ad* of the line *ed* are not equal. Therefore the line *db* touches the parabola without cutting it. Q. E. D.

SIMP. Your demonstration proceeds too rapidly and, it seems to me, you keep on assuming that all of Euclid's theorems are [272] as familiar and available to me as his first axioms, which is far from true. And now this fact which you spring upon us, that four times the rectangle *ea.ad* is less than the square of *de* because the two portions *ea* and *ad* of the line *de* are not equal brings me little composure of mind, but rather leaves me in suspense.

SALV. Indeed, all real mathematicians assume on the part of the reader perfect familiarity with at least the elements of Euclid; and here it is necessary in your case only to recall a proposition of the Second Book in which he proves that when a line is cut into equal and also into two unequal parts, the rectangle formed on the unequal parts is less than that formed on the equal (i. e., less than the square on half the line), by an amount which is the square of the difference between the equal and unequal segments. From this it is clear that the square of the whole line which is equal to four times the square of the half is greater than four times the rectangle of the unequal parts. In order to understand the following portions of this treatise it will be necessary to keep in mind the two elemental theorems from conic sections which we have just demonstrated; and these two theorems are indeed the only ones which the Author uses. We can now resume the text and see how he demonstrates his first proposition in which he shows that a body falling with a motion compounded of a uniform horizontal and a naturally accelerated [*naturale descendente*] one describes a semi-parabola.

Let us imagine an elevated horizontal line or plane *ab* along which a body moves with uniform speed from *a* to *b*. Suppose this

this plane to end abruptly at *b;* then at this point the body will, on account of its weight, acquire also a natural motion downwards along the perpendicular *bn.* Draw the line *be* along the plane *ba* to represent the flow, or measure, of time; divide this line into a number of segments, *bc*, *cd*, *de*, representing equal intervals of time; from the points *b*, *c*, *d*, *e*, let fall lines which are parallel to the perpendicular *bn.* On the first of these lay off any distance *ci*, on the second a distance four times as long, *df;* on [273] the third, one nine times as long, *eh;* and so on, in proportion to the squares of *cb*, *db*, *eb*, or, we may say, in the squared ratio of these same lines. Accordingly we see that while the body moves from *b* to *c* with uniform speed, it also falls perpendicularly through the distance *ci*, and at the end of the time-interval *bc* finds itself at the point *i.* In like manner at the end of the time-interval *bd*, which is the double of *bc*, the vertical fall will be four times the first distance *ci;* for it has been shown in a previous discussion that the distance traversed by a freely falling body varies as the square of the time; in like manner the space *eh* traversed during the time *be* will be nine times *ci;* thus it is evident that the distances *eh*, *df*, *ci* will be to one another as the squares of the lines *be*, *bd*, *bc*. Now from the points *i*, *f*, *h* draw the straight lines *io*, *fg*, *hl* parallel to *be;* these lines *hl*, *fg*, *io* are equal to *eb*, *db* and *cb*, respectively; so also are the lines *bo*, *bg*, *bl* respectively equal to *ci*, *df*, and *eh*. The square of *hl* is to that of *fg* as the line *lb* is to *bg;* and the square of *fg* is to that of *io* as *gb* is to *bo;* therefore the points *i*, *f*, *h*, lie on one and the same parabola. In like manner it may be shown that, if we take equal time-intervals of any size whatever, and if we imagine the particle to be carried by a similar compound motion, the

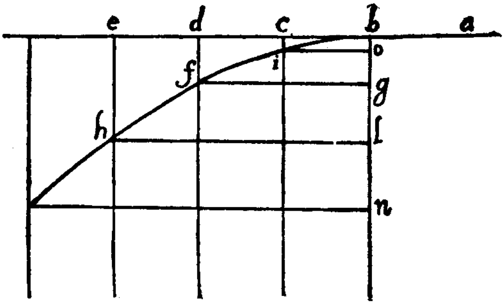

Fig. 108

the positions of this particle, at the ends of these time-intervals, will lie on one and the same parabola. Q. E. D.

SALV. This conclusion follows from the converse of the first of the two propositions given above. For, having drawn a parabola through the points *b* and *h*, any other two points, *f* and *i*, not falling on the parabola must lie either within or without; consequently the line *fg* is either longer or shorter than the line which terminates on the parabola. Therefore the square of *hl* will not bear to the square of *fg* the same ratio as the line *lb* to *bg*, but a greater or smaller; the fact is, however, that the square of *hl does* bear this same ratio to the square of *fg*. Hence the point *f* does lie on the parabola, and so do all the others.

SAGR. One cannot deny that the argument is new, subtle and conclusive, resting as it does upon this hypothesis, namely, that the horizontal motion remains uniform, that the vertical motion continues to be accelerated downwards in proportion to the square of the time, and that such motions and velocities as these combine without altering, disturbing, or hindering each other,* so that as the motion proceeds the path of the projectile does not change into a different curve: but this, in my opinion,

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is impossible. For the axis of the parabola along which we imagine the natural motion of a falling body to take place stands perpendicular to a horizontal surface and ends at the center of the earth; and since the parabola deviates more and more from its axis no projectile can ever reach the center of the earth or, if it does, as seems necessary, then the path of the projectile must transform itself into some other curve very different from the parabola.

SIMP. To these difficulties, I may add others. One of these is that we suppose the horizontal plane, which slopes neither up nor down, to be represented by a straight line as if each point on this line were equally distant from the center, which is not the case; for as one starts from the middle [of the line] and goes toward either end, he departs farther and farther from the center [of the earth] and is therefore constantly going uphill. Whence it follows that the motion cannot remain uniform

* A very near approach to Newton's Second Law of Motion. [*Trans.*]

through any distance whatever, but must continually diminish. Besides, I do not see how it is possible to avoid the resistance of the medium which must destroy the uniformity of the horizontal motion and change the law of acceleration of falling bodies. These various difficulties render it highly improbable that a result derived from such unreliable hypotheses should hold true in practice.

SALV. All these difficulties and objections which you urge are so well founded that it is impossible to remove them; and, as for me, I am ready to admit them all, which indeed I think our Author would also do. I grant that these conclusions proved in the abstract will be different when applied in the concrete and will be fallacious to this extent, that neither will the horizontal motion be uniform nor the natural acceleration be in the ratio assumed, nor the path of the projectile a parabola, etc. But, on the other hand, I ask you not to begrudge our Author that which other eminent men have assumed even if not strictly true. The authority of Archimedes alone will satisfy everybody. In his Mechanics and in his first quadrature of the parabola he takes for granted that the beam of a balance or steelyard is a straight line, every point of which is equidistant from the common center of all heavy bodies, and that the cords by which heavy bodies are suspended are parallel to each other.

Some consider this assumption permissible because, in practice, our instruments and the distances involved are so small in comparison with the enormous distance from the center of the earth that we may consider a minute of arc on a great circle as a straight line, and may regard the perpendiculars let fall from its two extremities as parallel. For if in actual practice one had to

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consider such small quantities, it would be necessary first of all to criticise the architects who presume, by use of a plumbline, to erect high towers with parallel sides. I may add that, in all their discussions, Archimedes and the others considered themselves as located at an infinite distance from the center of the earth, in which case their assumptions were not false, and therefore their conclusions were absolutely correct. When we wish

wish to apply our proven conclusions to distances which, though finite, are very large, it is necessary for us to infer, on the basis of demonstrated truth, what correction is to be made for the fact that our distance from the center of the earth is not really infinite, but merely very great in comparison with the small dimensions of our apparatus. The largest of these will be the range of our projectiles—and even here we need consider only the artillery—which, however great, will never exceed four of those miles of which as many thousand separate us from the center of the earth; and since these paths terminate upon the surface of the earth only very slight changes can take place in their parabolic figure which, it is conceded, would be greatly altered if they terminated at the center of the earth.

As to the perturbation arising from the resistance of the medium this is more considerable and does not, on account of its manifold forms, submit to fixed laws and exact description. Thus if we consider only the resistance which the air offers to the motions studied by us, we shall see that it disturbs them all and disturbs them in an infinite variety of ways corresponding to the infinite variety in the form, weight, and velocity of the projectiles. For as to velocity, the greater this is, the greater will be the resistance offered by the air; a resistance which will be greater as the moving bodies become less dense [*men gravi*]. So that although the falling body ought to be displaced [*andare accelerandosi*] in proportion to the square of the duration of its motion, yet no matter how heavy the body, if it falls from a very considerable height, the resistance of the air will be such as to prevent any increase in speed and will render the motion

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uniform; and in proportion as the moving body is less dense [*men grave*] this uniformity will be so much the more quickly attained and after a shorter fall. Even horizontal motion which, if no impediment were offered, would be uniform and constant is altered by the resistance of the air and finally ceases; and here again the less dense [*più leggiero*] the body the quicker the process. Of these properties [*accidenti*] of weight, of velocity, and also of form [*figura*], infinite in number, it is not possible to give

give any exact description; hence, in order to handle this matter in a scientific way, it is necessary to cut loose from these difficulties; and having discovered and demonstrated the theorems, in the case of no resistance, to use them and apply them with such limitations as experience will teach. And the advantage of this method will not be small; for the material and shape of the projectile may be chosen, as dense and round as possible, so that it will encounter the least resistance in the medium. Nor will the spaces and velocities in general be so great but that we shall be easily able to correct them with precision.

In the case of those projectiles which we use, made of dense [*grave*] material and round in shape, or of lighter material and cylindrical in shape, such as arrows, thrown from a sling or crossbow, the deviation from an exact parabolic path is quite insensible. Indeed, if you will allow me a little greater liberty, I can show you, by two experiments, that the dimensions of our apparatus are so small that these external and incidental resistances, among which that of the medium is the most considerable, are scarcely observable.

I now proceed to the consideration of motions through the air, since it is with these that we are now especially concerned; the resistance of the air exhibits itself in two ways: first by offering greater impedance to less dense than to very dense bodies, and secondly by offering greater resistance to a body in rapid motion than to the same body in slow motion.

Regarding the first of these, consider the case of two balls having the same dimensions, but one weighing ten or twelve times as much as the other; one, say, of lead, the other of oak, both allowed to fall from an elevation of 150 or 200 cubits.

Experiment shows that they will reach the earth with slight difference in speed, showing us that in both cases the retardation caused by the air is small; for if both balls start at the same moment and at the same elevation, and if the leaden one be slightly retarded and the wooden one greatly retarded, then the former ought to reach the earth a considerable distance in advance of the latter, since it is ten times as heavy. But this

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does

does not happen; indeed, the gain in distance of one over the other does not amount to the hundredth part of the entire fall. And in the case of a ball of stone weighing only a third or half as much as one of lead, the difference in their times of reaching the earth will be scarcely noticeable. Now since the speed [*impeto*] acquired by a leaden ball in falling from a height of 200 cubits is so great that if the motion remained uniform the ball would, in an interval of time equal to that of the fall, traverse 400 cubits, and since this speed is so considerable in comparison with those which, by use of bows or other machines except fire arms, we are able to give to our projectiles, it follows that we may, without sensible error, regard as absolutely true those propositions which we are about to prove without considering the resistance of the medium.

Passing now to the second case, where we have to show that the resistance of the air for a rapidly moving body is not very much greater than for one moving slowly, ample proof is given by the following experiment. Attach to two threads of equal length—say four or five yards—two equal leaden balls and suspend them from the ceiling; now pull them aside from the perpendicular, the one through 80 or more degrees, the other through not more than four or five degrees; so that, when set free, the one falls, passes through the perpendicular, and describes large but slowly decreasing arcs of 160, 150, 140 degrees, etc.; the other swinging through small and also slowly diminishing arcs of 10, 8, 6, degrees, etc.

In the first place it must be remarked that one pendulum passes through its arcs of 180°, 160°, etc., in the same time that the other swings through its 10°, 8°, etc., from which it follows that the speed of the first ball is 16 and 18 times greater than that of the second. Accordingly, if the air offers more resistance to the high speed than to the low, the frequency of vibration in the large arcs of 180° or 160°, etc., ought to be less than in the small arcs of 10°, 8°, 4°, etc., and even less than in arcs of 2°, or 1°; but this prediction is not verified by experiment; because if two persons start to count the vibrations, the one the large, the other the small, they will discover that after counting tens and

and even hundreds they will not differ by a single vibration, not even by a fraction of one.

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This observation justifies the two following propositions, namely, that vibrations of very large and very small amplitude all occupy the same time and that the resistance of the air does not affect motions of high speed more than those of low speed, contrary to the opinion hitherto generally entertained.

SAGR. On the contrary, since we cannot deny that the air hinders both of these motions, both becoming slower and finally vanishing, we have to admit that the retardation occurs in the same proportion in each case. But how? How, indeed, could the resistance offered to the one body be greater than that offered to the other except by the impartation of more momentum and speed [*impeto e velocità*] to the fast body than to the slow? And if this is so the speed with which a body moves is at once the cause and measure [*cagione e misura*] of the resistance which it meets. Therefore, all motions, fast or slow, are hindered and diminished in the same proportion; a result, it seems to me, of no small importance.

SALV. We are able, therefore, in this second case to say that the errors, neglecting those which are accidental, in the results which we are about to demonstrate are small in the case of our machines where the velocities employed are mostly very great and the distances negligible in comparison with the semi-diameter of the earth or one of its great circles.

SIMP. I would like to hear your reason for putting the projectiles of fire arms, i. e., those using powder, in a different class from the projectiles employed in bows, slings, and crossbows, on the ground of their not being equally subject to change and resistance from the air.

SALV. I am led to this view by the excessive and, so to speak, supernatural violence with which such projectiles are launched; for, indeed, it appears to me that without exaggeration one might say that the speed of a ball fired either from a musket or from a piece of ordnance is supernatural. For if such a ball be allowed to fall from some great elevation its speed will, owing to the resistance

resistance of the air, not go on increasing indefinitely; that which happens to bodies of small density in falling through short distances—I mean the reduction of their motion to uniformity—will also happen to a ball of iron or lead after it has fallen a few thousand cubits; this terminal or final speed [*terminata velocità*] is the maximum which such a heavy body can naturally acquire

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in falling through the air. This speed I estimate to be much smaller than that impressed upon the ball by the burning powder.

An appropriate experiment will serve to demonstrate this fact. From a height of one hundred or more cubits fire a gun [*archibuso*] loaded with a lead bullet, vertically downwards upon a stone pavement; with the same gun shoot against a similar stone from a distance of one or two cubits, and observe which of the two balls is the more flattened. Now if the ball which has come from the greater elevation is found to be the less flattened of the two, this will show that the air has hindered and diminished the speed initially imparted to the bullet by the powder, and that the air will not permit a bullet to acquire so great a speed, no matter from what height it falls; for if the speed impressed upon the ball by the fire does not exceed that acquired by it in falling freely [*naturalmente*] then its downward blow ought to be greater rather than less.

This experiment I have not performed, but I am of the opinion that a musket-ball or cannon-shot, falling from a height as great as you please, will not deliver so strong a blow as it would if fired into a wall only a few cubits distant, i. e., at such a short range that the splitting or rending of the air will not be sufficient to rob the shot of that excess of supernatural violence given it by the powder.

The enormous momentum [*impeto*] of these violent shots may cause some deformation of the trajectory, making the beginning of the parabola flatter and less curved than the end; but, so far as our Author is concerned, this is a matter of small consequence in practical operations, the main one of which is the preparation of a table of ranges for shots of high elevation, giving the distance

tance attained by the ball as a function of the angle of elevation; and since shots of this kind are fired from mortars [*mortari*] using small charges and imparting no supernatural momentum [*impeto sopranaturale*] they follow their prescribed paths very exactly.

But now let us proceed with the discussion in which the Author invites us to the study and investigation of the motion of a body [*impeto del mobile*] when that motion is compounded of two others; and first the case in which the two are uniform, the one horizontal, the other vertical.

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THEOREM II, PROPOSITION II

> When the motion of a body is the resultant of two uniform motions, one horizontal, the other perpendicular, the square of the resultant momentum is equal to the sum of the squares of the two component momenta.*

Let us imagine any body urged by two uniform motions and let *ab* represent the vertical displacement, while *bc* represents the displacement which, in the same interval of time, takes place in a horizontal direction. If then the distances *ab* and *bc* are traversed, during the same time-interval, with uniform motions the corresponding

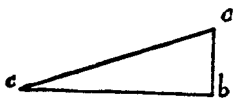

Fig. 109

momenta will be to each other as the distances *ab* and *bc* are to each other; but the body which is urged by these two motions describes the diagonal *ac*; its momentum is proportional to *ac*. Also the square of *ac* is equal to the sum of the squares of *ab* and *bc*. Hence the square of the resultant momentum is equal to the sum of the squares of the two momenta *ab* and *bc*. Q. E. D.

SIMP. At this point there is just one slight difficulty which needs to be cleared up; for it seems to me that the conclusion

* In the original this theorem reads as follows:

"*Si aliquod mobile duplici motu æquabili moveatur, nempe orizontali et perpendiculari, impetus seu momentum lationis ex utroque motu compositæ erit potentia æqualis ambobus momentis priorum motuum.*"

For the justification of this translation of the word "*potentia*" and of the use of the adjective "resultant" see p. 266 below. [*Trans.*]

just reached contradicts a previous proposition * in which it is claimed that the speed [*impeto*] of a body coming from *a* to *b* is equal to that in coming from *a* to *c;* while now you conclude that the speed [*impeto*] at *c* is greater than that at *b.*

SALV. Both propositions, Simplicio, are true, yet there is a great difference between them. Here we are speaking of a body urged by a single motion which is the resultant of two uniform motions, while there we were speaking of two bodies each urged with naturally accelerated motions, one along the vertical *ab* the other along the inclined plane *ac.* Besides the time-intervals were there not supposed to be equal, that along the incline *ac* being greater than that along the vertical *ab;* but the motions of which we now speak, those along *ab*, *bc*, *ac*, are uniform and simultaneous.

SIMP. Pardon me; I am satisfied; pray go on.

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SALV. Our Author next undertakes to explain what happens when a body is urged by a motion compounded of one which is horizontal and uniform and of another which is vertical but naturally accelerated; from these two components results the path of a projectile, which is a parabola. The problem is to determine the speed [*impeto*] of the projectile at each point. With this purpose in view our Author sets forth as follows the manner, or rather the method, of measuring such speed [*impeto*] along the path which is taken by a heavy body starting from rest and falling with a naturally accelerated motion.

THEOREM III, PROPOSITION III

Let the motion take place along the line *ab*, starting from rest at *a*, and in this line choose any point *c*. Let *ac* represent the time, or the measure of the time, required for the body to fall through the space *ac;* let *ac* also represent the velocity [*impetus seu momentum*] at *c* acquired by a fall through the distance *ac*. In the line *ab* select any other point *b*. The problem now is to determine the velocity at *b* acquired by a body in falling through the distance *ab* and to express this in terms of the velocity at *c*, the measure of which is the length *ac*. Take

* See p. 169 above. [*Trans.*]

as a mean proportional between *ac* and *ab*. We shall prove that the velocity at *b* is to that at *c* as the length *as* is to the length *ac*. Draw the horizontal line *cd*, having twice the length of *ac*, and *be*, having twice the length of *ba*. It then follows, from the preceding theorems, that a body falling through the distance *ac*, and turned so as to move along the horizontal *cd*

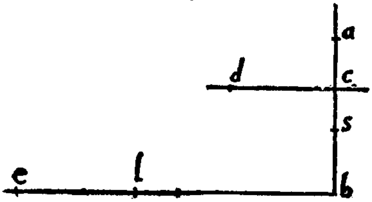

Fig. 110

with a uniform speed equal to that acquired on reaching *c*

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will traverse the distance *cd* in the same interval of time as that required to fall with accelerated motion from *a* to *c*. Likewise *be* will be traversed in the same time as *ba*. But the time of descent through *ab* is *as;* hence the horizontal distance *be* is also traversed in the time *as*. Take a point *l* such that the time *as* is to the time *ac* as *be* is to *bl;* since the motion along *be* is uniform, the distance *bl*, if traversed with the speed [*momentum celeritatis*] acquired at *b*, will occupy the time *ac;* but in this same time-interval, *ac*, the distance *cd* is traversed with the speed acquired in *c*. Now two speeds are to each other as the distances traversed in equal intervals of time. Hence the speed at *c* is to the speed at *b* as *cd* is to *bl*. But since *dc* is to *be* as their halves, namely, as *ca* is to *ba*, and since *be* is to *bl* as *ba* is to *sa;* it follows that *dc* is to *bl* as *ca* is to *sa*. In other words, the speed at *c* is to that at *b* as *ca* is to *sa*, that is, as the time of fall through *ab*.

The method of measuring the speed of a body along the direction of its fall is thus clear; the speed is assumed to increase directly as the time.

But before we proceed further, since this discussion is to deal with the motion compounded of a uniform horizontal one and one accelerated vertically downwards—the path of a projectile, namely, a parabola—it is necessary that we define some common standard by which we may estimate the velocity, or momentum [*velocitatem, impetum seu momentum*] of both motions

tions; and since from the innumerable uniform velocities one only, and that not selected at random, is to be compounded with a velocity acquired by naturally accelerated motion, I can think of no simpler way of selecting and measuring this than to assume another of the same kind.* For the sake of clearness, draw the vertical line *ac* to meet the horizontal line *bc*. *Ac* is the height and *bc* the amplitude of the semi-parabola *ab*, which is the resultant of the two motions, one that of a body falling

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from rest at *a*, through the distance *ac*, with naturally accelerated motion, the other a uniform motion along the horizontal *ad*. The speed acquired at *c* by a fall through the distance *ac* is determined by the height *ac;* for the speed of a body falling from the same elevation is always one and the same; but along the horizontal one may give a body an infinite number of uniform speeds. However, in order that I may select one out of this multitude and separate it from the rest in a perfectly definite manner, I will extend the height *ca* upwards to *e* just as far as is necessary and will call this distance *ae* the "sublimity." Imagine a body to fall from rest at *e;* it is clear that we may make its terminal speed at *a* the same as that with which the same body travels along the horizontal line *ad;* this speed will be such that, in the time of descent along *ea*, it will describe a horizontal distance twice the length of *ea*. This preliminary remark seems necessary.

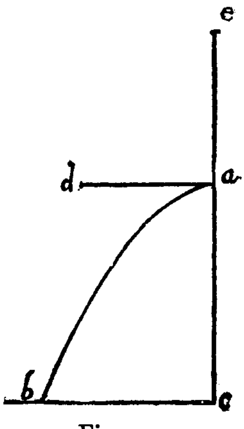

Fig. 111

The reader is reminded that above I have called the horizontal line *cb* the "amplitude" of the semi-parabola *ab;* the axis *ac* of this parabola, I have called its "altitude"; but the line *ea* the fall along which determines the horizontal speed I have called the "sublimity." These matters having been explained, I proceed with the demonstration.

* Galileo here proposes to employ as a standard of velocity the terminal speed of a body falling freely from a given height. [*Trans.*]

SAGR. Allow me, please, to interrupt in order that I may point out the beautiful agreement between this thought of the Author and the views of Plato concerning the origin of the various uniform speeds with which the heavenly bodies revolve. The latter chanced upon the idea that a body could not pass from rest to any given speed and maintain it uniformly except by passing through all the degrees of speed intermediate between the given speed and rest. Plato thought that God, after having created the heavenly bodies, assigned them the proper and uniform speeds with which they were forever to revolve; and that He made them start from rest and move over definite distances under a natural and rectilinear acceleration such as governs the motion of terrestrial bodies. He added that once these bodies had gained their proper and permanent speed, their rectilinear motion was converted into a circular one, the only

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motion capable of maintaining uniformity, a motion in which the body revolves without either receding from or approaching its desired goal. This conception is truly worthy of Plato; and it is to be all the more highly prized since its underlying principles remained hidden until discovered by our Author who removed from them the mask and poetical dress and set forth the idea in correct historical perspective. In view of the fact that astronomical science furnishes us such complete information concerning the size of the planetary orbits, the distances of these bodies from their centers of revolution, and their velocities, I cannot help thinking that our Author (to whom this idea of Plato was not unknown) had some curiosity to discover whether or not a definite "sublimity" might be assigned to each planet, such that, if it were to start from rest at this particular height and to fall with naturally accelerated motion along a straight line, and were later to change the speed thus acquired into uniform motion, the size of its orbit and its period of revolution would be those actually observed.

SALV. I think I remember his having told me that he once made the computation and found a satisfactory correspondence with observation. But he did not wish to speak of it, lest in

view

view of the odium which his many new discoveries had already brought upon him, this might be adding fuel to the fire. But if any one desires such information he can obtain it for himself from the theory set forth in the present treatment.

We now proceed with the matter in hand, which is to prove:

Problem I, Proposition IV

To determine the momentum of a projectile at each particular point in its given parabolic path.

Let *bec* be the semi-parabola whose amplitude is *cd* and whose height is *db*, which latter extended upwards cuts the tangent of the parabola *ca* in *a*. Through the vertex draw the horizontal line *bi* parallel to *cd*. Now if the amplitude *cd* is equal to the entire height *da*, then *bi* will be equal to *ba* and also to *bd;* and if we take *ab* as the measure of the time required for fall through the distance *ab* and also of the momentum acquired at *b* in consequence of its fall from rest at *a*, then if we turn into a horizontal direction the momentum acquired by fall through *ab* [*impetum ab*] the space traversed in the same interval of time will be represented by *dc* which is twice *bi*. But a body which falls from rest at *b* along the line *bd* will during the same time-interval fall through the height of the parabola

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bd. Hence a body falling from rest at *a*, turned into a horizontal direction with the speed *ab* will traverse a space equal to *dc*. Now if one superposes upon this motion a fall along *bd*, traversing the height *bd* while the parabola *bc* is described, then the momentum of the body at the terminal point *c* is the resultant of a uniform horizontal momentum, whose value is represented by *ab*, and of another momentum acquired by fall from *b* to the terminal point *d* or *c;* these two momenta are equal. If, therefore, we take *ab* to be the measure of one of these momenta, say, the uniform horizontal one, then *bi*, which is equal to *bd*, will represent the momentum acquired at *d* or *c;* and *ia* will represent the resultant of these two momenta, that is, the total momentum with which the projectile, travelling along the parabola, strikes at *c*.

With

With this in mind let us take any point on the parabola, say *e*, and determine the momentum with which the projectile passes that point. Draw the horizontal *ef* and take *bg* a mean proportional between *bd* and *bf*. Now since *ab*, or *bd*, is assumed to be the measure of the time and of the momentum [*momentum velocitatis*] acquired by falling from rest at *b* through the distance *bd*, it follows that *bg* will measure the time and also the momentum [*impetus*] acquired at *f* by fall from *b*. If therefore we lay off *bo*, equal to *bg*, the diagonal line joining *a* and *o* will represent the momentum at the point *e*; because the length *ab* has been assumed to represent the momentum at *b* which, after diversion into a horizontal direction, remains constant; and because *bo* measures the momentum at *f* or *e*, acquired by fall, from rest at *b*, through the height *bf*. But the square of *ao* equals the sum of the squares of *ab* and *bo*. Hence the theorem sought.

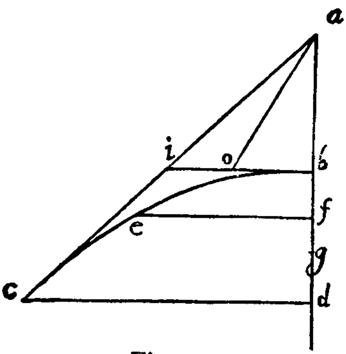

Fig. 112

SAGR. The manner in which you compound these different momenta to obtain their resultant strikes me as so novel that my mind is left in no small confusion. I do not refer to the composition of two uniform motions, even when unequal, and when one takes place along a horizontal, the other along a vertical direction; because in this case I am thoroughly convinced that the resultant is a motion whose square is equal to the sum of the squares of the two components. The confusion arises when one undertakes to compound a uniform horizontal motion with a vertical one which is naturally accelerated. I trust, therefore, we may pursue this discussion more at length.

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SIMP. And I need this even more than you since I am not yet as clear in my mind as I ought to be concerning those fundamental propositions upon which the others rest. Even in the case

case of the two uniform motions, one horizontal, the other perpendicular, I wish to understand better the manner in which you obtain the resultant from the components. Now, Salviati, you understand what we need and what we desire.

SALV. Your request is altogether reasonable and I will see whether my long consideration of these matters will enable me to make them clear to you. But you must excuse me if in the explanation I repeat many things already said by the Author.

Concerning motions and their velocities or momenta [*movimenti e lor velocità o impeti*] whether uniform or naturally accelerated, one cannot speak definitely until he has established a measure for such velocities and also for time. As for time we have the already widely adopted hours, first minutes and second minutes. So for velocities, just as for intervals of time, there is need of a common standard which shall be understood and accepted by everyone, and which shall be the same for all. As has already been stated, the Author considers the velocity of a freely falling body adapted to this purpose, since this velocity increases according to the same law in all parts of the world; thus for instance the speed acquired by a leaden ball of a pound weight starting from rest and falling vertically through the height of, say, a spear's length is the same in all places; it is therefore excellently adapted for representing the momentum [*impeto*] acquired in the case of natural fall.

It still remains for us to discover a method of measuring momentum in the case of uniform motion in such a way that all who discuss the subject will form the same conception of its size and velocity [*grandezza e velocità*]. This will prevent one person from imagining it larger, another smaller, than it really is; so that in the composition of a given uniform motion with one which is accelerated different men may not obtain different values for the resultant. In order to determine and represent such a momentum and particular speed [*impeto e velocità particolare*] our Author has found no better method than to use the momentum acquired by a body in naturally accelerated motion.

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The speed of a body which has in this manner acquired any momentum

momentum whatever will, when converted into uniform motion, retain precisely such a speed as, during a time-interval equal to that of the fall, will carry the body through a distance equal to twice that of the fall. But since this matter is one which is fundamental in our discussion it is well that we make it perfectly clear by means of some particular example.

Let us consider the speed and momentum acquired by a body falling through the height, say, of a spear [*picca*] as a standard which we may use in the measurement of other speeds and momenta as occasion demands; assume for instance that the time of such a fall is four seconds [*minuti secondi d'ora*]; now in order to measure the speed acquired from a fall through any other height, whether greater or less, one must not conclude that these speeds bear to one another the same ratio as the heights of fall; for instance, it is not true that a fall through four times a given height confers a speed four times as great as that acquired by descent through the given height; because the speed of a naturally accelerated motion does not vary in proportion to the time. As has been shown above, the ratio of the spaces is equal to the square of the ratio of the times.

If, then, as is often done for the sake of brevity, we take the same limited straight line as the measure of the speed, and of the time, and also of the space traversed during that time, it follows that the duration of fall and the speed acquired by the same body in passing over any other distance, is not represented by this second distance, but by a mean proportional between the two distances. This I can better illustrate by an example. In the vertical line *ac*, lay off the portion *ab* to represent the distance traversed by a body falling freely with accelerated motion: the time of fall may be represented by any limited straight line, but for the sake of brevity, we shall represent it by the same length *ab;* this length may also be employed as a measure of the momentum and speed acquired during the motion; in short, let *ab* be a measure of the various physical quantities which enter this discussion.

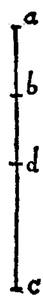

Fig. 113

Having agreed arbitrarily upon *ab* as a measure of these three

three different quantities, namely, space, time, and momentum, our next task is to find the time required for fall through a [288] given vertical distance *ac*, also the momentum acquired at the terminal point *c*, both of which are to be expressed in terms of the time and momentum represented by *ab*. These two required quantities are obtained by laying off *ad*, a mean proportional between *ab* and *ac;* in other words, the time of fall from *a* to *c* is represented by *ad* on the same scale on which we agreed that the time of fall from *a* to *b* should be represented by *ab*. In like manner we may say that the momentum [*impeto o grado di velocità*] acquired at *c* is related to that acquired at *b*, in the same manner that the line *ad* is related to *ab*, since the velocity varies directly as the time, a conclusion, which although employed as a postulate in Proposition III, is here amplified by the Author.

This point being clear and well-established we pass to the consideration of the momentum [*impeto*] in the case of two compound motions, one of which is compounded of a uniform horizontal and a uniform vertical motion, while the other is compounded of a uniform horizontal and a naturally accelerated vertical motion. If both components are uniform, and one at right angles to the other, we have already seen that the square of the resultant is obtained by adding the squares of the components [p. 257] as will be clear from the following illustration.

Let us imagine a body to move along the vertical *ab* with a uniform momentum [*impeto*] of 3, and on reaching *b* to move toward *c* with a momentum [*velocità ed impeto*] of 4, so that during the same time-interval it will traverse 3 cubits along the vertical and 4 along the horizontal. But a particle which moves with the resultant velocity [*velocità*] will, in the same time, traverse the diagonal *ac*, whose length is not 7 cubits—the sum of *ab* (3) and *bc* (4)—but 5, which is *in potenza* equal to the sum of 3 and 4, that is, the squares of 3 and 4 when added make 25, which is the square of *ac*, and is equal to the sum of the squares of

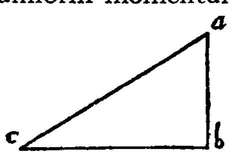

Fig. 114

of *ab* and *bc*. Hence *ac* is represented by the side—or we may say the root—of a square whose area is 25, namely 5.

As a fixed and certain rule for obtaining the momentum which

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results from two uniform momenta, one vertical, the other horizontal, we have therefore the following: take the square of each, add these together, and extract the square root of the sum, which will be the momentum resulting from the two. Thus, in the above example, the body which in virtue of its vertical motion would strike the horizontal plane with a momentum [*forza*] of 3, would owing to its horizontal motion alone strike at *c* with a momentum of 4; but if the body strikes with a momentum which is the resultant of these two, its blow will be that of a body moving with a momentum [*velocità e forza*] of 5; and such a blow will be the same at all points of the diagonal *ac*, since its components are always the same and never increase or diminish.

Let us now pass to the consideration of a uniform horizontal motion compounded with the vertical motion of a freely falling body starting from rest. It is at once clear that the diagonal which represents the motion compounded of these two is not a straight line, but, as has been demonstrated, a semi-parabola, in which the momentum [*impeto*] is always increasing because the speed [*velocità*] of the vertical component is always increasing. Wherefore, to determine the momentum [*impeto*] at any given point in the parabolic diagonal, it is necessary first to fix upon the uniform horizontal momentum [*impeto*] and then, treating the body as one falling freely, to find the vertical momentum at the given point; this latter can be determined only by taking into account the duration of fall, a consideration which does not enter into the composition of two uniform motions where the velocities and momenta are always the same; but here where one of the component motions has an initial value of zero and increases its speed [*velocità*] in direct proportion to the time, it follows that the time must determine the speed [*velocità*] at the assigned point. It only remains to obtain the momentum resulting from these two components (as in the case of uniform motions) by placing the square of the resultant equal

to

APPENDIX

Containing some theorems, and their proofs, dealing with centers of gravity of solid bodies, written by the same Author at an earlier date.*

* Following the example of the National Edition, this *Appendix* which covers 18 pages of the Leyden Edition of 1638 is here omitted as being of minor interest. [*Trans.*]

[FINIS]

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