THE FIRST DAY

from

Dialogues Concerning

TWO NEW SCIENCES

GALILEO GALILEI

TRANSLATED BY

Henry Crew & Alfonso de Salvio

WITH AN INTRODUCTION BY

Antonio Favaro

Galileo Galilei, *Dialogues Concerning Two New Sciences*, translated by Henry Crew & Alfonso de Salvio, with an introduction by Antonio Favaro, Dover Publications, Inc., New York, 1954: 1–108. (1st Day). Originally published in 1904 by the MacMillan company.

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TRANSCRIBER'S NOTES (Added)

The present treatment of Galileo's *Two New Sciences* (1638) follows the condensed format of the 1954 Dover publication with a number of minor cosmetic changes intended to render the work slightly more readable. To this end the *Two New Sciences* has also been split into shorter segments–here in separate parts—*First Day* to *Fourth Day* and *Introduction* from the Crew & De Salvio translation.

Denoted by {nnn} and [nnn] respectively, both modern and original page numbers have generally been incorporated *within* the text with the former also listed at the top of each page. For additional clarity and general consistency some figures have been redone and/or marginally repositioned.

Lastly, the bi-laterally dissimilar floral spira-form adornments and floral triangles from the title and end pages pages of earlier publications have been retained here for necessary completeness.

[*jnh*, 2014]

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.[50]{2}

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 [51] 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 {3} 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 not

[52] 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.*, {4} 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 [53] 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 {5} 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

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

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.[54]

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 {6} 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 [55] [*tenacita e coerenza*] {7} 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

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

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.

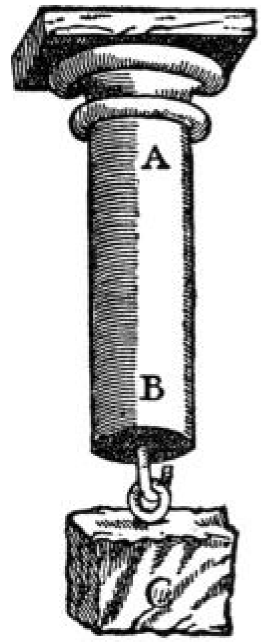

Fig. 1

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 Fig. I 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 [56] {8} 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 {9} 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 {10} becomes longer and in consequence more resistant; so that the cord slips and yields to the tractive force with increasing difficulty.

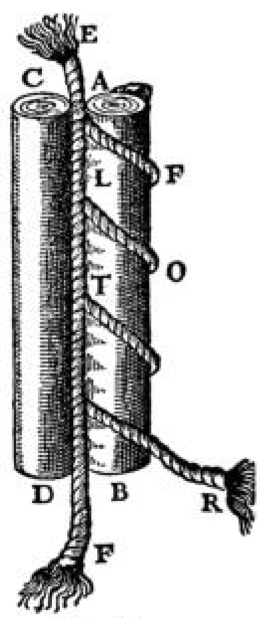

Fig. 2

[58]

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 Fig- 3 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 {11} more tightly and hold himself from slipping, or slacken his hold and descend as slowly as he wished. [59]

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.

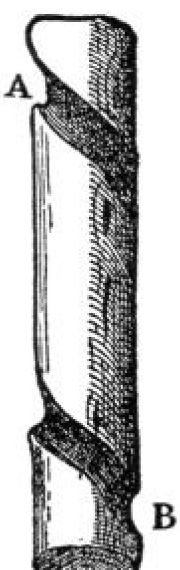

Fig. 3

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 {12} plates is doubtless likewise present between the parts of a solid, and enters, at least in part, as a concomitant cause of their coherence. [60]

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. {13}

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. [61]

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 {14} 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 [62] 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 D 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.

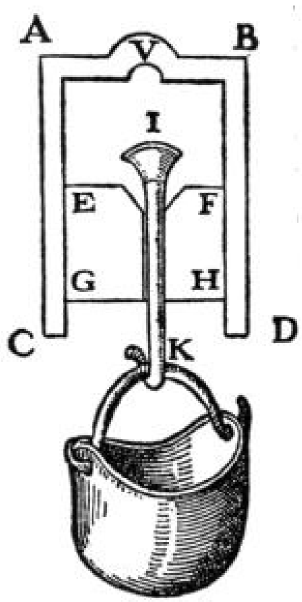

Fig. 4

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 {15} 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

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

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 [63] 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 {16} 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 [64] carried 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 {17} 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.[65]

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, Vs 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 Vs 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 {18} 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? [66]

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 {19} 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 [67] 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, multiplied 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 {20} 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. . . . [68]

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 {21} 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 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 [69] upon 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

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

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 {22} 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 .

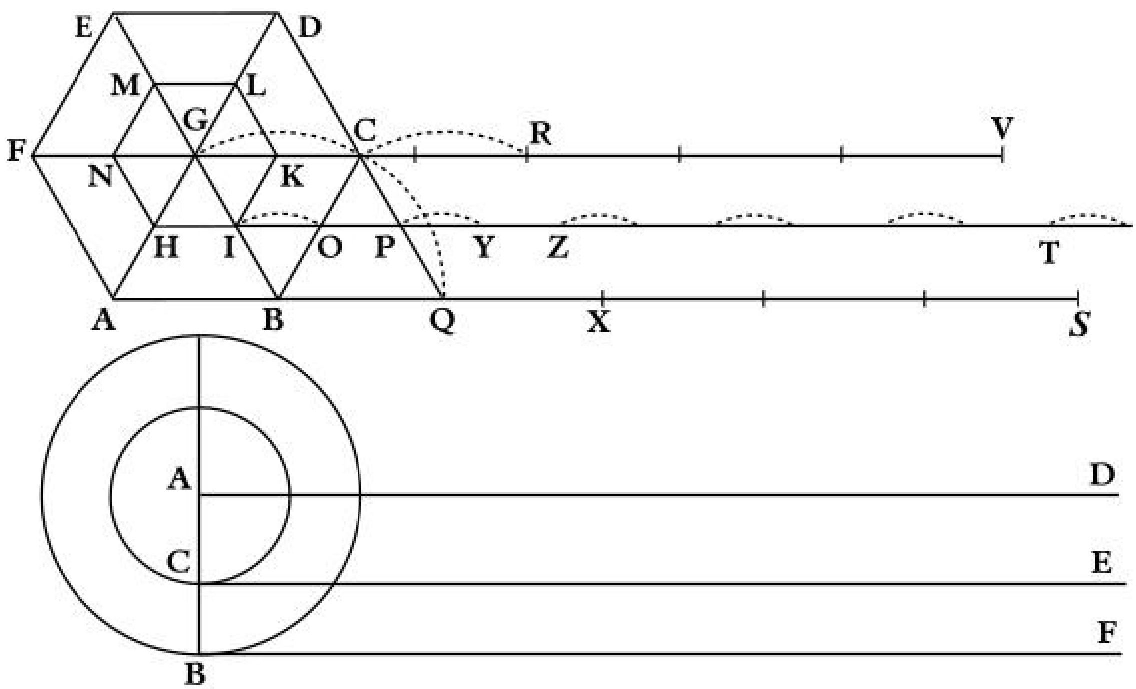

Fig. 5

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 JO 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 [70] 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. {23} 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. [71]

SALV. There are two reasons why this cannot happen. First {24} 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 100,000 sides, the line traversed by the perimeter of the greater, *i.e*., the line laid down by its 100,000 sides one after another, is equal to the line traced out by the 100,000 sides of the smaller, provided we include the 100,000 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 {25} 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 [72] is not possible to arrange them again into a greater length than that which they occupied when they formed a *continuum* [*continuaie*] 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.{26}

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. [73]

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 {27} 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. [74]

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

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

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 {28} 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 Fig. 6 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 [75] 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 {29} 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

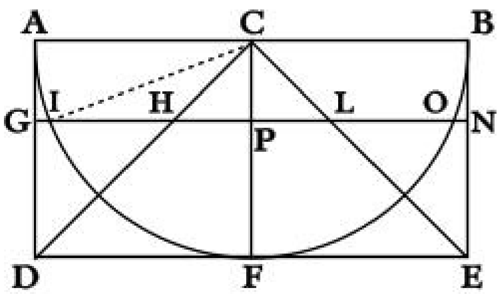

Fig. 6

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 satisfaction [76] 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 1C is equal to the sum of the squares on the two sides IP, PC; but the radius 1C 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 {30} because those who wish to follow it will find it in the twelfth proposition of the second book of *De centra 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 [77] 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

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

† *Cf.* p. 27 above. [*Trans.*]

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. {31}

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 [78] 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. {32}

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 [79] 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 {33} 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 [80] conclude that the

* 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.*]

attributes "larger," "smaller," and "equal" have no place either in comparing infinite quanti-ties 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 {34} 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 {35} 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] infinite 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* [*continue 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 {36} 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.

[82]

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 {37} 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 [83] 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 inumeri*].

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 [*dignita*] ; 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 {38} between 1 and 8 we have 2 and 4 intervening; and between 1 and 27 there lie 3 and 9. Therefore we conclude that unity is the only infinite number. These are some of the marvels which our imagination cannot grasp and which should warn us against the serious error of those who attempt to discuss the infinite by assigning to it the same properties which we employ for the finite, the natures of the two having nothing in common.

With regard to this subject I must tell you of a remarkable property which just now occurs to me and which will explain the vast alteration and change of character which a finite quantity would undergo in passing to infinity. Let us draw the straight line AB of arbitrary length and let the point C divide it into two unequal parts; then I say that, if pairs of lines be drawn, one from each of the terminal points A and B, and if the ratio between the lengths of these lines is the same as that between AC and CB, their points of intersection will all lie upon the circumference of one and the same circle. Thus, for example, [84] AL and BL drawn from A and B, meeting at the point L, bearing to one another the same ratio as AC to BC, and the pair AK and BK meeting at K also bearing to one another the same ratio, and likewise the pairs AI, BI, AH, BH, AG, BG, AF, BF, AE, BE, have their points of intersection L, K, I, H, G, F, E, all lying upon the circumference of one and the same circle. Accordingly if we imagine the point C to move continuously in such a manner that the lines drawn from it to the fixed terminal points, A and B, always maintain the same ratio between their lengths as exists between the original parts, AC and CB, then the point C will, as I shall presently prove, describe a circle. And the circle thus described will increase {39} in size without limit as the point C approaches the middle point which we may call O; but it will diminish in size as C approaches the end B. So that the infinite number of points located in the line OB will, if the motion be as explained above, describe circles of every size, some smaller than the pupil of the eye of a flea, others larger than the celestial equator. Now if we move any of the points lying between the two ends O and B they will all describe circles, those nearest O, immense circles; but if we move the point O itself, and continue to move it according to the aforesaid law, namely, that the lines drawn from O to the terminal points, A and B, maintain the same ratio as the original lines AO and OB, what kind of a line will be produced ? A circle will be drawn larger than the largest of the others, a circle which is therefore infinite. But from the point O a straight line will also be drawn perpendicular to BA and extending to infinity without ever turning, as did the others, to join its last end with its first; for the point C, with its limited motion, having described [85] the upper semi-circle, CHE, proceeds to describe the lower semi-circle EMC, thus returning to the starting point. But the point O having started to describe its circle, as did all the other points in the line AB, (for the points in the other portion OA describe their circles also, the largest being those nearest the point O) is unable to return to its starting point because the circle it describes, being the largest of all, is infinite; in fact, it describes an infinite straight line as circumference of its infinite circle. Think now what a difference there is between a finite and an infinite circle since the latter changes character in such a manner that it loses not only its existence but also its possibility of existence; indeed, we already clearly understand that there can be no such thing as an infinite circle; similarly there can be no infinite sphere, no infinite body, and no infinite surface of any shape. Now what shall we say concerning this metamorphosis in the transition from finite to infinite? And why should we feel greater repugnance, seeing that, in our search after the infinite among numbers we found it in unity? Having broken up a solid into many parts, having reduced it to the finest of powder {40} and having resolved it into its infinitely small indivisible atoms why may we not say that this solid has been reduced to a single *continuum* [*un solo*

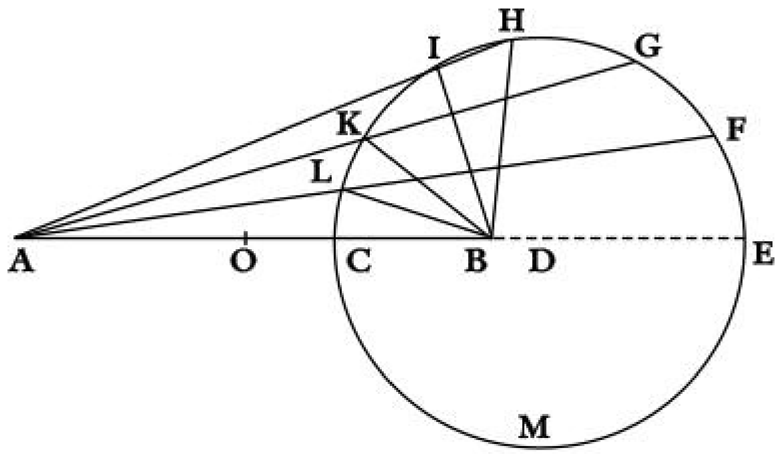

Fig. 7

continuo] perhaps a fluid like water or mercury or even a liquified metal? And do we not see stones melt into glass and the glass itself under strong heat become more fluid than water?

SAGR. Are we then to believe that substances become fluid in virtue of being resolved into their infinitely small indivisible components?

SALV. I am not able to find any better means of accounting for certain phenomena of which the following is one. When I take a hard substance such as stone or metal and when I reduce it by means of a hammer or fine file to the most minute and impalpable powder, it is clear that its finest particles, although when taken one by one are, on account of their smallness, imperceptible to our sight and touch, are nevertheless finite in size, possess shape, and capability of being counted. It is also true that when once heaped up they remain in a heap; and if an excavation be made within limits the cavity will remain and the surrounding particles will not rush in to fill it; if shaken the particles come to rest immediately after the external disturbing agent is removed; the same effects are observed in all piles of [86] larger and larger particles, of any shape, even if spherical, as is the case with piles of millet, wheat, lead shot, and every other material. But if we attempt to discover such properties in water we do not find them; for when once heaped up it immediately flattens out unless held up by some vessel or other external retaining body; when hollowed out it quickly rushes in to fill the cavity; and when disturbed it fluctuates for a long time and sends out its waves through great distances.

Seeing that water has less firmness [*consisttnza*] than the finest of powder, in fact has no consistence whatever, we may, it seems to me, very reasonably conclude that the smallest particles into which it can be resolved are quite different from finite and divisible particles; indeed the only difference I am able to discover is that the former are indivisible. The exquisite transparency { 41} of water also favors this view; for the most transparent crystal when broken and ground and reduced to powder loses its transparency; the finer the grinding the greater the loss; but in the case of water where the attrition is of the highest degree we have extreme transparency. Gold and silver when pulverized with acids [*acque forti*] more finely than is possible with any file still remain powders,* and do not become fluids until the finest particles [*gl' indivisibili*] of fire or of the rays of the sun dissolve them, as I think, into their ultimate, indivisible, and infinitely small components.

SAGR. This phenomenon of light which you mention is one which I have many times remarked with astonishment. I have, for instance, seen lead melted instantly by means of a concave mirror only three hands [*palmi*] in diameter. Hence I think that if the mirror were very large, well-polished and of a parabolic figure, it would just as readily and quickly melt any other metal, seeing that the small mirror, which was not well polished and had only a spherical shape, was able so energetically to melt lead and burn every combustible substance. Such effects as these render credible to me the marvels accomplished by the mirrors of Archimedes.

* It is not clear what Galileo here means by saying that gold and silver when treated with acids still remain powders. [*Trans.*]

SALV. Speaking of the effects produced by the mirrors of Archimedes, it was his own books (which I had already read and studied with infinite astonishment) that rendered credible to me all the miracles described by various writers. And if any doubt had remained the book which Father Buenaventura Cavalieri† [87] has recently published on the subject of the burning glass [*specchio ustorio*] and which I have read with admiration would have removed the last difficulty.

SAGR. I also have seen this treatise and have read it with {42} pleasure and astonishment and knowing the author I was confirmed in the opinion which I had already formed of him that he was destined to become one of the leading mathematicians of our age. But now, with regard to the surprising effect of solar rays in melting metals, must we believe that such a furious action is devoid of motion or that it is accompanied by the most rapid of motions ?

SALV. We observe that other combustions and resolutions are accompanied by motion, and that, the most rapid; note the action of lightning and of powder as used in mines and petards; note also how the charcoal flame, mixed as it is with heavy and impure vapors, increases its power to liquify metals whenever quickened by a pair of bellows. Hence I do not understand how the action of light, although very pure, can be devoid of motion and that of the swiftest type.

SAGR. But of what kind and how great must we consider this speed of light to be ? Is it instantaneous or momentary or does it like other motions require time? Can we not decide this by experiment ?

SIMP. Everyday experience shows that the propagation of light is instantaneous; for when we see a piece of artillery fired, at great distance, the flash reaches our eyes without lapse of time; but the sound reaches the ear only after a noticeable interval.

SAGR. Well, Simplicio, the only thing I am able to infer from this familiar bit of experience is that sound, in reaching our ear, travels more slowly than light; it does not inform me whether the coming of the light is instantaneous or whether, although extremely rapid, it still occupies time. An observation of this kind tells us nothing more than one in which it is claimed that "As soon as the sun reaches the horizon its light reaches our eyes"; but who will assure me that these rays had not reached this limit earlier than they reached our vision ?

SALV. The small conclusiveness of these and other similar observations once led me to devise a method by which one might accurately ascertain whether illumination, *i.e.*, the propagation of light, is really instantaneous. The fact that the speed of sound [88] {43} is as high as it is, assures us that the motion of light cannot fail to be extraordinarily swift. The experiment which I devised was as follows :

Let each of two persons take a light contained in a lantern, or other receptacle, such that by the interposition of the hand, the one can shut off or admit the light to the vision of the other. Next let them stand opposite each other at a distance of a few cubits and

† One of the most active investigators among Galileo's contemporaries; born at Milan 1598; died at Bologna 1647; a Jesuit father, first to introduce the use of logarithms into Italy and first to derive the expression for the focal length of a lens having unequal radii of curvature. His "method of indivisibles" is to be reckoned as a precursor of the infinitesimal calculus. [*Trans.*]

practice until they acquire such skill in uncovering and occulting their lights that the instant one sees the light of his companion he will uncover his own. After a few trials the response will be so prompt that without sensible error [*svario*] the uncovering of one light is immediately followed by the uncovering of the other, so that as soon as one exposes his light he will instantly see that of the other. Having acquired skill at this short distance let the two experimenters, equipped as before, take up positions separated by a distance of two or three miles and let them perform the same experiment at night, noting carefully whether the exposures and occultations occur in the same manner as at short distances; if they do, we may safely conclude that the propagation of light is instantaneous; but if time is required at a distance of three miles which, considering the going of one light and the coming of the other, really amounts to six, then the delay ought to be easily observable. If the experiment is to be made at still greater distances, say eight or ten miles, telescopes may be employed, each observer adjusting one for himself at the place where he is to make the experiment at night; then although the lights are not large and are therefore invisible to the naked eye at so great a distance, they can readily be covered and uncovered since by aid of the telescopes, once adjusted and fixed, they will become easily visible.

SAGR. This experiment strikes me as a clever and reliable invention. But tell us what you conclude from the results.

SALV. In fact I have tried the experiment only at a short distance, less than a mile, from which I have not been able to ascertain with certainty whether the appearance of the opposite {44} light was instantaneous or not; but if not instantaneous it is extraordinarily rapid—I should call it momentary; and for the present I should compare it to motion which we see in the lightning flash between clouds eight or ten miles distant from us. We see the beginning of this light—I might say its head and [89] source—located at a particular place among the clouds; but it immediately spreads to the surrounding ones, which seems to be an argument that at least some time is required for propagation; for if the illumination were instantaneous and not gradual, we should not be able to distinguish its origin—its center, so to speak—from its outlying portions. What a sea we are gradually slipping into without knowing it! With vacua and infinities and indivisibles and instantaneous motions, shall we ever be able, even by means of a thousand discussions, to reach dry land?

SAGR. Really these matters lie far beyond our grasp. Just think; when we seek the infinite among numbers we find it in unity; that which is ever divisible is derived from indivisibles; the vacuum is found inseparably connected with the plenum; indeed the views commonly held concerning the nature of these matters are so reversed that even the circumference of a circle turns out to be an infinite straight line, a fact which, if my memory serves me correctly, you, Salviati, were intending to demonstrate geometrically. Please therefore proceed without further digression.

SALV. I am at your service; but for the sake of greater clearness let me first demonstrate the following problem:

> Given a straight line divided into unequal parts which bear to each other any ratio whatever, to describe a circle such that two straight lines drawn from the ends of the given line to any point on the circumference will bear to each other the same ratio as the two parts of the given line, thus making those lines which are drawn from the same terminal points homologous.

Let AB represent the given straight line divided into any two unequal parts by the point C; the problem is to describe a circle such {45} such that two straight lines drawn from the terminal points, A and B, to any point on the circumference will bear to each other the same ratio as the part AC bears to BC, so that lines drawn from the same terminal points are homologous. About C as center describe a circle having the shorter part CB of the given line, as radius. Through A draw a straight line AD which [90] shall be tangent to the circle at D and indefinitely prolonged toward E. Draw the radius CD which will be perpendicular to AE. At B erect a perpendicular to AB; this perpendicular will intersect AE at some point since the angle at A is acute; call this point of intersection E, and from it draw a perpendicular to AE which will intersect AB prolonged in F. Now I say the two straight lines FE and FC are equal. For if we join E and C, we shall have two triangles, DEC and BEC, in which the two sides of the one, DE and EC, are equal to the two sides of the other, BE and EC, both DE and EB being tangents to the circle DB while the bases DC and CB are likewise equal; hence the two angles, DEC and BEC, will be equal. Now since the angle BCE differs from a right angle by the angle CEB, and the angle CEF also differs from a right angle by the angle CED, and since these differences are equal, it follows that the angle FCE is equal to CEF; consequently the sides FE and FC are equal. If we describe a circle with F as center and FE as radius it will pass through the point C; let CEG be such a circle. This is the circle sought, for if we draw lines from the terminal points A and B to any point on its circumference they will bear to each other the {46} same ratio as the two portions AC and BC which meet at the point C. This is manifest in the case of the two lines AE and BE, meeting at the point E, because the angle E of the triangle AEB is bisected by the line CE, and therefore AC: CB = AE: BE. The same may be proved of the two lines AG and BG terminating in the point G. For since the triangles AFE and EFB are similar, we have AF:FE=EF:FB, or AF:FC=CF:FB, and *dividendo* AC:CF=CB:BF, or AC:FG=CB:BF; also *componendo* we have both AB: BG=CB: BF and AG: GB=CF: FB =AE:EB=AC:BC.

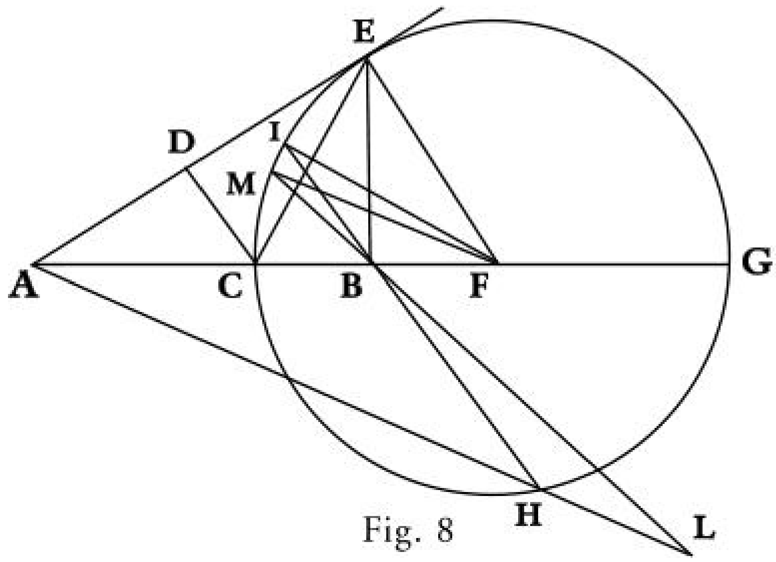

Fig. 8

Q. E. D.

[91] Take now any other point in the circumference, say H, where the two lines AH and BH intersect; in like manner we shall have AC: CB=AH: HB. Prolong HB until it meets the circumference at I and join IF; and since we have already found that AB:BG=CB:BF it follows that the rectangle AB.BF is equal to the rectangle CB.BG or IB.BH. Hence AB: BH=IB:BF. But the angles at B are equal and therefore AH:HB = IF:FB=EF:FB=AE:EB.

Besides, I may add, that it is impossible for lines which maintain this same ratio and which are drawn from the terminal points, A and B, to meet at any point either inside or outside the circle, CEG. For suppose this were possible; let AL and BL be two such lines intersecting at the point L outside the circle: prolong LB till it meets the circumference at M and join MF. If AL:BL=AC:BC=MF:FB, then we shall have two triangles ALB and MFB which have the sides about the two angles proportional, the angles at the vertex, B, equal, and the two remaining angles, FMB and LAB, less than right angles (because the right angle at M has for its base the entire diameter CG and not merely a part BF: and the other angle at the point A is acute because the line AL, the homologue of AC, is greater than BL, the homologue of BC). From this it follows that the triangles ABL and MBF are similar and therefore AB:BL= MB:BF, making the rectangle AB.BF =MB.BL; but it has been demonstrated that the rectangle AB.BF is equal to CB.BG; whence it would follow that the rectangle MB.BL is equal to the {47} rectangle CB.BG which is impossible; therefore the intersection cannot fall outside the circle. And in like manner we can show that it cannot fall inside; hence all these intersections fall on the circumference.

But now it is time for us to go back and grant the request of Simplicio by showing him that it is not only not impossible to resolve a line into an infinite number of points but that this is quite as easy as to divide it into its finite parts. This I will do under the following condition which I am sure, Simplicio, you will not deny me, namely, that you will not require me to separate the points, one from the other, and show them to you, [92] one by one, on this paper; for I should be content that you, without separating the four or six parts of a line from one another, should show me the marked divisions or at most that you should fold them at angles forming a square or a hexagon: for, then, I am certain you would consider the division distinctly and actually accomplished.

SIMP. I certainly should.

SALV. If now the change which takes place when you bend a line at angles so as to form now a square, now an octagon, now a polygon of forty, a hundred or a thousand angles, is sufficient to bring into actuality the four, eight, forty, hundred, and thousand parts which, according to you, existed at first only potentially in the straight line, may I not say, with equal right, that, when I have bent the straight line into a polygon having an infinite number of sides, *i.e.*, into a circle, I have reduced to actuality that infinite number of parts which you claimed, while it was straight, were contained in it only potentially? Nor can one deny that the division into an infinite number of points is just as truly accomplished as the one into four parts when the square is formed or into a thousand parts when the millagon is formed; for in such a division the same conditions are satisfied as in the case of a polygon of a thousand or a hundred thousand sides. Such a polygon laid upon a straight line touches it with one of its sides, *i.e.*, with one of its hundred thousand parts; while the circle which is a polygon of an infinite number of sides touches {48} the same straight line with one of its sides which is a single point different from all its neighbors and therefore separate and distinct in no less degree than is one side of a polygon from the other sides. And just as a polygon, when rolled along a plane, marks out upon this plane, by the successive contacts of its sides, a straight line equal to its perimeter, so the circle rolled upon such a plane also traces by its infinite succession of contacts a straight line equal in length to its own circumference. I am willing, Simplicio, at the outset, to grant to the Peripatetics the truth of their opinion that a continuous quantity [*il continue*] is divisible only into parts which are still further divisible so that however far the division and subdivision be continued no end will be reached; but I am not so certain that they will concede to me that none of these divisions of theirs can be a final one, as is surely the fact, because there always remains "another"; the final and ultimate division is rather one which resolves a continuous quantity into an infinite number of indivisible quantities, a result which I grant can never be reached by successive division into an ever-increasing number of parts. But if they employ the method which I propose for separating [93] and resolving the whole of infinity [*tutta la infinita*], at a single stroke (an artifice which surely ought not to be denied me), I think that they would be contented to admit that a continuous quantity is built up out of absolutely indivisible atoms, especially since this method, perhaps better than any other, enables us to avoid many intricate labyrinths, such as cohesion in solids, already mentioned, and the question of expansion and contraction, without forcing upon us the objectionable admission of empty spaces [in solids] which carries with it the penetrability of bodies. Both of these objections, it appears to me, are avoided if we accept the above-mentioned view of indivisible constituents.

SIMP. I hardly know what the Peripatetics would say since the views advanced by you would strike them as mostly new, and as such we must consider them. It is however not unlikely that they would find answers and solutions for these problems which {49} I, for want of time and critical ability, am at present unable to solve. Leaving this to one side for the moment, I should like to hear how the introduction of these indivisible quantities helps us to understand contraction and expansion avoiding at the same time the vacuum and the penetrability of bodies.

SAGR. I also shall listen with keen interest to this same matter which is far from clear in my mind; provided I am allowed to hear what, a moment ago, Simplicio suggested we omit, namely, the reasons which Aristotle offers against the existence of the vacuum and the arguments which you must advance in rebuttal.

SALV. I will do both. And first, just as, for the production of expansion, we employ the line described by the small circle during one rotation of the large one—a line greater than the circumference of the small circle—so, in order to explain contraction, we point out that, during each rotation of the smaller circle, the larger one describes a straight line which is shorter than its circumference.

For the better understanding of this we proceed to the consideration of what happens in the case of polygons. Employing [94] a figure similar to the earlier one, construct the two hexagons, ABC and HIK, about the common center L, and let them roll along the parallel lines HOM and ABC. Now holding the vertex I fixed, allow the smaller polygon to rotate until the side IK lies upon the parallel, during which motion the point K will BC will coincide with *be*, having advanced only through the distance Be, but having retreated through a portion of the line BA which subtends the arc B*b*. If we allow the rotation of the smaller polygon to go on it will traverse and describe along its parallel a line equal to its perimeter; while the larger one will traverse and describe a line less than its perimeter by as many times the length *bB* as there are {50} are sides less one; this line is approximately equal to that described by the smaller polygon exceeding it only by the distance *bB*. Here now we see, without any difficulty, why the larger polygon, when carried by the smaller, does not measure off with its sides a line longer than that traversed by the smaller one; this is because a portion of each side is super-posed upon its immediately preceding neighbor.

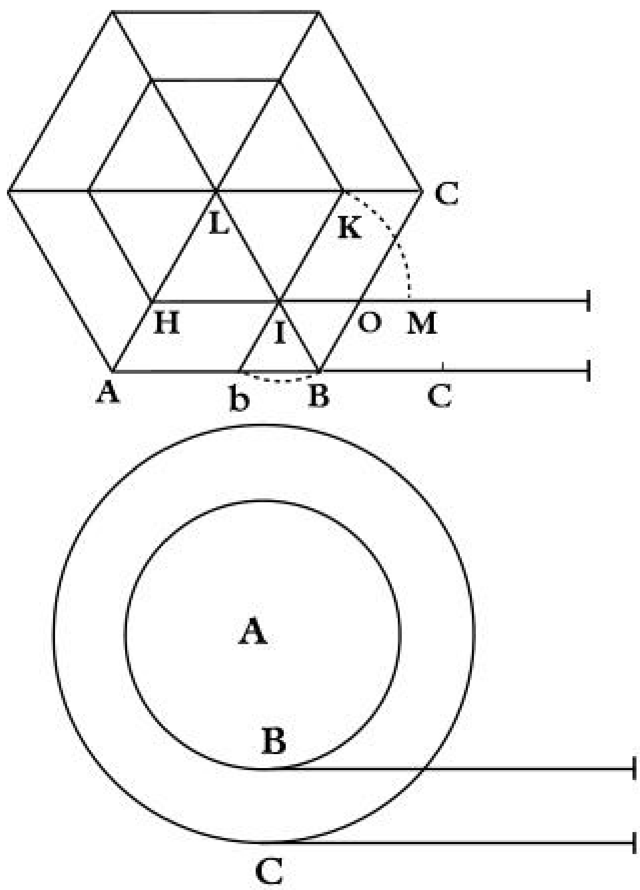

Fig. 9

Let us next consider two circles, having a common center at A, and lying upon their respective parallels, the smaller being tangent to its parallel at the point B; the larger, at the point C. Here when the small circle commences to roll the point B [95] does not remain at rest for a while so as to allow BC to move backward and carry with it the point C, as happened in the case of the polygons, where the point I remained fixed until the side KI coincided with MI and the line IB carried the terminal point B backward as far as *b*, so that the side BC fell upon *be*, thus superposing upon the line BA, the portion *Bb*, and advancing by an amount *Be*, equal to MI, that is, to one side of the smaller polygon. On account of these super-positions, which are the excesses of the sides of the larger over the smaller polygon, each net advance is equal to one side of the smaller polygon and, during one complete rotation, these amount to a straight line equal in length to the perimeter of the smaller polygon.{51}

But now reasoning in the same way concerning the circles, we must observe that whereas the number of sides in any polygon is comprised within a certain limit, the number of sides in a circle is infinite; the former are finite and divisible; the latter infinite and indivisible. In the case of the polygon, the vertices remain at rest during an interval of time which bears to the period of one complete rotation the same ratio which one side bears to the perimeter; likewise, in the case of the circles, the delay of each of the infinite number of vertices is merely instantaneous, because an instant is such a fraction of a finite interval as a point is of a line which contains an infinite number of points. The retrogression of the sides of the larger polygon is not equal to the length of one of its sides but merely to the excess of such a side over one side of the smaller polygon, the net advance being equal to this smaller side; but in the circle, the point or side C, during the instantaneous rest of B, recedes by an amount equal to its excess over the side B, making a net progress equal to B itself. In short the infinite number of indivisible sides of the greater circle with their infinite number of indivisible retrogressions, made during the infinite number of instantaneous delays of the infinite number of vertices of the smaller circle, together with the infinite number of progressions, equal to the infinite number of sides in the smaller circle—all these, I say, add up to a line equal to that described by the smaller circle, a line which contains an infinite number of infinitely small superpositions, thus bringing about a thickening or contraction without any overlapping or interpenetration of finite parts. This result could not be obtained in the case of a line divided [96] into finite parts such as is the perimeter of any polygon, which when laid out in a straight line cannot be shortened except by the overlapping and interpenetration of its sides. This contraction of an infinite number of infinitely small parts without the interpenetration or overlapping of finite parts and the previously mentioned [p. 70, Nat. Ed.] expansion of an infinite number of indivisible parts by the interposition of indivisible vacua is, in my opinion, the most that can be said concerning the contraction and {52} rarefaction of bodies, unless we give up the impenetrability of matter and introduce empty spaces of finite size. If you find anything here that you consider worth while, pray use it; if not regard it, together with my remarks, as idle talk; but this remember, we are dealing with the infinite and the indivisible.

SAGR. I frankly confess that your idea is subtle and that it impresses me as new and strange; but whether, as a matter of fact, nature actually behaves according to such a law I am unable to determine; however, until I find a more satisfactory explanation I shall hold fast to this one. Perhaps Simplicio can tell us something which I have not yet heard, namely, how to explain the explanation which the philosophers have given of this abstruse matter; for, indeed, all that I have hitherto read concerning contraction is so dense and that concerning expansion so thin that my poor brain can neither penetrate the former nor grasp the latter.

SIMP. I am all at sea and find difficulties in following either path, especially this new one; because according to this theory an ounce of gold might be rarefied and expanded until its size would exceed that of the earth, while the earth, in turn, might be condensed and reduced until it would become smaller than a walnut, something which I do not believe; nor do I believe that you believe it. The arguments and demonstrations which you have advanced are mathematical, abstract, and far removed from concrete matter; and I do not believe that when applied to the physical and natural world these laws will hold.

SALV. I am not able to render the invisible visible, nor do I think that you will ask this. But now that you mention gold, do not our senses tell us that that metal can be immensely expanded? I do not know whether you have observed the method [97] employed by those who are skilled in drawing gold wire, of which really only the surface is gold, the inside material being silver. The way they draw it is as follows: they take a cylinder or, if you please, a rod of silver, about half a cubit long and three or four times as wide as one's thumb; this rod they cover with gold-leaf which is so thin that it almost floats in air, putting on not {53} more than eight or ten thicknesses. Once gilded they begin to pull it, with great force, through the holes of a draw-plate; again and again it is made to pass through smaller and smaller holes, until, after very many passages, it is reduced to the fineness of a lady's hair, or perhaps even finer; yet the surface remains gilded. Imagine now how the substance of this gold has been expanded and to what fineness it has been reduced.

SIMP. I do not see that this process would produce, as a consequence, that marvellous thinning of the substance of the gold which you suggest: first, because the original gilding consisting of ten layers of gold-leaf has a sensible thickness; secondly, because in drawing out the silver it grows in length but at the same time diminishes proportionally in thickness; and, since one dimension thus compensates the other, the area will not be so increased as to make it necessary during the process of gilding to reduce the thinness of the gold beyond that of the original leaves.

SALV. You are greatly mistaken, Simplicio, because the surface increases directly as the square root of the length, a fact which I can demonstrate geometrically.

SAGR. Please give us the demonstration not only for my own sake but also for Simplicio provided you think we can understand it.

SALV. I'll see if I can recall it on the spur of the moment. At the outset, it is clear that the original thick rod of silver and the wire drawn out to an enormous length are two cylinders of the same volume, since they are the same body of silver. So [98] that, if I determine the ratio between the surfaces of cylinders of the same volume, the problem will be solved. I say then,

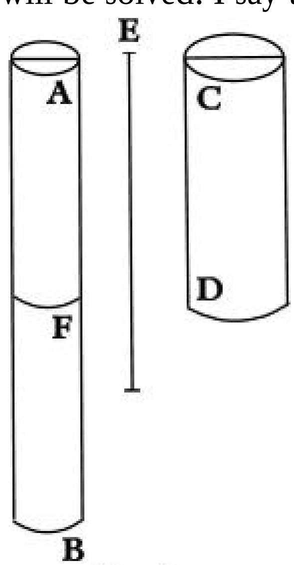

Fig. 10

> The areas of cylinders of equal volumes, neglecting the bases, bear to each other a ratio which is the square root of the ratio of their lengths.

Take two cylinders of equal volume having the altitudes AB and CD, between which the line E is a mean proportional. Then I claim that, omitting the bases of each cylinder, the surface of the cylinder AB is to that of the cylinder CD as the length AB is {54} to the line E, that is, as the square root of AB is to the square root of CD. Now cut off the cylinder AB at F so that the altitude AF is equal to CD. Then since the bases of cylinders of equal volume bear to one another the inverse ratio of their heights, it follows that the area of the circular base of the cylinder CD will be to the area of the circular base of AB as the altitude BA is to DC: moreover, since circles are to one another as the squares of their diameters, the said squares will be to each other as BA is to CD. But BA is to CD as the square of BA is to the square of E: and, therefore, these four squares will form a proportion; and likewise their sides; so the line AB is to E as the diameter of circle C is to the diameter of the circle A. But the diameters are proportional to the circumferences and the circumferences are proportional to the areas of cylinders of equal height; hence the line AB is to E as the surface of the cylinder CD is to the surface of the cylinder AF. Now since the height AF is to AB as the surface of AF is to the surface of AB; and since the height AB is to the line E as the surface CD is to AF, it follows, *ex oequali in proportione perturbata** that the height AF is to E as the surface CD is to the surface AB, and *convertendo,* the surface of the cylinder AB is to the surface of the cylinder CD as the line E is to AF, *i.e.*, to CD, or as AB is to E which is the square root of the ratio of AB to CD. Q. E. D.

If now we apply these results to the case in hand, and assume that the silver cylinder at the time of gilding had a length of only half a cubit and a thickness three or four times that of [99] one's thumb, we shall find that, when the wire has been reduced to the fineness of a hair and has been drawn out to a length of twenty thousand cubits (and perhaps more), the area of its surface will have been increased not less than two hundred times. Consequently the ten leaves of gold which were laid on {55} have been extended over a surface two hundred times greater, assuring us that the thickness of the gold which now covers the surface of so many cubits of wire cannot be greater than one twentieth that of an ordinary leaf of beaten gold. Consider now what degree of fineness it must have and whether one could conceive it to happen in any other way than by enormous expansion of parts; consider also whether this experiment does not suggest that physical bodies [*materie fisiche*] are composed of infinitely small indivisible particles, a view which is supported by other more striking and conclusive examples.

SAGR. This demonstration is so beautiful that, even if it does not have the cogency originally intended,—although to my mind, it is very forceful—the short time devoted to it has nevertheless been most happily spent.

SALV. Since you are so fond of these geometrical demonstrations, which carry with them distinct gain, I will give you a companion theorem which answers an extremely interesting query. We have seen above what relations hold between equal cylinders of different height or length; let us now see what holds when the cylinders are equal in area but unequal in height, understanding area to include the curved surface, but not the upper and lower bases. The theorem is:

> The volumes of right cylinders having equal curved surfaces are inversely proportional to their altitudes.

Let the surfaces of the two cylinders, AE and CF, be equal but let the height of the latter, CD, be greater than that of the former, AB: then I say that the volume of the cylinder AE is to that of the cylinder CF as the height CD is to AB. Now since the surface of CF is equal to the surface of AE, it follows that the volume of CF is less than that of AE; for, if they were equal, the surface of CF would, by the preceding proposition, exceed that of AE, and the excess would be so much the greater if the volume of the cylinder CF were

* See *Euclid*, Book V, Def. 20., Todhunter's Ed., p. 137 (London, 1877.) [*Trans.*]

greater than that [100] of AE. Let us now take a cylinder ID having a volume equal to that of AE; then, according to the preceding theorem, the surface of the cylinder ID is to the surface of AE as the altitude {56} IF is to the mean proportional between IF and AB. But since one datum of the problem is that the surface of AE is equal to that of CF, and since the surface ID is to the surface CF as the altitude IF is to the altitude CD, it follows that CD is a mean proportional between IF and AB. Not only so, but since the volume of the cylinder ID is equal to that of AE, each will bear the same ratio to the volume of the cylinder CF; but the volume ID is to the volume CF as the altitude IF is to the altitude CD; hence the volume of AE is to the volume of CF as the length IF is to the length CD, that is, as the length CD is to the length AB. Q. E. D.

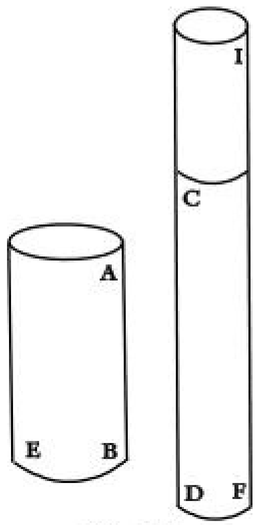

Fig. 11

This explains a phenomenon upon which the common people always look with wonder, namely, if we have a piece of stuff which has one side longer than the other, we can make from it a cornsack, using the customary wooden base, which will hold more when the short side of the cloth is used for the height of the sack and the long side is wrapped around the wooden base, than with the alternative arrangement. So that, for instance, from a piece of cloth which is six cubits on one side and twelve on the other, a sack can be made which will hold more when the side of twelve cubits is wrapped around the wooden base, leaving the sack six cubits high than when the six cubit side is put around the base making the sack twelve cubits high. From what has been proven above we learn not only the general fact that one sack holds more than the other, but we also get specific and particular information as to how much more, namely, just in proportion as the altitude of the sack diminishes the contents increase and *vice versa.* Thus if we use the figures given which make the cloth twice as long as wide and if we use the long side for the seam, the volume of the sack will be just one-half as great as with the opposite arrangement. Likewise [101] {57} if we have a piece of matting which measures 7 x 25 cubits and make from it a basket, the contents of the basket will, when the seam is lengthwise, be seven as compared with twenty-five when the seam runs endwise.

SAGR. It is with great pleasure that we continue thus to acquire new and useful information. But as regards the subject just discussed, I really believe that, among those who are not already familiar with geometry, you would scarcely find four persons in a hundred who would not, at first sight, make the mistake of believing that bodies having equal surfaces would be equal in other respects. Speaking of areas, the same error is made when one attempts, as often happens, to determine the sizes of various cities by measuring their boundary lines, forgetting that the circuit of one may be equal to the circuit of another while the area of the one is much greater than that of the other. And this is true not only in the case of irregular, but also of regular surfaces, where the polygon having the greater number of sides always contains a larger area than the one with the

* See interesting biographical note on Sacrobosco [John Holywood] in *Ency. Brit.*, nth Ed. [*Trans.*]

less number of sides, so that finally the circle which is a polygon of an infinite number of sides contains the largest area of all polygons of equal perimeter. I remember with particular pleasure having seen this demonstration when I was studying the sphere of Sacrobosco* with the aid of a learned commentary.

SALV. Very true! I too came across the same passage which suggested to me a method of showing how, by a single short demonstration, one can prove that the circle has the largest content of all regular isoperimetric figures; and that, of other [102] figures, the one which has the larger number of sides contains a greater area than that which has the smaller number.

SAGR. Being exceedingly fond of choice and uncommon propositions, I beseech you to let us have your demonstration.

SALV. I can do this in a few words by proving the following theorem:

> The area of a circle is a mean proportional between any {58} two regular and similar polygons of which one circumscribes it and the other is isoperimetric with it. In addition, the area of the circle is less than that of any circumscribed polygon and greater than that of any isoperimetric polygon. And further, of these circumscribed polygons, the one which has the greater number of sides is smaller than the one which has a less number; but, on the other hand, that isoperimetric polygon which has the greater number of sides is the larger.

Let A and B be two similar polygons of which A circumscribes the given circle and B is isoperimetric with it. The area of the circle will then be a mean proportional between the areas of the polygons. For if we indicate the radius of the circle by AC and if we remember that the area of the circle is equal to that of a right-angled triangle in which one of the sides about the right angle is equal to the radius, AC, and the other to the circumference; and if likewise we remember that the area of the polygon A is equal to the area of a right-angled triangle one of [103] whose sides about the right angle has the same length as AC and the other is equal to the perimeter of the polygon itself; it is then

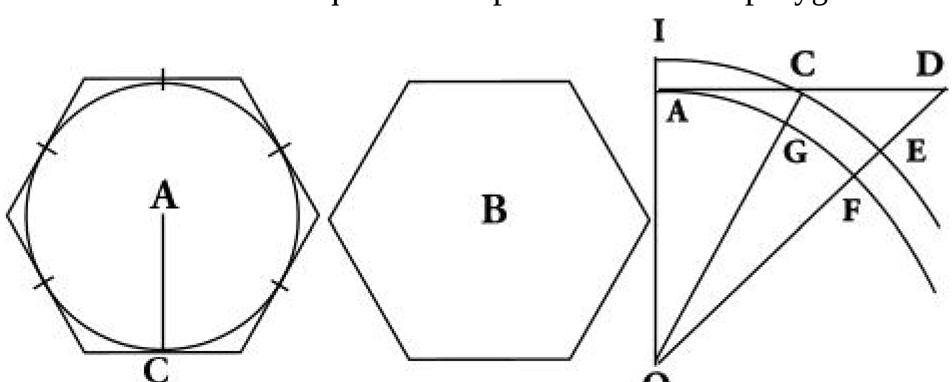

Fig. 12

manifest that the circum-scribed polygon bears to the circle the same ratio which its perimeter bears to the circumference of the circle, or to the perimeter of the polygon B

* See interesting biographical note on Sacrobosco [John Holywood] in *Enc. Brit*., 11th Ed. [*Trans.*]

which is, by hypothesis, equal to the circumference of the circle. But since the polygons A and B are similar their areas are to each other as the squares of their perimeters; hence the area of the circle A is a mean {59} mean proportional between the areas of the two polygons A and B. And since the area of the polygon A is greater than that of the circle A, it is clear that the area of the circle A is greater than that of the isoperimetric polygon B, and is therefore the greatest of all regular polygons having the same perimeter as the circle.

We now demonstrate the remaining portion of the theorem, which is to prove that, in the case of polygons circumscribing a given circle, the one having the smaller number of sides has a larger area than one having a greater number of sides; but that on the other hand, in the case of isoperimetric polygons, the one having the more sides has a larger area than the one with less sides. To the circle which has O for center and OA for radius draw the tangent AD; and on this tangent lay off, say, AD which shall represent one-half of the side of a circumscribed pentagon and AC which shall represent one-half of the side of a heptagon; draw the straight lines OGC and OFD; then with O as a center and OC as radius draw the arc ECI. Now since the triangle DOC is greater than the sector EOC and since the sector COI is greater than the triangle COA, it follows that the triangle DOC bears to the triangle COA a greater ratio than the sector EOC bears to the sector COI, that is, than the sector FOG bears to the sector GOA. Hence, *componendo et permutando,* the triangle DOA bears to the sector FOA a greater ratio than that which the triangle COA bears to the sector GOA, and also 10 such triangles DOA bear to 10 such sectors FOA a greater ratio than 14 such triangles COA bear to 14 such sectors GOA, that is to say, the circumscribed pentagon bears to the circle a greater ratio than does the heptagon. Hence the pentagon exceeds the heptagon in area.

But now let us assume that both the heptagon and the pentagon have the same perimeter as that of a given circle. Then I say the heptagon will contain a larger area than the pentagon. For since the area of the circle is a mean proportional between areas of the circumscribed and of the isoperimetric pentagons, [104] and since likewise it is a mean proportional between the {60} circumscribed and isoperimetric heptagons, and since also we have proved that the circumscribed pentagon is larger than the circumscribed heptagon, it follows that this circumscribed pentagon bears to the circle a larger ratio than does the heptagon, that is, the circle will bear to its isoperimetric pentagon a greater ratio than to its isoperimetric heptagon. Hence the pentagon is smaller than its isoperimetric heptagon. Q. E. D.

SAGR. A very clever and elegant demonstration! But how did we come to plunge into geometry while discussing the objections urged by Simplicio, objections of great moment, especially that one referring to density which strikes me as particularly difficult?

SALV. If contraction and expansion [*condensazione e rarefazzione*] consist in contrary motions, one ought to find for each great expansion a correspondingly large contraction. But our surprise is increased when, every day, we see enormous expansions taking place almost instantaneously. Think what a tremendous expansion occurs when a small quantity of gunpowder flares up into a vast volume of fire ! Think too of the almost limitless expansion of the light which it produces! Imagine the contraction which would take place if this fire and this light were to reunite, which, indeed, is not impossible since only a little while ago they were located together in this small space. You will find, upon observation, a thousand such expansions for they are more obvious than contractions since dense matter is more palpable and accessible to our senses. We can take wood and see it go up in fire and light, but we do not see [105] them recombine to form wood; we see fruits and flowers and a thousand other solid bodies dissolve largely into odors, but we do not observe these fragrant atoms coming together to form fragrant solids. But where the senses fail us reason must step in; for it will enable us to understand the motion involved in the condensation of extremely rarefied and tenuous substances just as clearly as that involved in the expansion and dissolution of solids. Moreover we are trying to find out how it is possible to produce expansion and contraction in bodies which are capable of such changes without introducing vacua and without giving up {61} up the impenetrability of matter; but this does not exclude the possibility of there being materials which possess no such properties and do not, therefore, carry with them consequences which you call inconvenient and impossible. And finally, Simplicio, I have, for the sake of you philosophers, taken pains to find an explanation of how expansion and contraction can take place without our admitting the penetrability of matter and introducing vacua, properties which you deny and dislike; if you were to admit them, I should not oppose you so vigorously. Now either admit these difficulties or accept my views or suggest something better.

SAGR. I quite agree with the peripatetic philosophers in denying the penetrability of matter. As to the vacua I should like to hear a thorough discussion of Aristotle's demonstration in which he opposes them, and what you, Salviati, have to say in reply. I beg of you, Simplicio, that you give us the precise proof of the Philosopher and that you, Salviati, give us the reply.

SIMP. So far as I remember, Aristotle inveighs against the ancient view that a vacuum is a necessary prerequisite for motion and that the latter could not occur without the former. In opposition to this view Aristotle shows that it is precisely the phenomenon of motion, as we shall see, which renders untenable the idea of a vacuum. His method is to divide the argument into two parts. He first supposes bodies of different weights to move in the same medium; then supposes, one and the same body to move in different media. In the first case, he [106] supposes bodies of different weight to move in one and the same medium with different speeds which stand to one another in the same ratio as the weights; so that, for example, a body which is ten times as heavy as another will move ten times as rapidly as the other. In the second case he assumes that the speeds of one and the same body moving in different media are in inverse ratio to the densities of these media; thus, for instance, if the density of water were ten times that of air, the speed in air would be ten times greater than in water. From this second supposition, {62} he shows that, since the tenuity of a vacuum differs infinitely from that of any medium filled with matter however rare, any body which moves in a plenum through a certain space in a certain time ought to move through a vacuum instantaneously; but instantaneous motion is an impossibility; it is therefore impossible that a vacuum should be produced by motion.

SALV. The argument is, as you see, *ad hominem*, that is, it is directed against those who thought the vacuum a prerequisite for motion. Now if I admit the argument to be conclusive and concede also that motion cannot take place in a vacuum, the assumption of a vacuum considered absolutely and not with reference to motion, is not thereby invalidated. But to tell you what the ancients might possibly have replied and in order to better understand just how conclusive Aristotle's demonstration is, we may, in my opinion, deny both of his assumptions. And as to the first, I greatly doubt that Aristotle ever tested by experiment whether it be true that two stones, one weighing ten times as much as the other, if allowed to fall, at the same instant, from a height of, say, 100 cubits, would so differ in speed that when the heavier had reached the ground, the other would not have fallen more than 10 cubits.

SIMP. His language would seem to indicate that he had tried the experiment, because he says: *We see the heavier;* now the word *see* shows that he had made the experiment.

SAGR. But I, Simplicio, who have made the test can assure [107] you that a cannon ball weighing one or two hundred pounds, or even more, will not reach the ground by as much as a span ahead of a musket ball weighing only half a pound, provided both are dropped from a height of 200 cubits.

SALV. But, even without further experiment, it is possible to prove clearly, by means of a short and conclusive argument, that a heavier body does not move more rapidly than a lighter one provided both bodies are of the same material and in short such as those mentioned by Aristotle. But tell me, Simplicio, whether you admit that each falling body acquires a definite speed {63} fixed by nature, a velocity which cannot be increased or diminished except by the use of force [*violenza*] or resistance.

SIMP. There can be no doubt but that one and the same body moving in a single medium has a fixed velocity which is determined by nature and which cannot be increased except by the addition of momentum [*impeto*] or diminished except by some resistance which retards it.

SALV. If then we take two bodies whose natural speeds are different, it is clear that on uniting the two, the more rapid one will be partly retarded by the slower, and the slower will be somewhat hastened by the swifter. Do you not agree with me in this opinion?

SIMP. You are unquestionably right.

SALV. But if this is true, and if a large stone moves with a speed of, say, eight while a smaller moves with a speed of four, then when they are united, the system will move with a speed less than eight; but the two stones when tied together make a stone larger than that which before moved with a speed of eight. Hence the heavier body moves with less speed than the lighter; an effect which is contrary to your supposition. Thus you see [108] how, from your assumption that the heavier body moves more rapidly than the lighter one, I infer that the heavier body moves more slowly.

SIMP. I am all at sea because it appears to me that the smaller stone when added to the larger increases its weight and by adding weight I do not see how it can fail to increase its speed or, at least, not to diminish it.

SALV. Here again you are in error, Simplicio, because it is not true that the smaller stone adds weight to the larger.

SIMP. This is, indeed, quite beyond my comprehension.

SALV. It will not be beyond you when I have once shown you the mistake under which you are laboring. Note that it is necessary to distinguish between heavy bodies in motion and the same bodies at rest. A large stone placed in a balance not only acquires additional weight by having another stone placed upon it, but even by the addition of a handful of hemp its weight is augmented {64} six to ten ounces according to the quantity of hemp. But if you tie the hemp to the stone and allow them to fall freely from some height, do you believe that the hemp will press down upon the stone and thus accelerate its motion or do you think the motion will be retarded by a partial upward pressure? One always feels the pressure upon his shoulders when he prevents the motion of a load resting upon him; but if one descends just as rapidly as the load would fall how can it gravitate or press upon him? Do you not see that this would be the same as trying to strike a man with a lance when he is running away from you with a speed which is equal to, or even greater, than that with which you are following him ? You must therefore conclude that, during free and natural fall, the small stone does not press upon the larger and consequently does not increase its weight as it does when at rest.

SIMP. But what if we should place the larger stone upon the smaller? [109]

SALV. Its weight would be increased if the larger stone moved more rapidly; but we have already concluded that when the small stone moves more slowly it retards to some extent the speed of the larger, so that the combination of the two, which is a heavier body than the larger of the two stones, would move less rapidly, a conclusion which is contrary to your hypothesis. We infer therefore that large and small bodies move with the same speed provided they are of the same specific gravity.

SIMP. Your discussion is really admirable; yet I do not find it easy to believe that a bird-shot falls as swiftly as a cannon ball.

SALV. Why not say a grain of sand as rapidly as a grindstone? But, Simplicio, I trust you will not follow the example of many others who divert the discussion from its main intent and fasten upon some statement of mine which lacks a hair's-breadth of the truth and, under this hair, hide the fault of another which is as big as a ship's cable. Aristotle says that "an iron ball of one hundred pounds falling from a height of one hundred cubits reaches the ground before a one-pound ball has fallen a single cubit." I say that they arrive at the same time. You find, on making {65} the experiment, that the larger outstrips the smaller by two finger-breadths, that is, when the larger has reached the ground, the other is short of it by two finger-breadths; now you would not hide behind these two fingers the ninety-nine cubits of Aristotle, nor would you mention my small error and at the same time pass over in silence his very large one. Aristotle declares that bodies of different weights, in the same medium, travel (in so far as their motion depends upon gravity) with speeds which are proportional to their weights; this he illustrates by use of bodies in which it is possible to perceive the pure and unadulterated effect of gravity, eliminating other considerations, for example, figure as being of small importance [*minimi momenti*], influences which are greatly dependent upon the medium which modifies the single effect of gravity alone. Thus we observe that gold, the densest of all substances, when beaten out into a very thin leaf, goes floating through the air; the same thing happens with stone when ground into a very fine powder. But if you wish to maintain the general proposition you will have to show that the same ratio of speeds is preserved in the [110] case of all heavy bodies, and that a stone of twenty pounds moves ten times as rapidly as one of two; but I claim that this is false and that, if they fall from a height of fifty or a hundred cubits, they will reach the earth at the same moment.

SIMP. Perhaps the result would be different if the fall took place not from a few cubits but from some thousands of cubits.

SALV. If this were what Aristotle meant you would burden him with another error which would amount to a falsehood; because, since there is no such sheer height available on earth, it is clear that Aristotle could not have made the experiment; yet he wishes to give us the impression of his having performed it when he speaks of such an effect as one which we see.

SIMP. In fact, Aristotle does not employ this principle, but uses the other one which is not, I believe, subject to these same difficulties.

SALV. But the one is as false as the other; and I am surprised that you yourself do not see the fallacy and that you do not perceive {66} that if it were true that, in media of different densities and different resistances, such as water and air, one and the same body moved in air more rapidly than in water, in proportion as the density of water is greater than that of air, then it would follow that any body which falls through air ought also to fall through water. But this conclusion is false inasmuch as many bodies which descend in air not only do not descend in water, but actually rise.

SIMP. I do not understand the necessity of your inference; and in addition I will say that Aristotle discusses only those bodies which fall in both media, not those which fall in air but rise in water,

SALV. The arguments which you advance for the Philosopher are such as he himself would have certainly avoided so as not to aggravate his first mistake. But tell me now whether the density [*corpulenza*] of the water, or whatever it may be that [111] retards the motion, bears a definite ratio to the density of air which is less retardative; and if so fix a value for it at your pleasure.

SIMP. Such a ratio does exist; let us assume it to be ten; then, for a body which falls in both these media, the speed in water will be ten times slower than in air.

SALV. I shall now take one of those bodies which fall in air but not in water, say a wooden ball, and I shall ask you to assign to it any speed you please for its descent through air.

SIMP. Let us suppose it moves with a speed of twenty.

SALV. Very well. Then it is clear that this speed bears to some smaller speed the same ratio as the density of water bears to that of air; and the value of this smaller speed is two. So that really if we follow exactly the assumption of Aristotle we ought to infer that the wooden ball which falls in air, a substance ten times less-resisting than water, with a speed of twenty would fall in water with a speed of two, instead of coming to the surface from the bottom as it does; unless perhaps you wish to reply, which I do not believe you will, that the rising of the wood through the water is the same as its falling with a speed of two. But {67} since the wooden ball does not go to the bottom, I think you will agree with me that we can find a ball of another material, not wood, which does fall in water with a speed of two.

SIMP. Undoubtedly we can; but it must be of a substance considerably heavier than wood.

SALV. That is it exactly. But if this second ball falls in water with a speed of two, what will be its speed of descent in air? If you hold to the rule of Aristotle you must reply that it will move at the rate of twenty; but twenty is the speed which you yourself have already assigned to the wooden ball; hence this and the other heavier ball will each move through air with the same speed. But now how does the Philosopher harmonize this result with his other, namely, that bodies of different weight move through the same medium with different speeds—speeds which are proportional to their weights? But without going into the matter more deeply, how have these common and [112] obvious properties escaped your notice? Have you not observed that two bodies which fall in water, one with a speed a hundred times as great as that of the other, will fall in air with speeds so nearly equal that one will not surpass the other by as much as one hundredth part? Thus, for example, an egg made of marble will descend in water one hundred times more rapidly than a hen's egg, while in air falling from a height of twenty cubits the one will fall short of the other by less than four finger-breadths. In short, a heavy body which sinks through ten cubits of water in three hours will traverse ten cubits of air in one or two pulse-beats; and if the heavy body be a ball of lead it will easily traverse the ten cubits of water in less than double the time required for ten cubits of air. And here, I am sure, Simplicio, you find no ground for difference or objection. We conclude, therefore, that the argument does not bear against the existence of a vacuum; but if it did, it would only do away with vacua of considerable size which neither I nor, in my opinion, the ancients ever believed to exist in nature, although they might possibly be produced by force [*violenza*] as may be gathered from various experiments whose description would here occupy too much time. {68}

SAGR. Seeing that Simplicio is silent, I will take the opportunity of saying something. Since you have clearly demonstrated that bodies of different weights do not move in one and the same medium with velocities proportional to their weights, but that they all move with the same speed, understanding of course that they are of the same substance or at least of the same specific gravity; certainly not of different specific gravities, for I hardly think you would have us believe a ball of cork moves [113] with the same speed as one of lead ; and again since you have clearly demonstrated that one and the same body moving through differently resisting media does not acquire speeds which are inversely proportional to the resistances, I am curious to learn what are the ratios actually observed in these cases.

SALV. These are interesting questions and I have thought much concerning them. I will give you the method of approach and the result which I finally reached. Having once established the falsity of the proposition that one and the same body moving through differently resisting media acquires speeds which are inversely proportional to the resistances of these media, and having also disproved the statement that in the same medium bodies of different weight acquire velocities proportional to their weights (understanding that this applies also to bodies which differ merely in specific gravity), I then began to combine these two facts and to consider what would happen if bodies of different weight were placed in media of different resistances; and I found that the differences in speed were greater in those media which were more resistant, that is, less yielding. This difference was such that two bodies which differed scarcely at all in their speed through air would, in water, fall the one with a speed ten times as great as that of the other. Further, there are bodies which will fall rapidly in air, whereas if placed in water not only will not sink but will remain at rest or will even rise to the top: for it is possible to find some kinds of wood, such as knots and roots, which remain at rest in water but fall rapidly in air.

SAGR. I have often tried with the utmost patience to add grains of sand to a ball of wax until it should acquire the same specific {69} gravity as water and would therefore remain at rest in this medium. But with all my care I was never able to accomplish this. Indeed, I do not know whether there is any solid substance whose specific gravity is, by nature, so nearly equal to that of water that if placed anywhere in water it will remain at rest.

SALV. I n this, as in a thousand other operations, men are surpassed by animals. In this problem of yours one may learn much from the fish which are very skillful in maintaining their equilibrium not only in one kind of water, but also in waters which are notably different either by their own nature or by [114] some accidental muddiness or through salinity, each of which produces a marked change. So perfectly indeed can fish keep their equilibrium that they are able to remain motionless in any position. This they accomplish, I believe, by means of an apparatus especially provided by nature, namely, a bladder located in the body and communicating with the mouth by means of a narrow tube through which they are able, at will, to expel a portion of the air contained in the bladder: by rising to the surface they can take in more air; thus they make themselves heavier or lighter than water at will and maintain equilibrium.

SAGR. By means of another device I was able to deceive some friends to whom I had boasted that I could make up a ball of wax that would be in equilibrium in water. In the bottom of a vessel I placed some salt water and upon this some fresh water; then I showed them that the ball stopped in the middle of the water, and that, when pushed to the bottom or lifted to the top, would not remain in either of these places but would return to the middle.

SALV. This experiment is not without usefulness. For when physicians are testing the various qualities of waters, especially their specific gravities, they employ a ball of this kind so adjusted that, in certain water, it will neither rise nor fall. Then in testing another water, differing ever so slightly in specific gravity [*peso*], the ball will sink if this water be lighter and rise if it be heavier. And so exact is this experiment that the addition {70} of two grains of salt to six pounds of water is sufficient to make the ball rise to the surface from the bottom to which it had fallen. To illustrate the precision of this experiment and also to clearly demonstrate the non-resistance of water to division, I wish to add that this notable difference in specific gravity can be produced not only by solution of some heavier substance, but also by merely heating or cooling; and so sensitive is water to this process that by simply adding four drops of another water which is slightly warmer or cooler than the six pounds one can cause the ball to sink or rise; it will sink when the warm water is poured in and will rise upon the addition of cold water. Now you [115] can see how mistaken are those philosophers who ascribe to water viscosity or some other coherence of parts which offers resistance to separation of parts and to penetration.

SAGR. With regard to this question I have found many convincing arguments in a treatise by our Academician; but there is one great difficulty of which I have not been able to rid myself, namely, if there be no tenacity or coherence between the particles of water how is it possible for those large drops of water to stand out in relief upon cabbage leaves without scattering or spreading out?

SALV. Although those who are in possession of the truth are able to solve all objections raised, I would not arrogate to myself such power; nevertheless my inability should not be allowed to becloud the truth. To begin with let me confess that I do not understand how these large globules of water stand out and hold themselves up, although I know for a certainty, that it is not owing to any internal tenacity acting between the particles of water; whence it must follow that the cause of this effect is external. Beside the experiments already shown to prove that the cause is not internal, I can offer another which is very convincing. If the particles of water which sustain themselves in a heap, while surrounded by air, did so in virtue of an internal cause then they would sustain themselves much more easily when surrounded by a medium in which they exhibit less tendency to fall than they do in air; such a medium would be any fluid heavier {71} than air, as, for instance, wine: and therefore if some wine be poured about such a drop of water, the wine might rise until the drop was entirely covered, without the particles of water, held together by this internal coherence, ever parting company. But this is not the fact; for as soon as the wine touches the water, the latter without waiting to be covered scatters and spreads out underneath the wine if it be red. The cause of this effect is therefore external and is possibly to be found in the surrounding air. Indeed there appears to be a considerable antagonism between air and water as I have observed in the following experiment. Having taken a glass globe which had a mouth of about the same diameter as a straw, I filled it with water and turned it mouth downwards; never-the-less, [116] the water, although quite heavy and prone to descend, and the air, which is very light and disposed to rise through the water, refused, the one to descend and the other to ascend through the opening, but both remained stubborn and defiant. On the other hand, as soon as I apply to this opening a glass of red wine, which is almost inappreciably lighter than water, red streaks are immediately observed to ascend slowly through the water while the water with equal slowness descends through the wine without mixing, until finally the globe is completely filled with wine and the water has all gone down into the vessel below. What then can we say except that there exists, between water and air, a certain incompatibility which I do not understand, but perhaps . . . .

SIMP. I feel almost like laughing at the great antipathy which Salviati exhibits against the use of the word antipathy; and yet it is excellently adapted to explain the difficulty.

SALV. Alright, if it please Simplicio, let this word antipathy be the solution of our difficulty. Returning from this digression, let us again take up our problem. We have already seen that the difference of speed between bodies of different specific gravities is most marked in those media which are the most resistant: thus, in a medium of quicksilver, gold not merely sinks to the bottom more rapidly than lead but it is the only substance {72} that will descend at all; all other metals and stones rise to the surface and float. On the other hand the variation of speed in air between balls of gold, lead, copper, porphyry, and other heavy materials is so slight that in a fall of 100 cubits a ball of gold would surely not outstrip one of copper by as much as four fingers. Having observed this I came to the conclusion that in a medium totally devoid of resistance all bodies would fall with the same speed.

SIMP. This is a remarkable statement, Salviati. But I shall never* believe that even in a vacuum, if motion in such a place were possible, a lock of wool and a bit of lead can fall with the same velocity.

SALV. A little more slowly, Simplicio. Your difficulty is not so recondite nor am I so imprudent as to warrant you in believing that I have not already considered this matter and found the proper solution. Hence for my justification and [117] for your enlightenment hear what I have to say. Our problem is to find out what happens to bodies of different weight moving in a medium devoid of resistance, so that the only difference in speed is that which arises from inequality of weight. Since no medium except one entirely free from air and other bodies, be it ever so tenuous and yielding, can furnish our senses with the evidence we are looking for, and since such a medium is not available, we shall observe what happens in the rarest and least resistant media as compared with what happens in denser and more resistant media. Because if we find as a fact that the variation of speed among bodies of different specific gravities is less and less according as the medium becomes more and more yielding, and if finally in a medium of extreme tenuity, though not a perfect vacuum, we find that, in spite of great diversity of specific gravity [*peso*], the difference in speed is very small and almost inappreciable, then we are justified in believing it highly probable that in a vacuum all bodies would fall with the same speed. Let us, in view of this, consider what takes place in air, where for the sake of a definite figure and light material imagine an inflated bladder. The air in this bladder when surrounded by air {73} will weigh little or nothing, since it can be only slightly compressed; its weight then is small being merely that of the skin which does not

amount to the thousandth part of a mass of lead having the same size as the inflated bladder. Now, Simplicio, if we allow these two bodies to fall from a height of four or six cubits, by what distance do you imagine the lead will anticipate the bladder? You may be sure
that the lead will not travel three times, or even twice, as swiftly as the bladder, although you would have made it move a thousand times as rapidly.

SIMP. It may be as you say during the first four or six cubits of the fall; but after the motion has continued a long while, I believe that the lead will have left the bladder behind not only six out of twelve parts of the distance but even eight or ten.

SALV. I quite agree with you and doubt not that, in very long distances, the lead might cover one hundred miles while [118] the bladder was traversing one; but, my dear Simplicio, this phenomenon which you adduce against my proposition is precisely the one which confirms it. Let me once more explain that the variation of speed observed in bodies of different specific gravities is not caused by the difference of specific gravity but depends upon external circumstances and, in particular, upon the resistance of the medium, so that if this is removed all bodies would fall with the same velocity; and this result I deduce mainly from the fact which you have just admitted and which is very true, namely, that, in the case of bodies which differ widely in weight, their velocities differ more and more as the spaces traversed increase, something which would not occur if the effect depended upon differences of specific gravity. For since these specific gravities remain constant, the ratio between the distances traversed ought to remain constant whereas the fact is that this ratio keeps on increasing as the motion continues. Thus a very heavy body in a fall of one cubit will not anticipate a very light one by so much as the tenth part of this space; but in a fall of twelve cubits the heavy body would out-strip {74} the other by one-third, and in a fall of one hundred cubits by 90/100, etc.

SIMP. Very well: but, following your own line of argument, if differences of weight in bodies of different specific gravities cannot produce a change in the ratio of their speeds, on the ground that their specific gravities do not change, how is it possible for the medium, which also we suppose to remain constant, to bring about any change in the ratio of these velocities ?

SALV. This objection with which you oppose my statement is clever; and I must meet it. I begin by saying that a heavy body has an inherent tendency to move with a constantly and uniformly accelerated motion toward the common center of gravity, that is, toward the center of our earth, so that during equal intervals of time it receives equal increments of momentum and velocity. This, you must understand, holds whenever all external and accidental hindrances have been removed; but of these there is one which we can never remove, namely, the medium which must be penetrated and thrust aside by the falling body. This quiet, yielding, fluid medium opposes motion, [119] through it with a resistance which is proportional to the rapidity with which the medium must give way to the passage of the body; which body, as I have said, is by nature continuously accelerated so that it meets with more and more resistance in the medium and hence a diminution in its rate of gain of speed until finally the speed reaches such a point and the resistance of the medium becomes so great that, balancing each other, they prevent any further acceleration and reduce the motion of the body to one which is uniform and which will thereafter maintain a constant value. There is, therefore, an increase in the resistance of the medium, not on account of any change in its essential properties, but on account of the change in rapidity with which it must yield and give way laterally to the passage of the falling body which is being constantly accelerated.

Now seeing how great is the resistance which the air offers to the slight momentum [*momenta*] of the bladder and how small that which it offers to the large weight [*peso*] of the lead, I am {75} convinced that, if the medium were entirely removed, the advantage received by the bladder would be so great and that coming to the lead so small that their speeds would be equalized. Assuming this principle, that all falling bodies acquire equal speeds in a medium which, on account of a vacuum or something else, offers no resistance to the speed of the motion, we shall be able accordingly to determine the ratios of the speeds of both similar and dissimilar bodies moving either through one and the same medium or through different space-filling, and therefore resistant, media. This result we may obtain by observing how much the weight of the medium detracts from the weight of the moving body, which weight is the means employed by the falling body to open a path for itself and to push aside the parts of the medium, something which does not happen in a vacuum where, therefore, no difference [of speed] is to be expected from a difference of specific gravity. And since it is known that the effect of the medium Is to diminish the weight of the body by the weight of the medium displaced, we may accomplish our purpose by diminishing in just this proportion the speeds of the falling bodies, which in a non-resisting medium we have assumed to be equal.

Thus, for example, imagine lead to be ten thousand times as heavy as air while ebony is only one thousand times as heavy.[120] Here we have two substances whose speeds of fall in a medium devoid of resistance are equal: but, when air is the medium, it will subtract from the speed of the lead one part in ten thousand, and from the speed of the ebony one part in one thousand, *i.e.* ten parts in ten thousand. While therefore lead and ebony would fall from any given height in the same interval of time, provided the retarding effect of the air were removed, the lead will, in air, lose in speed one part in ten thousand; and the ebony, ten parts in ten thousand. In other words, if the elevation from which the bodies start be divided into ten thousand parts, the lead will reach the ground leaving the ebony behind by as much as ten, or at least nine, of these parts. Is it not clear then that a leaden ball allowed to fall from a tower two hundred cubits high {76} will outstrip an ebony ball by less than four inches? Now ebony weighs a thousand times as much as air but this inflated bladder only four times as much; therefore air diminishes the inherent and natural speed of ebony by one part in a thousand; while that of the bladder which, if free from hindrance, would be the same, experiences a diminution in air amounting to one part in four. So that when the ebony ball, falling from the tower, has reached the earth, the bladder will have traversed only three-quarters of this distance. Lead is twelve times as heavy as water; but ivory is only twice as heavy. The speeds of these two substances which, when entirely unhindered, are equal will be diminished in water, that of lead by one part in twelve, that of ivory by half. Accordingly when the lead has fallen through eleven cubits of water the ivory will have fallen through only six. Employing this principle we shall, I believe, find a much closer agreement of experiment with our computation than with that of Aristotle.

In a similar manner we may find the ratio of the speeds of one and the same body in different fluid media, not by comparing the different resistances of the media, but by considering the excess of the specific gravity of the body above those of the media. Thus, for example, tin is one thousand times heavier than air and ten times heavier than water; hence, if we divide its unhindered speed into 1000 parts, air will rob it of one of these parts so that it will fall with a speed of 999, while in water its speed will be 900, seeing that water diminishes its weight by one part in ten while air by only one part in a thousand.

Again take a solid a little heavier than water, such as oak, a ball of which will weigh let us say 1000 drachms; suppose an [121] equal volume of water to weigh 950, and an equal volume of air, 2; then it is clear that if the unhindered speed of the ball is 1000, its speed in air will be 998, but in water only 50, seeing that the water removes 950 of the 1000 parts which the body weighs, leaving only 50.

Such a solid would therefore move almost twenty times as fast in air as in water, since its specific gravity exceeds that of water {77} by one part in twenty. And here we must consider the fact that only those substances which have a specific gravity greater than water can fall through it—substances which must, therefore, be hundreds of times heavier than air; hence when we try to obtain the ratio of the speed in air to that in water, we may, without appreciable error, assume that air does not, to any considerable extent, diminish the free weight [*assoluta gravita*], and consequently the unhindered speed [*assoluta velocita*] of such substances. Having thus easily found the excess of the weight of these substances over that of water, we can say that their speed in air is to their speed in water as their free weight [*totale gravita*} is to the excess of this weight over that of water. For example, a ball of ivory weighs 20 ounces; an equal volume of water weighs 17 ounces; hence the speed of ivory in air bears to its speed in water the approximate ratio of 20 : 3.

SAGR. I have made a great step forward in this truly interesting subject upon which I have long labored in vain. In order to put these theories into practice we need only discover a method of determining the specific gravity of air with reference to water and hence with reference to other heavy substances.

SIMP. But if we find that air has levity instead of gravity what then shall we say of the foregoing discussion which, in other respects, is very clever?

SALV. I should say that it was empty, vain, and trifling. But can you doubt that air has weight when you have the clear testimony of Aristotle affirming that all the elements have weight including air, and excepting only fire? As evidence of this he cites the fact that a leather bottle weighs more when inflated than when collapsed.[122]

SIMP. I am inclined to believe that the increase of weight observed in the inflated leather bottle or bladder arises, not from the gravity of the air, but from the many thick vapors mingled with it in' these lower regions. To this I would attribute the increase of weight in the leather bottle.

SALV. I would not have you say this, and much less attribute it to Aristotle; because, if speaking of the elements, he wished to persuade {78} me by experiment that air has weight and were to say to me : "Take a leather bottle, fill it with heavy vapors and observe how its weight increases," I would reply that the bottle would weigh still more if filled with bran; and would then add that this merely proves that bran and thick vapors are heavy, but in regard to air I should still remain in the same doubt as before. However, the experiment of Aristotle is good and the proposition is true. But I cannot say as much of a certain other consideration, taken at face value; this consideration was offered by a philosopher whose name slips me; but I know I have read his argument which is that air exhibits greater gravity than levity, because it carries heavy bodies downward more easily than it does light ones upward.

SAGR. Fine indeed! So according to this theory air is much heavier than water, since all heavy bodies are carried downward more easily through air than through water, and all light bodies buoyed up more easily through water than through air; further there is an infinite number of heavy bodies which fall through air but ascend in water and there is an infinite number of substances which rise in water and fall in air. But, Simplicio, the question as to whether the weight of the leather bottle is owing to thick vapors or to pure air does not affect our problem which is to discover how bodies move through this vapor-laden atmosphere of ours. Returning now to the question which interests me more, I should like, for the sake of more complete and thorough knowledge of this matter, not only to be strengthened in my belief that air has weight but also to learn, if possible, how great its specific gravity is. Therefore, Salviati, if you can satisfy my curiosity on this point pray do so.

SALV. The experiment with the inflated leather bottle of Aristotle proves conclusively that air possesses positive gravity and not, as some have believed, levity, a property possessed possibly by no substance whatever; for if air did possess this quality of absolute and positive levity, it should on compression [123] exhibit greater levity and, hence, a greater tendency to rise; but experiment shows precisely the opposite. {79}

As to the other question, namely, how to determine the specific gravity of air, I have employed the following method. I took a rather large glass bottle with a narrow neck and attached to it a leather cover, binding it tightly about the neck of the bottle: in the top of this cover I inserted and firmly fastened the valve of a leather bottle, through which I forced into the glass bottle, by means of a syringe, a large quantity of air. And since air is easily condensed one can pump into the bottle two or three times its own volume of air. After this I took an accurate balance and weighed this bottle of compressed air with the utmost precision, adjusting the weight with fine sand. I next opened the valve and allowed the compressed air to escape; then replaced the flask upon the balance and found it perceptibly lighter: from the sand which had been used as a counterweight I now removed and laid aside as much as was necessary to again secure balance. Under these conditions there can be no doubt but that the weight of the sand thus laid aside represents the weight of the air which had been forced into the flask and had afterwards escaped. But after all this experiment tells me merely that the weight of the compressed air is the same as that of the sand removed from the balance; when however it comes to knowing certainly and definitely the weight of air as compared with that of water or any other heavy substance this I cannot hope to do without first measuring the volume [*quantità*] of compressed air; for this measurement I have devised the two following methods.

According to the first method one takes a bottle with a narrow neck similar to the previous one; over the mouth of this bottle is slipped a leather tube which is bound tightly about the neck of the flask; the other end of this tube embraces the valve attached to the first flask and is tightly bound about it. This second flask is provided with a hole in the bottom through which an iron rod can be placed so as to open, at will, the valve above mentioned and thus permit the surplus air of the first to escape after it has once been weighed: but his second bottle must be filled with water. Having prepared everything in the manner [124] above {80} described, open the valve with the rod; the air will rush into the flask containing the water and will drive it through the hole at the bottom, it being clear that the volume [*quantità*] of water thus displaced is equal to the volume [*mole e quantità*] of air escaped from the other vessel. Having set aside this displaced water, weigh the vessel from which the air has escaped (which is supposed to have been weighed previously while containing the compressed air), and remove the surplus of sand as described above; It is then manifest that the weight of this sand is precisely the weight of a volume [*mole*] of air equal to the volume of water displaced and set aside; this water we can weigh and find how many times its weight contains the weight of the removed sand, thus determining definitely how many times heavier water is than air; and we shall find, contrary to the opinion of Aristotle, that this is not 10 times, but, as our experiment shows, more nearly 400 times.

The second method is more expeditious and can be carried out with a single vessel fitted up as the first was. Here no air is added to that which the vessel naturally contains but water is forced into it without allowing any air to escape; the water thus introduced necessarily compresses the air. Having forced into the vessel as much water as possible, filling it, say, three-fourths full, which does not require any extraordinary effort, place it upon the balance and weigh it accurately; next hold the vessel mouth up, open the valve, and allow the air to escape; the volume of the air thus escaping is precisely equal to the volume of water contained in the flask. Again weigh the vessel which will have diminished in weight on account of the escaped air; this loss in weight represents the weight of a volume of air equal to the volume of water contained in the vessel.

SIMP. No one can deny the cleverness and ingenuity of your devices; but while they appear to give complete intellectual satisfaction they confuse me in another direction. For since it is undoubtedly true that the elements when in their proper places have neither weight nor levity, I cannot understand how it is possible for that portion of air, which appeared to weigh, say, 4 drachms of sand, should really have such a weight in air as the sand {81} which counterbalances it. It seems to me, therefore, that the experiment should be carried out, not in air, but in a medium [125] in which the air could exhibit its property weight if such it really has.

SALV. The objection of Simplicio is certainly to the point and must therefore either be unanswerable or demand an equally clear solution. It is perfectly evident that that air which, under compression, weighed as much as the sand, loses this weight when once allowed to escape into its own element, while, indeed, the sand retains its weight. Hence for this experiment it becomes necessary to select a place where air as well as sand can gravitate; because, as has been often remarked, the medium diminishes the weight of any substance immersed in it by an amount equal to the weight of the displaced medium; so that air in air loses all its weight. If therefore this experiment is to be made with accuracy it should be performed in a vacuum where every heavy body exhibits its momentum without the slightest diminution. If then, Simplicio, we were to weigh a portion of air in a vacuum would you then be satisfied and assured of the fact?

SIMP. Yes truly: but this is to wish or ask the impossible.

SALV. Your obligation will then be very great if, for your sake, I accomplish the impossible. But I do not want to sell you something which I have already given you; for in the previous experiment we weighed the air in vacuum and not in air or other medium. The fact that any fluid medium diminishes the weight of a mass immersed in it, is due, Simplicio, to the resistance which this medium offers to its being opened up, driven aside, and finally lifted up. The evidence for this is seen in the readiness with which the fluid rushes to fill up any space formerly occupied by the mass; if the medium were not affected by such an immersion then it would not react against the immersed body. Tell me now, when you have a flask, in air, filled with its natural amount of air and then proceed to pump into the vessel more air, does this extra charge in any way separate or divide or change the circumambient air? Does the vessel perhaps expand so {82} that the surrounding medium is displaced in order to give more room? Certainly not. Therefore one is able to say that [126] this extra charge of air is not immersed in the surrounding medium for it occupies no space in it, but is, as it were, in a vacuum. Indeed, it is really in a vacuum; for it diffuses into the vacuities which are not completely filled by the original and uncondensed air. In fact I do not see any difference between the enclosed and the surrounding media: for the surrounding medium does not press upon the enclosed medium and, *vice versa*, the enclosed medium exerts no pressure against the surrounding one; this same relationship exists in the case of any matter in a vacuum, as well as in the case of the extra charge of air compressed into the flask. The weight of this condensed air is therefore the same as that which it would have if set free in a vacuum. It is true of course that the weight of the sand used as a counterpoise would be a little greater *in vacua* than in free air. We must, then, say that the air is slightly lighter than the sand required to counterbalance it, that is to say, by an amount equal to the weight *in vacua* of a volume of air equal to the volume of the sand.

At this point in an annotated copy of the original edition the following note by Galileo is found-.

[SAGR. A very clever discussion, solving a wonderful problem, because it demonstrates briefly and concisely the manner in which one may find the weight of a body *in vacuo* by simply weighing it in air. The explanation is as follows: when a heavy body is immersed in air it loses in weight an amount equal to the weight of a volume [*mole*] of air equivalent to the volume [*mole*] of the body itself. Hence if one adds to a body, without expanding
t, a quantity of air equal to that which it displaces and weighs it, he will obtain its absolute weight *in vacuo,* since, without increasing it in size, he has increased its weight by just the amount which it lost through immersion in air.

When therefore we force a quantity of water into a vessel which already contains its normal amount of air, without allowing any of this air to escape it is clear that this normal quantity of air will be compressed and condensed into a smaller space in order to make room for the water which is forced in: it is also clear that the volume of air thus compressed is equal to the volume of water added. If now the vessel be weighed {83} weighed in air in this condition, it is manifest that the weight of the water will be increased by that of an equal volume of air; the total weight of water and air thus obtained is equal to the weight of the water alone *in vacua.*

Now record the weight of the entire vessel and then allow the compressed air to escape; weigh the remainder; the difference of these two weights will be the weight of the compressed air which, in volume, is equal to that of the water. Next find the weight of the water alone and add to it that of the compressed air; we shall then have the water alone *in vacua.* To find the weight of the water we shall have to remove it from the vessel and weigh the vessel alone; subtract this weight from that of the vessel and water together. It is clear that the remainder will be the weight of the water alone in air.]

[127]

SIMP. The previous experiments, in my opinion, left something to be desired: but now I am fully satisfied.

SALV. The facts set forth by me up to this point and, in particular, the one which shows that difference of weight, even when very great, is without effect in changing the speed of falling bodies, so that as far as weight is concerned they all fall with equal speed: this idea is, I say, so new, and at first glance so remote from fact, that if we do not have the means of making it just as clear as sunlight, it had better not be mentioned; but having once allowed it to pass my lips I must neglect no experiment or argument to establish it.

SAGR. Not only this but also many other of your views are so far removed from the commonly accepted opinions and doctrines that if you were to publish them you would stir up a large number of antagonists; for human nature is such that men do not look with favor upon discoveries—either of truth or fallacy—in their own field, when made by others than themselves. They call him an innovator of doctrine, an unpleasant title, by which they hope to cut those knots which they cannot untie, and by subterranean mines they seek to destroy structures which patient artisans have built with customary tools. [128]

But as for ourselves who have no such thoughts, the experiments and arguments which you have thus far adduced are fully satisfactory; however if you have any experiments which are {84} more direct or any arguments which are more convincing we will hear them with pleasure.

SALV. The experiment made to ascertain whether two bodies, differing greatly in weight will fall from a given height with the same speed offers some difficulty; because, if the height is considerable, the retarding effect of the medium, which must be penetrated and thrust aside by the falling body, will be greater in the case of the small momentum of the very light body than in the case of the great force [*violenza*] of the heavy body; so that, in a long distance, the light body will be left behind; if the height be small, one may well doubt whether there is any difference; and if there be a difference it will be inappreciable.

It occurred to me therefore to repeat many times the fall through a small height in such a way that I might accumulate all those small intervals of time that elapse between the arrival of the heavy and light bodies respectively at their common terminus, so that this sum makes an interval of time which is not only observable, but easily observable. In order to employ the slowest speeds possible and thus reduce the change which the resisting medium produces upon the simple effect of gravity it occurred to me to allow the bodies to fall along a plane slightly inclined to the horizontal. For in such a plane, just as well as in a vertical plane, one may discover how bodies of different weight behave: and besides this, I also wished to rid myself of the resistance which might arise from contact of the moving body with the aforesaid inclined plane. Accordingly I took two balls, one of lead and one of cork, the former more than a hundred times heavier than the latter, and suspended them by means of two equal fine threads, each four or five cubits long. Pulling each ball aside from the perpendicular, I let them go at the same instant, and they, falling along the circumferences of circles having these equal strings for semi-diameters, passed beyond the perpendicular and returned along the same path. This free vibration [*per lor medesime le andate e le tornate*] repeated a hundred times showed clearly that the heavy body maintains so [129] nearly the period of the light body that neither in a hundred swings {85} nor even in a thousand will the former anticipate the latter by as much as a single moment [*minima momento*], so perfectly do they keep step. We can also observe the effect of the medium which, by the resistance which it offers to motion, diminishes the vibration of the cork more than that of the lead, but without altering the frequency of either; even when the arc traversed by the cork did not exceed five or six degrees while that of the lead was fifty or sixty, the swings were performed in equal times.

SIMP. If this be so, why is not the speed of the lead greater than that of the cork, seeing that the former traverses sixty degrees in the same interval in which the latter covers scarcely six?

SALV. But what would you say, Simplicio, if both covered their paths in the same time when the cork, drawn aside through thirty degrees, traverses an arc of sixty, while the lead pulled aside only two degrees traverses an arc of four? Would not then the cork be proportionately swifter? And yet such is the experimental fact. But observe this: having pulled aside the pendulum of lead, say through an arc of fifty degrees, and set it free, it swings beyond the perpendicular almost fifty degrees, thus describing an arc of nearly one hundred degrees; on the return swing it describes a little smaller arc; and after a large number of such vibrations it finally comes to rest. Each vibration, whether of ninety, fifty, twenty, ten, or four degrees occupies the same time: accordingly the speed of the moving body keeps on diminishing since in equal intervals of time, it traverses arcs which grow smaller and smaller.

Precisely the same things happen with the pendulum of cork, suspended by a string of equal length, except that a smaller number of vibrations is required to bring it to rest, since on account of its lightness it is less able to overcome the resistance of the air; nevertheless the vibrations, whether large or small, are all performed in time-intervals which are not only equal among themselves, but also equal to the period of the lead pendulum. Hence it is true that, if while the lead is traversing an arc of fifty degrees the cork covers one of only ten, the cork moves more slowly than the lead; but on the other hand it is also true that [130] {86} the cork may cover an arc of fifty while the lead passes over one of only ten or six; thus, at different times, we have now the cork, now the lead, moving more rapidly. But if these same bodies traverse equal arcs in equal times we may rest assured that their speeds are equal.

SIMP. I hesitate to admit the conclusiveness of this argument because of the confusion which arises from your making both bodies move now rapidly, now slowly and now very slowly, which leaves me in doubt as to whether their velocities are always equal.

SAGR. Allow me, if you please, Salviati, to say just a few words. Now tell me, Simplicio, whether you admit that one can say with certainty that the speeds of the cork and the lead are equal whenever both, starting from rest at the same moment and descending the same slopes, always traverse equal spaces in equal times?

SIMP. This can neither be doubted nor gainsaid.

SAGR. Now it happens, in the case of the pendulums, that each of them traverses now an arc of sixty degrees, now one of fifty, or thirty or ten or eight or four or two, etc.; and when they both swing through an arc of sixty degrees they do so in equal intervals of time; the same thing happens when the arc is fifty degrees or thirty or ten or any other number; and therefore we conclude that the speed of the lead in an arc of sixty degrees is equal to the speed of the cork when the latter also swings through an arc of sixty degrees; in the case of a fifty-degree arc these speeds are also equal to each other; so also in the case of other arcs. But this is not saying that the speed which occurs in an arc of sixty is the same as that which occurs in an arc of fifty; nor is the speed in an arc of fifty equal to that in one of thirty, etc.; but the smaller the arcs, the smaller the speeds; the fact observed is that one and the same moving body requires the same time for traversing a large arc of sixty degrees as for a small arc of fifty or even a very small arc of ten; all these arcs, indeed, are covered in the same interval of time. It is true therefore that the lead [131] {87} and the cork each diminish their speed [*moto*] in proportion as their arcs diminish; but this does not contradict the fact that they maintain equal speeds in equal arcs.

My reason for saying these things has been rather because I wanted to learn whether I had correctly understood Salviati, than because I thought Simplicio had any need of a clearer explanation than that given by Salviati which like everything else of his is extremely lucid, so lucid, indeed, that when he solves questions which are difficult not merely in appearance, but in reality and in fact, he does so with reasons, observations and experiments which are common and familiar to everyone.

In this manner he has, as I have learned from various sources, given occasion to a highly esteemed professor for undervaluing his discoveries on the ground that they are commonplace, and established upon a mean and vulgar basis; as if it were not a most admirable and praiseworthy feature of demonstrative science that it springs from and grows out of principles well-known, understood and conceded by all.

But let us continue with this light diet; and if Simplicio is satisfied to understand and admit that the gravity inherent [*interna gravità*] in various falling bodies has nothing to do with the difference of speed observed among them, and that all bodies, in so far as their speeds depend upon it, would move with the same velocity, pray tell us, Salviati, how you explain the appreciable and evident inequality of motion; please reply also to the objection urged by Simplicio—an objection in which I concur—namely, that a cannon ball falls more rapidly than a bird-shot. From my point of view, one might expect the difference of speed to be small in the case of bodies of the same substance moving through any single medium, whereas the larger ones will descend, during a single pulse-beat, a distance which the smaller ones will not traverse in an hour, or in four, or even in twenty hours; as for instance in the case of stones and fine sand and especially that very fine sand which produces muddy water and which in many hours will not fall through as much as two cubits, a distance which stones not much larger will traverse in a single pulse-beat. {88}

SALV. The action of the medium in producing a greater retardation upon those bodies which have a less specific gravity has already been explained by showing that they experience a diminution of weight. But to explain how one and the same [132] medium produces such different retardations in bodies which are made of the same material and have the same shape, but differ only in size, requires a discussion more clever than that by which one explains how a more expanded shape or an opposing motion of the medium retards the speed of the moving body. The solution of the present problem lies, I think, in the roughness and porosity which are generally and almost necessarily found in the surfaces of solid bodies. When the body is in motion these rough places strike the air or other ambient medium. The evidence for this is found in the humming which accompanies the rapid motion of a body through air, even when that body is as round as possible. One hears not only humming, but also hissing and whistling, whenever there is any appreciable cavity or elevation upon the body. We observe also that a round solid body rotating in a lathe produces a current of air. But what more do we need? When a top spins on the ground at its greatest speed do we not hear a distinct buzzing of high pitch? This sibilant note diminishes in pitch as the speed of rotation slackens, which is evidence that these small rugosities on the surface meet resistance in the air. There can be no doubt, therefore, that in the motion of falling bodies these rugosities strike the surrounding fluid and retard the speed; and this they do so much the more in proportion as the surface is larger, which is the case of small bodies as compared with greater.

SIMP. Stop a moment please, I am getting confused. For although I understand and admit that f riction of the medium upon the surface of the body retards its motion and that, if other things are the same, the larger surface suffers greater retardation, I do not see on what ground you say that the surface of the smaller body is larger. Besides if, as you say, the larger surface suffers greater retardation the larger solid should move more slowly, which is not the fact. But this objection can be easily met {89} by saying that, although the larger body has a larger surface, it has also a greater weight, in comparison with which the resistance of the larger surface is no more than the resistance of the small surface in comparison with its smaller weight; so that the speed of the larger solid does not become less. I therefore see no reason for expecting any difference of speed so long as the driving weight [*gravità movente*] diminishes in the same proportion [133] as the retarding power [*facoltà ritardante*] of the surface.

SALV. I shall answer all your objections at once. You will admit, of course, Simplicio, that if one takes two equal bodies, of the same material and same figure, bodies which would therefore fall with equal speeds, and if he diminishes the weight of one of them in the same proportion as its surface (maintaining the similarity of shape) he would not thereby diminish the speed of this body.

SIMP. This inference seems to be in harmony with your theory which states that the weight of a body has no effect in either accelerating or retarding its motion.

SALV. I quite agree with you in this opinion from which it appears to follow that, if the weight of a body is diminished in greater proportion than its surface, the motion is retarded to a certain extent; and this retardation is greater and greater in proportion as the diminution of weight exceeds that of the surface.

SIMP. This I admit without hesitation.

SALV. Now you must know, Simplicio, that it is not possible to diminish the surface of a solid body in the same ratio as the weight, and at the same time maintain similarity of figure. For since it is clear that in the case of a diminishing solid the weight grows less in proportion to the volume, and since the volume always diminishes more rapidly than the surface, when the same shape is maintained, the weight must therefore diminish more rapidly than the surface. But geometry teaches us that, in the case of similar solids, the ratio of two volumes is greater than the ratio of their surfaces; which, for the sake of better understanding, I shall illustrate by a particular case.{90}

Take, for example, a cube two inches on a side so that each face has an area of four square inches and the total area, *i.e.*, the sum of the six faces, amounts to twenty-four square inches; now imagine this cube to be sawed through three times so as to divide it into eight smaller cubes, each one inch on the side, each face one inch square, and the total surface of each cube six square inches instead of twenty-four as in the case of the larger cube. It is evident therefore that the surface of the little cube is only one-fourth that of the larger, namely, the ratio of six to twenty-four; but the volume of the solid cube itself is only one-eighth; the volume, and hence also the weight, diminishes therefore much more rapidly than the surface. If we again divide the little cube into eight others we shall have, for the total surface of one of these, one and one-half square inches, which is one-sixteenth of the surface of the original cube; but its volume is only one-sixty-fourth part. Thus, by two divisions, you see that the volume is diminished four times as much as the surface. And, if the subdivision be continued until the original solid be reduced to a fine powder, we shall find that the weight of one of these smallest particles has diminished hundreds and hundreds of times as much as its surface. And this which I have illustrated in the case of cubes holds also in the case of all similar solids, where the volumes stand in sesquialteral ratio to their surfaces. Observe then how much greater the resistance, arising from, contact of the surface of the moving body with the medium, in the case of small bodies than in the case of large; and when one considers that the rugosities on the very small surfaces of fine dust particles are perhaps no smaller than those on the surfaces of larger solids which have been carefully polished, he will see how important it is that the medium should be very fluid and offer no resistance to being thrust aside, easily yielding to a small force. You see, therefore, Simplicio, that I was not mistaken when, not long ago, I said that the surface of a small solid is comparatively greater than that of a large one.

SIMP. I am quite convinced; and, believe me, if I were again beginning my studies, I should follow the advice of Plato and {91} start with mathematics, a science which proceeds very cautiously and admits nothing as established until it has been rigidly demonstrated.

SAGR. This discussion has afforded me great pleasure; but before proceeding further I should like to hear the explanation of a phrase of yours which is new to me, namely, that similar solids are to each other in the sesquialteral ratio of their surfaces; for although I have seen and understood the proposition in which it is demonstrated that the surfaces of similar solids are in the duplicate ratio of their sides and also the proposition which proves that the volumes are in the triplicate ratio of their sides, yet I have not so much as heard mentioned the ratio of the volume of a solid to its surface.

SALV. You yourself have suggested the answer to your question and have removed every doubt. For if one quantity is the cube of something of which another quantity is the square does it not follow that the cube is the sesquialteral of the square? Surely. Now if the surface varies as the square of its linear dimensions while the volume varies as the cube of these dimensions may we not say that the volume stands in sesquialteral ratio to the surface?

SAGR. Quite so. And now although there are still some details, in connection with the subject under discussion, concerning which I might ask questions yet, if we keep making one digression after another, it will be long before we reach the main topic which has to do with the variety of properties found in the resistance which solid bodies offer to fracture; and, therefore, if you please, let us return to the subject which we originally proposed to discuss.

SALV. Very well; but the questions which we have already considered are so numerous and so varied, and have taken up so much time that there is not much of this day left to spend upon our main topic which abounds in geometrical demonstrations calling for careful consideration. May I, therefore, suggest that we postpone the meeting until tomorrow, not only for the reason just mentioned but also in order that I may bring with {92} me some papers in which I have set down in an orderly way the theorems and propositions dealing with the various phases of this subject, matters which, from memory alone, I could not present in the proper order.

SAGR. I fully concur in your opinion and all the more willingly because this will leave time to-day to take up some of my difficulties with the subject which we have just been discussing. One question is whether we are to consider the resistance of the medium as sufficient to destroy the acceleration of a body of very heavy material, very large volume, and spherical figure. I say *spherical* in order to select a volume which is contained within a minimum surface and therefore less subject to retardation.

Another question deals with the vibrations of pendulums which may be regarded from several viewpoints; the first is whether all vibrations, large, medium, and small, are performed in exactly and precisely equal times : another is to find the ratio of the times of vibration of pendulums supported by threads of unequal length.

SALV. These are interesting questions : but I fear that here, as in the case of all other facts, if we take up for discussion any one of them, it will carry in its wake so many other facts and curious consequences that time will not remain to-day for the discussion of all.

SAGR. If these are as full of interest as the foregoing, I would gladly spend as many days as there remain hours between now and nightfall; and I dare say that Simplicio would not be wearied by these discussions.

SIMP. Certainly not; especially when the questions pertain to natural science and have not been treated by other philosophers.

SALV. Now taking up the first question, I can assert without hesitation that there is no sphere so large, or composed of material so dense but that the resistance of the medium, although very slight, would check its acceleration and would, in time reduce its motion to uniformity; a statement which is strongly {93} supported by experiment. for if a falling body, as time goes on, were to acquire a speed as great as you please, no such speed, impressed by external forces [*motore esterno*], can be so great but that the body will first acquire it and then, owing to the resisting medium, lose it. Thus, for instance, if a cannon ball, having fallen a distance of four cubits through the air and having acquired a speed of, say, ten units [*gradi*] were to strike the surface of the water, and if the resistance of the water were not able to check the momentum [*impeto*] of the shot, it would either increase in speed or maintain a uniform motion until the bottom were reached: but such is not the observed fact; on the contrary, the water when only a few cubits deep hinders and diminishes the motion in such a way that the shot delivers to the bed of the river or lake a very slight impulse. Clearly then if a short fall through the water is sufficient to deprive a cannon ball of its speed, this speed cannot be regained by a fall of even a thousand cubits. How could a body acquire, in a fall of a thousand cubits, that which it loses in a fall of four? But what more is needed? Do we not observe that the enormous momentum, delivered to a shot by a cannon, is so deadened by passing through a few cubits of water that the ball, so far from injuring the ship, barely strikes it? Even the air, although a very yielding medium, can also diminish the speed of a falling body, as may be easily understood from similar experiments. For if a gun be fired downwards from the top of a very high tower the shot will make a smaller impression upon the ground than if the gun had been fired from an elevation of only four or six cubits; this is clear evidence that the momentum of the ball, fired from the top of the tower, diminishes continually from the instant it leaves the barrel until it reaches the ground. Therefore a fall from ever so great an altitude will not suffice to give to a body that momentum which it has once lost through the resistance of the air, no matter how it was originally acquired. In like manner, the destructive effect produced upon a wall by a shot fired from a gun at a distance of twenty cubits cannot be duplicated by the fall of the same shot from any altitude however {94} ever great. My opinion is, therefore, that under the circumstances which occur in nature, the acceleration of any body falling from rest reaches an end and that the resistance of the medium finally reduces its speed to a constant value which is thereafter maintained.

SAGR. These experiments are in my opinion much to the purpose; the only question is whether an opponent might not make bold to deny the fact in the case of bodies [*moli*] which are very large and heavy or to assert that a cannon ball, falling from the distance of the moon or from the upper regions of the atmosphere, would deliver a heavier blow than if just leaving the muzzle of the gun.

SALV. No doubt many objections may be raised not all of which can be refuted by experiment: however in this particular case the following consideration must be taken into account, namely, that it is very likely that a heavy body falling from a height will, on reaching the ground, have acquired just as much momentum as was necessary to carry it to that height; as may be clearly seen in the case of a rather heavy pendulum which, when pulled aside fifty or sixty degrees from the vertical, will acquire precisely that speed and force which are sufficient to carry it to an equal elevation save only that small portion which it loses through friction on the air. In order to place a cannon ball at such a height as might suffice to give it just that momentum which the powder imparted to it on leaving the gun we need only fire it vertically upwards from the same gun; and we can then observe whether on falling back it delivers a blow equal to that of the gun fired at close range; in my opinion it would be much weaker. The resistance of the air would, therefore, I think, prevent the muzzle velocity from being equalled by a natural fall from rest at any height whatsoever.

We come now to the other questions, relating to pendulums, a subject which may appear to many exceedingly arid, especially to those philosophers who are continually occupied with the more profound questions of nature. Nevertheless, the problem is one which I do not scorn. I am encouraged by the {95} example of Aristotle whom I admire especially because he did not fail to discuss every subject which he thought in any degree worthy of consideration.

Impelled by your queries I may give you some of my ideas concerning certain problems in music, a splendid subject, upon which so many eminent men have written: among these is Aristotle himself who has discussed numerous interesting acoustical questions. Accordingly, if on the basis of some easy and tangible experiments, I shall explain some striking phenomena in the domain of sound, I trust my explanations will meet your approval.

SAGR. I shall receive them not only gratefully but eagerly. For, although I take pleasure in every kind of musical instrument and have paid considerable attention to harmony, I have never been able to fully understand why some combinations of tones are more pleasing than others, or why certain combinations not only fail to please but are even highly offensive. Then there is the old problem of two stretched strings in unison; when one of them is sounded, the other begins to vibrate and to emit its note; nor do I understand the different ratios of harmony [*forme delle consonanze*] and some other details.

SALV. Let us see whether we cannot derive from the pendulum a satisfactory solution of all these difficulties. And first, as to the question whether one and the same pendulum really performs its vibrations, large, medium, and small, all in exactly the same time, I shall rely upon what I have already heard from our Academician. He has clearly shown that the time of descent is the same along all chords, whatever the arcs which subtend them, as well along an arc of 180° (*i.e.*, the whole diameter) as along one of 100°, 60°, 10°, 2°, ½°, or 4'. It is understood, of course, that these arcs all terminate at the lowest point of the circle, where it touches the horizontal plane.

If now we consider descent along arcs instead of their chords then, provided these do not exceed 90°, experiment shows that they are all traversed in equal times; but these times are greater for the chord than for the arc, an effect which is all the more {96} remarkable because at first glance one would think just the opposite to be true. For since the terminal points of the two motions are the same and since the straight line included between these two points is the shortest distance between them, it would seem reasonable that motion along this line should be executed in the shortest time; but this is not the case, for the shortest time—and therefore the most rapid motion—is that employed along the arc of which this straight line is the chord.

As to the times of vibration of bodies suspended by threads of different lengths, they bear to each other the same proportion as the square roots of the lengths of the thread; or one might say the lengths are to each other as the squares of the times; so that if one wishes to make the vibration-time of one pendulum twice that of another, he must make its suspension four times as long. In like manner, if one pendulum has a suspension nine times as long as another, this second pendulum will execute three vibrations during each one of the first; from which it follows that the lengths of the suspending cords bear to each other the [inverse] ratio of the squares of the number of vibrations performed in the same time.

SAGR. Then, if I understand you correctly, I can easily measure the length of a string whose upper end is attached at any height whatever even if this end were invisible and I could see only the lower extremity. For if I attach to the lower end of this string a rather heavy weight and give it a to-and-fro motion, and if I ask a friend to count a number of its vibrations, while I, during the same time-interval, count the number of vibrations of a pendulum which is exactly one cubit in length, then knowing the number of vibrations which each pendulum makes in the given interval of time one can determine the length of the string. Suppose, for example, that my friend counts 20 vibrations of the long cord during the same time in which I count 240 of my string which is one cubit in length; taking the squares of the two numbers, 20 and 240, namely 400 and 57600, then, I say, the long string contains 57600 units of such length that my pendulum will contain 400 of them; and since the length of {97} my string is one cubit, I shall divide 57600 by 400 and thus obtain 144. Accordingly I shall call the length of the string 144 cubits.

SALV. Nor will you miss it by as much as a hand's breadth, especially if you observe a large number of vibrations.

SAGR. You give me frequent occasion to admire the wealth and profusion of nature when, from such common and even trivial phenomena, you derive facts which are not only striking and new but which are often far removed from what we would have imagined. Thousands of times I have observed vibrations especially in churches where lamps, suspended by long cords, had been inadvertently set into motion; but the most which I could infer from these observations was that the view of those who think that such vibrations are maintained by the medium is, highly improbable: for, in that case, the air must needs have considerable judgment and little else to do but kill time by pushing to and fro a pendent weight with perfect regularity. But I never dreamed of learning that one and the same body, when suspended from a string a hundred cubits long and pulled aside through an arc of 90° or even 1° or $\frac{1}{2}$°, would employ the same time in passing through the least as through the largest of these arcs; and, indeed, it still strikes me as somewhat unlikely. Now I am waiting to hear how these same simple phenomena can furnish solutions for those acoustical problems—solutions which will be at least partly satisfactory.

SALV. First of all one must observe that each pendulum has its own time of vibration so definite and determinate that it is not possible to make it move with any other period [*altro periodo*] than that which nature has given it. For let any one take in his hand the cord to which the weight is attached and try, as much as he pleases, to increase or diminish the frequency [*frequenza*] of its vibrations; it will be time wasted. On the other hand, one can confer motion upon even a heavy pendulum which is at rest by simply blowing against it; by repeating these blasts with a frequency which is the same as that of the pendulum one can impart considerable motion. Suppose that by the {98} first puff we have displaced the pendulum from the vertical by, say, half an inch; then if, after the pendulum has returned and is about to begin the second vibration, we add a second puff, we shall impart additional motion; and so on with other blasts provided they are applied at the right instant, and not when the pendulum is coming toward us since in this case the blast would impede rather than aid the motion. Continuing thus with many impulses [*impulsi*] we impart to the pendulum such momentum [*impeto*] that a greater impulse [*forza*] than that of a single blast will be needed to stop it.

SAGR. Even as a boy, I observed that one man alone by giving these impulses at the right instant was able to ring a bell so large that when four, or even six, men seized the rope and tried to stop it they were lifted from the ground, all of them together being unable to counterbalance the momentum which a single man, by properly-timed pulls, had given it.

SALV. Your illustration makes my meaning clear and is quite as well fitted, as what I have just said, to explain the wonderful phenomenon of the strings of the cittern [*cetera*] or of the spinet [*cimbalo*], namely, the fact that a vibrating string will set another string in motion and cause it to sound not only when the latter is in unison but even when it differs from the former by an octave or a fifth. A string which has been struck begins to vibrate and continues the motion as long as one hears the sound [*risonanza*] ; these vibrations cause the immediately surrounding air to vibrate and quiver; then these ripples in the air expand far into space and strike not only all the strings of the same instrument but even those of neighboring instruments. Since that string which is tuned to unison with the one plucked is capable of vibrating with the same frequency, it acquires, at the first impulse, a slight oscillation; after receiving two, three, twenty, or more impulses, delivered at proper intervals, it finally accumulates a vibratory motion equal to that of the plucked string, as is clearly shown by equality of amplitude in their vibrations. This undulation expands through the air and sets into vibration not only strings, but also any other body {99} which happens to have the same period as that of the plucked string. Accordingly if we attach to the side of an instrument small pieces of bristle or other flexible bodies, we shall observe that, when a spinet is sounded, only those pieces respond that have the same period as the string which has been struck; the remaining pieces do not vibrate in response to this string, nor do the former pieces respond to any other tone.

If one bows the base string on a viola rather smartly and brings near it a goblet of fine, thin glass having the same tone [*tuono*] as that of the string, this goblet will vibrate and audibly resound. That the undulations of the medium are widely dispersed about the sounding body is evinced by the fact that a glass of water may be made to emit a tone merely by the friction of the finger-tip upon the rim of the glass; for in this water is produced a series of regular waves. The same phenomenon is observed to better advantage by fixing the base of the goblet upon the bottom of a rather large vessel of water filled nearly to the edge of the goblet; for if, as before, we sound the glass by friction of the finger, we shall see ripples spreading with the utmost regularity and with high speed to large distances about the glass. I have often remarked, in thus sounding a rather large glass nearly full of water, that at first the waves are spaced with great uniformity, and when, as sometimes happens, the tone of the glass jumps an octave higher I have noted that at this moment each of the aforesaid waves divides into two; a phenomenon which shows clearly that the ratio involved in the octave [*forma dell' ottava*] is two.

SAGR. More than once have I observed this same thing, much to my delight and also to my profit. For a long time I have been perplexed about these different harmonies since the explanations hitherto given by those learned in music impress me as not sufficiently conclusive. They tell us that the diapason, *i.e.* the octave, involves the ratio of two, that the diapente which we call the fifth involves a ratio of 3 : 2, etc.; because if the open string of a monochord be sounded and afterwards a bridge be placed in the middle and the half length be sounded {100} one hears the octave ; and if the bridge be placed at 1/3 the length of the string, then on plucking first the open string and afterwards 2/3 of its length the fifth is given; for this reason they say that the octave depends upon the ratio of two to one [*contenuta tra'l due e l'uno*] and the fifth upon the ratio of three to two. This explanation does not impress me as sufficient to establish 2 and 3/2 as the natural ratios of the octave and the fifth; and my reason for thinking so is as follows. There are three different ways in which the tone of a string may be sharpened, namely, by shortening it, by stretching it and by making it thinner. If the tension and size of the string remain constant one obtains the octave by shortening it to one-half, *i.e.*, by sounding first the open string and then one-half of it; but if length and size remain constant and one attempts to produce the octave by stretching he will find that it does not suffice to double the stretching weight; it must be quadrupled; so that, if the fundamental note is produced by a weight of one pound, four will be required to bring out the octave.

And finally if the length and tension remain constant, while one changes the size * of the string he will find that in order to produce the octave the size must be reduced to ¼ that which gave the fundamental. And what I have said concerning the octave, namely, that its ratio as derived from the tension and size of the string is the square of that derived from the length, applies equally well to all other musical intervals [*intervalli musici*]. Thus if one wishes to produce a fifth by changing the length he finds that the ratio of the lengths must be sesquialteral, in other words he sounds first the open string, then two-thirds of it; but if he wishes to produce this same result by stretching or thinning the string then it becomes necessary to square the ratio 3/2 that is by taking 9/4 [*dupla sesquiquarta*]; accordingly, if the fundamental requires a weight of 4 pounds, the higher note will be produced not by 6, but by 9 pounds; the same is true in regard to size, the string which gives the fundamental is larger than that which yields the fifth in the ratio of 9 to 4. In view of these facts, I see no reason why those wise {101} philosophers should adopt 2 rather than 4 as the ratio of the octave, or why in the case of the fifth they should employ the sesquialteral ratio, 3/2, rather than that of 9/4. Since it is impossible to count the vibrations of a sounding string on account of its high frequency, I should still have been in doubt as to whether a string, emitting the upper octave, made twice as many vibrations in the same time as one giving the fundamental, had it not been for the following fact, namely, that at the instant when the tone jumps to the octave, the waves which constantly accompany the vibrating glass divide up into smaller ones which are precisely half as long as the former.

SALV. This is a beautiful experiment enabling us to distinguish individually the waves which are produced by the vibrations of a sonorous body, which spread through the air, bringing to the tympanum of the ear a stimulus which the mind translates into sound. But since these waves in the water last only so long as the friction of the finger continues and are, even then, not constant but are always forming and disappearing, would it not be a fine thing if one had the ability to produce waves which would persist for a long while, even months and years, so as to easily measure and count them?

SAGR. Such an invention would, I assure you, command my admiration.

*For the exact meaning of "size" see p. 103 below. [*Trans.*]

SALV. The device is one which I hit upon by accident; my part consists merely in the observation of it and in the appreciation of its value as a confirmation of something to which I had given profound consideration; and yet the device is, in itself, rather common. As I was scraping a brass plate with a sharp iron chisel in order to remove some spots from it and was running the chisel rather rapidly over it, I once or twice, during many strokes, heard the plate emit a rather strong and clear whistling sound; on looking at the plate more carefully, I noticed a long row of fine streaks parallel and equidistant from one another. Scraping with the chisel over and over again, I noticed that it was only when the plate emitted this hissing noise that any marks were left upon it; when the scraping was not accompanied by {102} this sibilant note there was not the least trace of such marks. Repeating the trick several times and making the stroke, now with greater now with less speed, the whistling followed with a pitch which was correspondingly higher and lower. I noted also that the marks made when the tones were higher were closer together; but when the tones were deeper, they were farther apart. I also observed that when, during a single stroke, the speed increased toward the end the sound became sharper and the streaks grew closer together, but always in such a way as to remain sharply defined and equidistant. Besides whenever the stroke was accompanied by hissing I felt the chisel tremble in my grasp and a sort of shiver run through my hand. In short we see and hear in the case of the chisel precisely that which is seen and heard in the case of a whisper followed by a loud voice; for, when the breath is emitted without the production of a tone, one does not feel either in the throat or mouth any motion to speak of in comparison with that which is felt in the larynx and upper part of the throat when the voice is used, especially when the tones employed are low and strong.

At times I have also observed among the strings of the spinet two which were in unison with two of the tones produced by the aforesaid scraping; and among those which differed most in pitch I found two which were separated by an interval of a perfect fifth. Upon measuring the distance between the markings produced by the two scrapings it was found that the space which contained 45 of one contained 30 of the other, which is precisely the ratio assigned to the fifth.

But now before proceeding any farther I want to call your attention to the fact that, of the three methods for sharpening a tone, the one which you refer to as the fineness of the string should be attributed to its weight. So long as the material of the string is unchanged, the size and weight vary in the same ratio. Thus in the case of gut-strings, we obtain the octave by making one string 4 times as large as the other; so also in the case of brass one wire must have 4 times the size of the other; but if now we wish to obtain the octave of a gut-string, by use of {103} brass wire, we must make it, not four times as large, but four times as heavy as the gut-string: as regards size therefore the metal string is not four times as big but four times as heavy. The wire may therefore be even thinner than the gut notwithstanding the fact that the latter gives the higher note. Hence if two spinets are strung, one with gold wire the other with brass, and if the corresponding strings each have the same length, diameter, and tension it follows that the instrument strung with gold will have a pitch about one-fifth lower than the other because gold has a density almost twice that of brass. And here it is to be noted that it is the weight rather than the size of a moving body which offers resistance to change of motion [*velocità del moto*] contrary to what one might at first glance think. For it seems reasonable to believe that a body which is large and light should suffer greater retardation of motion in thrusting aside the medium than would one which is thin and heavy; yet here exactly the opposite is true.

Returning now to the original subject of discussion, I assert that the ratio of a musical interval is not immediately determined either by the length, size, or tension of the strings but rather by the ratio of their frequencies, that is, by the number of pulses of air waves which strike the tympanum of the ear, causing it also to vibrate with the same frequency. This fact established, we may possibly explain why certain pairs of notes, differing in pitch produce a pleasing sensation, others a less pleasant effect, and still others a disagreeable sensation. Such an explanation would be tantamount to an explanation of the more or less perfect consonances and of dissonances. The unpleasant sensation produced by the latter arises, I think, from the discordant vibrations of two different tones which strike the ear out of time [*sproporzionatamente*]. Especially harsh is the dissonance between notes whose frequencies are incom-mensurable; such a case occurs when one has two strings in unison and sounds one of them open, together with a part of the other which bears the same ratio to its whole length as the side of a square bears to the diagonal; this yields a dissonance similar {104} to the augmented fourth or diminished fifth [*tritono o semi-diapente*].

Agreeable consonances are pairs of tones which strike the ear with a certain regularity; this regularity consists in the fact that the pulses delivered by the two tones, in the same interval of time, shall be commensurable in number, so as not to keep the ear drum in perpetual torment, bending in two different directions in order to yield to the ever-discordant impulses.

The first and most pleasing consonance is, therefore, the octave since, for every pulse given to the tympanum by the lower string, the sharp string delivers two; accordingly at every other vibration of the upper string both pulses are delivered simultaneously so that one-half the entire number of pulses are delivered in unison. But when two strings are in unison their vibrations always coincide and the effect is that of a single string; hence we do not refer to it as consonance. The fifth is also a pleasing interval since for every two vibrations of the lower string the upper one gives three, so that considering the entire number of pulses from the upper string one-third of them will strike in unison, *i.e.*, between each pair of concordant vibrations there intervene two single vibrations; and when the interval is a fourth, three single vibrations intervene. In case the interval is a second where the ratio is 9/8 it is only every ninth vibration of the upper string which reaches the ear simultaneously with one of the lower; all the others are discordant and produce a harsh effect upon the recipient ear which interprets them as dissonances.

SIMP. Won't you be good enough to explain this argument a little more clearly?

SALV. Let AB denote the length of a wave [*lo spazio e la dilatazione d'una vibrazione*] emitted by the lower string and CD that of a higher string which is emitting the octave of AB; divide AB in the middle at E. If the two strings begin their motions at A and C, it is clear that when the sharp vibration has reached the end D, the other vibration will have travelled only as far as E, which, not being a terminal point, will emit no pulse; but there is a blow delivered at D. Accordingly when the one {105} wave comes back from D to C, the other passes on from E to B; hence the two pulses from B and C strike the drum of the ear simultaneously. Seeing that these vibrations are repeated again and again in the same manner, we conclude that each alternate pulse from CD falls in unison with one from AB. But each of the pulsations at the terminal points, A and B, is constantly accompanied by one which leaves always from C or always from D. This is clear because if we suppose the waves to reach A and *C* at the same instant, then, while one wave Fig. 13 travels from A to B, the other will proceed from C to D and back to C, so that waves strike at C and B simultaneously; during the passage of the wave from B back to A the disturbance at C goes to D and again returns to C, so that once more the pulses at A and C are simultaneous.

Next let the vibrations AB and CD be separated by an interval of a fifth, that is, by a ratio of 3/2; choose the points E and O such that they will divide the wave length of the lower string into three equal parts and imagine the vibrations to start at the same instant from each of the terminals A and C. It is evident that when the pulse has been delivered at the terminal D, the wave in AB has travelled only as far as O; the drum of the ear receives, therefore, only the pulse from D. Then during the return of the one vibration from D to C, the other will pass from O to B and then back to O, producing an isolated pulse at B—a pulse which is out of time but one which must be taken into consideration.

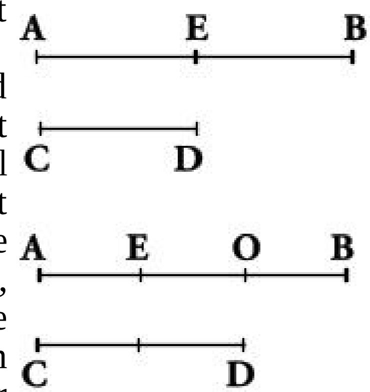

Fig. 13

Now since we have assumed that the first pulsations started from the terminals A and C at the same instant, it follows that the second pulsation, isolated at D, occurred after an interval of time equal to that required for passage from C to D or, what is the same thing, from A to O; but the next pulsation, the one at B, is separated from the preceding by only half this interval, namely, the time required for passage from O to B. Next while the one vibration travels from O to A, the other travels from C to {106} D, the result of which is that two pulsations occur simultaneously at A and D. Cycles of this kind follow one after another, *i.e.*, one solitary pulse of the lower string interposed between two solitary pulses of the upper string. Let us now imagine time to be divided into very small equal intervals; then if we assume that, during the first two of these intervals, the disturbances which occurred simultaneously at A and C have travelled as far as O and D and have produced a pulse at D; and if we assume that during the third and fourth intervals one disturbance returns from D to C, producing a pulse at C, while the other, passing on from O to B and back to O, produces a pulse at B; and if finally, during the fifth and sixth intervals, the disturbances travel from O and C to A and D, producing a pulse at each of the latter two, then the sequence in which the pulses strike the ear will be such that, if we begin to count time from any instant where two pulses are simultaneous, the ear drum will, after the lapse of two of the said intervals, receive a solitary pulse; at the end of the third interval, another solitary pulse; so also at the end of the fourth interval; and two intervals later, *i.e.*, at the end of the sixth interval, will be heard two pulses in unison. Here ends the cycle—the anomaly, so to speak—which repeats itself over and over again.

SAGR. I can no longer remain silent; for I must express to you the great pleasure I have in hearing such a complete explanation of phenomena with regard to which I have so long been in darkness. Now I understand why unison does not differ from a single tone; I understand why the octave is the principal harmony, but so like unison as often to be mistaken for it and also why it occurs with the other harmonies. It resembles unison because the pulsations of strings in unison always occur simultaneously, and those of the lower string of the octave are always accompanied by those of the upper string; and among the latter is interposed a solitary pulse at equal intervals and in such a manner as to produce no disturbance; the result is that such a harmony is rather too much softened and lacks fire. But the fifth is characterized by its displaced beats and by the interposition {107} of two solitary beats of the upper string and one solitary beat of the lower string between each pair of simultaneous pulses; these three solitary pulses are separated by intervals of time equal to half the interval which separates each pair of simultaneous beats from the solitary beats of the upper string. Thus the effect of the fifth is to produce a tickling of the ear drum such that its softness is modified with sprightliness, giving at the same moment the impression of a gentle kiss and of a bite.

SALV. Seeing that you have derived so much pleasure from these novelties, I must show you a method by which the eye may enjoy the same game as the ear. Suspend three balls of lead, or other heavy material, by means of strings of different length such that while the longest makes two vibrations the shortest will make four and the medium three; this will take place when the longest string measures 16, either in hand breadths or in any other unit, the medium 9 and the shortest 4, all measured in the same unit.

Now pull all these pendulums aside from the perpendicular and release them at the same instant; you will see a curious interplay of the threads passing each other in various manners but such that at the completion of every fourth vibration of the longest pendulum, all three will arrive simultaneously at the same terminus, whence they start over again to repeat the same cycle. This combination of vibrations, when produced on strings is precisely that which yields the interval of the octave and the intermediate fifth. If we employ the same disposition of apparatus but change the lengths of the threads, always however in such a way that their vibrations correspond to those of agreeable musical intervals, we shall see a different crossing of these threads but always such that, after a definite interval of time and after a definite number of vibrations, all the threads, whether three or four, will reach the same terminus at the same instant, and then begin a repetition of the cycle.

If however the vibrations of two or more strings are incommensurable so that they never complete a definite number of vibrations at the same instant, or if commensurable they return only {108} after a long interval of time and after a large number of vibrations, then the eye is confused by the disorderly succession of crossed threads. In like manner the ear is pained by an irregular sequence of air waves which strike the tympanum without any fixed order.

But, gentlemen, whither have we drifted during these many hours lured on by various problems and unexpected digressions ? The day is already ended and we have scarcely touched the subject proposed for discussion. Indeed we have deviated so far that I remember only with difficulty our early introduction and the little progress made in the way of hypotheses and principles for use in later demonstrations.

SAGR. Let us then adjourn for to-day in order that our minds may find refreshment in sleep and that we may return tomorrow, if so please you, and resume the discussion of the main question.

SALV. I shall not fail to be here to-morrow at the same hour, hoping not only to render you service but also to enjoy your company.

END OF THE FIRST DAY.

Galileo Galilei, *Dialogues Concerning Two New Sciences*, translated by Henry Crew & Alfonso de Salvio with an introduction by Antonio Favaro, Dover Publications, Inc., New York, 1954 (INTRODUCTION). Originally published in 1904 by the MacMillan company.

INTRODUCTION FIRST DAY SECOND DAY THIRD DAY FOURTH DAY

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THE ADDED (OR "FIFTH" DAY) BY STILLMAN DRAKE (1974)

Galileo Galilei, *Discourses and Mathematical Demonstrations Concerning Two New Sciences Pertaining to Mechanics and Local Motions.* Translated by Stillman Drake, University of Wisconsin Press, Madison, 1974: 281–303. (ADDED DAY)