Metaphysics and Measurement

Alexandre Koyré

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FOREWORD

In 1939 there appeared in the series 'Actualités scientifiques et industrielles' three slim volumes of *Études galiléennes* by Alexandre Koyré, who was then distinguished mainly as an historian of philosophy. In these Professor Koyré examined Galileo's role in the Scientific Revolution with the eyes of a philosopher. He was not the first to do this, but his *Études* argued for the dominating role of ideas over experience in Galileo's scientific thought with a penetration and a singlemindedness that were to have the most profound influence on the next generation of historians of science.

As it happened, the impact of the *Études* themselves was postponed: these "ill-fated" volumes, as Koyré described them, were temporarily victims of the German invasion of France. Koyré himself distilled something of their essence into two essays, "Galileo and the Scientific Revolution of the Seventeenth Century" and "Galileo and Plato," which he published in American journals. In these he argued, in particular, that an experiment is a question put to nature, and that before he can put the question the scientist (or, better, natural philosopher) must decide the language that nature understands: so that Galileo could deal in *measurement* of nature because as a *metaphysician* he already shared with contemporary Platonists the conviction that nature is fundamentally mathematical. Indeed, Koyré maintained in these and later essays, that Galileo was (like some of his distinguished contemporaries) primarily an intellectual dissector of nature for whom experiments—*if* actually carried out—were for confirmation of conclusions previously reached by hard thinking.

Today the historian of science almost instinctively gives full weight to the ideological context of his subject, and that this is so is due more to Koyré than to any other writer. It was not to be expected, or even desired, that so uncompromising and challenging

a thesis would be accepted without qualification, and in re-reading these essays one may perhaps be tempted to demur or call for qualification: a quarter of a century after the publication of the *Études* the pendulum is perceptibly swinging back towards a greater appreciation, not simply of the importance of observation and experiment, but also of the social, economic and technological context of scientific development. This is as it should be. But each year a new intake of students encounters history of science for the first time, and the need remains and no doubt always will remain of bringing them quickly to realize the intellectual challenge of the subject: that as historians of science they are not to be mere chroniclers of facts and dates, but must creatively reinterpret the past in an effort to understand the present. For this purpose I know of no finer initiation than the essays in this volume, and it was with beginning students in mind that I approached Professor Koyré with a view to publishing a suitable selection of his essays. He was able to make minor corrections to the original texts, but the publication of the volume is, unhappily, posthumous. Four of his intimate friends, Professor I. Bernard Cohen, Father Pierre Costabel, Dr Alistair Crombie and Professor René Taton, have assisted with the publication of the volume.

M.A. Hoskin

NOTE

Essays I, II and IV were originally written and published in English by Professor Koyré. Essays III, V and VI have been translated from the French for this edition by Dr R.E.W. Maddison. The Index has been prepared by Miss M.A. Hennings.

I

Galileo and the Scientific Revolution of the Seventeenth Century

Modern science did not spring perfect and complete, as Athena from the head of Zeus, from the minds of Galileo and Descartes. On the contrary, the Galilean and Cartesian revolution – which remains, nevertheless, a revolution – had been prepared by a strenuous effort of thought. And there is nothing more interesting, more instructive, nor more thrilling, than to study the history of that effort; to write the story of the human mind dealing obstinately with the same everlasting problems, encountering the same difficulties, struggling untiringly with the same obstacles, and slowly and progressively forging for itself instruments and tools, new concepts, new methods of thinking, which will enable it to overcome them.

It is a long and thrilling story; too long to be told here. Yet, in order to understand the origin, the bearing and the meaning of the Galileo-Cartesian revolution, we cannot dispense with throwing at least a glance backwards, on some of the contemporaries and predecessors of Galileo.

Modern physics studies, in the first line, the motion of ponderous bodies, i.e., the motion of bodies which surround us. Thus it is from the effort to explain the facts and the phenomena of common, everyday, experience – the act of falling, the act of throwing – that proceeds the trend of ideas which leads to the establishment of its fundamental laws. Yet it does not proceed therefrom exclusively, or even principally, or in a direct way. Modern physics does not originate from earth alone. It comes, just as well, from the skies. And it is in the skies that it finds its perfection and end.

This fact, the fact that modern physics has its "prologue" and its "epilogue" in the skies, or, to speak a more sober language, the fact that modern physics takes its origin from the study of astronomical problems and maintains this tie throughout its history, has a deep meaning, and carries important consequences. It expresses the replacement of the classic and medieval conception of the Cosmos –

closed unity of a qualitatively determined and hierarchically well ordered whole in which different parts (heaven and earth) are subject to different laws – by that of the Universe, that is of an open and indefinitely extended entirety of Being, governed and united by the identity of its fundamental laws; it determines the merging of the *Physica coelestis* with *Physica terrestris*, which enables the latter to use and to apply to its problems the methods – the hypothetico-deductive mathematical treatment – developed by the former; it implies the impossibility of establishing and elaborating a terrestrial physics, or, at least, a terrestrial mechanics, without a celestial one; it explains the partial failure of Galileo and Descartes.

Modern physics, which, in my opinion, is born with, and in, the works of Galileo Galilei, looks upon the law of inertial motion as its basic and fundamental law. It does so quite correctly, for *ignorato motu ignoratur natura*, and modern science aims at the explaining of everything by "number, figure, and motion". True, it was Descartes, and not Galileo – as I believe I have established in my *Galilean Studies*[1] – who for the first time fully understood its bearing and its meaning. And yet Newton is not wholly incorrect in giving full credit for it to Galileo. As a matter of fact, though Galileo never explicitly formulated this principle – nor could he have done so – his mechanics, implicitly, is based upon it. And it is only his reluctance to draw, or to admit, the ultimate consequences – or implications – of his own conception of movement, his reluctance to discard completely and radically the experiential data for the theoretical postulate he worked so hard to establish, that prevented him from making the last step on the road which leads from the finite Cosmos of the Greeks to the infinite Universe of the Moderns.

The principle of inertial motion is very simple. It states that a body, left to itself, remains in its state of rest or of motion as long as it is not interfered with by some external force. In other words, a body at rest will remain eternally at rest unless it is "put in motion". And a body in motion will continue to move, and to persist in its rectilinear uniform motion, as long as nothing prevents it from doing so.[2]

[1] A. Koyré, *Études galiléennes* (Paris: Hermann, 1939). See my *From the Closed World to the Infinite Universe* (Baltimore: Johns Hopkins University Press, 1957).

[2] Sir Isaac Newton, *Philosophiae Naturalis Principia Mathematica*; Axiomata sive leges motus; Lex I: Corpus omne perseverare in statu suo quiescendi vel movendi uniformiter in directum, nisi quatenus a viribus impressis cogitur statum ille mutare. See "Newton and Descartes" in A. Koyré, *Newtonian Studies* (London;

The principle of inertial motion appears to us perfectly clear, plausible, and even, practically, self-evident. It seems to us pretty obvious that a body at rest will remain at rest, i.e. will stay where it is – wherever that may be – and will not move away on its own accord. And that, *converso modo*, once put in motion, it will continue to move, and to move in the same direction and with the same speed, because, as a matter of fact, we do not see any reason nor cause why it should change either. All that appears to us not only plausible, but even natural. Yet it is nothing less than that. In fact, the "evidence" and the "naturalness" which these conceptions and considerations are enjoying are very young: we owe them to Galileo and Descartes, whereas to the Greeks, as well as to the Middle Ages, they would appear as "evidently" false, and even absurd.

This fact can only be explained if we admit – or recognize – that all these "clear" and "simple" notions, which form the basis of modern science, are not "clear" and "simple" *per se et in se*, but only as a part of a certain set of concepts and axioms, apart from which they are not "simple" at all. This, in turn, enables us to understand why the discovery of such simple and easy things as, for instance, the fundamental laws of motion, which today are taught to, and understood by, children, has needed such a tremendous effort – and an effort which often remained unsuccessful – by some of the deepest and mightiest minds ever produced by mankind: they had not to "discover" or to "establish" these simple and evident laws, but to work out and to build up the very framework which made these discoveries possible. They had, to begin with, to reshape and to re-form our intellect itself; to give to it a series of new concepts, to evolve a new approach to being, a new concept of nature, a new concept of science, in other words, a new philosophy.

We are so well acquainted with, or rather so well accustomed to, the concepts and principles which form the basis of modern science, that it is nearly impossible for us to appreciate rightly either the obstacles that had to be overcome for their establishment, or the difficulties that they imply and encompass. The Galilean concept of motion (as well as that of space) seems to us so "natural" that we even believe we have derived it from experience and observation, though, obviously, nobody has ever encountered an inertial motion for the simple reason that such a motion is utterly and absolutely

Chapman and Hall, 1965; Cambridge, Massachusetts: Harvard University Press, 1965).

impossible. We are equally well accustomed to the mathematical approach to nature, so well that we are not aware of the boldness of Galileo's statement that "the book of nature is written in geometrical characters", any more than we are conscious of the paradoxical daring of his decision to treat mechanics as mathematics, that is, to substitute for the real, experienced world a world of geometry made real, and to explain the real by the impossible.

In modern science, as well we know, motion is considered as purely geometrical translation from one point to another. Motion, therefore, in no way affects the body which is endowed with it; to be in motion or to be at rest does not make any difference to, or produce a change in, the body whether in motion or at rest. The body, as such, is utterly indifferent to both. Consequently, we are unable to ascribe motion to a determined body considered in itself. A body is only in motion in its relation to something else – some other body – which we assume to be at rest. We can, therefore, ascribe it to the one or to the other of the two bodies, *ad libitum.* All motion is relative.

Just as it does not affect the body which is endowed with it, a given motion of a body in no way interferes with other movements that it may execute at the same time. Thus a body may be endowed with any number of motions, which combine to produce a result according to purely geometrical rules; and, *vice versa*, every given motion can be decomposed, according to these same rules, into any number of component ones.

Yet, all this notwithstanding, motion is considered to be a *state*, and rest another state, utterly and absolutely opposed to the former, so that we must apply a *force* in order to change a state of motion of a given body to that of rest, and *vice versa.*

It is therefore perfectly evident that a body in a state of motion will persist in this state forever; and that it will no more need a force or a cause by which to explain, or to maintain, its uniform, rectilinear movement, than it will need one by which to explain or to maintain its rest.

Thus, in order to appear evident, the principle of inertial motion presupposes (*a*) the possibility of isolating a given body from all its physical environment, (*b*) the conception of space which identifies it with the homogeneous, infinite space of Euclidian geometry, and (*c*) a conception of movement – and of rest – which considers them as *states* and places them on the same ontological level of being.

No wonder that these conceptions appeared pretty difficult to ad-

mit – and even to understand – to the contemporaries and predecessors of Galileo; no wonder that to his Aristotelian adversaries the notion of motion as a persistent, substantial relation-state appeared just as abstruse and contradictory as the famous substantial forms of the scholastics appear to us; no wonder that Galileo Galilei had to struggle before he succeeded in forming that conception, and that great, but somewhat lesser, minds, such as Bruno and even Kepler, failed to reach that goal. As a matter of fact, even today, the conception we are describing is by no means easy to grasp, as anyone who ever attempted to teach physics to students who did not learn it at school will certainly testify. Common sense, indeed, is – as it always was – medieval and Aristotelian.

We must now give our attention to the pre-Galilean, chiefly Aristotelian, conception of motion and of space. I will not, of course, endeavour to give here an exposition of Aristotelian physics; I will only point out some of its characteristic features as opposed to the modern; and I would like to stress, because it is fairly widely misappreciated, that the Aristotelian physics is a very thoroughly thought out, and very coherent, body of theoretical knowledge, which, besides having a very deep philosophical foundation, is, as stated by P. Duhem and P. Tannery,[1] in pretty good accordance – a much better one, indeed, than the Galilean – with the experience, at least with the common-sense experience, of our everyday life.

Aristotelian physics is based on sense-perception, and is therefore decidedly non-mathematical. It refuses to substitute mathematical abstractions for the colourful, qualitatively determined facts of common experience, and it denies the very possibility of a mathematical physics on the ground (*a*) of the nonconformity of mathematical concepts to the data of sense-experience, (*b*) of the inability of mathematics to explain quality and to deduce movement. There is no quality, and no motion, in the timeless realm of figure and number.

As for motion – κίνησις – or rather "local motion" – Aristotelian physics considers it a kind of process of change – in contradistinction with *rest*, which, being the goal and the end of motion, is to be recognized as a *state*.[2] Motion is change (actualization or decay) and

[1] P. Duhem, *Le Système du monde*, I (Paris: Hermann, 1915), pp. 194 ff; P. Tannery, "Galilée et les principes de la dynamique", *Mémoires scientifiques*, VI (Toulouse: Privat; Paris: Gauthier-Villars, 1926), pp. 399 ff.

[2] For Aristotle rest, being a deficiency, *privatio*, is on a lower ontological level than motion, *actus entis in potentia in quantum est in potentia.*

consequently a body in motion changes not only its *relations* to other bodies, but, at the same time, undergoes itself a processus of change. Motion, therefore, always affects the body which endures it, and, consequently, if a body is endowed with two (or more) movements, these movements interfere with each other, impede each other, and even are, sometimes, incompatible with each other. Besides, Aristotelian physics does not admit the right, or even the possibility, of identifying the concrete world-space of its well ordered and finite Cosmos with the "space" of geometry, any more than it admits the possibility of isolating a given body from its physical (and Cosmical) environment. In dealing with a concrete physical problem it is, therefore, always necessary to take into account the world order, to consider the realm of being (the "natural place") to which a given body belongs by its nature; and, on the other hand, it is impossible to try to subject these different realms to the same laws, even – and perhaps especially – to the same laws of motion. E.g. *heavy* things descend whereas *light* ones ascend; terrestrial bodies move in right lines, celestial ones in circles, and so on.

It is evident, even from this brief account, that motion, considered as *processus of change* (and not as *state*), cannot go on spontaneously and automatically, that it requires, for its persistence, a continuous action of a mover or cause, and that it stops dead as soon as this action does not exercise itself upon the body in motion, i.e. as soon as the body in question is separated from its mover. *Cessante causa, cessat effectus.* It follows therefrom, with absolute necessity, that the kind of motion which is postulated by the principle of inertia is utterly and perfectly impossible, and even contradictory.

And now we must come to the facts. I have said already that modern science originated in close connection with astronomy; more precisely it takes its origin in, and from, the necessity of meeting the *physical* objections formulated by some of the leading scientists of the time against the Copernican astronomy. As a matter of fact, these objections were nothing less than new: quite to the contrary, though presented sometimes in a slightly modernized form, as by replacing the throwing of a stone of the older argument by the firing of a cannon-ball, they were fundamentally identical with those that Aristotle and Ptolemy raised against the possibility that the earth moves. It is very interesting, and very instructive, to see them dis-

cussed and rediscussed by Copernicus himself, by Bruno, Tycho Brahe, Kepler, and Galileo.[1]

Divested from the imaginative clothing which they gave them, the arguments of Aristotle and Ptolemy can be boiled down to the statement that, if the earth were moving, this movement would affect the phenomena occurring on its surface in two perfectly definite ways: (1) the tremendous velocity of this (rotational) movement would develop a centrifugal force of such a magnitude that all the bodies *not connected* with the earth would fly away, and (2) this same movement would cause all bodies not connected, or temporarily disconnected with it, to lag behind. Therefore, a stone falling from the summit of a tower would never land at its foot, and, *a fortiori*, a stone (or a bullet) thrown (or shot) perpendicularly into the air would never fall back to the place from which it departed, because, during the time of its fall or flight, this place would be "quickly removed from below it and rapidly moved away".

We must not smile at this argument. From the point of view of the Aristotelian physics it is perfectly sound. So sound that, on the basis of this physics, it is utterly irrefutable. In order to destroy it we must change the system as a whole and evolve a new concept of movement: the concept of movement of Galileo.

As we have already seen, motion for the Aristotelian is a process which affects the moved, which takes place "in" the body in motion. A falling body moves from A to B, from a certain place, situated above the earth, toward the latter, or, more exactly, towards its centre. It follows the straight line which connects these two points. If during this movement the earth revolved around its axis, it would describe, in respect to this line (the line leading from A to the centre of the earth) a movement in which neither this line, nor the body which follows it, takes any part whatever: the movement of the earth does not affect the body which is separated from it. The fact that the earth beneath it moves away has no effect on its trajectory. The body cannot run after the earth. It follows its path as if nothing happened because, in fact, nothing happened *to it*. Even the fact that the point A (the summit of the tower) did not stay still, but participated in the movement of the earth, does not have any bearing on its motion: what happened to the point of departure of the body (after it left it) has not the slightest influence on its behaviour.

[1] See A. Koyré, *Études galiléennes*, part III, Galilée et la loi d'inertie (Paris: Hermann, 1939).

This conception may appear strange to us. But it is by no means absurd: it is exactly in that way that we represent to ourselves the movement – or propagation – of a ray of light. And it implies that, if the earth were moving, a body thrown from the top of a tower would never fall at its foot; and that a stone, or a cannon ball, shot vertically in the air, would never fall back to the place where it went from. It implies, *a fortiori*, that a stone or a ball falling from the top of the mast of a moving ship will never fall at its foot.

What Copernicus himself has to reply to the Aristotelian is very poor. He argues that the unhappy consequences deduced by this latter would follow, indeed, in the case of a "violent" movement. But not in that of the movement of the earth, and to the things that belong to the earth: for them it is indeed a *natural* movement. This is the reason why all these things, clouds, birds, stones, etc., etc., partake in the movement, and do not lag behind.

The arguments of Copernicus are very poor. And yet they bear the seed of a new conception which will be developed by later thinkers. The reasonings of Copernicus apply the laws of "celestial mechanics" to terrestrial phenomena, a step which, at least implicitly, involves abandoning the old, qualitative division of the Cosmos into two different worlds. Besides this, Copernicus explains the *apparently rectilinear* path of the falling body by its participation in the movement of the earth; this movement, being common to the earth, to the body, and to ourselves, remains for us "as if it were non-existent".

The arguments of Copernicus are based on the mythical conception of a "community of nature" between the earth and "earthen" things. Later science will have to replace it by the concept of the physical system, of the system of things sharing the same movement; it will have to rely upon the *physical* and not only upon the *optical* relativity of motion. All of which is impossible on the basis of the Aristotelian philosophy of motion and makes it necessary to adopt another philosophy. As a matter of fact, as we shall see more and more clearly, it is with a philosophical problem that we are dealing in this discussion.

The conception of physical or, rather, mechanical system, which was implicitly present in the arguments of Copernicus, was worked out by Giordano Bruno. By a stroke of genius Bruno saw that it was necessary for the new astronomy to abandon outright the conception of a closed and finite world, and to replace it by that of an open infinite Universe. This involves the abandonment of the notions of

"natural" places and motions as opposed to non-natural, violent ones. In the infinite universe of Bruno, in which the Platonic conception of space as "receptacle" (χώρα) takes the place of the Aristotelian conception of space as envelope, all "places" are perfectly equivalent and therefore perfectly natural for all bodies. Therefore, whereas Copernicus distinguishes between the "natural" movement of the earth and the "violent" movement of the things upon it, Bruno expressly assimilates them. All that happens on the earth if we suppose it in movement has, as he explains, its exact counterpart in what happens on a ship gliding on the surface of the sea; and the movement *of* the earth has no more influence upon the movement *on* the earth than the movement of the ship on those of the things that are in the ship. The consequences deduced by Aristotle would only take place if the origin, i.e. the place of departure, of the moving body were external to, and not connected with, the earth.

Bruno states that the place of origin *as such* does not play any role in the determination of the motion (the path) of the moving body, that what is important is the connection – or lack of connection – of this "place" with the mechanical system. It is even possible – *horribile dictu* – for the selfsame "place" to pertain to two or more systems. Thus, for instance, if we imagine two men, one of them on the top of the mast of a ship passing under a bridge, and the other on that bridge, we may imagine, further, that at a certain moment, the hands of both of them will be in the selfsame place. If, at that moment, each of them shall let a stone fall, the stone of the man on the bridge will fall down (and in the water), but the stone of the man on the mast will follow the movement of the ship, and (describing, relatively to the bridge, a peculiar curve) fall at the foot of the mast. The reason for this different behaviour, explains Bruno, is simply the fact that the last stone having shared the movement of the ship retains in it a part of the "moving virtue" which has been impressed into it.

As we see, Bruno substitutes for the Aristotelian dynamics the *impetus*-dynamics of the Parisian nominalists. It seems to him that this dynamics provides a sufficient basis for his construction. A belief which, as history has shown us, was an error. It is true that the conception of the *impetus*, virtue, or power, which animates the moving body, produces its motion, and uses itself up in this production, enabled him to refute the arguments of Aristotle; at least some of them. Yet it was not able to meet all of them; still less was it able to carry the structure of modern science.

The arguments of Giordano Bruno appear to us perfectly reasonable. Yet in his time they made no impression whatever; neither on Tycho Brahe, who in his polemics with Rothmann repeats imperturbably the old Aristotelian objections (though in a somewhat modernized presentation); nor even on Kepler, who, though influenced by Bruno, deems himself obliged to return to those of Copernicus, replacing, indeed, the great astronomer's mythical conception of the community of nature by a physical conception, that of the force of attraction.

Tycho Brahe flatly denies that a bullet falling from the top of the mast of a moving ship will come down at its foot. He affirms that, quite on the contrary, it will lag behind, and lag behind the more the faster the ship is moving. Just as cannon-balls, shot vertically in the air, would never – on a moving earth – be able to come back to the cannon.

Tycho Brahe adds that, if the earth were moving, as Copernicus wants it, it would never be possible to send a cannon-ball to the same distance to the east and to the west: the extremely rapid movement of the earth, if it were shared by the ball, would impede its own movement and even, if the ball had to move in a direction opposite to that of the movement of the earth, render it utterly impossible. The point of view of Tycho Brahe appears to us pretty strange. Yet we must not forget that to him the theories of Bruno seemed utterly unbelievable and even exaggeratedly anthropomorphic. To pretend that two bodies, falling from the same place and going to the same point (the centre of the earth), will follow two different paths, describe two different trajectories, for the reason that one of them was associated with the ship, whereas the other was not, means for the Aristotelian to pretend that the bullet in question *remembers* its past association, knows where it has to go, and is endowed with the power and the ability to do so. Which, in turn, implies that it is endowed with a soul.

Besides, as we have already mentioned, from the point of view of the Aristotelian dynamics – as well as from the point of view of the dynamics of the *impetus* – two different movements always impede each other, which is proved by the well-known fact that the speedy motion of the bullet (in a horizontal flight) prevents it from moving downwards and enables it to stay in the air much longer than it would be able to do if we simply let it fall to the bottom.

In short, Tycho Brahe does not admit the mutual independence

of motions – nobody did till Galileo; he is therefore perfectly right not to admit the facts, and the theories, which imply it.

The position taken by Kepler is of a quite particular interest and importance. It shows us, better than any other, the ultimate *philosophical* roots of the Galilean revolution. From a purely scientific point of view, Kepler – to whom we owe, *inter alia*, the very term *inertia* – is, undoubtedly, one of the foremost – if not *the* foremost – genius of his time: it is needless to insist upon his outstanding mathematical gifts, equalled only by the intrepidity of his thought. The very title of one of his works, *Physica coelestis*, is a challenge to his contemporaries. And yet, philosophically, he is much nearer to Aristotle and the Middle Ages than to Galileo and Descartes. He still reasons in terms of the Cosmos; for him motion and rest are still opposed as light and darkness, as being and privation of being. Consequently, the term *inertia* means for him the resistance that bodies oppose, not to change of state, as for Newton, but only and solely to movement; therefore, just like Aristotle and the physicists of the Middle Ages, he needs a cause or a force to explain motion, and does not need one to explain rest; just like them, he believes that, separated from the mover, or deprived from the influence of the moving virtue or power, bodies in motion will not continue their movement, but, on the contrary, will immediately stop. Therefore, in order to explain the fact that, on the moving earth, bodies, even if they are not attached to it by material bounds, do not "lag behind", at least *not perceptibly*[1]; that stones thrown upwards come down to the spot they were thrown from; that cannon-balls fly (nearly) as far to the west as to the east, he must admit – or find out – a real force which binds them to the earth, and pulls them along.

This force is found by Kepler in the mutual attraction of all material, or at least of all terrestrial, bodies, which means, for all practical purposes, in the attraction of all terrestrial things by the earth. Kepler conceives all these things as bound to the earth by innumerable elastic chains; it is the traction of these chains which explains that clouds, vapours, etc., stones, and bullets, do not stay immobile in the air, but follow the earth in its movement; and the fact that these chains are everywhere explains, in Kepler's opinion, the possibility of throwing a stone or firing a cannon *against* its movement: the attracting chains pull the bullet to the East as well as to the West and thus their influence is nearly neutralized. The real move-

[1] See A. Koyré, *Études galiléennes* (Paris: Hermann, 1939), pp. 172-94.

ment of the body (the cannon-ball shot vertically) is, of course, a combination or mixture of (*a*) its own movement and (*b*) that of the earth. But, as the latter is common to all the examined cases, it is the former only that counts. It is therefore clear (though Tycho Brahe did not grasp it) that, while the length of the path of a bullet shot to the east and of another shot to the west differ, as measured in the space of the universe, nevertheless their paths on the earth are the same or nearly the same. Which explains why the same force, produced by the same amount of powder, can throw them to the same distance in both directions.

The Aristotelian or Tychonian objections against the movement of the earth are thus satisfactorily disposed of. And Kepler points out that it was an error to assimilate the earth to the moving ship: in fact, the earth "magnetically attracts" the bodies it transports, the ship does not. Therefore, on a ship we need a material bond, which is perfectly useless in the case of the earth.

We need not dwell upon this point any longer: we see that Kepler, the great Kepler, the founder of modern astronomy, the same man who proclaimed the unity of matter in the whole universe and stated that *ubi materia, ibi geometria,* failed to establish the basis of modern physical science for one and only one reason: he still believed that motion is, ontologically, on a higher level of being than rest.

If now, after our brief historical summary, we turn our attention to Galileo Galilei, we shall not be surprised that he, too, discusses at great, and even at a very great, length, the time-worn objections of the Aristotelians. We shall, moreover, be able to appreciate the consummate skill with which, in his *Dialogue on the two greatest world systems,* he marshals his arguments and prepares for the final assault on Aristotelianism.

Galileo is well aware of the tremendous difficulty of his task. He knows perfectly well that he has to deal with powerful enemies: authority, tradition, and – worst of them all – common sense. It is useless to present proofs to minds not able to grasp their value. Useless, for instance, to explain the difference between linear and radial velocity (the confusion between which is the whole basis of the first of the Aristotelian and Ptolemaic objections) to people not accustomed to mathematical thinking. You must begin by educating them. You must proceed slowly, step by step, discussing and rediscussing the old and the new arguments; you must present them in various forms; you must multiply examples, invent new and striking ones:

the example of the rider throwing his spear in the air and catching it again; the example of the bowman straining his bow more or less and thus giving to the arrow a greater or a lesser *speed*; the example of the bow placed on a moving carriage and able to compensate the *speed* of the carriage by a greater or lesser *speed* given to his arrows. Innumerable other examples which, step by step, lead us, or rather his contemporaries, to the acceptance of this paradoxical, unheard of point of view, according to which motion is something which persists in being *in se et per se* and does not require any cause, or force, for its persistence. A hard task. Because, as I have already said, it is not natural to think of motion in terms of speed and of direction instead of those of effort, of impetus, and of momentum.

But, as a matter of fact, we cannot *think* of motion in terms of effort and impetus: we only can *imagine* in this way. Thus we must choose: either to think or to imagine. To think with Galileo, or to imagine with common sense.

For it is thought, pure unadulterated thought, and not experience or sense-perception, as until then, that gives the basis for the "new science" of Galileo Galilei.

Galileo is perfectly clear about it. Thus discussing the famous example of the ball falling from the top of a mast of a moving ship, Galileo explains at length the principle of the physical relativity of motion, the difference between the motion of the body as relative to the earth, and as relative to the ship, and then, *without making any appeal to experience*, concludes that the motion of the ball, *in relation to the ship*, does not change with the motion of the latter. Moreover, when his empirically minded Aristotelian opponent asks him, "Did you make an experiment?" Galileo proudly declares: "No, and I do not need it, as without any experience I can affirm that it is so, because it cannot be otherwise."

Thus *necesse* determines *esse*. Good physics is made *a priori*. Theory precedes fact. Experience is useless because before any experience we are already in possession of the knowledge we are seeking for. Fundamental laws of motion (and of rest), laws that determine the spatio-temporal behaviour of material bodies, are laws of a mathematical nature. Of the same nature as those which govern relations and laws of figures and of numbers. We find and discover them not in Nature, but in ourselves, in our mind, in our memory, as Plato long ago has taught us.

And it is *therefore* that, as Galileo proclaims it to the greatest dis-

may of the Aristotelian, we are able to give to propositions which describe the "symptoms" of motion strictly and purely mathematical proofs, to develop the language of natural science, to question Nature by mathematically conducted experiments,[1] and to read the great book of Nature which is "written in geometrical characters".

The book of Nature is written in geometrical characters: the new, Galilean, physics is a geometry of motion, just as the physics of his true master, the *divus Archimedes*, was a geometry of rest.

Geometry of motion, *a priori*, mathematical science of nature. . . . How is it possible? The old, Aristotelian objections against the mathematization of nature by Plato, have they, at last, been disproved and refuted? Not quite. There is, indeed, no quality in the realm of number, and therefore Galileo – as, for the same reason, Descartes – is obliged to renounce it, to renounce the variegated, qualitative world of sense-perception and common experience and to substitute for it the colourless, abstract Archimedian world. And as for motion . . . there is, quite certainly, no motion in numbers. Yet motion – at least the motion of Archimedian bodies in the infinite homogeneous space of the new science – is governed by number. By the *leges et rationes numerorum.*

Motion is subjected to number; that is something which even the greatest of the old Platonists, the superhuman Archimedes himself, did not know, something which was left to discover to this "marvellous Assayer of Nature", as his pupil and friend Cavallieri calls him, the Platonist Galileo Galilei.

The Platonism of Galileo Galilei (a problem discussed by me elsewhere[2]) is, indeed, quite different from that of the Florentine Academy, just as his mathematical philosophy of nature is different from their neo-pythagorean arithmology. But in the history of philosophy there is more than one Platonic school, more than one Platonic tradition, and it is still a question whether the trend of ideas represented by Iamblichus and Proclus is more or less Platonic than the trend represented by Archimedes.[3]

[1] Experiment – in contradistinction to mere experience – is a question we put to Nature. In order to receive an answer we must formulate it in some definite language. The Galilean revolution can be boiled down to the discovery of that language, to the discovery of the fact that mathematics is the grammar of science. It is this discovery of the rational structure of Nature which gave the *a priori* foundations to the modern *experimental* science and made its constitution possible.

[2] See "Galileo and Plato", below.

[3] For the whole doxographic tradition Archimedes is a *philosophus platonicus.*

I will not discuss this problem here. Yet I must point out that for the contemporaries and pupils of Galileo, as well as for Galileo himself, the dividing line between Aristotelianism and Platonism was perfectly clear. In their opinion the opposition between these two philosophies was determined by a different appreciation of mathematics as science, and of its role for the constitution of the science of Nature. According to them, if one sees in mathematics an auxiliary science which deals with abstractions and is, therefore, of a lesser value than sciences dealing with real being, such as physics, if one affirms that physics can and must be built directly on experience and sense-perception, one is an Aristotelian. If, on the contrary, one claims for mathematics a superior value, and a commanding position in the study of things natural, one is a Platonist. Accordingly, for the contemporaries and pupils of Galileo, as well as for Galileo himself, the Galilean science, the Galilean philosophy of Nature, appeared as a return to Plato, a victory of Plato over Aristotle.

I must confess that, to me, this interpretation seems to be perfectly sensible.

II

Galileo and Plato

The name of Galileo Galilei is indissolubly linked with the scientific revolution of the sixteenth century, one of the profoundest, if not the most profound, revolutions of human thought since the invention of the Cosmos by Greek thought: a revolution which implies a radical intellectual "mutation", of which modern physical science is at once the expression and the fruit.[1]

This revolution is sometimes characterized, and at the same time explained, as a kind of spiritual upheaval, an utter transformation of the whole fundamental attitude of the human mind: the active life, the *vita activa* taking the place of the *Θεωρία*, the *vita contemplativa*, which until then had been considered its highest form. Modern man seeks the domination of nature, whereas medieval or ancient man attempted above all its contemplation. The mechanistic trend of classical physics – of the Galilean, Cartesian, Hobbesian physics, *scientia activa, operativa*, which was to render man "master and possessor of nature" – has, therefore, to be explained by this desire to dominate, to act; it has to be considered purely and simply an outflow of this attitude, an application to nature of the categories of thinking of *homo faber*.[2] The science of Descartes – and *a fortiori* that of Galileo – is nothing else than (as has been said) the science of the craftsman or of the engineer.[3]

I must confess that I do not believe this explanation to be entirely correct. It is true, of course, that modern philosophy, as well as modern ethics and modern religion, lays much more stress on action, on *πράξις*, than ancient and medieval thought. And it is just as true of

[1] See J. H. Randall, Jr., *The Making of the Modern Mind* (Boston: 1926), pp. 220 ff., 231 ff; see also A. N. Whitehead, *Science and the Modern World* (New York: 1925).

[2] This widespread conception must not be confused with that of Bergson, for whom all physics, the Aristotelian just as much as the Newtonian, is in the last analysis the work of *homo faber*.

[3] See L. Laberthonnière, *Études sur Descartes* (Paris: Vrin, 1935), II, pp. 288 ff, 297, 304: "physique de l'exploitation des choses".

modern science: I am thinking of the Cartesian physics and its analogies of pulleys, strings and levers. Still the attitude we have just described is much more that of Bacon – whose role in the history of science is not of the same order[1] – than that of Galileo or Descartes. Their science is made not by engineers or craftsmen, but by men who seldom built or made anything more real than a theory.[2] The new ballistics was made not by artificers and gunners, but against them. And Galileo did not learn *his* business from people who toiled in the arsenals and shipyards of Venice. Quite the contrary: he taught them *theirs*.[3] Moreover, this theory explains too much and too little. It explains the tremendous scientific progress of the seventeenth century by that of technology. And yet the latter was infinitely less conspicuous than the former. Besides, it forgets the technological achievements of the Middle Ages. It neglects the lust for power and wealth which, throughout its history, inspired alchemy.

Other scholars have insisted on the Galilean fight against authority, especially against that of Aristotle: against the scientific and philosophical tradition, upheld by the Church and taught in the univer-

[1] Bacon is the announcer, the *buccinator*, of modern science, not one of its creators.

[2] The Cartesian and Galilean science has, of course, been of extreme importance for the engineer and the technician; ultimately it has produced a technical revolution. Yet it was created and developed neither by engineers nor technicians, but by theorists and philosophers.

[3] "Descartes artisan" is the conception of Cartesianism developed by Leroy in his *Descartes social* (Paris: 1931), and brought to absurdity by F. Borkenau in his book *Der Übergang vom feudalen zum bürgerlichen Weltbild* (Paris: 1934). Borkenau explains the birth of the Cartesian philosophy and science by that of a new form of economic enterprise, i.e. manufacturing. See the criticism of the work of Borkenau, a criticism much more interesting and instructive than the book itself, by H. Grossmann, "Die gesellschaftlichen Grundlagen der mechanistischen Philosophie und die Manufaktur", *Zeitschrift für Sozialforschung* (Paris: 1935).

As for Galileo, he is linked with the traditions of the artisans, builders, engineers, etc., of the Renaissance by L. Olschki, *Galileo und seine Zeit* (Halle: 1927), and more recently by E. Zilsel, "The Sociological Roots of Science", *The American Journal of Sociology*, XLVII (1942). Zilsel stresses the role played by the "superior artisans" of the Renaissance in the development of the modern scientific mentality. It is, of course, perfectly true that the artists, engineers, architects, etc., of the Renaissance played an important part in the struggle against the Aristotelian tradition, and that some of them – like Leonardo da Vinci and Benedetti – attempted even to develop a new, anti-Aristotelian dynamics; yet this dynamics, as was conclusively shown by Duhem, was in its main features that of the Parisian nominalists, the *impetus* dynamics of John Buridan and Nicole Oresme. And if Benedetti, by far the most remarkable of these "forerunners" of Galileo, transcends sometimes the level of the "Parisian" dynamics, it is not because of his work as engineer and gunner but because of his study of Archimedes and his decision to apply "mathematical philosophy" to the investigation of nature.

sities. They have stressed the role of observation and experience in the new science of nature.[1] It is perfectly true, of course, that observation and experimentation form one of the most characteristic features of modern science. It is certain that in the writings of Galileo we find innumerable appeals to observation and to experience, and bitter irony toward men who didn't believe their eyes because what they saw was contrary to the teaching of the authorities, or, even worse, who (like Cremonini) did not want to look through Galileo's telescope for fear of seeing something which would contradict their traditional theories and beliefs. It is obvious that it was just by building a telescope and by looking through it, by careful observation of the moon and the planets, by his discovery of the satellites of Jupiter, that Galileo dealt a crushing blow to the astronomy and the cosmology of his times.

Still one must not forget that observation and experience, in the sense of brute, common-sense experience, did not play a major role – or, if it did, it was a negative one, the role of obstacle – in the foundation of modern science.[2] The physics of Aristotle, and even more that of the Parisian Nominalists, of Buridan and Nicole Oresme, was, as stated by Tannery and Duhem, much nearer to commonsense experience than those of Galileo and Descartes.[3] It is not "experience" but "experiment" which played – but only later – a great positive role. Experimentation is the methodical interrogation of nature, an

[1] A friendly critic has reproached me for having neglected this side of Galileo's teaching. (See L. Olschki, "The Scientific Personality of Galileo", *Bulletin of the History of Medicine*, XII [1942].) I must confess I do not believe I have merited this reproach, though I do indeed believe that science is primarily theory and not the gathering of "facts".

[2] É. Meyerson, *Identité et réalité* (Paris: Alcan, 1926), p. 156, 3rd ed., shows very convincingly the lack of accord between "experience" and the principles of modern physics.

[3] P. Duhem, *Le Système du Monde* (Paris: Hermann, 1913), I, pp. 194 ff: "Cette dynamique, en effet, semble s'adapter si heureusement aux observations courantes qu'elle ne pouvait manquer de s'imposer, tout d'abord, à l'acceptation des premiers qui aient spéculé sur les forces et les mouvements. . . . Pour que les physiciens en viennent à rejeter la Dynamique d'Aristote et à construire la Dynamique moderne, il leur faudra comprendre que les faits dont ils sont chaque jour les témoins ne sont aucunement les faits simples, élémentaires, auxquelles les lois fondamentales de la Dynamique se doivent immédiatement appliquer; que la marche du navire tiré par les haleurs, que le roulement sur une route de la voiture attelée doivent être regardés comme des mouvements d'une extrême complexité; en un mot que pour le principe de la science du mouvement, on doit, par abstraction, considérer un mobile qui, sous l'action d'une force unique, se meut dans le vide. Or, de sa Dynamique Aristote va jusqu'à conclure qu'un tel mouvement est impossible."

interrogation which presupposes and implies a *language* in which to formulate the questions, and a dictionary which enables us to read and to interpret the answers. For Galileo, as we know well, it was in curves and circles and triangles, in mathematical or even more precisely, in *geometrical language* – not in the language of common sense or in that of pure symbols – that we must speak to Nature and receive her answers. Yet obviously the choice of the language, the decision to employ it, could not be determined by the experience which its use was to make possible. It had to come from other sources.

Still other historians of science and philosophy[1] have more modestly tried to characterize modern physics, as *physics*, by some of its salient traits: for instance, by the role which the principle of *inertia* plays in it. Perfectly right, once more: the principle of *inertia*, in contradistinction to that of the Ancients, holds an outstanding place in classical mechanics. It is its fundamental law of motion; it implicitly pervades Galilean physics and quite explicitly that of Descartes and of Newton. But this characteristic seems to me to be somewhat superficial. In my opinion it is not enough simply to state the fact. We have to understand and to explain it – to explain why *modern* physics was able to adopt this principle; to understand why, and how, the principle of inertial motion, which to us appears so simple, so clear, so plausible and even self-evident, acquired this status of self-evidence and *a priori* truth whereas for the Greeks as well as for the thinkers of the Middle Ages the idea that a body once put in motion will continue to move forever, appeared as obviously and evidently false, and even absurd.[2]

I shall not try to explain here the reasons and causes that produced the spiritual revolution of the sixteenth century. It is for our purpose sufficient to describe it, to describe the mental or intellectual attitude of modern science by two (connected) characteristics. They are: (1) the destruction of the Cosmos, and therefore the disappearance from science of all considerations based on that notion[3]; (2) the geometrization of space – that is, the substitution of the homogeneous and

[1] See Kurd Lasswitz, *Geschichte der Atomistik* (Hamburg and Leipzig: 1890), II, pp. 23 ff; E. Mach, *Die Mechanik in ihrer Entwicklung* (Leipzig: Brockhaus, 1921), pp. 117 ff, 8th Ed.; E. Wohlwill, "Die Entdeckung des Beharrunggesetzes", *Zeitschrift für Völkerpsychologie und Sprachwissenschaft*, vols. XIV and XV (1883 and 1884), and E. Cassirer, *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit* (Berlin: 1911), I, pp. 394 ff, 2nd Ed.

[2] See E. Meyerson, *Identité et réalité* (Paris: Alcan, 1926), pp. 124 ff.

[3] The *term* remains, of course, and Newton still speaks of the Cosmos and its order (as he speaks of *impetus*), but in an entirely new meaning.

abstract space of Euclidian geometry for the qualitatively differentiated and concrete world-space conception of the pre-galilean physics. These two characteristics may be summed up and expressed as follows: the mathematization (geometrization) of nature and, therefore, the mathematization (geometrization) of science.

The dissolution of the Cosmos means the destruction of the idea of a hierarchically-ordered finite world-structure, of the idea of a qualitatively and ontologically differentiated world, and its replacement by that of an open, indefinite and even infinite universe, united and governed by the same universal laws; a universe in which, in contradiction to the traditional conception with its distinction and opposition of the two worlds of Heaven and of Earth, all things are on the same level of Being. The laws of Heaven and the laws of Earth are merged together. Astronomy and physics become interdependent, and even unified and united.[1] And this implies the disappearance from the scientific outlook of all considerations based on value, on perfection, on harmony, on meaning and on purpose.[2] They disappear in the infinite space of the new Universe. It is in this new Universe, in this new world of a geometry made real, that the laws of classical physics are valid and find their application.

The dissolution of the Cosmos – I repeat what I have already said: this seems to me to be the most profound revolution achieved or suffered by the human mind since the invention of the Cosmos by the Greeks. It is a revolution so profound and so far-reaching that mankind – with very few exceptions, of whom Pascal was one – for centuries did not grasp its bearing and its meaning; which, even now, is often misvalued and misunderstood.

Therefore what the founders of modern science, among them Galileo, had to do, was not to criticize and to combat certain faulty theories, and to correct or to replace them by better ones. They had

[1] As I have endeavoured to show elsewhere (*Études galiléennes*, part III, Galilée et la loi d'inertie [Paris: Hermann, 1939]) modern science results from this unification of astronomy and physics which enables it to apply the methods of mathematical investigation, till then employed in the study of celestial phenomena, to the study of the phenomena of the sublunar world.

[2] See É. Bréhier, *Histoire de la philosophie*, vol. II, fasc. 1 (Paris: 1929), p. 95: "Descartes dégage la physique de la hantise du Cosmos hellénique, c'est-à-dire de l'image d'un certain état privilégié des choses qui satisfait nos besoins esthétiques. . . . Il n'y a pas d'état privilégié puisque tous les états sont équivalents. Il n'y a donc aucune place en physique pour la recherche des causes finales et la considération du meilleur."

to do something quite different. They had to destroy one world and to replace it by another. They had to reshape the framework of our intellect itself, to restate and to reform its concepts, to evolve a new approach to Being, a new concept of knowledge, a new concept of science – and even to replace a pretty natural approach, that of common sense, by another which is not natural at all.[1]

This explains why the discovery of things, of laws, which today appear so simple and so easy as to be taught to children – the laws of motion, the law of falling bodies – required such a long, strenuous, and often unsuccessful effort of some of the greatest geniuses of mankind, a Galileo, a Descartes.[2] This fact in turn seems to me to disprove the modern attempt to minimize, or even to deny, the originality, or at least the revolutionary character, of Galileo's thinking; and to make clear that the apparent continuity in the development of medieval and modern physics (a continuity so emphatically stressed by Caverni and Duhem)[3] is an illusion. It is true, of course, that an unbroken tradition leads from the works of the Parisian Nominalists to those of Benedetti, Bruno, Galileo and Descartes.

[1] See P. Tannery, "Galilée et les principes de la dynamique", in *Mémoires scientifiques*, VI (Toulouse: 1926), p. 399: "Si pour juger le système dynamique d'Aristote, on fait abstraction des préjugés qui dérivent de notre éducation moderne, si on cherche à se replacer dans l'état d'esprit que pouvait avoir un penseur indépendant au commencement du XVIIe siècle, il est difficile de méconnaitre que ce système est beaucoup plus conforme que le nôtre à l'observation immédiate des faits."

[2] See my *Études galiléennes*, part II, La loi de la chute des corps (Paris: Hermann, 1939).

[3] See Caverni, *Storia del metodo sperimentale in Italia*, 5 vols. (Firenze: 1891-6), particularly vols. IV and V; P. Duhem, *Le mouvement absolu et le mouvement relatif* (Paris: 1905); "De l'accélération produite par une force constante", *Congrès International de l'histoire des sciences, IIIe session* (Geneva: 1906); *Études sur Léonard de Vinci: Ceux qu'il a lu et ceux qui l'ont lu*, 3 vols. (Paris: 1909-13), particularly vol. III, *Les précurseurs parisiens de Galilée*. More recently the thesis of continuity has been upheld by J. H. Randall, Jr., in his brilliant article "Scientific Method in the School of Padua", *Journal of the History of Ideas*, I (1940); Randall convincingly shows the progressive elaboration of the method of "resolution and composition" in the teaching of the great logicians of the Renaissance. Yet Randall himself states that there was "one element lacking in Zabarella's formulation of method: he did not insist that the principles of natural science be mathematical" (p. 204), and that Cremonini's *Tractatus de paedia* "sounds like the solemn warning of the great tradition of Aristotelian rational empiricism to the triumphant mathematicians" (ibid.). As a matter of fact, it is just this "mathematical emphasis added to the logical methodology of Zabarella" (p. 205) which forms, in my opinion, the content of the scientific revolution of the seventeenth century; and, in the opinion of the time, the dividing line between the followers of Plato and those of Aristotle.

(I myself have added a link to the history of that tradition.)[1] Still the conclusion drawn therefrom by Duhem is a delusion: a well-prepared revolution is nevertheless a revolution, and in spite of the fact that Galileo himself in his youth (as well as at times Descartes) shared the views and taught the theories of the medieval critics of Aristotle, modern science, the science born from his efforts and discoveries, *does not* follow the inspiration of the "Parisian forerunners of Galileo"; it places itself at once on a quite different level – on a level which I should like to call the Archimedian one. The true forerunner of modern physics is neither Buridan, nor Nicole Oresme, nor even John Philoponos, but Archimedes.[2]

I

The history of the scientific thought of the Middle Ages and of the Renaissance, now beginning to be somewhat better known,[3] can be divided into two periods. Or better, as the chronological order corresponds only very roughly to that division, the history of scientific thought may be, *grosso modo*, divided into three stages or epochs, which correspond in turn to three different types of thinking: the Aristotelian physics first; then the physics of *impetus*, inaugurated, like everything else, by the Greeks, and elaborated in the current of the fourteenth century by the Parisian nominalists; and finally modern, mathematical, Archimedian or Galilean physics.

It is these stages that we find represented in the works of the young Galileo, which thus not only give us information on the history – or the prehistory – of his thought, on the *mobiles* and motives which dominated and inspired it, but present us at the same time, condensed and as it were clarified by the admirable mind of its author, a striking and deeply instructive picture of the whole history of pre-Galilean physics. Let us briefly follow this story, beginning with Aristotelian physics.

Aristotelian physics is false, of course; and utterly obsolete. Never-

[1] See my *Études galiléennes*, part I, À l'aube de la science classique (Paris: Hermann, 1939).

[2] The sixteenth century, at least its latter half, is the period of the reception of the study and of the gradual understanding of Archimedes.

[3] We owe that knowledge chiefly to the works of P. Duhem (to the works cited above, p. 21 n. 3, must be added: *Les Origines de la statique*, 2 vols. [Paris: 1905], and *Le Système du monde*, 5 vols. [Paris: 1913-17]) and to those of Lynn Thorndike (see his monumental *History of Magic and Experimental Science*, 6 vols. [New York; 1923-41]). See also E. J. Dijksterhuis, *Wal en Worp* (Groningen: 1924).

theless, it is a "physics", that is, a highly though non-mathematically elaborated[1] science. It is not a childish phantasy, nor a brute and verbal restatement of common sense, but a theory, that is, a doctrine which, starting of course with the data of common sense, subjects them to an extremely coherent and systematic treatment.[2]

The facts or data which serve as a basis for this theoretical elaboration are very simple, and in practice we admit them just as did Aristotle. It still seems to all of us "natural" to see a heavy body fall "down". And just like Aristotle or St. Thomas, we should be deeply astonished to see a ponderous body – a stone or a bull – rise freely in the air. This would seem to us pretty "unnatural"; and we would look for an explanation in the action of some hidden mechanism.

In the same way we still find it "natural" that the flame of a match points "up", and that we place our pots and pans "on" the fire. We should be astonished and should seek for an explanation if, for instance, we saw the flame turn about and point "down". Shall we call this conception, or rather this attitude, childish and simple? Perhaps. We can even point out that according to Aristotle himself science begins precisely by looking for an explanation for things that appear natural. Still, when thermodynamics asserts as a principle that "heat" passes from a hot to a cold body, but not from the cold to a hot one, does it not simply translate an intuition of common sense that a "hot" body "naturally" becomes cold, but that a cold one does not "naturally" become hot? And even when we are stating that the centre of gravity of a system tends to take the lowest position and does not rise by itself, are we not simply translating an intuition of common sense, the self-same intuition which Aristotelian physics expresses by its distinction of movement into "natural" and "violent"?[3]

Moreover, Aristotelian physics no more rests content than thermodynamics with merely expressing in its language the "fact" of common sense just mentioned; it transposes it, and the distinction between "natural" and "violent" movements takes its place in a general conception of physical reality, a conception of which the principal features seem to be: (*a*) the belief in the existence of qualitatively determined "natures", and (*b*) the belief in the existence

[1] P. Duhem, "De l'accélération produite par une force constante", *Comptes rendus du IIe congrès international de philosophie* (Genava: 1904).

[2] The systematic character of Aristotelian physics is often not sufficiently appreciated by the modern historian of scientific thought.

[3] See E. Mach, *Die Mechanik in ihrer Entwicklung*, 8th Ed. (Leipzig: Brockhaus, 1921), pp. 124 ff.

of a Cosmos – that is, the belief in the existence of principles of order in virtue of which the entirety of real beings forms a hierarchically ordered whole.

Whole, cosmic order, and harmony: these concepts imply that in the Universe things are (or should be) distributed and disposed in a certain determined order; that their location is not a matter of indifference (neither for them, nor for the Universe); that on the contrary each thing has, according to its nature, a determined "place" in the Universe, which is in some sense its own.[1] A place for everything, and everything in its place: the concept of "natural place" expresses this theoretical demand of Aristotelian physics.

The conception of "natural place" is based on a purely static conception of order. Indeed, if everything were "in order", everything would be in its natural place, and, of course, would remain and stay there forever. Why should it depart from it? On the contrary, it would offer a resistance to any attempt to expel it therefrom. This expulsion could be effected only by exerting some kind of *violence*, and the body would seek to come back, if, and when, owing to such a *violence*, it found itself out of "its" place.

Thus every movement implies some kind of cosmic disorder, a disturbance of the world-equilibrium, being either a direct effect of *violence*, or, on the contrary, the effect of the effort of Being to compensate for the *violence*, to recover its lost and troubled order and balance, to bring things back to their natural places, places where they can rest and remain. It is this returning to order which constitutes precisely what we have called "natural" movement.[2]

Upsetting equilibrium, returning to order: it is perfectly clear that order constitutes a firm and durable state which tends to extend itself indefinitely. There is therefore no need to explain the state of rest, at least the state of a body at rest in its natural, proper place; it is its own nature which explains it, which explains, for instance, the earth's being at rest in the centre of the world. It is obvious likewise that movement is necessarily a transitory state: natural movement ends naturally when it reaches its goal. And as for violent movement, Aristotle is too optimistic to admit that this abnormal status could endure; moreover, violent movement is disorder creating disorder,

[1] It is only in "its" place that a being comes to its accomplishment and becomes truly itself. And that is the reason why it tends to reach that place.

[2] The conceptions of "natural places" and "natural motions" imply that of a finite Universe.

and to admit that it could endure indefinitely would mean, in fact, to abandon the very idea of a well-ordered Cosmos. Aristotle therefore holds the reassuring belief that nothing which is *contra naturam possit esse perpetuum*.[1]

Thus, as we have just said, in the Aristotelian physics movement is an essentially transitory state. Taken literally, however, this statement would be incorrect, and even doubly incorrect. As a matter of fact movement, though it is for *each of the moved bodies*, or at least for those of the sublunar world, for the movable things of our experience, a necessarily transitory and ephemeral state, is nevertheless for the whole of the world a necessarily eternal, and therefore an eternally necessary phenomenon[2] – a phenomenon which we cannot explain without discovering its origin and cause in the physical as well as the metaphysical structure of the Cosmos. Such an analysis would show that the ontological structure of material Being prevents it from reaching the state of perfection implied in the notion of absolute rest, and would enable us to see the ultimate physical cause of the temporary, ephemeral and variable movements of sublunar bodies in the continuous, uniform, and perpetual movement of the heavenly spheres.[3] On the other hand, movement strictly speaking is not a *state*: it is a process, a flux, a *becoming*, in and by which things constitute, actualize and accomplish themselves.[4] It is perfectly true that becoming has Being as its end; and that movement has rest as its goal. Yet this immutable rest of a fully actualized being is something utterly different from the heavy and impotent immobility of a being unable to move itself; the first is something positive, is "perfection and *actus*"; the second is only a "privation". Movement, therefore – a *processus*, a becoming, a change – finds itself placed ontologically between the two. It is the being of everything that changes, of which the being is alteration and modification and which *is* only in changing and in modifying itself. The famous Aristotelian definition of movement – *actus entis in potentia in quantum est in potentia* – which Descartes will find perfectly unintelligible – expresses admirably the

[1] Aristotle, *Physique* (Paris: Société d'Édition des Belles Lettres, vol. I (books I-IV) 1952, vol. II (books V-VII) 1956. Aristotle, *The Physics* (Cambridge, Mass.: Harvard University Press, 1952).

[2] Movement can result only from a previous movement. Therefore every actual motion implies an infinite series of preceding ones.

[3] In a finite Universe the only uniform movement which can persist indefinitely is a circular one.

[4] See Kurt Riezler, *Physics and Reality* (New Haven, 1940).

fact: movement is the being – or the *actus* – of everything which is not God.

To move is thus to change, *aliter et aliter se habere*, to change in itself and in respect to others. This implies on the one hand a term of relation or of comparison, with respect to which the thing moved changes its being or relation; which implies – if we are dealing with local movement[1] – the existence of a fixed point with respect to which the moved moves itself, a fixed unmovable point; which obviously can only be the centre of the Universe. On the other hand the fact that every change, every process needs a cause to explain it, implies that every movement needs a mover to produce it, which, as long as the movement endures, keeps it going. Movement indeed does not maintain itself, as rest does. Rest – a state or a privation – does not need the action of any cause to explain its persistence. Movement, change, any process of actualization (or of decay), and even of continuous actualization or decay cannot dispense with such action. If you remove the cause, movement will stop. *Cessante causa cessat effectus.*[2]

If we are dealing with "natural" movement, this cause, this motor is the very nature of the body, its "form", which seeks to bring it back to its place, and thus keeps the movement going. *Vice versa*, movement which is *contra naturam* requires throughout its duration the *continuous* action of an *external* mover conjoint to the moved. Remove the mover, and the movement will stop. Detach it from the moved, and the movement will equally stop. Aristotle, as we know well, does not admit action at a distance[3]; every transmission of movement implies according to him a contact. Therefore there are only two kinds of such transmission: pressure and traction. To move a body you have either to push or to pull it. There is no other means.

Aristotelian physics thus forms an admirable and perfectly coherent theory which, to tell the truth, has only one flaw (besides that of being false); that of being contradicted by everyday practice, by the practice of throwing. But a theoretician deserving the name does

[1] Local movement – locomotion – is only one, though a particularly important, kind of "motion" (κίνησις), motion in the realm of space, in contradistinction to alteration, motion in the realm of quality, and generation and decay, motion in the realm of being.

[2] Aristotle is perfectly right. No process of change or becoming can dispense with a cause. And if motion, in modern physics, persists by itself, it is because it is no longer a process.

[3] The body *tends* to its natural place, but it is not *attracted* by it.

not allow himself to be troubled by an objection from common sense. If and when he encounters a "fact" that does not fit into his theory, he denies its existence. And if he cannot deny it, he explains it. And it is in the explanation of this everyday fact, the fact of throwing, a movement continuing in spite of the absence of a "mover", a fact apparently incompatible with his theory, that Aristotle gives us the measure of his genius. This answer consists in the explanation of the apparently motorless movement of the projectile by the reaction of the ambient medium, the air, or the water.[1] The theory is a stroke of genius. Unfortunately (besides being false), from the point of view of common sense it is utterly impossible. No wonder therefore that the criticism of Aristotelian dynamics turns always to the same *questio disputata*: *a quo moveantur projecta*?

II

We shall come back in a moment to this *questio*, but we must first turn our attention to another detail of Aristotelian dynamics: the negation of any vacuum and of movement in a vacuum. In this dynamics, indeed, a vacuum does not enable movement to proceed more easily; on the contrary, it renders it utterly impossible; this for very profound reasons.

We have already said that in Aristotelian dynamics, every body is conceived as endowed with a tendency to find itself in its natural place, and to come back to it when, and if, by violence it is moved away from it. This tendency explains its (natural) movement: a movement which brings it to its natural place by the shortest and the speediest way. It follows that every natural movement proceeds in a straight line, and that every body travels to its natural place as fast as possible; that is, as fast as its environment, which resists and opposes its movement, allows it to do. If therefore there were nothing to arrest it, if the surrounding medium did not oppose any resistance to its movement through it (as would be the case in a vacuum) the body would travel to "its" place with an infinite speed.[2] But such a movement would be instantaneous and this – with good reason – seems to Aristotle to be utterly impossible. The conclusion is obvious: no (natural) movement can possibly take place in the void. As for violent movement, that, for example, of throwing, movement in a

[1] See Aristotle, *Physics*, IV, 8, 215 a; VIII, 10, 267 a; *De Coelo*, III, 2, 310 b; É. Meyerson, *Identité et réalité* (Paris: Alcan 1926), p. 84, 3rd Ed.

[2] See Aristotle, *Physics*, VII, 5, 249 b, 250 a; *De Coelo*, III, 2, 301 e.

vacuum would be equivalent to movement without a motor; it is obvious that the vacuum is not a physical medium and cannot receive, transmit and keep up a movement. Moreover, in a vacuum (as in the space of the Euclidian geometry) there are no privileged places or directions. In a vacuum there are not, and there cannot be, "natural" places. Therefore a body put into a vacuum would not know where to go, would not have any reason to move in one direction rather than in any other, and thus would not have any reason to move at all. *Vice versa*, once moved, it would have no more reason to stop here rather than there, and thus it would have no reason to stop at all.[1] Both of which are utterly absurd.

Aristotle is once more perfectly right. An empty space (the space of geometry) is utterly destructive of the conception of a cosmic order: in an empty space there are not only no natural places,[2] there are no *places* at all. The idea of a vacuum is not compatible with the interpretation of movement as change and as process – perhaps not even with that of the concrete movement of concrete "real", perceptible, bodies: I mean the bodies of our common everyday experience. The vacuum is a *non ens*[3]; and to place things in such a *non-ens* is absurd.[4] Geometrical bodies alone can be "placed" in a geometrical space.

The physicist investigates real things, the geometer reasons about abstractions. Therefore, contends Aristotle, nothing could be more dangerous than to mingle together geometry and physics, and to apply purely geometrical method and reasoning to the study of physical reality.

III

I have already mentioned that Aristotelian dynamics, in spite – or perhaps because – of its theoretical perfection, was burdened with an important drawback; that of being utterly implausible and completely unbelievable and unacceptable to plain sound common sense, and obviously contradictory to the commonest everyday experience. No wonder therefore that it never enjoyed universal recognition, and that the critics and adversaries of the dynamics of Aristotle always opposed to it the commonsense fact of the persistence of movement

[1] See Aristotle, *Physics*, IV, 8, 214 b; 215 b.

[2] If one likes it better, one can say that in a vacuum all places are the natural places of every kind of body.

[3] Kant called empty space an "*Unding*".

[4] Such was, as we know, the opinion of Descartes; and of Spinoza.

separated from its original motor. Thus the classical examples of such movement, for instance the continuing rotation of the wheel, the flight of the arrow, the throwing of a stone, were persistently marshalled against it, beginning with Hipparchus and John Philoponos, through John Buridan and Nicole Oresme, down to Leonardo da Vinci, Benedetti and Galileo.[1]

I do not propose to analyse here the traditional arguments which since John Philoponos[2] have been repeated by the partisans of his dynamics. *Grosso modo* they can be classified into two groups: (*a*) the first arguments are material and stress the improbability of the assumption that a big and heavy body, a bullet, a revolving millstone, an arrow flying against the wind, could be moved by the reaction of the air; (*b*) the others are formal and point out the contradiction involved in attributing to the air a double role, that of resistance and that of being a mover, as well as the illusory character of the whole theory which only shifts the problem from the body to the air and is, in fact, obliged to endow the air with the same ability to maintain its movement in spite of its separation from its external cause which it denies to other bodies. If so, they ask, why not assume that the mover transmits to the moved, or impresses it with, something which enables it to move – a something which is called δύναμις, *virtus motiva, virtus impressa, impetus, impetus impressus*, sometimes *forza* or even *motio*, and which is always thought of as some kind of power or force, which passes from the mover to the *mobile*, and

[1] For the history of the medieval criticism of Aristotle see the works cited above, p. 21, n. 3, and B. Jansen, "Olivi, der älteste scholastische Vertreter des heutigen Bewegungsbegriffes", *Philosophisches Jahrbuch* (1920); K. Michalsky, "La physique nouvelle et les différents courants philosophiques au XIVe siècle", *Bulletin international de l'Académie polonaise des sciences et des lettres* (Cracow: 1927); S. Moser, *Grundbegriffe der Naturphilosophie bei Wilhelm von Occam* (Innsbruck: 1932); E. Borchert, *Die Lehre von der Bewegung bei Nicolaus Oresme* (Münster: 1934); R. Marcolongo, "La Meccanica di Leonardo da Vinci", *Atti della reale accademia delle scienze fisiche e matematiche*, XIX (Naples: 1933).

[2] On John Philoponos, who seems to be the real inventor of the theory of the *impetus*, see E. Wohlwill, "Ein Vorgänger Galileis im VI. Jahrhundert", *Physicalische Zeitschrift*, VII (1906), and P. Duhem, *Le Système du Monde*, I. The *Physics* of John Philoponos, not having been translated into Latin, remained inaccessible to the scholastics, who had at their disposal only the brief account given by Simplicius. But it was well known to the Arabs, and the Arabic tradition, directly and through the translation of Avicenna, seems to have influenced the "Parisian" school to a hitherto unsuspected degree. See the very important article of S. Pines, "Études sur Awhad al-Zamān Abu'l Barakat al-Baghdadi", *Revue des Études Juives* (1938).

which then carries on the movement, or better, which produces the movement as its cause.

It is obvious, as Duhem himself recognized, that we are back with common sense. The partisans of the *impetus* physics are thinking in terms of everyday experience. Is it not clear that we need an *effort*, a deployment and an expenditure of force, in order to move a body, for instance in order to push a carriage along its path, to throw a stone or to bend a bow? Is it not clear that it is this force which moves the body, or better, which makes it move? – that it is this force which the body receives from the mover that enables it to overcome resistance (like that of the air) and to strike at obstacles?

The medieval followers of *impetus* dynamics discuss at great length, and without success, the ontological status of *impetus*. They try to fit it into the Aristotelian classification, to interpret it as some kind of *form*, or as a kind of *habitus*, or as a kind of quality such as heat (like Hipparchus and Galileo). These discussions only show the confused, imaginative nature of the conception, which is a direct product or, if one may say so, a condensation, of common sense.

As such it is even more in accord than the Aristotelian view with the "facts" – real or imaginary – which form the experiential basis of medieval dynamics; and particularly with the well-known "fact" that every projectile begins by increasing its speed and acquires the maximum of its velocity some time after its separation from the mover.[1]

[1] It is interesting to note that this absurd belief, shared and taught by Aristotle (*De Coelo*, II, 6), was so deeply rooted and so universally accepted that Descartes himself did not dare to deny it outright, and as so often with him preferred to explain it. In 1630 he writes to Mersenne (A.T., I, 110): "Je voudrais bien aussi sçavoir si vous n'avez point expérimenté si une pierre jettée avec une fronde, ou la bale d'un mousquet, ou un traist d'arbaleste, vont plus viste et ont plus de force au milieu de leur mouvement qu'ils n'en ont au commencement, et s'ils font plus d'effet. Car c'est là la créance du vulgaire, avec laquelle toutefois mes raisons ne s'accordent pas; et je trouve que les choses qui sont poussées et qui ne se meuvent pas d'elles mêmes, doivent avoir plus de force au commencement qu'incontinent après." In 1632 (A.T., I, 259) and once more in 1640 (A.T., II, 37 ff) he explains to his friend what is true in this belief: "*In motu projectorum*, ie ne croie point que le Missile aille jamais moins vite au commencement qu'à la fin, à conter dès le premier moment qu'il cesse d'être poussé par la main ou la machine; mais je crois bien qu'un mousquet, n'estant éloigné que d'un pied et demi d'une muraille n'aura pas tant d'effet que s'il en était éloigné de quinze ou de vingt pas, à cause que la bale, en sortant du mousquet ne peut si aisement chasser l'air qui est entre lui et cette muraille et ainsi doit aller moins viste que si cette muraille estoit moins proche. Toutefois c'est à l'expérience de déterminer si cette différence est sensible et je doute fort de toutes celles que je n'ai pas faites moi-même." Descartes' friend, Beekmann, on the contrary, denies flatly the possibility of an acceleration of the projectile and writes (*Beekmann à Mersenne*, Apr. 30, 1630, see *Correspondance du*

Everybody knows that in order to jump an obstacle one has to "make a take-off"; that a chariot which one pushes, or pulls, starts slowly and little by little increases its speed; it too takes off and gathers momentum; just as everybody – even a child throwing a ball – knows that in order to hit the goal hard he has to place himself at a certain distance from it, and not too near, in order to allow the ball to gather momentum. The physics of *impetus* is not at pains to explain this phenomenon; from its standpoint it is perfectly natural that *impetus* should require some time before it "takes hold" of the *mobile* – just as, for example, heat needs time to permeate a body.

The conception of movement underlying and supporting *impetus* physics is quite different from that of the Aristotelian view. Movement is no longer understood as a process of actualization. Yet it is still a change, and as such it must be explained by the action of a definite force or cause. *Impetus* is just that immanent cause which produces the movement, which is *converso modo* the effect produced by it. Thus the *impetus impressus produces* the movement; it *moves* the body. But at the same time it plays another very important role: it overcomes the resistance opposed by the medium to the movement.

Owing to the confused and ambiguous character of the *impetus* conception, it is rather natural that the two aspects and roles should merge together, and that some of the partisans of the *impetus* dynamics should come to the conclusion that, at least in some special cases such as the circular movement of the heavenly spheres, or, more generally, the rolling movement of a circular body on a level plane, or even more generally in all the cases where there is no external resistance to movement, such as would be the case in a *vacuum*, the *impetus* does not weaken but remains "immortal". This seems to be a close approach to the law of inertia, and it is therefore of particular interest and importance to note that Galileo himself, who in his *De Motu* gives us one of the best expositions of *impetus* dynamics, resolutely denies the possibility of such an assumption, and asserts most vigorously the essentially perishable nature of *impetus*.

Père Mersenne [Paris: 1936], II, 437): "Funditores vero ac pueri omnes qui existimant remotiora fortius ferire quam eadem propinquiora, certo certius falluntur." Yet he admits that there must be something true in this belief and tries to explain: "Non dixeram plenitudinem nimiam aeris impedire effectum tormentorii globi, sed pulverem pyrium extra bombardam jam existentem forsitan adhuc rarefieri, ideoque fieri posse ut globus tormentarius extra bombardam nova vi (simili tandem) propulsus velocitate aliquamdiu cresceret."

Galileo is obviously perfectly right. If movement is understood as the effect of *impetus* considered as its immanent – and not natural – cause, it is unthinkable and absurd not to admit that the cause or force which produces it must necessarily spend and finally exhaust itself in this production. It can never remain unchanged for two consecutive moments, and therefore the movement which it produces must necessarily slow down and come to an end.[1] Thus it is a very important lesson that we learn from the young Galileo. He teaches us that *impetus* physics, though compatible with movement in a *vacuum*, is like that of Aristotle *incompatible* with the principle of inertia. And this is not the only lesson that Galileo teaches with regard to *impetus* physics. The second is at least as valuable as the first. It runs that, like that of Aristotle, the dynamics of *impetus* is incompatible with mathematical treatment. It leads nowhere. It is a blind alley.

Impetus physics, during the thousand years that separate John Philoponos from Benedetti, made very little progress. But in the latter's works, and even more clearly, more consistently and consciously, in those of the young Galileo, we find – under the obvious and unmistakable influence of the "suprahuman Archimedes"[2] a determined attempt to apply to this physics the principles of "mathematical philosophy".[3]

Nothing is more instructive than the study of this attempt – or, more exactly, of these attempts – and of their failure. They show us that it is impossible to mathematize, i.e. to transform into an exact, mathematical concept, the rude, vague and confused conception of *impetus*. In order to build up a mathematical physics following the lines of the statics of Archimedes, this conception had to be dropped altogether.[4] A new and original concept of motion had to be formed and developed. It is this new concept that we owe to Galileo.

IV

We are too well acquainted with, or rather too well accustomed to,

[1] See *De Motu Gravium* in *Le Opere di Galileo Galilei* (Firenze: Edizione Nazionale, 1898), I, pp. 314 ff.

[2] See *De Motu Gravium* in *Le Opere di Galileo Galilei* (Firenze: Edizione Nazionale, 1898), I, p. 300.

[3] J. B. Benedetti, *Diversarum speculationum mathematicarum liber* (Taurini: 1585), p. 168.

[4] The persistence of the terminology – the word *impetus* is used by Galileo and his pupils and even by Newton – must not prevent us from recognizing the disappearance of the idea.

the principles and concepts of modern mechanics, so well that it is almost impossible for us to see the difficulties which had to be overcome for their establishment. They seem to us so simple, so natural, that we do not notice the paradoxes they imply and contain. Yet the mere fact that the greatest and mightiest minds of mankind – Galileo, Descartes – had to struggle in order to make them theirs, is in itself sufficient to indicate that these clear and simple notions – the notion of movement or that of space – are not so clear and simple as they seem to be. Or they are clear and simple only from a certain point of view, only as part of a certain set of concepts and axioms, apart from which they are not simple at all. Or, perhaps, they are too clear and too simple: so clear and so simple that, like all prime notions, they are very difficult to grasp.

Movement, space – let us try to forget for a while all we have learnt at school; let us try to think out what they mean in mechanics. Let us try to place ourselves in the situation of a contemporary of Galileo, a man accustomed to the concepts of Aristotelian physics which *he* learnt at *his* school, and who encounters for the first time the modern concept of motion. What is it? In fact something pretty strange. It is something which in no way affects the body which is endowed with it: to be in motion or to be at rest does not make any difference for, nor any change in, the body in motion or at rest. The body, as such, is utterly and absolutely indifferent to both.[1] Therefore, we are not able to ascribe motion to a determined body considered in itself. A body is in motion only in relation to some other body which we assume to be at rest. All motion is relative. And therefore we may ascribe it to the one or to the other of the two bodies, *ad libitum*.[2]

Thus motion seems to be a relation. But at the same time it is a *state*, just as rest is another *state*, utterly and absolutely opposed to the former; besides which they are both *persistent states*.[3] The famous first law of motion, the law of inertia, teaches us that a body left to itself persists eternally in its state of motion or of rest, and that we must apply a force in order to change a state of motion to a state

[1] In the Aristotelian physics, motion is a process of change and always affects the body in motion.

[2] A given body, therefore, can be endowed with any number of different motions, which do not interfere with each other. In the Aristotelian as well as in the *impetus* physics every motion interferes with every other and sometimes even prevents it from taking place.

[3] Motion and rest are thus placed on the same ontological level, and therefore persistence of *motion* becomes just as self-evident and without need of explanation as persistence of *rest* had previously been.

of rest, and *vice versa*.[1] Yet not every kind of motion is thus endowed with an eternal being, but only uniform movement in a straight line. Modern physics affirms, as well we know, that a body once set in motion conserves eternally its direction and speed, provided of course it is not subject to the action of any external force.[2] Moreover, to the objection of the Aristotelian that though as a matter of fact he is acquainted with eternal motion, the eternal circular motion of the heavenly spheres, he has never yet encountered a persistent rectilinear one, modern physics replies: of course! rectilinear, uniform motion is utterly impossible, and can take place only in a vacuum.

Let us think it over, and perhaps we will not be too harsh on the Aristotelian who felt himself unable to grasp and to accept this unheard-of notion, the notion of a persistent, substantial relation-state, the concept of something which to him seemed just as abstruse, and just as impossible, as the ill-fated substantial forms of the scholastics appear to us. No wonder that the Aristotelian felt himself astonished and bewildered by this amazing attempt to explain the real by the impossible – or, which is the same thing, to explain real being by mathematical being, because, as I have mentioned already, these bodies moving in straight lines in infinite empty space are not *real* bodies moving in *real* space, but *mathematical* bodies moving in *mathematical* space.

Once more, we are so accustomed to mathematical science, to mathematical physics, that we no longer feel the strangeness of a mathematical approach to Being, the paradoxical daring of Galileo's utterance that the book of Nature is written in geometrical characters.[3] For us it is a foregone conclusion. But not for the contemporaries of Galileo. Therefore it is the right of mathematical science, of the mathematical explanation of Nature, in opposition to the non-mathematical one of common sense and of Aristotelian physics, much more than the opposition between two astronomical systems, that forms the real subject of the *Dialogue on the two greatest*

[1] In modern terms: in the Aristotelian and *impetus* dynamics, force produces motion; in modern dynamics, force produces acceleration.

[2] This implies necessarily the infinity of the Universe.

[3] *Il Saggiatore* in *Le Opere di Galileo Galilei* (Firenze: Edizione Nazionale, 1898), VI, p. 232: "La filosofia è scritta in questo grandissimo libro, che continuamente ci sta aperto innanzi a gli occhi (io dico l'universo), ma non si può intendere se prima non s'impara a intender la lingua, e conoscer i caratteri, ne' quali è scritto. Egli è scritto in lingua matematica, e i caratteri son triangoli, cerchi, ed altre figure geometriche, senza i quali mezi è impossibile a intenderne umanamente parola." See *Letter to Liceti* of Jan. 11, 1641 (ibid., XVIII, p. 293).

systems of the world. As a matter of fact the *Dialogue*, as I believe I have shown in my *Études*, is not so much a book on *science* in our meaning of the term as a book on philosophy – or to be quite correct and to employ a disused but time-honoured expression, a book on *natural philosophy* – for the simple reason that the solution of the astronomical problem depends on the constitution of a new Physics; which in turn implies the solution of the *philosophical* question of the role played by mathematics in the constitution of the science of Nature.

The role and the place of mathematics in science is not in fact a very new problem. Quite the contrary: for more than two thousand years it has formed the object of philosophical meditation, inquiry and discussion. And Galileo is perfectly aware of it. No wonder! Even as a young boy, a student in the University of Pisa, he could have learned from the lectures of his master, Francesco Buonamici, that the "question" about the role and the nature of mathematics, constitutes the principal subject of opposition between Aristotle and Plato.[1] And some years later when he came back to Pisa, this time a

[1] The enormous compilation of Buonamici (1011 pages *in folio*) is an invaluable source-book for the study of medieval theories of motion. Though frequently *mentioned* by historians of Galileo it has never been utilized by them. Buonamici's book is very rare. I allow myself therefore to quote it at some length: Francisci Bonamici, Florentini, e primo loco philosophiam ordinariam in Almo Gymnasio Pisano profitentis, *De Motu, libri X, quibus generalia naturalis philosophiae principia summo studio collecta continentur* (Florentiae: 1591), lib. X, cap. XI. *Jurene mathematicae ex ordine scientiarum expurgantur*, p. 56: . . . "Itaque veluti ministri sunt mathematicae, nec honore dignae et habitae προπαιδεία, id est apparatus quidam ad alias disciplinas. Ob eamque potissime caussam, quod de bono mentionem facere non videntur. Etenim omne bonum est finis, is vero cuiusdam actus est. Omnis vero actus est cum motu. Mathematicae autem motum non respiciunt. Haec nostri addunt. Omnem scientiam ex propriis effici: propria vero sunt necessaria quae alicui [?] quatenus ipsum et per se insunt. Atqui talia principia mathematicae non habent. . . . Nullum caussae genus accipit . . . proptereaquod omnes caussae definiuntur per motum: efficiens enim est principium motus, finis cuius gratia motus est, forma et materia sunt naturae; et motus igitur principia sint necesse est. At vero mathematica sunt immobilia. Et nullum igitur ibi caussae genus existit." Ibid., lib. I, p. 54: "Mathematicae cum ex notis nobis et natura simul efficiant id quod cupiunt, sed caeteris demonstrationis perspicuitate praeponentur, nam vis rerum quas ipsae tractant non est admodum nobilis; quippe quod sunt accidentia, id est habeant rationem substantiae quatenus subiicitur et determinatur quanto; eaque considerentur longe secus atque in natura existant. Attamen nonnullarum rerum ingenium tale esse comperimus ut ad certam materiam sese non applicent, neque motum consequantur, quia tamen in natura quicquid est, cum motu existit; opus est abstractione cuius beneficio quantum motu non comprehenso in eo munere contemplamur; et cum talis sit earum natura nihil absurdi exoritur. Quod item confirmatur, quod mens in omni habitu verum dicit; atqui verum est ex eo, quod res ita est. Huc accedit quod Aristoteles distinguit scientias non ex ratione notionum sed entium."

professor himself, he could have learned from his friend and colleague, Jacopo Mazzoni, author of a book on Plato and Aristotle, that "there is no other question which has given place to more noble and beautiful speculations ... than the question whether the use of mathematics in physical science as an instrument of proof and a middle term of demonstration, is opportune or not; in other words, whether it brings us some profit, or on the contrary is dangerous and harmful". "It is well known," says Mazzoni, "that Plato believed that mathematics was quite particularly appropriate for physical investigations, which was the reason why he himself had many times recourse to it for the explanation of physical mysteries. But Aristotle held a quite different view and he explained the errors of Plato by his too great attachment to mathematics."[1]

One sees that for the scientific and philosophical consciousness of the time – Buonamici and Mazzoni are only giving expression to the *communis opinio* – the opposition, or rather the dividing line, between the Aristotelian and the Platonist is perfectly clear. If you claim for mathematics a superior status, if more than that you attribute to it a real value and a commanding position in physics, you are a Platonist. If on the contrary you see in mathematics an abstract science, which is therefore of a lesser value than those – physics and metaphysics – which deal with real being; if in particular you pretend that physics needs no other basis than experience and must be built directly on perception, that mathematics has to content itself

[1] Jacobi Mazzoni, Caesenatis, in Almo Gymnasio Pisano Aristotelem ordinarie Platonem vero extra ordinem profitentis, *In Universam Platonis et Aristotelis Philosophiam Praeludia, sive de comparatione Platonis et Aristotelis* (Venetiis: 1597), pp. 187 ff: "*Disputatur utrum usus mathematicarum in Physica utilitatem vel detrimentum afferat, et in hoc Platonis et Aristotelis comparatio.* Non est enim inter Platonem et Aristotelem quaestio, seu differentia, quae tot pulchris, et nobilissimis speculationibus scateat, ut cum ista, ne in minima quidem parte comparari possit. Est autem differentia, utrum usus mathematicarum in scientia Physica tanquam ratio probandi et medius terminus demonstrationum sit opportunus, vel inopportunus, id est, an utilitatem aliquam afferat, vel potius detrimentum et damnum. Credidit Plato Mathematicas ad speculationes physicas apprime esse accommodatas. Quapropter passim eas adhibet in reserandis mysteriis physicis. At Aristoteles omnino secus sentire videtur, erroresque Platonis adscribet amori Mathematicarum. ... Sed si quis voluerit hanc rem diligentius considerare, forsan, et Platonis defensionem inveniet, videbit Aristotelem in nonnullos errorum scopulos impegisse, quod quibusdam in locis Mathematicas demonstrationes proprio consilio valde consentaneas, aut non intellexerit, aut certe non adhibuerit. Utramque conclusionem, quarum prima ad Platonis tutelam attinet, secunda errores Aristotelis ob Mathematicas male rejectas profitetur, brevissime demonstrabo."

with the secondary and subsidiary role of a mere auxiliary, you are an Aristotelian.

What is in question in this discussion is not certainty – no Aristotelian has ever doubted the certainty of geometrical propositions or demonstrations – but Being; not even the use of mathematics in physical science – no Aristotelian has ever denied our right to measure what is measurable and to count what is numerable – but the structure of science, and therefore the structure of Being.

These are the discussions to which Galileo alludes continuously in the course of his *Dialogue*. Thus at the very beginning Simplicio, the Aristotelian, points out that "concerning natural things we need not always seek the necessity of mathematical demonstrations".[1] To which Sagredo, who allows himself the pleasure of misunderstanding Simplicio, replies: "Of course, when you cannot reach it. But, if you can, why not?" Of course. If it is possible in questions pertaining to natural things to achieve a demonstration possessing a mathematical necessity, why shouldn't we try to do it? But is it possible? That is precisely the problem, and Galileo, in the margin of the book, sums up the discussion and formulates the real meaning of the Aristotelian: "In natural demonstrations," says he, "one must not seek mathematical exactitude."

One must not. Why? Because it is impossible. Because the nature of physical being is qualitative and vague. It does not conform to the rigidity and the precision of mathematical concepts. It is always "more or less". Therefore, as the Aristotelian will explain to us later, philosophy, that is the science of the real, does not need to look at details, nor need it have recourse to numerical determinations in formulating its theories of motion; all that it has to do is to develop its chief categories (natural, violent, rectilinear, circular) and to describe its general qualitative and abstract features.[2]

The modern reader is probably far from being convinced. He finds it difficult to admit that "philosophy" had to content itself with abstract and vague generalization and not try to establish precise and concrete universal laws. The modern reader does not know the real reason of this necessity, but Galileo's contemporaries knew it quite well. They knew that quality, as well as form, being non-mathematical by nature, cannot be treated in terms of mathematics. Physics is

[1] See *Dialogo sopra i due Massimi Sistemi del Mondo*, in *Le Opere di Galileo Galilei* (Firenze: Edizione Nazionale, 1898), VII, p. 38; see p. 256.

[2] Ibid., p. 242.

not applied geometry. Terrestrial matter can never exhibit exact mathematical figures; the "forms" never "inform" it completely and perfectly. There always remains a gap. In the skies, of course, it is different; and therefore mathematical astronomy is possible. But astronomy is not physics. To have missed that point is precisely the error of Plato and of those who follow Plato. It is useless to attempt to build up a mathematical philosophy of nature. The enterprise is doomed even before it starts. It does not lead us to truth but to error.

"All these mathematical subtleties", explains Simplicio, "are true *in abstracto.* But applied to sensible and physical matter, they do not work."[1] In real nature there are no circles, no triangles, no straight lines. Therefore it is useless to learn the language of mathematical figures: the book of Nature, in spite of Galileo and Plato, is not written in them. In fact, it is not only useless, it is dangerous: the more a mind is accustomed to the precision and to the rigidity of geometrical thought, the less it will be able to grasp the mobile, changing, qualitatively determined variety of Being.

This attitude of the Aristotelian is very far from being ridiculous.[2] To me, at least, it seems perfectly sensible. You cannot establish a mathematical theory of quality, objects Aristotle to Plato; not even one of motion. There is no motion in numbers. But *ignorato motu ignoratur natura.* And the Aristotelian of Galileo's time could add that the greatest of the Platonists, the *divus* Archimedes himself,[3] was never able to establish more than a statics. Not a dynamics. A theory of rest. Not one of motion.

The Aristotelian was perfectly right. It is impossible to furnish a mathematical deduction of quality. And well we know that Galileo, like Descartes somewhat later, and for just the same reason, was forced to drop the notion of quality, to declare it subjective, to ban it from the realm of nature.[4] This at the same time implies that he was obliged to drop sense-perception as the source of knowledge and to proclaim that intellectual, and even *a priori* knowledge, is our sole and only means of apprehending the essence of the real.

As for dynamics, and the laws of motion – the *posse* is only to be

[1] Ibid., pp. 229, 423.

[2] As we know, it was shared by Pascal, and even by Leibniz.

[3] It is perhaps worth mentioning that for all the doxographic tradition, Archimedes is a *philosophus platonicus.*

[4] See E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science* (London and New York: 1925).

proved by the *esse*; in order to show that it is possible to establish mathematical laws of nature, you have to do it. There is no other way and Galileo is perfectly conscious of it. It is therefore by giving mathematical solutions to concrete physical problems – the problem of falling bodies, the problem of projectile motion – that he leads Simplicio to the confession "that to want to study natural problems without mathematics is to attempt something that cannot be done".

It seems to me that we are now able to understand the meaning of this significant text of Cavalieri, who in 1630 writes in his *Specchio Ustorio*: "How much is added by the knowledge of the mathematical sciences, which the famous schools of Pythagoreans and Platonists considered supremely necessary for the comprehension of physical things, I hope will shortly become clear with the publication of the new science of movement promised by this marvellous Assayer of Nature, Galileo Galilei."[1]

And we understand too the pride of Galileo the Platonist, who in his *Discourses and Demonstrations* announces that "about a most ancient subject he will promote a quite new science", and will prove something that nobody has proven till then, namely that the movement of falling bodies is subjected to the law of numbers.[2] Movement governed by numbers; the Aristotelian objection had at last met its refutation.

It is obvious that for the disciples of Galileo just as for his contemporaries and elders mathematicism means Platonism. Therefore when Torricelli tells us "that among the liberal disciplines geometry *alone* exercises and sharpens the mind and renders it able to be an ornament of the City in time of peace and to defend it in time of war", and that "*caeteris paribus*, a mind trained in geometrical gymnastics is endowed with a quite particular and *virile* strength",[3] not

[1] Bonaventura Cavalieri, *Lo Specchio Ustorio overo trattato Delle Settioni Coniche e alcuni loro mirabili effetti intorno al Lume* etc. (Bologna: 1632), pp. 152 ff: "Ma quanto vi aggiunga la cognitione delle scienze Matematiche, giudicate da quelle famosissime scuole de' Pithogorici et de' 'Platonici', sommamente necessarie per intender le cose Fisiche, spero in breve sarà manifesto, per la nuova dottrina del moto promessaci dall'esquisitissimo Saggiatore della Natura, dico dal Sig. Galileo Galilei, ne' suoi Dialoghi. . . ."

[2] *Discorsi e dimostrazioni mathematiche intorno a due nuove scienze* in *Le Opere di Galileo Galilei* (Firenze: Edizione Nazionale, 1898), VIII, p. 190: "nullus enim, quod sciam, demonstravit, spatia a mobile descendente ex quiete peracta in temporibus aequalibus, eam inter se retinere rationem, quam habent numeri impares ab unitate consequentes."

[3] Evangelista Torricelli, *Opera Geometrica* (Florentiae: 1644), II, p. 7: "Sola enim Geometria inter liberales disciplinas acriter exacuit ingenium, idoneumque

only does he show himself an authentic disciple of Plato, he acknowledges and proclaims himself to be one. And in doing it he remains a faithful disciple of his master Galileo, who in his *Response to the Philosophical Exercitations* of Antonio Rocco addresses himself to the latter, asking him to judge for himself the value of the two rival methods, i.e. the purely physical and empirical method and the mathematical one, adding: "and decide at the same time who reasoned better, Plato, who said that without mathematics one could not learn philosophy, or Aristotle, who reproached this same Plato for having too much studied Geometry".[1]

I have just called Galileo a Platonist. And I believe that nobody will doubt that he is one.[2] Moreover, he says so himself. In the very first pages of the *Dialogue* Simplicio makes the remark that Galileo, being a mathematician, is probably sympathetic to the numerical speculations of the Pythagoreans. This enables Galileo to declare that he deems them perfectly meaningless, and to say at the same time: "I know perfectly well that the Pythagoreans had the highest esteem for the science of number and that Plato himself admired the human intellect and believed that it participates in divinity solely because it is able to understand the nature of

reddit ad civitates adornandas in pace et in bello defendendas: caeteris enim paribus, ingenium quod exercitatum sit in Geometrica palestra, peculiare quoddam et virile robur habere solet: praestabitque semper et antecellet, circa studia Architecturae, rei bellicae, nauticaeque, etc."

[1] *Esercitazioni filosofiche di Antonio Rocco* in *Le Opere di Galileo Galilei* (Firenze: Edizione Nazionale, 1898), VII, p. 744.

[2] The Platonism of Galileo Galilei has been more or less clearly recognized by certain modern historians of science and philosophy. Thus the author of the German translation of the *Dialogo* notes the Platonic influence (the doctrine of anamnesis) on the very form of the book (see G. Galilei, *Dialog über die beiden hauptsächlichsten Weltsysteme*, aus dem italienischen übersetzt und erläutert von E. Strauss [Leipzig: 1891], p. XLIX); E. Cassirer (*Das Erkenntnisproblem in der Philosophie und Wissenschaft der neuren Zeit* 2nd Ed. [Berlin: 1911], I, pp. 389 ff,) insists upon the Platonism of Galileo's ideal of knowledge; L. Olschki (*Galileo und seine Zeit* [Leipzig: 1927]) speaks about the "Platonic vision of Nature" of Galileo etc. It is E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science* (New York: 1925), who seems to me to have given the best account of the metaphysical substructure (Platonic mathematicism) of modern science. Unfortunately Burtt failed to recognize the existence of *two* (and not one) Platonic traditions, that of mystical arithmology, and that of mathematical science. The same error, which in the case of Burtt was a venial sin, was made by his critic, E. W. Strong, *Procedures and Metaphysics* (Berkeley, Cal.: 1936), and in this case it was a mortal one. On the distinction between the two Platonisms see L. Brunschvicg, *Les Étapes de la philosophie mathématique* (Paris: 1922) pp. 69 ff, and *Le progrès de la conscience dans la philosophie occidentale* (Paris: 1937), pp. 37 ff.

numbers. And I myself am well inclined to make the same judgement."[1]

How could he be of a different opinion, he who believed that in mathematical knowledge the human mind attains the very perfection of the divine understanding? Does he not say that "*extensive*, that is in respect of the multiplicity of things to be known, which is infinite, the human mind is as nothing (even if it understood a thousand propositions, because a thousand compared with infinity is like zero): but taking the understanding *intensive*, in so far as this term means to grasp intensely, that is, perfectly a given proposition, I say that the human mind understands some propositions as perfectly and has of them as absolute certainty as Nature herself can have; and of that kind are the pure mathematical sciences, that is, geometry and arithmetic, of which the divine intellect knows of course infinitely more propositions, for the simple reason that it knows them all; but as for those few understood by the human intellect, I believe that our knowledge equals the divine in objective certainty, because it succeeds in understanding their necessity, beyond which it does not seem that there can exist a greater certainty".[2]

Galileo could have added that the human understanding is so excellent a work of God that *ab initio* it is in possession of these clear and simple ideas of which the very simplicity is a guarantee of truth, and that it has only to turn to itself in order to find in its "memory" the true foundations of science and knowledge, the alphabet, i.e. the elements, of the language – the mathematical language – spoken by the Nature God has created. There is to be found the true foundation of a *real* science, a science of the *real* world – not of a science endowed with a purely formal truth, the intrinsic truth of mathematical reasoning and deduction, a truth which would not be affected by the non-existence in Nature of the objects studied by it: it is obvious that Galileo would no more than Descartes ever rest content with such an *Ersatz* for real science and knowledge.

It is of this science, the true "philosophic" knowledge which is knowledge of the very essence of Being, that Galileo proclaims: "And I, I say to you that if one does not know the truth by himself, it is impossible for anyone else to give him that knowledge. It is in-

[1] See *Dialogo sopra i due Massimi Sistemi del Mondo*, in *Le Opere di Galileo Galilei* VII (Firenze: Edizione Nazionale, 1898), p. 35.

[2] See *Dialogo sopra i due Massimi Sistemi del Mondo*, in *Le Opere di Galileo Galilei* VII (Firenze: Edizione Nazionale, 1898), pp. 128 ff.

deed possible to teach those things that are neither true nor false; but the true, by which I mean necessary things, that is, those for which it is impossible to be otherwise, every average mind either knows by itself, or it is impossible for it ever to learn them."[1] Assuredly. A Platonist cannot be of a different opinion because for him to know is nothing else than to understand.

The allusions to Plato so numerous in the works of Galileo, and the repeated mention of the Socratic maieutics and of the doctrine of reminiscence, are not superficial ornaments born from his desire to conform to the literary mode inherited from the concern of Renaissance thought with Plato. Nor are they meant to gain for the new science the sympathy of the "common reader", tired and disgusted by the aridity of Aristotelian scholastics; nor to cloak himself against Aristotle in the authority of his master and rival, Plato. Quite the contrary: they are perfectly serious, and must be taken at their face value. Thus, that no-one might have the slightest doubt concerning his philosophical standpoint, Galileo insists[2]:

SALVIATI: The solution of the question under discussion implies the knowledge of certain truths that are just as well known to you as to me. But, as you do not remember them, you do not see that solution. In this way, without teaching you, because you know them already, but only by recalling them to you, I shall make you solve the problem yourself.
SIMPLICIO: Several times I have been struck by your manner of reasoning, which makes me think that you incline to the opinion of Plato that *nostrum scire sit quoddam reminisci*; pray, free me from this doubt and tell me your own view.
SALVIATI: What I think of this opinion of Plato I can explain by words, and also by facts. In the arguments so far advanced I have already more than once declared myself by fact. Now I will apply the same method in the inquiry we have in hand, an inquiry which may serve as an example to help you more easily to understand my ideas concerning the acquisition of science. . . .

The inquiry "we have in hand" is nothing else than the deduction of the fundamental propositions of mechanics. We are informed that Galileo judges he has done more than merely declare himself a follower and a partisan of Platonic epistemology. In addition, by applying it, by discovering the true laws of physics, by letting them be deduced by Sagredo and Simplicio, that is, *by the reader* himself,

[1] See *Dialogo sopra i due Massimi Sistemi del Mondo*, in *Le Opere di Galileo Galilei* VII (Firenze: Edizione Nazionale, 1898), p. 183.
[2] See *Dialogo sopra i due Massimi Sistemi del Mondo*, in *Le Opere di Galileo Galilei*, VII (Firenze: Edizione Nazionale, 1898), p. 217.

by *us*, he believes he has demonstrated the truth of Platonism "by fact". The *Dialogue* and the *Discourses* give us the history of an intellectual experiment – of a conclusive experiment, because it ends with the wistful confession of the Aristotelian Simplicio, acknowledging the necessity of the study of mathematics, and regretting that he himself had not learned it in his youth.

The *Dialogue* and the *Discourses* tell us the history of the discovery, or better still, of the rediscovery of the language spoken by Nature. They explain to us the manner of questioning her, i.e. the theory of that scientific experimentation in which the formulation of postulates and the deduction of their implications precedes and guides the recourse to observation. This too, at least for Galileo, is a proof "by fact". The new science is for him an experimental proof of Platonism.

III

Galileo's Treatise *De Motu Gravium:* the use and abuse of imaginary experiment

The law governing the free fall of bodies, which sounded the knell of Aristotelian physics, contains two statements which are independent of each other, even though they were intimately linked together in the mind of Galileo. For this reason, a careful distinction should be made between them.

The first statement concerns the mathematical and dynamic nature of the motion of fall. It asserts that this motion obeys the law of number, and that the distances traversed in successive equal intervals of time are *ut numeri impares ab unitate*[1]; in other words, a constant force, contrary to the teaching of Aristotle, does not produce a uniform motion, but a uniformly accelerated motion[2]: that is to say, the motive force does not produce a velocity, but an acceleration.

The second statement adds that during fall, again contrary to the teaching of Aristotle, all bodies, large or small, heavy or light, that is to say, whatever their size or kind, fall, in principle if not in fact,[3] with the same velocity: in other words, the acceleration during fall is a universal constant.[4]

[1] During free fall, the speed increases in direct proportion to the time, that is to say, proportionally to the natural numbers; the distances traversed in successive equal intervals of time are proportional to the odd numbers; and the distances traversed from the commencement of fall are proportional to the squares of the natural numbers.

[2] In the last analysis, the law governing the fall of bodies implies that of inertia, that is to say, the conservation of motion. In Aristotle's view, such conservation is impossible: motion implies the action of a motive force, a prime-mover attached to the moving body; when the former is detached, the latter stops.

[3] On account of the resistance of the air, heavy and light bodies can acquire the same speed of fall only in a vacuum.

[4] As Kepler did previously, we reduce gravity to terrestrial attraction, and this "constant" depends on the distance of the heavy body from the centre of the earth. For Galileo, who does not admit attraction, the acceleration constant has a universal value. Moreover, this constant is implied by Galileo in the deduction itself of the law of fall.

Historical study of the first of these two statements has frequently been made[1]; on the other hand, the second has been somewhat neglected by historians.[2] However, such a study is rather interesting, not only because it provides a brilliant example of the use – and abuse – by Galileo of the method of imaginary experiment, but also because it enables us to define to some extent the relationship between Galilean thought and that of his immediate, and even more remote, predecessors.

Imaginary experiments, which Mach has called "thought experiments" (*Gedankenexperimente*), and to which Popper has recently directed attention, have played an important part in the history of scientific thought.[3] This fact is easily understood. Real experiments are often very difficult to carry out; and just as often involve complicated and expensive equipment. Furthermore, they are necessarily attended by a certain lack of precision, and so by a certain element of doubt.

It is impossible in practice to produce a plane surface which is truly plane; or to make a spherical surface which is so in reality. Perfectly rigid bodies do not, and cannot, exist *in rerum natura*; nor can perfectly elastic bodies; and it is not possible to make an absolutely correct measurement. Perfection is not of this world: no doubt we can approach it, but we cannot attain it. Between empirical fact and theoretical concept there remains, and will always remain, a gap that cannot be bridged.

That is where imagination appears on the scene. It cheerfully closes the gap. It is not embarrassed by the limitations imposed on us by reality. It "achieves" the ideal, and even the impossible. It operates by means of theoretically perfect concepts, and these are the very concepts that are brought into play by the imaginary experiment.[4] Thus, it rolls perfect spheres on perfectly smooth, perfectly hard planes; it hangs weights from perfectly rigid weightless levers; it causes light to be emitted from point sources; it sends bodies to move eternally in infinite space; it times Galilean reference systems in

[1] Recently by myself: see *Études galiléennes*, part II, La loi de la chute des corps (Paris: Hermann, 1939).

[2] They usually limit themselves to the "experiment at the Leaning Tower of Pisa", which Galileo never performed and never mentions; see my "Galilée et l'expérience de Pise", *Annales de l'Université de Paris*, 1937; Lane Cooper, *Aristotle, Galileo and the Tower of Pisa* (New York, Ithaca: 1935).

[3] See K. Popper, *The Logic of Scientific Discovery* (New York: 1959), App. XI, pp. 442 ff.

[4] The part played is thus intermediate between the mathematical and the real.

inertial motion with synchronous watches; it hurls photons one at a time on to a screen pierced with a single slit, or perhaps two. Having done this, it obtains results of perfect precision; though it is not prevented thereby from being wrong sometimes, at least, with respect to *rerum natura*. Undoubtedly, it is on account of these perfect results that imaginary experiments so often underlie the fundamental laws of the great systems of natural philosophy, such as those of Descartes, Newton, Einstein . . . and Galileo too.

Let us go back to Galileo, and particularly to the first book of the *Discorsi e dimostrazioni matematiche intorno a due nuove scienze*, which like the *Dialogo . . . sopra i due massimi sistemi del mondo Tolemaico, e Copernicano*, is a friendly conversation between three symbolic characters: Salviati, who represents the new science and is the spokesman for Galileo; Sagredo, the *bona mens*, open-minded and free from scholastic prejudices, who is able for this reason to understand, and receive instruction from, Salviati; and Simplicio, supporter of university tradition dominated by Aristotle, whose views he defends, though not with great fervour.[1]

After having talked about various matters,[2] they start to discuss the fall of heavy bodies. In order to disprove the Aristotelian assertion that the speed of free fall of bodies is proportional to their weight, and inversely proportional to the resistance offered by the media in which they move (hence the impossibility of motion in a vacuum), Galileo first of all makes this statement through Simplicio (the spokesman for Aristotelianism), and then opposes it through Sagredo by the results of a genuine experiment, and through Salviati by the results of an imaginary experiment.[3]

SIMPLICIO: Aristotle, as far as I remember, inveighs against some of the ancients, who used to introduce the idea of the vacuum as a necessary prerequisite for motion, saying that motion could not take place without

[1] The first two characters are not only symbolic, but also real persons. Sagredo (1571-1620) was a Venetian, Salviati (1582-1614) was a Florentine. Both were friends of Galileo, who wished to perpetuate their memory in this manner. Simplicio, however, is purely symbolic. It is rather unlikely that Galileo, when he chose the name, had in mind Simplicius, the great commentator of Aristotle; it is more likely that he wanted to show that the Aristotelian mind is, by definition, over-simple; or perhaps, by a play on the similarity of names, he wanted to suggest that the spiritual descendents of *Simplicius* were *over-simple*.

[2] Cohesion; resistance of materials to fracture (title of the First Day); vacuum; paradoxes concerning infinity (Aristotle's wheel); experiment to show that the propagation of light is not instantaneous, but requires time; etc.

[3] See *Discorsi e dimostrazioni matematiche intorno a due nuove scienze*, in *Le Opere de Galileo Galilei*, VIII (Firenze: Edizione Nazional, 1897), pp. 105 ff.

it. Aristotle, opposing this view, shows that, on the contrary, the fact of movement proves (as we shall see) that a vacuum is inconceivable; and his argument is as follows. He supposes two cases: the first, of objects of different weight moving in the same medium; the second, of the same object moving in different media. In the first case, he supposes that objects of different weight move in the same medium at different speeds, which are themselves in the same proportion as the weights; so that, for instance, an object ten times as heavy as another should move ten times as fast.[1] In the other case, he assumes that the speeds of the same object in different media are in inverse proportion to the thicknesses or densities of the media; so that, for example, if the resistance of water were ten times greater than that of air, he would say that the speed in air would be ten times that in water.[2] He uses this second case to prove his point, in the following way. As the tenuity of vacuum is infinitely greater than that of any medium filled with matter, however rare, any body which moved for a certain distance in a certain time in a medium filled with matter should, in a vacuum, move instantaneously[3]; but instantaneous motion is impossible; consequently the idea of a vacuum, owing to movement, is impossible.

SALVIATI: The argument is plainly *ad hominem* – directed, that is, against those who propose a vacuum as necessary for motion: so that if I agree that the argument is conclusive, and that motion in a vacuum is impossible, the conception of the vacuum, taken in the absolute sense and not in relation to movement, is still not invalidated.[4] But to reply as the ancients themselves might have replied, it seems to me that in order to see how much Aristotle's argument really proves, one might reject his assumptions, indeed deny them both. And as for the first assumption, I very much doubt whether Aristotle ever tried to find out whether it is true that two stones, one ten times as heavy as the other, allowed to fall at the same instant from a height of, say, a hundred cubits, would fall at such different speeds that when the larger one reached the ground the other would have fallen only ten cubits.

SIMPLICIO: You can even see from the words he uses that he must have tried it, because he says: "*We see the heavier*"; now, that word "see" implies that he must have tried it.

SAGREDO: But I, Signor Simplicio, have myself tried it, and I assure you that a cannon-ball weighing a hundred, two hundred pounds, or even more, will not reach the ground a palm's breadth sooner than a musket-ball weighing half a pound – even from a height of two hundred cubits.[5]

[1] Assuming that the medium offers a constant resistance, we have $V_1 = P_1/R$ and $V_2 = P_2/R$.

[2] $V_1 = P/R_1$ and $V_2 = P/R_2$. In general $V = P/R$, always assuming that $P > R$.

[3] $V = P/O = \infty$. The speed in vacuum would be infinite.

[4] Galileo admitted the existence, not only of infinitesimal voids, which explain the cohesion of bodies, but also finite voids, such as those produced by a suction pump.

[5] It is very doubtful if Sagredo ever carried out these experiments. The first to be made in a systematic manner seem to be those of Riccioli and Mersenne; see "An Experiment in Measurement", below.

SALVIATI: Anyway, without further experiment, we can clearly prove by a brief, conclusive argument that it is not true that a heavier body moves faster than a lighter one – meaning always bodies of the same material, in fact the ones discussed by Aristotle. Tell me, Signor Simplicio, whether you admit that every falling body has a speed determined by nature, which cannot be increased except by applying violence or presenting some obstacle to its movement.

SIMPLICIO: There can be no doubt that the same body, in the same medium, will have a speed which is determined and appointed by nature, and cannot be increased except by conferring some new impetus to it, nor diminished save by some impediment slowing it down.

SALVIATI: If therefore we have two bodies, whose natural speeds are unequal, it is evident that if we join the slower to the faster, the faster will be partially slowed by it, and the slower hastened to some extent by the other, faster body. Are you not with me in this opinion?

SIMPLICIO: It does seem to me that that must undoubtedly follow.

SALVIATI: But if this is so, and if a large stone moves at a speed of, for instance, eight degrees, and a smaller one at a speed of four, on joining them the composite of the two will move at a speed less than eight: but the two stones, when joined together, make a stone larger than the first stone which moved at eight degrees of speed. Consequently this larger stone (although it is larger than the first stone by itself) will move more slowly than the first on its own, which was smaller; which is against your supposition. Hence you see how from the supposition that the heavier body moves faster than the lighter, I conclude that it moves more slowly.[1]

Simplicio is quite bewildered. Is it not obvious that a smaller stone added to a larger one increases the weight, and therefore increases the speed as asserted by Aristotle? But Salviati silences him by proclaiming that it is not true that a small stone added to a larger one increases the weight of the latter, as it does when at rest. The truth is that a distinction must be made between heavy bodies in motion and the same bodies at rest.[2]

A large stone placed in a balance does not only acquire extra weight when another stone is placed on top of it – for even the addition of a plume of flax will make it weigh the six or ten ounces more that the flax weighs; however, if you let the stone, tied to the flax, fall freely from a height, do you believe that, in motion, the flax would weigh on the stone, thus accelerating its motion, or do you think that, by partially sustaining it, the flax would slow it down? We feel a weight on our shoulders when we try

[1] It is interesting to note that this argument was put forward by Galileo in his youthful work *De Motu Gravium* (see *Le Opere di Galileo Galilei*, I (Firenze: Edizione Nazionale, 1898), p. 265) written probably about 1590, though he did draw the conclusion that the speed of fall of heavy bodies is the same.

[2] See *Le Opere di Galileo Galilei*, VIII (Firenze: Edizione Nazionale, 1898), pp. 108 ff.

to oppose the movement that the weight we are carrying is trying to make; but if we were to fall at the speed at which the weight would naturally fall, how do you imagine it could press or weigh down upon us? Do you not see that this would be like trying to lance a man running ahead of you at a speed as great as or greater than that at which you are following?[1] You may conclude, therefore, that in free and natural fall the smaller stone does not weigh down upon the larger, and does not in consequence increase its weight, as it does at rest.

Simplicio, however, does not give in:

Let us say, then, that the smaller stone does not weigh down upon the larger. But what if you put the larger on top of the smaller?

Of course–replies Salviati[2]–it would add to the weight (of the little stone), if it moved faster: but it has already been concluded that if the smaller were slower it would partially retard the speed of the larger, so that the combination of the two would move more slowly although greater; which is contrary to your assumption. We may therefore conclude that bodies large and small, if they are of the same specific gravity, move at equal speed.

It is most curious that Galileo should mention specific gravity at this point, where it is not relevant to the argument. Historically, however, it is very important, because it reveals the inspirational source of Galileo's reasoning, not only in the passage just quoted but also in the one that I shall quote shortly. The source is Giambattista Benedetti.[3] As early as 1553 in his *Resolutio omnium Euclidis problematum*[4] Benedetti, for the purpose of analysing the fall of heavy bodies, substituted an Archimedean scheme for that of Aristotle; and Galileo did the same, as we shall see shortly. In the dedicatory preface to the fore-mentioned work Benedetti wrote:

I now assert that if there are two bodies of the same form[5] and species (specific gravity), either equal or unequal, then, if in the same medium, they will be borne over an equal space in an equal time. This proposition is most

[1] It is amusing to note that this striking example was later used by Stefano degli Angeli in his polemic with Riccioli; see my "*De Motu Gravium* . . .", *American Philosophical Society, Transactions*, 1955.

[2] *Le Opere di Galileo Galilei*, VIII (Firenze: Edizione Nazionale, 1898), p. 109.

[3] The influence of Benedetti on Galileo has already been emphasized by G. Vailati: see "Le Speculazioni di Giovanni Benedetti sul Moto dei Gravi", *Scritti* (Firenze: 1911), pp. 161 ff; and more recently R. Giacomelli, *Galileo Galilei giovane e il suo "De Motu"* (Pisa: 1949).

[4] *Resolutio omnium Euclidis problematum aliorumque una tantum modo circini apertura* (Venetiis: 1553). For G. B. Benedetti, see my study "Jean Baptiste Benedetti, critique d'Aristote", *Mélanges offerts à Étienne Gilson* (Paris: 1959); and earlier my *Études galiléennes*, I and II, where there is a bibliography of this author; see also the works of Vailati and Giacomelli cited in the previous note.

[5] If they were not, their shape would affect their motion.

evident, because, if they did not move in equal times, then they would necessarily be of different species . . ., or else the medium would not be uniform or the spaces would be unequal.[1] . . .

Benedetti, as well as Galileo, considered the simultaneous fall of large and small heavy bodies (of identical kind, i.e. of the same specific gravity) to be contrary to Aristotle's teaching. They were unquestionably right. Aristotle, in actual fact, taught that large stones fall more rapidly than small ones.[2] Nevertheless, we might ask ourselves if Simplicio were not wrong to accept Salviati's reasoning so easily, and to allow himself to be confounded by the paradoxical "experiment" of a body weighed down by the addition of another moving slower than, or as quickly as, the former. Could not he, ought not he, to have answered Salviati by saying that in his analysis of the fall he had neglected a factor of fundamental, and vital, importance, namely, the resistance to motion? All motion, in fact, implies action and resistance; and he had furthermore admitted, as a matter of course, that the weight of an *assembly of bodies* behaves in the same way with regard to the assembly as the weight of a single body does with regard to its own weight. Could not he have said, for example, that Benedetti's "experiment" as put forward in his book *Diversarum speculationum mathematicarum et physicarum liber*[3] was an excellent one, but of no value as an argument against Aristotle?[4] In the experiment in question two equal bodies of identical material are first of all allowed to fall singly, and afterwards made to fall together, being tied together with a mathematical string. It was concluded, that there is no reason why they should fall more rapidly in the second instance; that is to say, more rapidly together than separately. In fact, the two bodies in question remain *two* bodies whether they are tied together or not; and they do not constitute a *single body*. Two similar horses connected by a bridle do not make one horse of twice the size, and the two together do not run twice as fast as each of them, but with the same speed. Even if we regard Bene-

[1] Benedetti published the preface separately at Venice in 1554 under the title, *Demonstratio proportionum motuum localium contra Aristotelem*; it is very rare. It was republished by G. Libri in the third volume of his *Histoire des sciences mathématiques en Italie* (Paris: 1838), pp. 258 ff. The passage quoted here will be found on p. 261. I have reproduced it in translation in my article on Benedetti cited above.

[2] He was right.

[3] *Diversarum Speculationum Mathematicarum et Physicarum Liber* (Taurini: 1585).

[4] Ibid., p. 174; see p. 371 of my article on Benedetti cited above.

detti's two bodies as one single body, the latter would have no real reason to move faster than either body by itself; neither in a vacuum, where the speed in any case would be infinite, nor in a plenum, because then they would incur twice the resistance.[1] Now, the speed being proportional to the force, or inversely proportional to the resistance, it would be the same in both cases.[2]

As for Salviati's "experiment", Simplicio could have answered that a bundle of straw attached to a cannon-ball remains a bundle of straw, in the same way that a cannon-ball remains a cannon-ball; and if the bundle of straw falls slowly by itself, whilst the cannon-ball falls rapidly, it is reasonable, and by no means contrary to the teaching of Aristotle, to conclude that when the two are tied together the cannon-ball will accelerate the motion of the bundle of straw, and the latter will retard the motion of the former, in spite of the fact that the weight of the assembly is greater than that of the individual components, particularly of the cannon-ball. An assembly comprising a bundle of straw and a cannon-ball is not a cannon-ball of greater weight, because the assembly is not a natural object. Again, in the hypothetical reply to Benedetti, Simplicio could have added that even if we do persist, contrary to common-sense and the teaching of Aristotle, in attributing to an assembly behaviour which is valid only for its components, then we should have taken account of the fact that, the volume of the assembly with respect to the cannon-ball having been increased to a greater extent than the weight of the latter, the resistance of the assembly to motion will have been increased to a greater extent than its increase in weight. Consequently, it is quite normal (and yet again in agreement with Aristotle's dynamics), if the ratio of the motive force to the resistance decreases, for the motion, i.e. the speed, to decrease in the same proportion.

Simplicio could have said all that, or something similar. It is rather a pity that he did not. The Aristotelian standpoint would have been clarified thereby, without, however, being strengthened. Salviati, in his turn, by invoking the case of the hen's egg and the marble egg, as he does in a slightly different context,[3] would have been able to retort that proportionality between speed and weight is not affected by taking this resistance into account, because the resistance is the same

[1] If they were to be considered as connected by a physical rod, this would offer additional resistance to the ambient air.

[2] Two men holding hands do not fall any faster; neither in fact, nor according to the teaching of Aristotle.

[3] *Vide infra*, p. 55.

for both bodies (the hen's egg and the marble egg), and so does not enter into the argument. The speed of fall of the said bodies is not proportional to their weight. In fact, instead of moving much slower than the marble egg, the hen's egg moves almost as rapidly as the former, and reaches the ground in nearly the same time.

If, in his *reductio ad absurdum* criticism of Aristotle's dynamics, which we have just examined, Galileo has not taken into account the resistance offered by the medium to a falling body, we must not conclude therefrom that he has in general failed to recognize the part that it plays in dynamics. Quite the contrary! By criticizing the Aristotelian concept of the relationship between force and resistance he was led to prove, by means of an imaginary experiment, not only the possibility of motion in a vacuum, but also the fact that all bodies fall *in vacuo* with the same velocity, and that it is precisely the resistance of the medium that provides the reason why they do not do so in a plenum.

Why, then, has he neglected so far to speak of it? Perhaps it was because he had set forth Aristotelian dynamics as being based on two axiomatic principles: (*a*) that speed is proportional to motive force, and (*b*) that it is inversely proportional to the resistance; and having done so, he thought it necessary to criticize them separately[1]; perhaps also, because this resistance of the air is usually trivial, and may in fact be neglected. Indeed, when Simplicio, instead of presenting Salviati with the arguments that we have been obliged to develop for him, restricts himself in spite of all arguments to the remark that he cannot believe that a leaden shot falls just as rapidly as a cannon-ball, he draws upon himself the following tirade from Salviati[2]:

SALVIATI: You should have said: "A grain of sand as swiftly as a grindstone." But I would not wish you, Signor Simplicio, to do what many others do, and divert the argument from the main point by attacking some statement of mine that is a hair's breadth from the truth, and hide beneath this hair's breadth the error of another, which is as big as a ship's cable. Aristotle says: "An iron ball weighing a hundred pounds, falling from a height of a hundred cubits, reaches earth before one weighing one pound has fallen even a single cubit." I say they arrive at the same time: you find, on carrying out the experiment, that the larger arrives two fingers' breadths before the smaller – that is, when the larger strikes the ground, the smaller

[1] His own theory takes account of these two factors.

[2] *Discorsi*, First Day, *Le Opere di Galileo Galilei*, VIII (Firenze: Edizione Nazionale, 1898), p. 110.

is two fingers' breadths from the ground: now you are trying to hide behind these two fingers' breadths the ninety-nine cubits of Aristotle, and by speaking only of my slight error, to pass over his, which is enormous, in silence. Aristotle declares that bodies of different heaviness move in the same medium (in so far as their motion depends on gravity) at speeds proportionate to their weights, and he gives as examples bodies in which the pure and absolute effect of weight can be discerned, neglecting, as of minimal importance, other considerations such as shape, which are greatly affected by the medium, which alters the simple effect of gravity alone: which is why gold, the heaviest of all materials, when reduced to a very thin leaf, floats through the air; and stones ground to a very fine powder do the same. But if you want to maintain the general proposition you must show that the proportionality of speeds can be seen in all heavy bodies, and that a stone of twenty pounds moves ten times as fast as one of two.

The resistance of the medium does, then, play some effective part in determining the speed of fall. Aristotle, in admitting it, was not entirely mistaken. Nevertheless, he made a great error in saying that the speed of fall of a heavy body is inversely proportional to the resistance, that is to say, to the density, of the medium in which the fall takes place. This error involves inadmissible consequences. In fact, Salviati continues with these words[1]:

If it were true that in media of different degrees of thinness and rarefaction, in other words of different degrees of resistance, like, for instance, air and water, the same body moved faster in air than in water, according to the proportion between the rarity of air and water, it would follow that any body which fell in air would fall in water as well: which is false, in that a great many bodies fall in air, which in water not only do not fall, but rise upwards.

Simplicio does not rightly understand Salviati's reasoning. Furthermore, he thinks it is unwarranted, seeing that Aristotle was concerned only with the fall of bodies in two media (water and air), and not with bodies that fall in one and rise in the other.

Taken literally, Simplicio's objection is undoubtedly rather weak; and Salviati is quite justified in bringing home to him the fact that he is making a bad defence of his master. As a matter of fact, Simplicio ought to have replied – as he had already done[2] – that the physics of Aristotle is not mathematical physics, and for this reason the formulae of proportionality that it puts forward are not to be taken

[1] Ibid., p. 110; see the same argument in *De Motu Gravium*, *Le Opere di Galileo Galilei*, I (Firenze: Edizione Nazionale, 1898), pp. 263 ff.

[2] See *Dialogo . . . sopra i due Massimi Sistemi del Mondo, Tolemaico, e Copernicano*, First Day, *Le Opere di Galileo Galilei*, VII (Firenze: Edizione Nazionale, 1898), p. 38; Second Day, p. 242.

literally in the mathematical sense, because they are in fact only qualitative and vague; they are approximations.[1] Galileo, of course, knew this quite well. No doubt he thought it unnecessary to pursue the matter in the *Discorsi*, seeing that he had already treated the general problem of the mathematization of physical science in his *Dialogo*.[2] He could have added that he was not alone in accepting Aristotle's pseudo-mathematical expressions literally, and that Aristotle's commentators had done so long before.[3] Salviati then proceeds to demonstrate the absurd, and even contradictory, conclusions from Aristotle's thesis by means of a "concrete" example.

But tell me, then – he says to Simplicio[4] – whether there is some proportion between the grossness of the water – or whatever it is that retards the motion – and the grossness of air; and if there is, assign one at your pleasure.

There is – replies Simplicio – and let us say it is tenfold; and that moreover the speed of a body which falls in both elements is ten times slower in water than in air.

Let us now take one of those bodies which falls in air but not in water, continues Salviati, such as a ball of wood, and I ask you to assign to it whatever speed you please for its fall through the air.

[1] Fundamentally, Simplicio is right. By substituting a strictly quantitative scheme for Aristotle's semi-qualitative ideas, we are obliged to add "more or less" to the conclusions, whereby the meaning is considerably altered; concerning this aspect see my *Études galiléennes*, III, pp. 120 ff; and more recently, E. J. Dijksterhuis, "The Origins of Classical Mechanics" in *Critical Problems in the History of Science* (Madison, Wisconsin: 1959).

[2] See *Dialogo*, First Day, *Le Opere di Galileo Galilei*, VII (Firenze: Edizione Nazionale, 1898), pp. 38 ff; Second Day, pp. 229 ff; 242 ff; Third Day, pp. 423 ff; also *Il Saggiatore*, *Le Opere di Galileo Galilei*, VI (Firenze: Edizione Nazionale, 1898), p. 232, etc.

[3] Medieval critics as well had pointed out the following contradictory considerations. When the force equals the resistance, the speed is unity ($V = P/R$; if $P = R$, then $V = 1$); now, it is obvious that, if the resistance equals the force, there can be no motion. The formula speed = force/resistance itself implies something still more absurd, namely, that any force, no matter how small, always produces motion, no matter how great the opposing resistance. Since the time of Averroes, sundry formulae have been proposed to take account of these circumstances, especially one that expresses speed as proportional, not to the force, but to the excess of the force over the resistance – a formula which is very similar to that adopted by Benedetti (*vide infra*). Bradwardine, too, adopted one which was more complicated, and in modern notation is equivalent to a logarithmic function. See Marshall Clagett, *Giovanni Marliani and late Medieval Physics* (New York: 1941), pp. 129 ff; Anneliese Maier, *Die Vorläufer Galileis im XIV Jahrhundert* (Rome: 1949), pp. 81 ff; Marshall Clagett, *The Science of Mechanics in the Middle Ages* (Madison, Wisconsin: 1959).

[4] *Discorsi*, p. 111. [*Publisher's note*: The following passage was to some extent paraphrased by Professor Koyré from the original Italian. The spirit of this paraphrase has here been reproduced in this editorial retranslation from the Italian.]

Let us say that it moves with a speed of twenty, Simplicio proposes.

Then, concludes Salviati, the body should, to conform with Aristotle's assumption, have fallen in water at a speed of two, instead of floating up from the bottom, as it actually does: *vice versa*, a body, heavier than wood, which in water would fall at a speed of two, ought to fall in air at a speed of twenty – that is to say at the speed of the wooden ball – which contradicts Aristotle's teaching concerning the proportionality of speed and weight. Besides, everyday experience is there to show us that Aristotle's assertion is false, and that the relation between the speed of objects falling in water is very different from that between the speed of their falling in air. For example,[1] a marble egg will fall in water a hundred times faster than a hen's egg, which, falling through the air from a height of twenty cubits, will not arrive so much as four fingers' breadths before it; and, in short, such a body will take three hours to reach the bottom of ten cubits of water, while in the air it would take one or two pulse-beats to traverse a similar distance, and another (like a ball of lead) would easily traverse [the ten cubits of water] in less than twice the time [required to fall through the air].

It follows, therefore, that the Aristotelian objection to the possibility of motion in a vacuum, based on an increase in speed proportional to the decrease in resistance, is not valid: the speed in a vacuum will never be infinite.[2]

We know that the possibility of motion in a vacuum had already been asserted by Benedetti, who came to the same conclusion by a critical examination of Aristotelian "proportionality". Benedetti, however, proved, as did Galileo also, that the motive force, being taken as equal to, or identical with the weight, should be reduced, and not divided, by the value of the resistance. Hence he concluded that bodies would fall in a vacuum with speeds proportional to their specific gravities; and not all with the same speed, as stated by Galileo. It is interesting to note that Sagredo (without acknowledgement) invokes Benedetti's argument, which as we have seen seemed to have the support of Salviati.[3]

You have clearly shown – he says – that it is not true that bodies of different weight move in the same medium with speeds proportional to their weights, but at the same speed – meaning always bodies of the same material, or the same specific gravity (because I do not think you mean to

[1] Ibid., p. 112.

[2] Ibid., p. 112.

[3] Ibid., pp. 112 ff. It was a regular pedagogic practice of Galileo to make the reader trace the phases of his own thought, fall into the same errors that he himself had fallen into, and then free him from them. Sagredo is always allotted the intermediate phase, and Salviati the final.

conclude that a ball of cork moves at the same speed as a ball of lead); and now that you have also clearly demonstrated that it is not true that the same body moves in media of different resistance with speed or slowness proportional to the resistance, I should be extremely pleased to hear what proportions are actually observed in each case.

We know perfectly well that Galileo intends to prove precisely that which Sagredo finds unbelievable – as it was also to him formerly – namely, that a ball of cork and one of lead do not fall with different speeds, but with the same speed, in a vacuum. This was an astonishing proposition; and he would have had more justification for claiming that it, rather than his law of the acceleration of falling bodies, had not been put forward by anyone previously.[1] It is, moreover, interesting to analyse his proof closely, particularly as it reveals the development of his thought.[2]

After having assured myself that it is not true that the same body moving in media of different resistance keeps its speed proportional to the yieldingness of the media; nor that bodies of different weight maintain, in the same medium, speeds in proportion to their gravity (meaning bodies of different specific gravity as well), I began to combine these two phenomena together, and observe what happened to bodies of different gravity placed in media of different resistance: and I noticed that the inequality of speeds was actually greater in more resistant media than in more yielding ones. This difference was such that, if two bodies fall through the air with only very slightly different speeds, in water one will fall ten times as fast as the other; in fact, the one which falls swiftly in air, in water not only will not fall, but will remain absolutely motionless, and what is more, move upwards: because there are some kinds of timber – perhaps a knot or a root – which in water remain motionless, and in the air fall rapidly.

This mention of bodies that remain in equilibrium in water gives rise to a digression which interrupts the line of argument, so it will not be considered for the moment. The digression is of interest because it provides Sagredo and Salviati with the opportunity to discourse on rather surprising experiments relating to hydrostatical equilibrium, as well as other matters.[3] Salviati continues as follows[4]:

[1] The rule for finding the distance traversed by a body, or a point, in uniform accelerated motion was known in the Middle Ages, first at Oxford, then at Paris, from the first half of the fourteenth century. It was even applied to the motion of fall by Dominico Soto in the sixteenth century. See the well-known works of P. Duhem, *Études sur Léonard de Vinci*, 3 vols. (Paris: 1908-13); his *Système du monde*, VII and VIII (Paris: 1956 and 1958); and the works quoted on p. 54, n. 3.

[2] *Discorsi*, p. 113.

[3] See the appendix to this paper.

[4] *Discorsi*, p. 116.

We have already seen that the difference between the speeds of bodies of different gravity becomes outstandingly greater as the resistance of the medium increases; but what is more, in the medium of quicksilver, gold not only sinks more swiftly than lead, but is the only substance that *does* fall – all other metals and stones rise to the surface and float on it; whereas with balls of gold, lead, copper, porphyry, or other heavy materials, the difference between the way they move through the air is almost indistinguishable – for a gold ball, at the end of a descent of a hundred cubits, will not arrive four fingers' breadths before one of copper. Seeing, as I say, that this is so, I immediately fell to the conclusion that if the resistance of the medium were removed, all materials would fall at the same speed.

Galileo's argument is thus put forward as a kind of progression to the limiting case. Two sets of magnitudes, one the resistance of the media in which heavy bodies move, the other the difference in their speeds, develop in a consistent manner; the greater the resistance, the greater the difference in speeds; and conversely, as the former decreases, so the latter decreases.[1] If we suppress the former, then most probably we shall see the latter disappear also.

Of course, this proof is not satisfactory logically; and the Aristotelian was not entirely wrong in declining to accept it as such.[2] To prove the Galilean supposition, an experiment is necessary. But that is impossible; we cannot work in a vacuum. So Galileo found himself obliged to reverse the procedure and show that, by assuming that heavy bodies, in accordance with his hypothesis, fall with the same speed in a vacuum, he can predict the results of actual experiment; and furthermore that he can explain the true part played by resistance in the effective retardation of motion. He continues thus[3]:

We are trying to find out what would happen to bodies of very different weights in a medium of no resistance, so that any difference of speed which was found would have to be attributed to the difference of weight alone; and since only a space completely empty of air or of anything else, even of a thin and yielding nature, would be able to give us a visible demonstration of what we are seeking – and we lack such a space – we shall try to observe what happens in the thinnest and least resistant media, compared to what we see occurring in others less thin and more resistant. If we find, in fact, that bodies of different weight differ less and less in speed as they are placed in less and less resistant media – and that, finally, in the finest medium of all, even if it is not a vacuum, the difference in speed, even between bodies of very different weight, is extremely small, – almost inappreciable –

[1] In the Aristotelian scheme this difference – proportional to the weight of the falling body and the density of the medium – should remain unchanged.

[2] Especially as it contradicts Benedetti's theory, which, at first sight, is so seductive.

[3] *Discorsi*, p. 117.

it seems to me that it would be a very safe guess to believe that in a vacuum their speeds would be exactly the same. Let us therefore consider what happens in air. To choose a shape of well determined surface and very light material, I should like to consider a blown-up bladder in which the air which is inside it will weigh, in the medium of air itself, nothing – or very little, since it can only be very slightly compressed[1] – so that the weight is only that tiny amount constituted by the skin itself, which would not be a thousandth part of a mass of lead as big as the inflated bladder. If you allowed these two, Signor Simplicio, to fall from a height of four or six cubits, by what distance do you think the lead would anticipate the bladder in their fall? You may be sure it would not get there three times sooner, nor even twice as soon, although you would have made it a thousand times faster.

It may be – replies Simplicio[2] – that at the beginning of their movement, that is in the first four or six cubits of movement, what you say might occur: but afterwards, and in an extensive continuation [of the movement], I believe that the lead would leave [the bladder] behind not only by six parts out of the twelve, but by eight or ten.

Salviati agrees, and goes one better by saying[3]:

. . . in very long distances, the lead might cover one hundred miles while the bladder was traversing one.

But that by no means contradicts the proposition: quite the contrary, as is confirmed by the fact that the speed of bodies when falling freely varies progressively during fall. This variation is precisely what should occur if the difference in speed is not caused by the difference in weight, but is dependent solely on external conditions, namely, the resistance of the medium. It would not occur if the speed depended on the heaviness of the falling bodies.

Because, if they were the same [their gravities], the proportion between the spaces they traverse ought to be the same, whereas we see this proportion continuing to grow as their movement progresses.

Galileo's argument is specious. It may even seem rather trivial, and purely polemical, that is to say, designed to silence his adversary, or to defeat him by meeting him on his own ground; for that is what it really comes to. Undoubtedly he does meet his adversary on his own ground, as all critical argument should, but in the mind of

[1] The weight of the air is not under discussion; nor the fact that it weighs more when it is compressed: these are matters admitted by everyone. On the other hand, we must admire all the more Galileo's ingenuity in setting up his experiment – imaginary, of course – even though it clearly cannot make claims to precision.

[2] *Discorsi*, p. 117.

[3] Ibid., p. 118.

Galileo his argument is to be taken seriously. For what he charges against Aristotelianism (and implicitly against Benedetti-Sagredo, also) involves conflict with the fundamental principle of all scientific explanation, i.e. constant causes produce constant effects. He prided himself upon having established a doctrine in agreement with this principle.

In Aristotelian dynamics a constant force produces a uniform motion. The constant weight of a body should not, therefore, produce an *accelerated* motion during fall.[1] However, let us go further, and admit that it does "cause" an accelerated motion. Then, at least, the ratio between the speeds "caused" by different weights should be constant, and hence the ratio between the distances traversed should also be constant. In fact, they are not.

It would seem, moreover, that the Galilean doctrine of fall is subject to the same objection. For if the first and immediate effect of gravity is not motion, but acceleration, and the increase in speed of fall is only a secondary effect,[2] then this acceleration, supposing it to be different for different bodies, e.g. an inflated bladder and a leaden ball in the same medium, nevertheless remains constant for each of them. The result is – or appears to be – that the ratio between the speeds and the distances traversed should remain constant also.

Whether the part played by resistance with respect to the motive force was improperly understood by Aristotle and properly understood by Galileo (or Benedetti) does not alter the basic position, namely, constant factors cannot produce variable effects.

Now, this is exactly what Simplicio says, and he, being a good logician, has no need of the explanation of Galileo's argument which we have been constrained to make[3]:

> All right; but, to follow in your own tracks, if, in bodies of different gravities it cannot be the difference in weight that is the reason for the change in the proportion of their speeds, on the grounds that their gravities do not change, neither can the medium, which is supposedly always the same, cause any alteration in the proportion between their speeds.

As we see, Simplicio is quite right. If the resistance of the medium has a constant value, as admitted by Aristotle (Galileo himself, following Benedetti in this matter, did so also in a different context), the ratio of the speeds resulting from two constant "causes" should

[1] The explanation of this acceleration is a *crux* in Aristotelian dynamics.

[2] The increase in speed, and the speed itself, are only the result, or accumulated result, of acceleration.

[3] *Discorsi*, p. 118.

remain constant. It is precisely at this point that the error arises: the resistance of the medium is not constant, but variable; and it varies as a function of the speed of motion. Salviati explains as follows[1]:

I would say for one thing that a heavy body has an inherent tendency to move towards the common centre of all bodies – the centre of the earth – with a motion which is continually, and uniformly, accelerated, so that in equal spaces of time, equal additions of speed and momentum are made.[2] And this must be understood to occur whenever all accidental and external impediments are removed; and there is one which we cannot remove: the impediment of the pervading medium, which must be opened and pushed aside by the falling body. Though it is a yielding and passive fluid, the medium opposes this motion with a resistance which is at first small, but grows greater and greater, as it opens, first slowly, then faster and faster, in order to allow the body to pass; and, because, as I have said, the body is continually accelerating, owing to its nature, as a consequence it encounters ever greater resistance from the medium, and the amounts of speed acquired grow ever less, so that finally the speed and the resistance of the medium reach values which, balancing each other, prevent further acceleration.[3]

From this point onwards the moving body moves uniformly with the speed it has acquired during fall.[4] The increase in resistance does not derive from any change in the nature of the medium, but solely from the increase in speed with which it must be displaced from the path of the falling body. In this way, we may add, the basic principle of proportionality between cause and effect has been safeguarded: at the same time it has been explained how a constant cause can produce a variable effect.

The passage which has just been quoted from Galileo is very interesting and significant. Not only does it give a mechanical explanation of the resistance of the medium – a concept whose great importance[5] is not in the least diminished by the fact that Galileo makes

[1] Ibid., cf. also *De Motu Gravium*, pp. 255 ff.

[2] Ignorance of the nature of gravity, as proclaimed, in the *Dialogo*, does not prevent Galileo from recognizing it as an inherent principle of bodies. Moreover, that is an indispensable requirement for the constancy of acceleration.

[3] It is interesting to note that if the resistance to acceleration is proportional to the speed, and hence increases proportionally to the latter, then its own value (or its "speed") decreases inversely; the ratio between resistance and acceleration is therefore exactly the same as that postulated by Aristotle between resistance and speed. We shall return to this matter later.

[4] *Vide infra*, Appendix, II, p. 84, n. 2. I. B. Cohen in a recent book (*The Birth of a New Physics* (New York: 1960), pp. 117 ff) makes the very pertinent remark that this "equable and uniform" downward motion is an inertial motion, the only inertial motion that can be realized in Galilean physics.

[5] This is the starting-point for all subsequent studies on the resistance of media

a most curious mistake[1] in applying it to reality – but it also reveals what I should like to call the underlying axioms of his thought, axioms which he fails to formulate clearly and explicitly, or does not always formulate, but which show evidence of their activity in some way or other in the course of his reasoning.

We know that Galileo, as well as Benedetti and Archimedes, consider all bodies to be "heavy", and that light ones do not exist.[2] We can therefore extend to all bodies, or to a body *qua* body, the remark he makes about "gravity", and say that every body "possesses" an internal principle by virtue of which it moves towards the centre of the earth with a uniformly accelerated motion.[3] In other words, "gravity", concerning whose "nature"[4] we are in other respects ignorant, can, nevertheless, be defined as a "cause" or *inherent* "principle" and con-substantial with the body; and as a force which is not only *constant*, but also *the same* in all bodies, whatever their kind. It is for this very reason that acceleration has a constant value, and hence is the same for all bodies, whatever their nature; or wherever they are placed, for this would not be so if "gravity" were the

to the motion of bodies. It hardly matters, as Newton remarks in his *Philosophiae Naturalis Principia Mathematica* (Book II, Section I, Of the motion of bodies that are resisted in the ratio of the velocity), that "the resistance of bodies is in the ratio of the velocity, is more a mathematical hypothesis than a physical one. In media void of all tenacity, the resistances made to bodies are in the duplicate ratio of the velocities", and not in direct ratio to the speeds, as Galileo thought.

[1] *Vide infra*, Appendix, II.

[2] See previously in *De Motu Gravium*, p. 360: "*Concludamus itaque, gravitatis nullum corpum expers esse, sed gravia esse omnia, haec quidem magis, haec autem minus, prout eorum materia magis est constipata et compressa, vel diffusa et extensa, fuerit.*"

[3] We might even say that heaviness *constitutes* the *physical* body.

[4] See *Dialogo*, *Le Opere di Galileo Galilei* (Firenze: Edizione Nazionale, 1898), VII, pp. 260 ff:

"SIMPLICIO: *La causa di quest'effetto* (the motion of earthly things downwards) *è notissima, e ciaschedun sa che è la gravità.*

"SALVIATI: *Voi errate, Sig. Simplicio; voi dovevi dire che ciaschedun sa ch'ella si chiama gravità. Ma io non vi domando del nome, ma dell'essenza della cosa: della quale essenza voi non sapete punto più di quello che voi sappiate dell' essenza del movente delle stelle in giro, eccettuatone il nome, che a questa è stato posto e fatto familiare e domestico per la frequente esperienza che mille volte il giorno ne veggiamo; ma non è che realmente noi intendiamo più, che principio o che virtù sia quella che muove la pietra in giù, di quel che noi sappiamo chi la muova in su, separata dal proiciente, o chi muova la Luna in grio, eccèttoche (come ho detto) i nome, che più singulare e proprio gli abbiamo assegnato di* gravità, *dovechè a quello con termine più generico assegnamo* virtù impressa, *a quello diamo* intelligenza, *o* assistente, *o* informante, *ed a infiniti altri moti diamo loro per cagione la* natura."

effect of an external force, such as attraction, for example.[1] In the last analysis, this implies that the matter of which bodies (at least terrestrial bodies) are composed is identical in them all, and is not accompanied by qualitative differences. The "gravity" of a body (in a vacuum) is therefore strictly proportional to the quantity of matter it contains. Anticipating somewhat, and ascribing to Galileo a terminology of which he knew nothing, we could say that he regarded the mass of a body and its heaviness to be the same thing.[2]

We could even go further, and anticipate once more by saying that Galileo regards inertial mass and gravific mass as being essentially the same, although this identity appears only when motion occurs in a vacuum, and not when it occurs in a plenum, as we shall see shortly. Inertial mass! Even though Galileo does not employ this term,[3] it is nonetheless true that he constantly makes use of this concept in the course of his arguments.[4] In fact, apart from the "inherent principle"

[1] It is in this refusal to seek an explanation of gravity on which to build a theory that we find the source of sterility of Galilean philosophy in astronomy, and the cause of Borelli's failure. A bad theory is always preferable to no theory at all. See my *Révolution astronomique, Borelli et la mécanique céleste* (Paris: Hermann, 1961).

[2] The history of the concept "mass = quantity of matter" is still rather obscure. On the one hand, we could maintain that it was already implicit in the teaching of Aristotle; on the other hand, we could say the same of its determination from "volume × density", as well as by "weight": it is also the very basis of the art of the assayer. We could also give Kepler the credit for having discovered this concept; and that is my belief. In fact, he was the first to give a correct definition of the concept by distinguishing "mass = quantity of matter = volume × density" from "weight"; the former is concerned in dynamic relationships, and remains constant, whereas the latter does not. The achievement is all the more meritorious seeing that, in dynamics, Kepler was still an Aristotelian, even though a heretic. For the prehistory of the concept of mass, see Anneliese Maier, *Die Vorläufer Galileis im XIV Jahrhundert* (Rome: 1949); Max Jammer, *Concepts of Mass* (Harvard University Press, 1961).

[3] The term "inertia" (*inertia*) derives from Kepler; and in the sense used by him has very nearly the opposite meaning to that of the present day. He means *inclinatio ad quietem*, as Nicole Oresme would say, and not resistance of a body to motion. It is understood that I use the term in its modern sense.

[4] It could be maintained that the *concept* of "inertia = resistance to motion = inclination to rest" serves only to clarify an essential fundamental idea of Aristotle. Indeed, why should forces be necessary to set bodies in motion, if bodies offer no resistance to being set in motion? and why should they cease their motion, when deprived of the motive force, unless they have, inherently, an inclination to rest? Finally, to take the matter further, if motion is the actualization of a force, how can the latter be conceived to offer no resistance to actualization?

It might be said that the modern concept of inertia is not so far from that of Kepler (or Aristotle) as would appear at first sight, as I have just said; in both cases it is *resistance to change*. See Newton, *Principia Mathematica*, Book I, Definition III.

of "gravity", the Galilean body possesses a second internal principle, namely, that of resistance to the acceleration, or deceleration, which we impose on it, or cause to be imposed on it[1]; and it is even proportional to this acceleration (positive or negative)[2] and the weight, or let us say its mass, i.e. the quantity of matter contained in it.[3] For this reason, and in this way, a medium at rest, or yielding, resists the motion of a falling body. The particles of the medium, which Galileo treats as a perfect fluid, have no bond one with another (no viscosity), and resist being set in lateral motion; and this resistance is greater the more rapid the lateral motion, for the latter is a function of the speed of fall of the body in question; or more exactly, it is in ratio to the acceleration of the particles of the medium from rest to motion. We could also say the greater the action of the falling body on the medium, the greater is the reaction of the medium, or its particles.[4] It is obvious that this reaction is greater when the resistance of the medium is greater, that is to say, when the medium is denser; or (which comes to the same thing) the heavier the medium is. Finally, it is obvious that a falling body will overcome the resistance of the medium more easily when it is impelled by a greater force; in other words, the heavier it is; or, more exactly, when it is heavier than the medium in question. We have seen that the very small *momentum* of an inflated bladder encountered great resistance from the air, whilst the great weight of a leaden ball encountered little resistance. From a consideration of these facts we come to the conclusion that if there were no medium the benefit accruing to the bladder would be so great, and to the leaden ball so small, that their speeds would become equal. Now, if we admit the principle that "in a medium which, on account of a vacuum or something else",[5] offers no resistance to motion all bodies will fall with the same speed,[6] we should be in a position to determine the ratio of the speeds of similar and dissimilar

[1] We are concerned here with acceleration, and not the motion which the body conserves, and to which it is "indifferent". It is for this very reason that a heavy body continues its motion with a uniform speed, when its acceleration during fall is neutralized by the resistance of the medium.

[2] A greater force is required to impart a greater acceleration to a given body (i.e. a faster motion); acceleration is proportional to the force.

[3] Hence, for a given force, the acceleration is inversely proportional to the mass (weight) of the body on which it acts.

[4] The statement that action and reaction are equal is implicit here. It had previously been stated by Leonardo da Vinci for impact.

[5] *Discorsi*, p. 119. Something else!?

[6] The resistance of falling bodies to acceleration is proportional to their mass, and so, too, is the motive force, i.e. gravity; whence uniform acceleration.

bodies when they move in the same, or in different, media; in other words, we should be able to give a correct answer to the problem to which Aristotle gave a solution, which we have proved to be wrong.

Indeed, in the considerations destined to replace the error of Aristotle by the truth of Galileo, the part played by the medium will appear in a noticeably different aspect. Its action will no longer be dynamic, but static, or, if preferred, hydrostatic, as it was with Benedetti. The medium will not offer resistance to the falling body; it will deprive it of part of its weight.

Did Galileo take account of this change in the way he represented the action of the medium? He says nothing about it, and passes straight from one to the other. It seems to me impossible for him to have confused the two views.[1] It is more likely that he regarded the two as being superimposed; and trusted the reader to make the necessary distinction. However that may be, he says that we can solve the problem in question

> by observing how much the heaviness of the medium detracts from the heaviness of the falling body: its heaviness being the means by which it opens up a path in the medium and pushes aside the parts of the medium . . . 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.[2]

The reasoning[3] is curious, and confirms the interpretation of the Galilean concept that I gave above: "the weight" is "the cause" or "the reason" of the acceleration. If the "weight" acting on the body is decreased, then the acceleration, and hence the speed, will be decreased by the same amount, provided that the resistance which the body offers to the action of this weight remains unaltered.

Thus, for example, say lead were ten thousand times as heavy as air, but

[1] The part played by the resistance of air to the motion of a pendulum or projectiles is purely mechanical; *vide infra*, pp. 76-7.

[2] *Discorsi*, p. 119.

[3] According to this argument, taken literally, the speeds are proportional to the weights, as maintained by Aristotle, or Benedetti. But properly speaking, Galileo's reasoning must not be taken literally, for it is not the "speed", but the "acceleration", that is in question here. In modern terms, we could say that submersion of a heavy body in a medium that "lightens" it, separates the gravific mass from the inertial mass.

ebony only a thousand times as heavy.[1] Now, while the speeds of these two materials, taken absolutely, that is if every resistance were removed, would be equal, the air detracts from the speed of lead one part in ten thousand, while from the speed of ebony it detracts one part in a thousand, that is, ten parts in ten thousand. Consequently, when lead and ebony fall through the air from any given height, from which, if the slowing down effect of air were removed, they would have fallen equally quickly, the air will detract one ten-thousandth part of the speed of the lead, but ten ten-thousandths from the speed of the ebony[2]; which means that if the height from which the bodies fell were divided into ten thousand parts, the lead would reach the earth ten – or rather nine – of the ten thousand parts before the ebony. And is this not the same as finding that a ball of lead falling from a tower two hundred cubits high outstrips a ball of ebony by scarcely four fingers' breadths? Now, ebony weighs a thousand times as much as air; but our well-inflated bladder only weighs four times as much: therefore the air takes one thousandth part of the speed from the natural speed of ebony; but from [the speed] of the bladder, which should be exactly the same, it takes one part out of four: therefore when the ebony ball, falling from the tower, reaches the ground, the bladder will only have traversed three-quarters of the distance. Lead is twelve times as heavy as water, but ivory only twice as heavy; therefore water takes from their absolute velocities – which would be the same – from lead a twelfth part, but from ivory a half: so that in water, if lead had fallen eleven cubits, ivory would have fallen six. And following this principle, I think we will find that experience corresponds much more closely to our calculations than to those of Aristotle.

Undoubtedly the Galilean calculation would agree with experiment much better than that of Aristotle's, provided always that we do carry out the experiment, and in place of Salviati's round numbers of 1,000 and 10,000 we substitute actual numbers, properly measured, expressing the true ratios between the specific weights[3] of the various bodies falling in the same medium. On the available evidence, such experiments were not carried out by Galileo; and he does not even claim to have done so. We are still, as always, in the realm of imaginary experiment.

Similarly, there are imaginary experiments which will enable us to find the ratio of the speeds of bodies falling in different media,

[1] *Discorsi*, pp. 119 ff. Once again, the imaginary nature of Galileo's numerical data may be pointed out.

[2] By being placed in air, lead will lose one ten-thousandth part of its weight, and ebony one thousandth part. The effective weight of the former will therefore be $10{,}000 - 1 = 9{,}999$; and of the latter $1{,}000 - 10 = 990$; or, in both cases, equal to the excess of the weight of the body over that of the medium ($P - R$).

[3] Galileo *does not say*: specific weights, as he is made to say by H. Crew and A. de Salvio in their translation of the *Discorsi* (*Dialogues Concerning Two New Sciences* (New York: 1914)). However, the meaning is clear.

not by comparing the various resistances of the media, but by considering the excess heaviness of the body above that of the medium.[1] For instance, tin is a thousand times heavier than air, and ten times heavier than water; then, dividing the absolute speed of tin into a thousand parts, in air, which takes away a thousandth part, it will move at a speed of nine hundred and ninety-nine, but in water at a speed of only nine hundred, because water takes from it a tenth part of its heaviness, and air only a thousandth. To take a solid only slightly heavier than water, such as oak: say a ball weighed a thousand drachms – if a similar amount of water weighed nine hundred and fifty, but a similar amount of air only two, it is plain that, if the absolute speed [of the ball of oak] were a thousand, in air its speed would be nine hundred and ninety-eight, but in water only fifty – on the grounds that the water takes nine hundred and fifty parts of its heaviness out of a thousand, and leaves it only fifty. Such a solid would therefore move almost twenty times as fast in air as in water, because the excess of its heaviness over that of water is only a twentieth part of its own heaviness. And here let us note that, as only materials which are heavier in kind than water can move downwards in it, and as these bodies are in consequence many hundreds of times heavier than air, in seeking the proportion between their speeds in air and in water, we may calculate, without significant error, that the air does not reduce the absolute heaviness – and hence the absolute speed – of such materials by any important amount; hence, having easily found the excess of their heaviness over that of water, we may say that their speed in air is to their speed in water in the same proportion as their total heaviness is to the excess of their heaviness above that of water. For example, an ivory ball weighs twenty ounces; the same amount of water weighs seventeen; therefore the velocity of ivory in air to the velocity of ivory in water is, approximately, in the proportion of twenty to three.

Galileo's "hydrostatic" argument follows very closely that of Benedetti. The only differences are the use of dialogue, the elegant style, the number and variety of examples. If Benedetti, when discussing the problem of the speed of fall in a vacuum and in a resistant medium, nearly always speaks of specific gravity, whereas Galileo nearly always simply says gravity, his examples generally imply a reference to specific gravity; and in such a manner that the reader cannot be misled. Now, by tracing the same path and by employing the same Archimedean scheme in their arguments, Benedetti and Galileo arrive at noticeably different conclusions. Whereas the former, as I have said, asserts that bodies, large or small, heavy or light, but of the same material or *specific gravity*, fall with the same speed, and that bodies of different specific gravities fall with different speeds, not only in a plenum but also in a vacuum, Galileo maintains that their speed in a vacuum is the same. How do we explain this differ-

[1] *Discorsi*, p. 120.

ence in conclusion? Has Benedetti simply made a mistake? Or is there some other reason?

First of all, let us quote Benedetti[1]:

I further suppose that the proportion between the motions of similar bodies, having different homogeneities and moving in the same medium over equal spaces, is that which exists between their excesses (in ponderosity, that is, or in lightness) over the medium. . . . Also the converse, namely, that the proportion between the excesses over the medium, as described above, is the same as that between the motions of the bodies.

This is made clear as follows. Consider a uniform medium *bfg*, let us say water, in which are located two bodies of different homogeneities, that is to say of different species. For example, let the body *dec* be of lead, and the body *aui* of wood, and let each body be heavier than a body equal to it but made of water.[2] Further, let there be given such spherical bodies of water, and call them *m* and *n*. . . . Let *m* be the aqueous body equal to *aui* and *n* the one equal to *dec*. Let *dec* be eight times *n* in ponderosity, and *aui* double *m*. I now assert, therefore, that the proportion of the motion of the body *dec* to the motion of the body *aui* (holding to the same hypothesis) is the same as that between the superfluities of the bodies *dec* and *aui* over the bodies *n* and *m*: that is to say that the time in which the body *aui* will be moved will be seven times that in which the body *dec* will be moved. This is because it is obvious by the third proposition of Archimedes' book *De insidentibus* that if the bodies *aui* and *dec* respectively were equal in heaviness to the bodies *m* and *n*, then they would in no wise move, neither upwards nor downwards. Further by the seventh proposition of the same book, which states that bodies heavier than the medium are borne downwards, the bodies *aui* and *dec* will be borne downwards. Therefore the resistance of the damp (that is of the water) is of a subduple proportion as compared with the body *aui* . . ., and of a suboctuple proportion as compared with the body *dec*. Thus the time which the centre of the body *dec* takes to cross the given space will be found to be in septuple proportion (in length) to that in which the centre of the body *aui* measures the aforementioned space. . . . This follows, because, from the book of Archimedes already cited, it may be gathered that the proportion of motion to motion does not have reference to the proportion of the heaviness between *aui* and *dec*, but to the proportion which is between the heaviness of *aui* as compared with *m* and the heaviness of *dec* as compared with *n*. The converse of this supposition is obvious enough, since the above considerations are clear. . . .

From all this it is evident that swifter motion is not caused by an excess of heaviness or lightness on the part of the swifter body as compared with

[1] G. B. Benedetti, *Resolutio omnium Euclidis problematum* (Libri, op. cit., pp. 259 ff) p. 50, n. 1.; see pp. 353 ff of my article on Benedetti quoted p. 49, n.4. In the original there is a diagram which I have thought it unnecessary to reproduce here.

[2] Wood twice as heavy as water! Benedetti is exaggerating a little.

the slower one, . . . but by the difference in species between the two bodies with respect to heaviness or lightness.

In short, if Benedetti agrees with Aristotle that the "virtue" or "quality" actuating a falling body is proportional to its weight, then it is not a question of the absolute weight of the body, but its specific weight. Furthermore, as this weight is decreased according to Archimedes by the action of the medium in which it is placed, it is only the residual weight (the "excess" of the specific weight of the body over the specific weight of the medium) which has to be taken into account; and it is the ratio of these different "excess" weights provided by bodies of different specific gravity that determines the ratio of their speeds. Thus, the weight of a body falling in a given medium should be decreased, and not divided, by the resistance of the medium; that is to say, decreased by the weight of an equal volume of the medium.[1] The speed will then be $P - R$, and not P/R; and the speeds of different bodies in the same medium will be in the ratio of the excess of their weights over that of the medium[2]

$$V_1/V_2 = (P_1 - R)/(P_2 - R).$$

That is exactly how Galileo explained it.

Benedetti, therefore, has good reasons for remarking that his concept "is not in accordance with Aristotle's teaching". And might not he have added, "nor with that of any of his commentators", that he had had the opportunity to read, or to converse with? We certainly have no reason to suspect his lack of truthfulness. Furthermore, his theory in its entirety is not found in any of the commentators of the Stagyrite. Nevertheless, rather similar things relating to the doctrine of fall are found in the works of Joannes Philoponus, particularly in his commentary on Aristotle's *Physica*, which was easily available to Benedetti.[3] Moreover, certain of Aristotle's medieval commentators in the course of subjecting his dynamics to penetrating criticism con-

[1] Arithmetic, not geometric, proportion.

[2] Or more accurately, if P_c is the weight of the body, and P_m that of the medium then $V_c = P_c - P_m$ and $V_c^1/V_c^2 = (P^1_c - P_m)/(P^2_c - P_m)$.

[3] This commentary by Joannes Philoponus was printed for the first time in 1535 (in Greek); Latin translations appeared in 1539, 1546, 1550, 1554, 1558 and 1569. Moreover, it is quoted by Galileo in his *De Motu Gravium*, p. 284, when dealing with motion in a vacuum (*vide infra*, p. 72): "*Tanta est veritatis vis ut doctissimi etiam viri et Peripatetici huius sententiae Aristotelis falsitatem cognoverunt, quamvis eorum nullus commode Aristotelis argumenta diluere potuerit . . . Scotus, D. Thomas, Philoponus. . . .*"

cluded that it was necessary to make the speed of a body depend, not on the ratio (F/R) of the force to the resistance, but on the excess of the former over the latter.[1] It is true, however, that Philoponus does not refer to Archimedes; and that the medievalists did not apply their ideas to motion of fall.

Thirty years later in his *Diversarum speculationum mathematicarum et physicarum liber* Benedetti returned to this question and said[2]:

whenever two bodies have or receive one and the same resistance on their surfaces, their motions will be proportionally related in precisely the same way as their motive powers: and, conversely, whenever two bodies have one and the same heaviness or lightness and different resistances, their motions will have mutually the same proportion as their resistances in the converse mode.

But wait, this must not be interpreted according to Aristotle. In fact,

it is also to be supposed that the speeds of the natural motions of any heavy body in different media are proportional to the weights of the body in those media. For example, if the total weight of any heavy body is denoted by *ai*, and if the body is placed in some medium less dense than itself (for, if it were placed in a denser medium it would not be heavy but light, as Archimedes shows), then that medium subtracts a part *ei* from the weight of the body so that the part *ae* remains free to act. If the body is then placed in some denser medium, but less dense than the body itself, that medium subtracts a part *ui* of the said weight, and so the part *au* of the weight will remain.

I assert that the proportion of the speed of the body through the less dense medium to its subsequent speed through the more dense one is as *ae* to *au*. This is more in accord with reason than if we were to say that speeds of this kind were as *ui* to *ai*, for speeds are proportioned solely by motive powers. . . . What we have now said clearly resembles what we wrote above, because it is the same thing to say that the proportion of the speeds of two heterogeneous bodies, which are nevertheless similar in figure and equal in magnitude and move in the same medium, is equal to the proportion of their weights, as it is to say that the proportion of the speeds of one sole body in different media is the same as that between its weights in those media.

[1] *Vide supra*, p. 54, n. 3.

[2] *Diversarum Speculationum Mathematicarum et Physicarum Liber* (Taurini: 1585), pp. 168 ff. In analysing the speed of falling bodies, Benedetti takes account only of the motive forces, because he is dealing with natural motion to which the body offers no resistance of its own. This is not the case when the motion is attended by extraneous force, in which case the internal resistance of the body to motion (e.g. to being raised, or even moved laterally) is added to the external resistance of the medium.

What has been said there does not agree with the teaching of Aristotle. But it is Aristotle who is wrong, especially by asserting that heaviness and lightness are opposite qualities peculiar to a body. In fact, this is not so: all bodies are heavy, more or less; and their primary qualities consist in their density or their tenuity. Light bodies are simply bodies that are not so heavy because they are placed in a heavier medium; or more exactly, they are more "tenuous" bodies placed in denser media.[1] Now, we can, at least in imagination, vary the density or tenuity of the medium, and in this way transform a heavy body (relative to the medium) into a light one, and *vice versa.* Similarly, we can alter the speed of fall of a given body in different media by altering the density of the medium. In particular, we can increase the speed by making the medium more tenuous. However, it will never become infinite, not even in a vacuum; which fact renders the Aristotelian objection against motion in a vacuum null and void. On the contrary, it is in a vacuum that bodies of different specific weights will fall with different speeds, and at speeds which are peculiar to those bodies. In fact

> in a plenum the proportion of the external resistances to the bodies is subtracted from the proportion of their weights, and what remains determines the proportion of their speeds, which would be nil if the proportion of the resistances were equal to the proportion of the weights[2]; and for this reason the speeds in vacuum would not be the same as in a plenum. The speeds of different bodies (i.e. bodies composed of different substances) will be proportional to their absolute specific weights.

On the other hand, in a vacuum, bodies composed of the same material will have the same speed, whether they are large or small; and a cannon-ball will not fall any faster than a musket-ball.

Benedetti's reasoning seems faultless, and so, too, the conclusion he reaches. We shall find the same again in the writings of the young Galileo. In fact, if the medium "removes" weight, and hence speed, from bodies moving through it; and if, moreover, it removes a different percentage from bodies of the same dimensions but different kinds (i.e. different specific weights), even though the absolute amount of weight "removed" be the same, with the result that the different speeds with which they fall in the medium are proportional to the different values of the *excess* of their different weights over the weight of the medium ($V_1 = P_1 - R$ and $V_2 = P_2 - R$), does it not

[1] Ibid., pp. 174 ff; and *supra* p. 61, n. 2.

[2] That is to say, if the motive force equals the resistance.

follow that when the effect of the medium is suppressed by placing the bodies in a vacuum we add an identical amount to their weights and speeds, and so obtain different results? This is precisely what the young Galileo says in his *De motu gravium*[1]:

> the speeds with which one and the same body will fall through different media are in the same ratio as the amounts by which its weight is in *excess* of those of the media; thus, if the weight of the moving body is 8, and that of an equal volume of the medium is 6, its speed will be 2 units; and if the weight of an equal volume of another medium is 4, its speed will be 4 units. The speeds will therefore be to each other as 2 is to 4; and not in the ratio of their densities, as maintained by Aristotle. The answer to another question will be equally evident: What will be the ratio of the speeds of moving bodies which have the same volume, but different weights when they fall in one and the same medium? The speeds of these moving bodies will be in the same ratio as the amounts by which the weights of the bodies are in excess of that of the medium. For example, let there be two moving bodies having the same volume, but different weights; let the weight of one of them be 8, and of the other 6; and let the weight of an equal volume of the medium be 4: then the speed of one body will be 4 units, and of the other 2 units. These speeds will therefore be in the ratio of 2 to 4, and not in the ratio of their weights, which is 8 to 6.

Galileo adds that the concept developed by him makes it possible to calculate the ratio of the speeds with which different bodies fall (or rise) in different media, and concludes as follows[2]:

> These are therefore the universal rules for the ratio of motions of bodies, whether they be of the same kind, or of different kinds, when they fall or rise in the same medium, or in different media.

Nevertheless, Galileo warns us that these rules are in no wise verified by experiment: light bodies fall much faster than they ought, and even fall faster than heavy ones at the beginning of their motion. So, we are obliged to explain the non-agreement of observed facts with theory, without in any way implying that it is wrong. The discrepancy implies the presence and effect of an additional factor. . . .[3]

[1] See *De Motu Gravium*, pp. 272 ff.

[2] Ibid., p. 273.

[3] Ibid., p. 273: "*Sed animadvertendum est quod magna hic oritur difficultas: quod proportiones istae, ab eo qui periculum fecerit, non observari comperientur. Si enim duo diversa mobilia accipiet quae tales habeant conditiones ut alterum altero duplo citius feratur, et ex turri dimittat, non certe velocius, duplo citius, terram pertinget: quin etiam sin observetur, id quod levius est, in principio motus praeibit gravius et velocius erit. Quae quidem diversitates et, quodammodo prodigia, unde accidant (per accidens enim haec sunt) non est hic inquirendi. Videndum enim prius est, cur motus naturalis tardius sit in principio.*"

Galileo's "rules", which as we have seen are really those of Benedetti, imply for both of them the possibility of motion in a vacuum: bodies will fall in a vacuum with finite but different speeds. Aristotle's error arose from not realizing that heaviness and lightness are not final qualities of bodies, but only relative ones, which express ratios between their own proper densities and those of the media in which they are present. In particular, he made the mistake of expressing the relationship between the force and the resistance as a geometric, instead of an arithmetic, proportion. For this reason he was led to conclude that the speed would be infinite in a vacuum.[1]

In the case of geometric proportion the smaller quantity must be capable of being multiplied many times so as to exceed any given magnitude. The said quantity must therefore be something; it cannot be nothing; for nothing multiplied by itself does not exceed any quantity. But this requirement does not hold in arithmetic proportion, where a number can have the same relationship with respect to another number as some other number has to nothing. . . . Thus 20 is to 12 as 8 is to 0. For this reason, if, as maintained by Aristotle, the motions bear the same geometric proportion to each other as the density of one medium does to another, we should be justified in concluding that no motion can take place in a vacuum. In fact, the ratio of the time [of motion] in a plenum to the time in a vacuum cannot be the same as that of the density of a plenum to the density of a vacuum. But if the speeds depend on an arithmetic, and not a geometric, proportion, then there is no absurdity in the conclusion.

In actual fact that is what happens.[2]

Hence, a body will move in a vacuum in the same way as in a plenum. Now, in a plenum the motion of a body depends on the amount by which its weight is in excess of the weight of the medium in which it moves; similarly, in a vacuum the motion depends on the excess of the weight of the moving body over the weight of a vacuum; and as the weight of a vacuum is nothing, the excess of the weight of the moving body over that of the vaccum is [equal to] its total weight. Hence, it will move faster [than in a plenum] in ratio to its total weight. In fact, it would not be able to move so fast in any plenum, seeing that the excess of the weight of the moving body over that of the medium will be less than the total weight of the moving body; also, its speed likewise will be less. . . .

Therefore, the speeds of bodies moving in a vacuum will not be infinite – which would be absurd – nor the same. On the contrary, they will depend on their specific gravities: a body of specific gravity 8 will fall with 8 units of speed, and one with a specific gravity of 4

[1] Ibid., pp. 278 ff. [2] Ibid., p. 281.

will fall with 4 units of speed; whilst those of the same specific gravity will fall in the same time.[1]

I do not think it is necessary to dwell on the matter. The young Galileo, as we see, had adopted Benedetti's doctrine in all its details[2]; and what is more, it is by no means absurd. In fact, let us suppose that heaviness is caused by a terrestrial attraction, something like magnetic attraction, or simply the attraction of "similar things" between themselves. Then, there would not be anything surprising in the fact that certain of these bodies are more strongly attracted by the earth than others – they would be "heavier" – and consequently fall with different speeds, assuming the same internal resistance to motion. We should then say that the inert mass of a body and its gravific mass are not equal.[3] No doubt it may be objected that Benedetti, no more than Galileo, does not explain heaviness by attraction, but regards it as a natural property of bodies, linked with, or even identical with, their density; as is perfectly true. Also, I have not attributed to Benedetti a theory of attraction by gravitation: I have only suggested it as an example in order to prove that it is not absolutely necessary for bodies to fall in a vacuum with the same speed; and that they could quite well do so with different speeds.[4] Furthermore, we could add that Benedetti was unaware of the concept of inert mass, as well as that of internal resistance of bodies to acceleration, and even *resistance to motion* during "natural" motion; and consequently, that he could neither distinguish inert mass from gravific mass, nor identify one with the other. This is a fact; and it provides the explanation why Galileo, who reasoned in an apparently identical manner to Benedetti, and did the same in his *De Motu Gravium*, was able to reach a totally different conclusion in the *Discorsi*, as he had done previously in the *Dialogo*. In fact, if we do not endow the falling body with an internal resistance opposing the force acting on it[5]; if, with Aristotle, we take account only of external resistance; and if we accept a simple proportionality *between the*

[1] Ibid., p. 283.

[2] Galileo does not quote him, but the connection is obvious.

[3] *Vide supra*, p. 64. n. 3.

[4] Benedetti, who "explained" accelerated motion of fall by the continual production of fresh *impetus* on the part of the body, could easily admit that bodies of greater specific gravity (denser) would produce greater *impetus*.

[5] Under natural motion this "force" is consubstantial with the body. This is not the case when the motion is subjected to extraneous influences: consequently bodies offer an internal resistance to the force acting on them from without.

speeds and the motive forces, then we are concerned solely with variation in the motive force. The arithmetic argument then assumes its full significance. The concept of Benedetti, and of the young Galileo, then demands proper recognition.

If, on the other hand, as Galileo does in the passage quoted above, we admit (be it only implicitly) that a body falling under natural motion is possessed of an internal resistance to its change of state (i.e. acceleration); and, furthermore, if we suppose the resistance to be proportional to the mass of the body (i.e. its absolute weight), we immediately arrive at the proposition of the simultaneous fall of heavy bodies in a vacuum; by so doing, we transfer to the *internal* resistance the *geometric* proportionality that Aristotle had assigned to the *exterior* resistance.[1] The reintroduction of *geometric* proportionality into the dynamic scheme does not stop here. When we come to study the fall of bodies in resistant media instead of in a vacuum, that is to say, when we come to study the part played by external resistance, we cannot limit ourselves to confirming that it manifests itself by an arithmetic decrease in the motive force; or, more exactly, we cannot conclude that there will be a similar *arithmetic* decrease for all bodies in the speed of their downward motion. This decrease in the motive force will be evaluated in relation to the unchanged internal resistance, and it is from this geometric relationship that we shall determine the resulting speed; or, what comes to the same thing, we shall determine it as a function of the weight of the body in question, decreased by that of the medium, to its absolute weight; or, again, what is the same thing, by the ratio of its motive force in the medium to its absolute motive force in a vacuum; or, in modern terms, by the ratio of the effective weight in the medium to its inertial mass.[2] So, when we reach a vacuum at the end of the infinite range of continuously decreasing resistances offered to the motion of falling bodies by media becoming more and more tenuous, we shall find not different speeds, but the same speed. We arrive at this result only because it was our starting-point.

[1] Both the motive force and the internal resistance being proportional to the weight (absolute) of the body, their ratio, that is to say, the acceleration is constant. Hence g is a universal constant.

[2] The speed of a body of given specific weight in a given medium will therefore be determined by the ratio (weight of body-weight of medium)/(weight of body); or, by the ratio of the excess of the force over the external resistance to the internal resistance, $(P - P/n)/P = (n - 1)/n$ or $(F - R_{ext})/R_{int}$, see *supra*, p. 64, n. 3.

Galileo's assertion of the simultaneous fall of heavy bodies, in the form in which it has so far been put forward in the *Discorsi*, rests only on *a priori* arguments and imaginary experiments,[1] as we are now well aware. Up to the present, we have never been confronted with a real experiment[2]; and none of the numerical data invoked by Galileo relates to measurements actually made. I do not reproach him on this account; on the contrary, I should like to claim for him the glory and merit of having known how to dispense with experiments (shown to be nowise indispensable by the very fact of his having been able to dispense with them): yet the experiments were unrealizable in practice with the facilities at his disposal. How, indeed, could a fall in vacuum be realized before the invention of pneumatic pumps? And as for experiments in a plenum, how could an insignificant loss or gain in motion of bodies thrown from the top of some tower be measured *exactly* before the invention of precision clocks?[3] Furthermore, in spite of the ingenious methods which he describes,[4] how could a proper measurement of the weight or density of air be made? For, if the measurements had not been *correct*, they would have been of little or no value. Galileo knew this as well as, or better than, anyone.

Naturally, there is no question of neglecting, or minimizing, the part played by experiment. It is obvious that experiment alone can provide the numerical data without which our knowledge of nature remains incomplete and imperfect. It is also true – and Galileo has expressed himself with sufficient clarity on the matter – that experiment alone is able to reveal which of the many means, all suitable for a certain purpose, have been rightly selected in any given case.[5] Even

[1] Did Galileo abandon Benedetti's concept used in *De Motu Gravium*, because he saw that it did not give much better agreement with experiment than Aristotle's? It is possible, and the quotation given above (p. 71, n. 3) rather points in that direction. On the other hand, it is clear that the discovery of the conservation of motion, and the substitution of accelerated motion as the prime and proper effect of the motive force (principle of inertia), could not make him do so.

[2] And even later. A century was to elapse before a real experiment was made by Atwood.

[3] A human clock was used by Riccioli in his experiments; see "An experiment in measurement", below.

[4] For example, weighing air by forcing a definite volume of air into a leather bottle already full of air; the excess weight corresponds to the excess quantity of air in the bottle (*Discorsi*, pp. 121 ff).

[5] The example of the grass-hopper in *Il Saggiatore* is classic. The recourse to experiment depends on the fertility introduced into classical science by mathematics. On this subject, Descartes does not differ from Galileo; see my "Galilée

when dealing with the fundamental laws of nature, such as that of fall, where pure reasoning suffices in principle, experiment alone can ensure that other unseen factors are not present to interfere with their application; and that matters take place in tangible reality, *in hoc vero aere*, very nearly as they do in the Archimedean world of reified geometry on which our deductions are grounded. Moreover, from another point of view that might be called pedagogic, nothing can take the place of experiment: it was experiment that pointed out the inadequacy of Aristotelian doctrine with respect to reality, and which, as much as its inherent contradictions, convinced Simplicio that it was wrong. The Galilean doctrine of the simultaneous fall of heavy bodies was so novel, and at first sight so contrary to fact and common-sense, that experimental confirmation alone could make it acceptable. No doubt, the arguments and "experiments" adduced by Galileo are sufficient for enlightened minds free from prejudice, as represented by Sagredo. But what about the others? For them, something more is required, namely, a real experiment.

So, we are not unduly surprised to find Galileo seeking experimental proof of his doctrine; and we cannot but admire the supreme ingenuity with which, seeing that it was impossible to proceed to direct experiment, he found a phenomenon in nature, which, when properly interpreted and slightly "corrected" (let us admit it in the proper place), provided him with indirect confirmation. This phenomenon was the isochronous movement of a pendulum, which he had discovered, or believed he had discovered.[1]

SALVIATI: The experiment of allowing two bodies, as different in weight as possible, to fall from a height, to determine whether their speeds are the same, presents some difficulties: because if the height is considerable, the medium, which must be opened and pushed aside by the falling body, will affect the slight momentum of the very light body much more than the violence of the very heavy body, so that the light body will remain behind in a long fall; while from a small height one might doubt whether there really was no difference at all, or whether there was some, but it was indiscernible. I therefore thought of repeating time after time falls from small heights, and accumulating a large number of those supposed tiny differ-

et Descartes", *Congrès International de Philosophie* (*Congrès Descartes*) (Paris: 1937).

[1] *Discorsi*, pp. 128 ff. The isochronism of the pendulum seems to have been generally admitted at the beginning of the seventeenth century; Baliano even laid it down as a principle. What characterizes Galileo, is that he attempted a demonstration of it. On Baliano, see S. Moscovici, "Sur l'incertitude des rapports entre expérience et théorie au XVII[e] siècle", *Physics*, 1960.

ences in time between the arrival of the heavy body and the light, so that together they would amount to a period of time not only discernible, but readily discernible. Moreover, in order to be able to make use of movements as slow as possible, in which the resistance of the medium does not work to alter the effect either, and the effect depends solely upon gravity, I thought of allowing bodies to fall on a flat slope, not much raised above the horizontal; for on such a slope one can observe how bodies of different weights behave no less than in the perpendicular.[1] To go further, I also wanted to get rid of any impediment that might result from the contact of these bodies against the inclined plane: and finally I took two balls, one of lead and the other of cork, the one at least a hundred times as heavy as the other, and I attached each of them to two identical thin strings, about four or five cubits long, fixed at the top.[2] Then, having drawn them both back from the perpendicular, I let them go at the same instant, so that they fell through the circumferences of the circles described by the identical strings, and, passing through the perpendicular, returned by the same path; and repeating these back-and-forth movements at least a hundred times on their own, they plainly showed that the heavy body follows the time of the light body so well that it does not anticipate it by the smallest amount even in a hundred or a thousand vibrations[3] – they kept perfectly in step. The effect of the medium is also evident, as, by causing some impediment to movement, it reduces the vibrations of the cork much more than those of the lead – not that it makes them any more or less frequent: in fact, when the arcs described by the cork were not more than five or six degrees, and those of the lead fifty or sixty, they were traversed in the same period of time.

Not without reason, Simplicio was somewhat confounded by the paradoxical nature of this demonstration. How, indeed, can it be claimed that the two balls move with the same speed, when in the same time one of them describes an arc of five degrees, and the other one of sixty? Is it not obvious that the leaden ball goes much faster? Undoubtedly, but this greater speed has nothing to do with the weight of the ball (at least not directly); it is a function of the height through which it falls. The proof lies in the fact that the roles can be reversed, that is to say, the cork ball can be made to describe an arc of fifty degrees, and the leaden ball one of five degrees. They will take

[1] The substitution of motion on an inclined plane for that of free fall is one of Galileo's claims to fame. By his experiments on the inclined plane he was able to verify the validity of his law governing the fall of bodies; on this point, see my *Études galiléennes*, II: and "An experiment in measurement".

[2] *Discorsi*, p. 128. "... *ciascheduna di loro ho attacata a due sottili spaghetti eguali*." So this is a bifilar pendulum, the invention of which, generally attributed to the *Accademia del Cimento*, should therefore be restored to the credit of Galileo.

[3] It may be asked if Galileo really observed *one thousand* oscillations of his pendulum.

the same time, for they will describe equal arcs in the same time, whether they be of five or fifty degrees. Salviati then replies[1]:

But what would you say, Signor Simplicio, if they still covered their paths in the same time, when the cork, drawn back thirty degrees from the perpendicular, had to traverse an arc of sixty degrees,[2] while the lead, drawn back from the same centre point by only two degrees, traversed an arc of four degrees? Would not the cork then, equally, be the faster? And yet experiment shows this to occur. But note this: the lead pendulum, drawn back, for instance, fifty degrees from the perpendicular, and then freed, falls, and passes the perpendicular again by almost another fifty, describing an arc of almost a hundred degrees; then, returning of its own accord, describes another, slightly lesser arc, and continuing its vibrations, after a great number finally comes to rest. Each one of these vibrations takes an equal amount of time, one of ninety degrees as much as one of fifty, twenty, ten, or four; so that, in consequence, the speed of the body continually decreases as it traverses, in the same space of time, ever smaller and smaller arcs. The cork, hanging from an equally long string, does a similar thing, in fact the same thing, except that it reaches the state of rest in a lesser number of movements, as, owing to its light weight, it is less able to overcome the obstacle of the air[3]: but in any case all the vibrations, large and small, are performed in times which are themselves equal, and also equal to the times of the lead. It is thus true that, when the lead traverses an arc of fifty degrees and the cork one of ten, the cork is slower than the lead; on the other hand, it will also happen that the cork traverses an arc of fifty degrees and the lead one of ten or six: so that at different times either the lead or the cork will be the faster. But if these same bodies traverse equal arcs in equal times, one can safely say that their speeds are equal.

The proof is complete[4]; the law of the simultaneous fall of heavy

[1] *Discorsi*, p. 129.

[2] The complete arc of oscillation.

[3] On the Fourth Day of the *Discorsi* (see p. 277) Galileo states, curiously enough, that the leaden ball makes the same number of oscillations as the cork ball.

[4] The theory of isochronism of the pendulum, previously treated in the *Dialogo* in different contexts, such as motion of a material pendulum (Second Day, p. 257), and fall along the circumference of a circle (Fourth Day, pp. 474 ff), is presented here as being based solely on experiment. Similarly, on the Fourth Day of the *Discorsi* (pp. 277 ff), where, in regard to the matters I have just mentioned, Galileo adds only the following comment: the resistance offered by the air being proportional to the speed of the moving body, its retarding action on fast and slow motions (large and small oscillations) will be the same, and consequently, without effect on their duration. But the discussions on the first and fourth days are conducted on a relatively popular level; and they are written in Italian. The proper proof, the mathematical proof, of isochronism is given on the Third Day; and it is in Latin. It is based on the following propositions (Theorem VI, Proposition VI, pp. 221 ff): (1) The time of descent of a heavy body along the

bodies is established; the discrepencies observed in tangible reality are easily explained by the resistance of the air, which is greater for more rapid motion, and more easily overcome by heavy bodies than by light ones. It depends also, of course, on the shape of the moving bodies (a fact which had long been known), and the ease with which they can penetrate and push aside the ambient air.[1] Finally, it depends on the nature of their surface, whether smooth, or rough, and on their volume, provided that all other things are similar. The mechanical resistance is in effect a function of the ratio between the surface of the body and its weight; which ratio is less for large bodies than for small ones; and this fact answers the question put sometime previously by Sagredo, and to which he now returns: Why does a cannon-ball fall more quickly than a musket-ball? Once again, the weight of the body does not come into the question.

We might express the opinion that the isochronism of the pendulum, on which Galileo insists so strongly, is not necessary for his proof; and that it is only necessary to verify the fact that two balls, one of

diameter of a vertical circle, and along any chord passing through its lowest point, is the same; (2) the time of descent along two successive chords is less than that along one only. Whence it follows that descent along the circumference will take place with *maximum* speed, and the time will always be the same. We can only admire the elegance and ingenuity of Galileo's proof; even though (as was shown later) descent along the circumference is not the fastest and does not take place in equal times (the cycloid has these properties), it remains nonetheless true, speaking in eighteenth-century language, that the "tautochrone" and "brachistochrone" are but one and the same curve.

[1] On account of the resistance offered by the air, bodies falling on our planet cannot conform entirely to the mathematical law of fall; they can do so only partially and approximately. In fact: (*a*) because of the hydrostatic action of the air, which "lightens" bodies placed in it, the gravific mass is not identical with the inertial mass: heavy bodies fall faster than light ones; (*b*) the resistance of the medium increases with the rate of acceleration, and so is not constant, but increases; the falling motion therefore is not "uniformly", but "non-uniformly", accelerated, and after a certain lapse of time changes into a uniform motion. For this reason, every medium, particularly air, imposes a *maximum* speed, which cannot be exceeded by any body during free fall, no matter from what height it falls, or how long the fall persists. It can, however, be exceeded by artificial means, as, for example, by shells from cannon. Galileo calls such speeds: *supernatural* speeds (see *Discorsi*, pp. 275-8).

It is amusing to note that Galilean physics, as a result of substituting acceleration for motion, and by transferring the resistance to change from without, to within, the body, finds itself in a position of accepting the consequences which, in Aristotelian physics, led to absurdities (see p. 47, n. 2). In particular: (*a*) that any force, however small, applied to a resistance (inertial), however great, produces motion, and (*b*) that, when the force equals the resistance, the resulting motion always has equal or constant, speed.

cork and the other of lead, dropped from the same height will reach the end of their travel (in the perpendicular) in the same time. No doubt, this is so.[1] But as far as Galileo was concerned, the isochronism of the pendulum is not only a great discovery of which he is quite justifiably proud, but it also presents one of the very rare instances where agreement between experiment and theory is almost perfect. Furthermore, it provides a means (*a*) of eliminating as far as possible the pernicious effects of secondary causes (in this case the resistance of the air) which affect the primary factors under investigation; and (*b*) of rendering observable, as a result of their accumulation, those small effects which taken in isolation would not be detectable. Now, this is unquestionably a matter of extreme importance; it is a capital improvement, which it would not be an exaggeration to call revolutionary in experimental technique; it is an improvement which far surpasses that achieved by Galileo when he substituted fall on an inclined plane for free fall. So, we can understand the value he attributed to it, as well as his desire to put it into effect.

But is this agreement between theory and experiment in fact genuine? In other words, could the experiment prove the isochronism of the pendulum? Or, at least, confirm the theoretical proof? Unfortunately, no. For the pendulum is not isochronous, as Mersenne was able to verify *by experiment* (and Huygens proved it theoretically). Now, if the methods employed by Mersenne were different from, and more accurate than, those of Galileo,[2] it remains nevertheless true that the difference between the duration of long and short oscillations is quite noticeable, and consequently could not have failed to be observable in the oscillations produced by Galileo.[3] What did he do then? He "corrects" the experiment; he holds it in his imagination and suppresses the experimental deviation. Was he wrong to do so? Not at all! for it is not by following experiment, but by outstripping experiment, that the scientific mind makes progress.

Let us now look back. In the course of this study we have tried to characterize Aristotelian dynamics by its fundamental axiom: the speed of a moving body is proportional to the motive force, and inversely proportional to the resistance ($V = F/R$); and consequently

[1] More correctly: would be so – if they did so in fact.

[2] See "An experiment in measurement".

[3] Or: the *differences*, for the non-isochronism of the large and small oscillations, is covered twice over by the cork and lead.

a constant force in a constant medium produces uniform motion. Against this we have urged the fundamental axiom of classical dynamics according to which, motion being conserved in the moving body, a constant force produces a motion that is no longer uniform, but accelerated. We have traced the criticism of Aristotelian dynamics through Benedetti and the young Galileo, a criticism which first substitutes an Archimedean scheme for that of Aristotle ($V=F/R$), and ends with the scheme: acceleration is proportional to motive force . . . and inversely proportional to the resistance (internal and external): $A=F/R$, or $A=F/(R_i+R_e)$. The similarity of this formula with that of Aristotle would not have escaped anyone.

Let us look closer; it is not a similarity, it is a formal identity that we discover in these two formulae. In fact, the second can be derived from the first by making an addition and a substitution: addition of the internal resistance to the external resistance, and substitution of acceleration for motion. The addition of the internal resistance to that of the external medium does not alter the structure of Aristotle's dynamics; it could even be considered as implicit in it. In the same way Kepler's dynamics in the deepest roots of its inspiration is Aristotelian[1]: in this instance speed is always proportional to force, and a constant force produces a uniform motion. On the other hand, the substitution of acceleration for motion is a complete upheaval: it is no longer a modification of the ancient dynamics, it is a replacement.

And yet, Why did Aristotle put motion (or speed) proportional to the motive force? Because he conceived it as a change, κίνησις, a process in which the moving body is never in the same state (*semper aliter et aliter se habet*); and because all change necessarily requires a cause – a cause proportional to its effect. Whence, necessarily, the proportionality of motion to the motive cause, and its cessation in the absence of the latter. There is not, and there cannot be, motion without driving power: *sine causa non est effectus*, and *cessante causa, cessat effectus.*

Galilean physics, classical physics, no longer conceives motion as a change, but as a true "state" – at least when the motion is uniform.[2] Not only that, it is able to persist and conserve itself without "cause":

[1] For Kepler, motion contrasts rest in the same way that light contrasts darkness; see my *Études galiléennes*, III.

[2] Though without using this term, which derives from Descartes, as is well known.

deprived of, or separated from, its driving power, the moving body will therefore continue its motion. On the other hand, acceleration is a change; in fact, the moving body does not remain in the same state: *se habet aliter et aliter*. Furthermore, acceleration requires a "cause" or a "force" strictly proportional to itself; and it ceases to be produced when the action of the latter ceases. *Sine causa non est effectus; cessante causa quidem cessat effectus.*

If we replace the relatively concrete terms V and A, speed and acceleration, in our formulae by a more abstract, vital term, κ (κίνησις) we shall obtain $\kappa = F/R$, which is the same for Galileo as well as for Aristotle. From a philosophical point of view, this seems to me to be an eminently satisfactory result.

APPENDIX

In the foregoing pages I have tried to describe and justify Galileo's use of the method of imaginary experiment concurrently with, and even in preference to, real experiment. In fact, it is an extremely fruitful method which incarnates, as it were, the demands of theory in imaginary objects, thereby allowing the former to be put in concrete form; and enables us to understand tangible reality as a deviation from the perfect model which it provides.[1] It must be confessed, however, that the method is not without its dangers; and that the tendency to go to the extreme in putting ideas into concrete form, to which tendency it is quite easy to succumb, sometimes plays some rather annoying tricks, and leads to assertions that reality persistently refutes. Alas! it has to be admitted that Galileo did not always escape this danger.

I am not going to enumerate all the instances where the great Florentine succumbed to temptation; I shall limit myself to two examples, both of them rather striking.

[I]. In the hydrostatic "digression",[2] which I did not analyse in the course of this study because it interrupts the development of the theory of fall, Sagredo tells how he put salt into a glass vessel before filling it with water in such a way that the vessel then contained the heavier salt-water at the bottom, and the lighter fresh water at the top: and how, to the astonishment of his friends, he succeeded in keeping

[1] It therefore plays a part intermediate between pure thought and tangible experiment.

[2] *Discorsi*, First Day, pp. 113 ff.

a ball of wax, made heavy by grains of sand, in equilibrium midway in the liquid. Salviati improves on this account by describing how it is possible to make the ball rise or fall by adding salt-water or fresh-water to the liquid contained in the vessel – and he no doubt causes even greater astonishment. Then, relying on the fact(?) that the same results can be produced by adding four drops of hot water to six pounds of cold water, or *vice versa*, he concludes that water possesses no viscosity, and offers no resistance (other than mechanical) to penetration or separation of its parts; and that philosophers who teach to the contrary are greatly mistaken. Sagredo agrees. However, if this be so, he asks how it is possible for drops of water, even quite large ones, to form on cabbage leaves, and remain intact without spreading or scattering. Salviati admits that he cannot explain it. He is certain, however, that the effect results from an external cause, and not from any internal quality; and in justification of this assertion offers a "very convincing" experimental proof. In fact,[1]

if the particles of water in a globule surrounded by air had some internal reason for sustaining themselves, they would do so much more easily when surrounded by a medium in which they had less tendency to fall than they have in the ambient air – like wine, for instance; and so, pouring wine round such a globule of water, one could fill the wine up right round it, without the particles of water, stuck together as they are by their internal viscosity, dissolving. However, this does not occur; in fact, as soon as the liquor spreads round and touches it, without waiting for it to rise much, it will dissolve and spread out, lying underneath, if it is red wine. The cause of this behaviour is therefore external, and possibly an attribute of the surrounding air. Indeed, a considerable antipathy between air and water may actually be observed – I observed it myself in another experiment: that is to say, if I take a glass globe with a neck as narrow as a straw, fill it with water, and then turn it, fully filled, so that its opening points downwards, although the water is comparatively heavy and in air tends to fall, and although air tends equally to rise through water, being comparatively light, the two fail to agree with one another, and instead of the water falling and the air rising through the hole, they both remain stubborn and defiant. On the other hand, however, if I apply a jar of red wine to the opening, although red wine is scarcely any lighter than water, we will immediately see it slowly rising through the water in red streaks, and the water will as slowly fall through the wine, without in the least mixing, until finally the globe fills itself entirely with wine and the water falls down to the bottom of the jar beneath. Now what can one say, what arguments can one propose, other than an incompatibility between water and air, which I do not understand, but which, perhaps. . . .

[1] Ibid., pp. 115 ff.

I confess that I share Salviati's perplexity. It is, indeed, difficult to put forward an explanation of the astonishing experiment he has just reported; particularly, because, if we repeated it *exactly as described*, we should see the wine rise in the glass globe (filled with water), and water fall into the vessel (full of wine); but we should not see the water and the wine simply replacing each other; we should see the formation of a mixture.[1]

What is the conclusion? Do we have to admit that red wines of the seventeenth century had properties no longer possessed by the wines of today – properties that made them, like oil, immiscible with water? Or can we suppose that Galileo, who undoubtedly never mixed water with his wine (for wine to him was "the incarnation of the light of the sun"), had never made the experiment; but, having heard of it, reconstructed it in his imagination, accepting the complete and essential incompatibility of water with wine as an indubitable fact? – Personally, I feel that the latter supposition is the right one.

[II]. The fact that the resistance of a medium to the motion of a moving body does not have a constant value, but increases as a function of the speed of the motion, and proportionally to it, involves a train of paradoxical consequences which Salviati takes great pleasure in setting before his companions.[2]

SALVIATI: Now . . . I can affirm without the slightest hesitation that there is no sphere so large, nor of so heavy a material, that the resistance of the medium, however tenuous, does not brake its acceleration, and continued motion does not bring it to a state of equilibrium: and of this we can obtain a very clear proof from experience. Because, [if any falling body were able, by continued motion, to acquire any speed you like], no speed [which was impressed upon it by any external engine] could be large enough for it to refuse the speed and lose it owing to the impediment of the medium.[3] Thus, for instance, if an artillery ball which had fallen through the air for four cubits, and had acquired ten degrees of speed, were to enter the water

[1] Results more nearly in agreement with Salviati's assertion would be obtained by having *two* openings, instead of one, in the glass flask, and fitting a straw, or a narrow tube, to each in such a manner that one (A) is directed to the interior of the flask, and the other (B) to the exterior. We should then see a streak of wine streaming from tube A towards the top of the flask, and a streak of water streaming to the bottom of the vessel, with the result that the wine would collect at the top, and the water at the bottom. Unfortunately, even in this case, there would be mixing. Furthermore, Salviati provides *one* orifice only to his flask, not *two*; nor does he provide any straw.

[2] *Discorsi*, First Day, pp. 136 ff.

[3] *Translator's note*: Professor Koyré's translation of this passage abbreviates this sentence by omitting to translate the words here bracketed.

at that speed, if the impediment of the water were not strong enough to stop the ball's impetus, the ball would go on accelerating, or at least it would continue at the same speed until it reached the bottom. One does not see this happen. On the contrary, the water, even if not more than a few cubits deep, impedes and weakens its motion, so that it only strikes the bed of the river or lake very lightly. It is therefore plain that the water would hardly allow the shot even at a depth of a thousand cubits, to acquire the speed of which it was able to deprive it in so short a journey. And why permit it to gain the speed in a thousand cubits only to remove it in four? Moreover, we may observe that the immense impetus of the ball, hurled from the cannon, is so reduced by the interposition of a few cubits of water, that it succeeds scarcely in bumping the ship, without any damage to it at all. Air too, although very yielding, lowers the speed of the falling body, heavy though it may be, as may be seen from similar experiments; for if we let off an arquebus towards the ground from the top of a tower, it will make much less of a thud on the earth than if we let it off from only four or six cubits from ground level; an evident sign that the impetus with which the ball left the barrel at the top of the tower diminished as it descended through the air. Consequently a descent from a very great height will not be enough to make it acquire a speed, of which the resistance of the air deprives it when it has already got it, no matter how. Equally the damage done to a wall by being struck by a shot from a carbine from a distance of twenty cubits would not, I believe, be caused by it falling perpendicularly from any height, however immense. I therefore believe that there is a limit to the acceleration of any natural body starting from rest, and that the impediment of the medium finally reduces it to a steady state, in which it thenceforth remains.

This long passage provides a good example of the mind of Galileo at work and in action: power – and imprudence; use – and abuse of the imagination. Is there anything finer, or more profound, than the considerations that lead him to assert that it is *only in a vacuum* that the accelerated motion of fall can obey the mathematical law which he has established for it; whilst in every other medium there is some deviation from the law, and finally the accelerated motion changes into a uniform motion having a speed determined by the nature of the falling body and the ambient medium (i.e. ratio of weight)? Consequently, this speed could be called the "natural speed" of the body in the medium in question. What could be more ingenious than the reasoning which demonstrates that it is impossible for a given body penetrating a particular medium to exceed its "natural" speed in that medium; and that it is impossible for the body to reacquire that greater speed which it possessed before entering the medium, and which the medium reduced by retarding the motion of the body? What could be more striking than the experiments which,

in Galileo's view, illustrate and provide proof of his proposition?

Yet, if it is quite true that bodies fall more rapidly in air than in water, and that in passing from the one to the other their motion is retarded, are we justified in raising this observation to the dignity of a general law, and in saying that when a freely falling body passes from a tenuous into a denser medium its motion is retarded? Could not we infer the arrest of motion as a consequence of the impossibility of such a passage? In fact, in Galileo's example or "experiment" we could let a cannon-ball fall not from four cubits, but from one, one-half, one-quarter, whereby in the course of its travel through the air it would acquire not ten units of speed, but five, one, one-half, and so on to infinity. If the speed in water must always be less than that in air, it should end by being infinitely small, and drop to zero. How, then, could a heavy body, whether of lead or gold, acquire its "natural" speed in water? Isn't it obvious, even if it is surprising, that Galileo confuses "speed" with "acceleration"? Isn't it even more surprising that, in his example of the cannon-ball arrested in its motion by several cubits of water, he ignores the difference between the effect of impact and that of hydrostatic resistance, effects which he so well distinguishes elsewhere? On the other hand, if it is quite definite that the speed, and hence the *impetus*, of a cannon-ball is greatest at the moment of leaving the mouth of the cannon, and that a passage of 20 cubits through air suffices to retard its motion; and, furthermore, if it is equally definite that a cannon-ball thrown vertically upwards in air, no matter to what height it rises and from what height it falls again, will never reach the ground again with its initial speed, can we draw the same conclusion therefrom as Galileo? That is to say, the speed (and hence the *impetus*) of the cannon-ball will never equal the speed with which it leaves the cannon, no matter from what height it falls, even if this height be several times greater than that which it could attain by being *fired* vertically. Obviously, we cannot. Galileo does, however. Why? Because he believes that the speed of the shot leaving a culverin is a "supernatural" speed, and greatly exceeds that which the shot would attain as a freely falling body, even if it fell from the Moon.[1] How does he demonstrate

[1] Ibid., Fourth Day, pp. 275 ff: "*Quanto poi al perturbamento procedente dall'impedimento del mezo, questo è più considerabile, e, per la sua tanto moltiplice varietà, incapace di poter sotto regole ferme esser compreso e datone scienza; atteso che, se noi metteremo in considerazione il solo impedimento che arreca l'aria a i moti considerati da noi, questo si troverà perturbargli tutti, e perturbargli in modi infiniti, secondo che in infiniti modi si variano le figure, le gravità e le velocità de i*

the "supernatural" character of the speed of the cannon-ball? He does so, as a matter of fact, by means of the experiment in which the shot is fired directly downwards; the shot from the cannon, or from the arquebus, is retarded in travelling from the top of the tower to the bottom. This would not be the result if the initial speed were less than the limiting speed acquired during fall.

Although Galileo does not say so, it is easy to make up for his silence, and say that that is the condition for the validity of his argument according to which the resistance offered by the water irremediably retards the falling cannon-ball, and the resistance of the air retards the shot from the arquebus, when both are fired directly downwards.[1] In fact, in a vacuum, where the speed of fall experiences no such limiting factor, a cannon-ball fired directly downwards would not be retarded; on the contrary, the normal acceleration of fall would be added to its initial speed. The result would be the same, or nearly so, if, instead of projecting it downwards with a "supernatural" speed conferred on it by the combustion of the powder, we limited ourselves to throwing it with such speed as human arms could confer: let us say, ten cubits per second. It is obvious that the resistance of the air proportional to this extremely low speed, and therefore almost nothing, would not be able to prevent the shot from

mobili. Imperò che, quanto alla velocità, secondo che questa sarà maggiore, maggiore sarà il contrasto fattogli dall'aria; la quale anco impedirà più i mobili, scondo che saranno men gravi: talchè, se bene il grave descendente dovrebbe andare accelerandosi in duplicata proporzione della durazion del suo moto, tuttavia, per gravissimo che fusse il mobile, nel venir da grandissime altezze sarà tale l'impedimento dell'aria, che gli torrà il poter crescere più la sua velocità, e la ridurrà ad un moto uniforme ed equabile; e questa adequazione tanto più presto ed in minori altezze si otterrà, quanto il mobile sarà men grave. . . . De i quali accidenti di gravità, di velocità, ed anco di figura, come variabili in modi infiniti, non si può dar ferma scienza: e però, per poter scientificamente trattar cotal materia, bisogna astrar da essi, e ritrovate e dimostrate le conclusioni astratte da gl'impedimenti, servircene, nel praticarle, con quelle limitazioni che l'esperienza ci verrà insegnando."

[1] Ibid., pp. 278 ff: Salviati explains that projectiles of fire-arms should be put in a different category from those of ballistae, cross-bows, etc. on account of "*l'eccessiva e, per via di dire, furia sopranaturale con la quale tali proietti vengono cacciati; chè bene anco fuora d'iperbole mi par che la velocità con la quale vien cacciata la palla fuori d'un moschetto o d'una artigleria, si possa chiamar sopranaturale. Imperò che, scendendo naturalmente per l'aria da qualche altezza immensa una tal palla, la velocità sua, merce del contrasto dell'aria, non si andrà accrescendo perpetuamente: ma quello che ne i cadenti poco gravi si vede in non molto spazio accadere, dico di ridursi finalmente a un moto equabile, accader ancora, dopo la scesa di qualche migliara di braccia, in una palla di ferro o di piombo; e questa terminata ed ultima velocità si può dire esser la massima che naturalmente può ottener tal grave per aria: la qual velocità io reputo assai minor di quella che alla medesima palla viene impressa dalla polvere accesa.*"

reaching the ground with a speed greater than the initial speed, as well as greater than that which it would have attained in free fall. Now, experiment shows that it is in fact retarded. Experiment also demonstrates the "supernatural" speed of the shot from the arquebus and of the cannon-ball from the culverin.

I said: the experiment demonstrates; but I should have said: *would demonstrate.* It would demonstrate – if it were performed. For, as Galileo honestly admits on the Fourth Day of these same *Discorsi* (from the First Day of which I have made such long quotations), he did not perform it.[1] Yet he is sure of the result. We have no difficulty in understanding the reason: that which should happen, does happen; and that which cannot happen, does not happen. Now, the speed of fall of a heavy body, even if it were to fall from the Moon, cannot exceed, as we have seen, a certain limit. It follows that the cannon-ball is retarded. Experiment merely confirms the deduction.

We know quite well that Galileo was right. Good physics is made *a priori.* As I have already said, it must at all costs avoid the temptation and fault of extreme concretism, and must not allow imagination to take the place of theory.

[1] Ibid., p. 279: "*Io non ho fatto tale esperienza, ma inclino a credere che una palla d'archibuso o d'artiglieria, cadendo da un' altezza quanto si voglia grande, non farà quella percossa che ella fa in una muraglia in lontananza di poche braccia, cioè dicosi poche, che'l breve sdrucito, o vogliam dire scissura, da farsi nell'aria non basti a levar l'eccesso della furia sopranaturale impressagli dal fuoco.*"

IV

An Experiment in Measurement

Historians of modern science,[1] when trying to determine its essence and structure, and thus to opppose it to the medieval and classical ones, insist, as often as not, in contradistinction to the abstract and bookish character of the latter, upon the empirical and concrete character of the former. Observation and experience leading a vigorous – and victorious – assault upon tradition and authority: such is the image, itself traditional, that we are usually given of the spiritual revolution of the seventeenth century, of which modern science is, at the same time, the root and the fruit.

This picture is by no means wrong. Quite on the contrary: it is perfectly obvious that modern science has immeasurably – and even beyond measure – enlarged our knowledge of the world, increased the number of "facts" – all kinds of facts – that it has discovered, observed and collected. Besides, it is just in this way that some of the founders of modern science have seen and understood themselves and their work. Gilbert and Kepler, Harvey and Galileo – they all extol the admirable fecundity of experience and direct observation, as they oppose it all to the sterility of abstract and speculative thought.[2]

Yet whatever the importance of the new "facts" discovered and brought together by the *venatores*, a simple amount of "facts", that is, a mere collection of observational or experiential data, does not constitute a science: they have to be ordered, interpreted, explained.

[1] I shall use the term "modern science" for the science of the seventeenth and eighteenth centuries, i.e. for the period which goes, roughly, from Galileo to Einstein. This science is sometimes called "classical" science in contradistinction to the contemporary one; I will not follow this usage and will reserve the designation "classical science" to the science of the classical world, chiefly to that of the Greeks.

[2] See for instance W. Whewell, *History of the inductive sciences*, 3 vols. (London: T. W. Parker, 1837); E. Mach, *Die Mechanik in ihrer Entwicklung, historisch-kritisch dargestellt* (Leipzig: F. A. Brockhaus, 1883; 9th Ed., Leipzig: F. A. Brockhaus, 1933): in English under the title: *The Science of Mechanics* (Chicago: Open Court, 1883; 5th Ed., La Salle: Open Court, 1943).

In other words, it is only when subjected to theoretical treatment that a knowledge of facts becomes science.

Besides, observation and experience – in the meaning of brute, common-sense observation and experience – had a very small part in the edification of modern science[1]; one could even say that they constituted the chief obstacles that it encountered on its way. It was not *experience*, but *experiment* that had nourished its growth and decided the struggle: the empiricism of the modern science is not *experiential*; it is *experimental.*

I certainly don't need to insist here upon the difference between "experience" and "experiment". Yet I should like to stress the close connection between this latter and the building of theory. Far from being opposed to each other, experiment and theory are bound together and mutually interdetermined, and it is with the growth of precision and refinement of theory that grow the precision and refinement of the experiments. Indeed, an experiment – as Galileo so beautifully has expressed it – being a question put before nature, it is perfectly clear that the activity which results in the asking of this question is a function of the elaboration of the language in which it is formulated. Experimentation is a teleological process of which the goal is determined by theory. The "activism" of modern science, so well noticed – *scientia activa, operativa* – and so deeply misinterpreted by Bacon, is only the counterpart of its theoretic development.

We have to add, moreover – and this determines the characteristic features of modern science – that, for its theoretical work, it adopts and develops the pattern of thinking of the mathematician. This is the reason why its "empiricism" differs *toto caelo* from that of the Aristotelian tradition[2]: "the book of nature is written in geometrical characters" declared Galileo; this implies that in order to reach its goal modern science is bound to replace the system of flexible and semi-qualitative concepts of the Aristotelian science by a system of rigid and strictly quantitative ones. Which means that modern science constitutes itself in substituting for the qualitative or, more exactly,

[1] As recognized already by Tannery and Duhem, the Aristotelian science is in much better accordance with common experience than that of Galileo and Descartes. See P. Tannery, "Galilée et les principes de la dynamique", in *Mémoires Scientifiques* VI, pp. 400 ff (Toulouse: E. Privat, 1926); P. Duhem, *Le Système du Monde* (Paris: Herrman, 1913), I, pp. 194-5.

[2] It is an empiricism that the Aristotelian tradition opposes to the abstract mathematism of the Galilean dynamics. See on the empiricism of the Aristotelians, J. H. Randall, Jr., "Scientific method in the School of Padua", *Journal of the History of Ideas*, I, pp. 177-206 (1940).

for the *mixed* world of common-sense (and Aristotelian science) an Archimedean world of geometry made real; or – which is exactly the same thing – in substituting for the world of the more-or-less of our daily life a universe of measurement and precision. Indeed this substitution implies automatically the exclusion from – or the relativation in – this universe of everything that cannot be subjected to exact measurement.[1]

It is this research of quantitative precision, of the discovery of exact numerical data, of these "numbers, weights, measures" upon which God has built the world that forms the goal, and thus determines the very structure of the experiments of modern science. This procedure is not co-extensive to experimentation in general: neither alchemy, nor Cardano, nor Giambattista Porta – nor even Gilbert – is looking for numerical results. This because they think the world as an ensemble of qualities much more than as an ensemble of magnitudes. Quality, indeed, is repugnant to the precision of measure.[2] Nothing is more significant in this respect than the fact that Boyle and Hooke, both of them experimenters of the first rank, men who know the value of precise measurement, make a purely qualitative study of the spectral colours. Nothing reveals better the incomparable greatness of Newton than his ability to transcend the realm of quality and to break through into the realm of physical, that is quantitatively determined, reality. But besides the theoretical (conceptual) and psychological difficulties that hinder the application of the idea of mathematical rigour to the world of perception and action, the actual performance of a correct measurement encounters in the seventeenth century technical difficulties of which, living as we do in a world overcrowded with, and dominated by, precision instruments, we have, I am afraid, a very bad understanding. Even historians, who – as Professor I. Bernard Cohen pointed out – only too often present us with the decisive experiments of the past not as they *were* actually performed *then*, but as they *are* performed *now* in our laboratories

[1] This applies, of course, only to the so called "exact sciences" (physico-chemical) in contradistinction to "natural science" or "history" (sciences dealing with the "natural" world of our perception and life) which does not – and perhaps cannot – discard quality and substitute a world of exact measures for the world of the "more-or-less". In any case neither in botany nor in zoology, nor even in physiology and biology, did exact measurements play any role; their concepts are still the non-mathematical concepts of the Aristotelian logic.

[2] Quality can be ordered, but not measured. The "more or less" we are using in respect to quality enable us to build a scale, but not to apply exact measurement.

and classrooms, do not realize the real conditions, and therefore the real meaning, of experimentation in the heroic epoch of modern science.[1] And it is in order to bring a contribution to the history of the constitution of the experimental methods of science that I will try, today, to tell the story of the first conscious and sustained attempt of an experimental measurement. The measurement of a universal constant, the constant of acceleration of bodies in their free fall.

Everybody knows the historical importance of the law of fall, the first of the mathematical laws of the new dynamics developed by Galileo, the law which established, once and forever, that "motion is subjected to the law of number".[2] This law presupposes that gravity, though by no means an essential property of bodies (and of which, moreover, we ignore the nature), is, nevertheless, their universal property (all bodies are "heavy" and there are no "light" ones); besides, for every one of them it is an invariable and constant property. It is only on these conditions that the Galilean law is valid (in the vacuum).

Yet, in spite of the mathematical elegance and physical plausibility of the Galilean law, it is obvious that it is not the only possible one.[3] Besides, we are not in the vacuum but in the air, and not in the abstract space, but on the earth, and even, perhaps, on a moving one. It is quite clear that an experimental verification of the law, as well as of its applicability to bodies falling in our space, *in hoc vero aere*, is indispensable. Just as the determination of the concrete value of the acceleration (of g) is indispensable.

It is well known with what extreme ingenuity, being unable to perform direct measurements, Galileo substitutes for the free fall the motion on an inclined plane on one hand, and that of the pendulum on the other. It is only justice to recognize his immense merit and genial insight, which are not diminished by the fact that they are based on two wrong assumptions.[4] But it is justice too to turn our attention

[1] See I. Bernard Cohen, "A sense of history in science", *American Journal of Physics*, XVIII (6), pp. 343 ff (1950).

[2] *Discorsi e Dimostrazioni matematiche intorno a due nuove scienze*, in *Le Opere di Galileo Galilei*, VIII (Firenze: Edizione Nazionale, 1898), p. 190.

[3] Thus G. B. Baliano proposes a law according to which traversed spaces are *ut numeri* and not *ut numeri impares*; Descartes and Torricelli discuss the possibility of the spaces being in cubical and not in quadratic proportion to the time; in the Newtonian physics acceleration is a function of attraction and therefore not constant. Moreover, as Newton himself does not fail to point out, the inverse square law of attraction is by no means the only possible one.

[4] Galileo's experiments are based on the assumptions (*a*) that the motion of a

to the amazing and pitiful poverty of the experimental means at his disposal.

Let us learn from himself his *modus procedendi*[1]:

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

Next we tried other distances, comparing the time for the whole length with that for the half, or with that for two-thirds, or indeed for any fraction; in such experiments repeated a full hundred times we always found that the spaces traversed were to each other as the squares of the times, and this was true for all inclinations of the plane, i.e. of the channel along which we rolled the ball. We also observed that the times of descent, for various inclinations of the plane, bore to one another precisely that ratio which, as we shall see later, the author has predicted and demonstrated for them.[2]

For the measurement of time, we employed a large vessel of water placed in an elevated position; to the bottom of this vessel was soldered a pipe of small diameter giving a thin jet of water which we collected in a

ball *rolling* down on an inclined plane is equivalent to that of a body *gliding* down (without friction) on the selfsame plane and (*b*) that the pendular motion is perfectly isochronous. This isochronism being a consequence of his law of fall, an experimental confirmation of the former would therefore confirm the latter. Unfortunately, no direct measurement of consecutive oscillation-periods is possible: just because there are no clocks with which we could measure them. Galileo, therefore – and one cannot but admire his experimental genius – substitutes for the direct measurement the comparison of the motion of two different pendula (of equal length), the bobs of which, though having performed oscillations of different amplitudes, arrive nevertheless at the same moment at their position of equilibrium (the lowest point of the curve); the same experiment made with pendula, the bobs of which are constituted by bodies of different weight, demonstrates experimentally that bodies heavy and light (individually as well as specifically) fall with the same speed. See *Discorsi*, pp. 128 ff.

[1] See *Discorsi*, pp. 212 ff. I am quoting the translation of Henry Crew and Alfonso de Salvio, *Dialogues concerning two new sciences* (New York, Macmillan, 1914; reprinted, New York: Dover Publications, 1952), pp. 178 ff.

[2] The speed of the descent is proportional to the sine of the angle of inclination. See ibid., pp. 215, 219; pp. 181, 185 of the translation.

small glass during the time of each descent, whether for the whole length of the channel or for a part of its length; the water thus collected was weighed, after each descent, on a very accurate balance; the differences and the ratios of these weights gave us the difference and the ratios of the times, and this with such accuracy that although the operation was performed many, many times there was no appreciable discrepancy in the results.

A bronze ball rolling in a "smooth and polished" wooden groove! A vessel of water with a small hole through which it runs out and which one collects in a small glass in order to weigh it afterwards and thus measure the times of descent (the Roman water-clock, that of Ctesebius, had been already a much better instrument): what an accumulation of sources of error and inexactitude!

It is obvious that the Galilean experiments are completely worthless: the very perfection of their results is a rigorous proof of their incorrection.[1]

No wonder that Galileo who, doubtless, is fully aware of all that, refrains, as far as possible (thus in the *Discourses*), from giving a concrete value for the acceleration; and that whenever he gives it (as in the *Dialogue*), it is completely and utterly false. So false that Father Mersenne has been unable to hide his surprise: "He supposes," writes he to Peyresc,[2] "that a bullet falls one hundred cubits in 5″ [seconds]; wherefrom it follows that the bullet will fall not more than four cubits in one second, though I am certain that it will fall from a greater height."

Indeed, four cubits – not even seven feet[3] – is less than the half of the true value; and about half the value that Father Mersenne will establish himself. And yet, that the figures given by Galileo are grossly inaccurate is by no means surprising; quite the contrary: it would be surprising, and even miraculous, if they were not. What is surprising, that is the fact that Mersenne, whose experimental means

[1] Modern historians, accustomed to see the Galilean experiments performed for the benefit of students in our school laboratories, accept indeed this astonishing report as gospel truth and even praise Galileo for having thus experimentally established not only the empirical validity of the law of fall, but even this law itself. (See among countless others, N. Bourbaki, *Éléments de mathématique* IX, première partie, livre IV, chap. I-III, Note historique, p. 150 (Actualités scientifiques et industrielles, No. 1074 [Paris: Herrman, 1949]). See Appendix, 1.

[2] F.M. Marin Mersenne, *Lettre à Peyresc* of 15 January 1635; see Tamizey de Larroque, *La Correspondance de Peyresc*, XIX, 112 (Paris: A. Picard, 1892): see *Harmonie Universelle*, I (2), pp. 85, 95, 108, 112, 144, 156, 221 (Paris: 1636).

[3] The Florentine cubit, doubtless used by Galileo, contains 20 inches, i.e. 1 foot, 8 inches; and the Florentine foot is equal to the Roman one, that is equal to 29.57 cm.

were not much richer than those of Galileo, could have obtained so much better results.

Thus modern science finds itself at its beginnings in a rather strange and even paradoxical situation: it has precision for principle; it asserts that the real is, in its essence, geometrical and, consequently, subject of rigorous determination and measurement (*vice-versa*, mathematicians like Barrow and Newton see in geometry itself a science of measurement[1]); it discovers and formulates (mathematically) laws that allow it to deduce and to calculate the position and speed of a body at each point of its trajectory and at each moment of its motion, and it is not able to use them because it has no ways to determine a moment, nor to measure a speed. Yet, without these measures the laws of the new dynamics remain abstract and void. In order to give them a real content it is indispensable to possess the means of measuring time (space is easy to measure), that is *organa chronou*, *horologia*, timekeepers as Galileo has called them; in other words: reliable clocks.[2]

Time, of course, cannot be measured directly but only through something else in which we find it embodied. That is either (*a*) a constant and uniform process, such for instance as the constant and uniform motion of the heavenly sphere or the constant and uniform outflow of water in the water-clock of Ctesebius[3]; or (*b*) a process which, though not uniform in itself, can be repeated, or repeats itself, automatically; or finally (*c*) a process which, though not repeating

[1] See Isaac Barrow, *Lectiones Mathematicae* of 1664-6 (*The Mathematical Works of Isaac Barrow, D.B.*, ed. by W. Whewell [Cambridge: C.U.P., 1860]), pp. 216 ff; Isaac Newton, *Philosophiae naturalis principia mathematica, preface* (London; 1687).

[2] The unreliability of the clocks of the sixteenth and seventeenth centuries is well known; precision clocks are by-products of scientific development (see Willis I. Milham, *Time and timekeepers* [New York: Macmillan, 1923]; L. Defossez, *Les savants du XVIIe siècle et la mesure du temps* [Lausanne: ed. *Journal Suisse d'horlogerie*, 1946]), yet their building is usually explained by the urge of solving the problem of longitude, i.e. the pressure of practical needs of navigation, the economical importance of which grew considerably since the circumnavigation of Africa and the discovery of America (see for instance Lancelot Hogben, *Science for the citizen* (London: G. Allen and Unwin, 1946), pp. 235 ff, 2nd Ed., 7th imp. Without denying the importance of practical needs or economic factors on the development of science, I believe this explanation, which combines Baconian and Marxist prejudices for *praxis* against *theoria*, to be at least 50 per cent false: the motives for building correct time measuring instruments were, and still are, immanent to the scientific development itself. See my paper, "Du monde de l'à peu près à l'univers de la précision", *Critique*, n. 28 (1946).

[3] See its description in H. Diels, *Antike Technik* (Leipzig: Teubner, 1924) 3rd Ed.

itself as completely identical, employs for its completion the same amount of time, presenting us thus, so to say, an atom or unity of duration.

It is in the pendular motion that Galileo found such a process. Indeed a pendulum, provided of course all external and internal impediments, such, for instance, as friction or the resistance of air, were eliminated, would reproduce and repeat its oscillations, in a perfectly identical manner, till the end of time. Moreover, even *in hoc vero aere* where its motion is continuously retarded and where no two oscillations are strictly identical, the period of these oscillations remains constant.

Or to put it in Galileo's own words[1]:

> 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 than that which nature has given it, and which depends neither on the weight of bob, nor on the amplitude of the oscillation, but only and solely on the length of the pendulum.

This great discovery, by the way, has been made by Galileo not by gazing at the oscillations of the great candelabra of the cathedral of Pisa and stating their isochronism by comparing them with the beats of his pulse, as following Viviani is still told in the textbooks,[2] but by extremely ingenious experiments in which he compares the oscillations of two pendula of the same length but with bobs of different matter and thus of different weight (cork and lead),[3] and first and foremost by hard mathematical thinking. Thus says Salviati[4]:

> 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.

[1] See *Discorsi e Dimostrazioni matematiche intorno a due nuove scienze*, in *Le Opere di Galileo Galilei* (Firenze: Edizione Nazionale, 1898), VIII, p. 141; English translation, p. 141.

[2] The famous candelabrum was put into the Cathedral of Pisa three years after Galileo's departure from that city; at the time at which Viviani places the discovery, the cupola of the Cathedral of Pisa was still bare and void. See E. Wohlwill, "Über einen Grundfehler aller neueren Galilei-Biographien", *Münchener medizinische Wochenschrift* (1903), and *Galilei und sein Kampf für die Copernicanische Lehre*, I (Hamburg and Leipzig: L. Voss, 1909); R. Giacomelli, "Galileo Galilei Giovane e il suo De Motu", *Quaderni di storia e critica della scienza*, I (Pisa: 1949).

[3] See *supra*, p. 92, n. 4.

[4] See *Discorsi e Dimostrazioni matematiche intorno a due nuove scienze*, in *Le Opere di Galileo Galilei* (Firenze: Edizione Nazionale, 1898), VIII, p. 139; English translation, p. 95.

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°, $\frac{1}{2}$° 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 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 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 the 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 threads; 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 this 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 chords bear to each other the (inverse) ratio of the squares of the number of vibrations performed in the same time.

One cannot but admire the depth of the Galilean thinking which shows itself in its very error; the oscillations of the pendulum are, of course, not isochronous; and the circle is not the line of the quickest descent; but, to use the terms of the eighteenth century, the "brachistochrone" curve, and the curve upon which oscillations are performed in the same time, or the "tautochrone" one, are recognized by Galileo to be the same line.[1]

[1] The times of descent on all the chords being equal and the motion along the (circular) arc being quicker than that along the chord, it was reasonable for Galilei to assume that the descent along the arc was the quickest possible and that the motion of the pendulum was, therefore, isochronous. That it is not the case was discovered experimentally by Mersenne in 1644 (see *Cogitata Physico-Mathematica, Phenomena Ballistica* (Parisiis: 1644), propositio XV, septimo, p. 42), and theoretically by Huygens who, in 1659, demonstrated that the "tautochrone" line of descent is the cycloid and not the circle (the same discovery was made independently by Lord Brouncker, in 1662). As for the cycloid being at the same time the curve of the quickest descent ("brachistochrone"), this was demonstrated by J. Bernoulli in 1696, and independently – answering the challenge of Bernoulli – by Leibniz, de l'Hôpital and Newton.

It is rather strange that, having discovered the isochronism of the pendulum – the very basis of all modern chronometry – Galileo, though he tried to achieve with its help a timekeeper, and even to construct a mechanical pendulum-clock[1] – never used it in his own experiments. It seems that it was Father Mersenne who first got this idea.

Mersenne, as a matter of fact, does not tell us *expressis verbis* that he employed the pendulum as a means of measuring the time of descent of heavy bodies in the experiments he reports about in his *Harmonie Universelle*.[2] But as in the same work he gives a careful description of the motion of the semicircular pendulum and insists upon the various utilizations of the same in medicine (for the determination of the variations in speed of the pulse beats), in astronomy (for the observation of the eclipses of the moon and the sun), etc.,[3] it is practically certain, and moreover confirmed by another passage of the *Harmonie Universelle*, not only that he did use a pendulum but even that this pendulum was three and a half feet long.[4] It is indeed

[1] This clock, or, more exactly, its central regulating mechanism, was constructed by Viviani; see *Lettera di Vincenzio Viviani al Principe Leopoldo de' Medici intorno all' applicazione del pendolo all' orologio*, in *Le Opere di Galileo Galilei* (Firenze: Edizione Nazionale, 1907), XIX, pp. 647 ff; see equally E. Gerland-F. Traumüller, *Geschichte der physikalischen Experimentierkunst* (Leipzig: W. Engelmann, 1899), pp. 120 ff; L. Defossez, *Les savants du XVIIe siècle et la mesure du temps* (Lausanne: ed. *Journal Suisse d'horlogerie*, 1946), pp. 113 ff.

[2] See *Harmonie Universelle* I, pp. 132 ff (Paris: 1636).

[3] Ibid., p. 136: "Quoy qu'il en soit, cette manière d'Horologe peut servir aux observations des Eclypses de Soleil, & de la Lune, car l'on peut conter les secondes minutes par les tours de la chorde, tandis que l'autre fera les observations, & marquer combien il y aura de secondes, de la première à la troisiesme observation, etc.

"Les Médecins pourront semblablement user de cette methode pour reconnoitre de combien le poux de leur malades sera plus viste ou plus tardif à diverses heures, et divers jours, et combien les passions de cholere, et les autres le hastent ou le retardent; par exemple qu'il faut une chorde de trois pieds de long pour marquer la durée du poux d'aujourd'hui par l'un de ses tours, et qu'il en faille deux, c'est à dire un tour et un retour pour le marquer demain, ou qu'il ne faille plus qu'une chorde longue de 3/4 de pied pour faire un tour en mesme temps que le poux bat une fois, il est certain que le poux bat fois plus viste."

[4] Ibid., p. 220, Corollaire 9: "Lorsque j'ay dit que la chorde de 3 pieds & demy marque les secondes par les tours ou retours, je n'empesche nullement que l'on n'accourcisse la chorde, si l'on trouve qu'elle soit trop longue, et chacun de ses tours dure un peu trop pour une seconde, comme i'ay quelquefois remarqué, suivant les différentes horologes communes ou faites exprez: par exemple le mesme horloge commun, dont i'ay souvent mesuré l'heure entière avec 3600 tours de la chorde de 3 pieds & demy, n'a pas fait d'autresfois son heure si longue: car il a fallu seulement faire la chorde de 3 pieds pour avoir 900 retours dans l'un des quarts d'heure dudit horologe: et i'ay experimenté sur une monstre à rouë faite

of such a pendulum that, according to Mersenne, the period is exactly equal to one second of the prime mobile.[1]

The results of Mersenne's experiences, "performed more than 50 times", are quite consistent; the falling body traverses 3 feet in half a second, 12 in a second, 48 in two, 108 in three, and 147 in three and a half. Which is nearly twice as much (80 per cent) as it should be according to the figures given by Galileo.

Thus Mersenne writes[2]:

> But concerning the experiences of Galileo, one cannot imagine where the great difference that one finds here in Paris and in the surroundings concerning the times of the falls, which have always appeared to us to be much smaller than his, comes from: not that I should like to reproach such a great man for little care in his experiences, but we have made ours many times from different heights, in the presence of many persons and they always succeeded in the same way. Therefore if the cubit which Galileo has used has only one foot and two thirds, that is twenty inches of the royal foot which we use in Paris, it is certain that the bullet descends more than one hundred cubits in 5 seconds.

Indeed, explains Mersenne, "the hundred cubits of Galileo" are equal to $166\frac{2}{3}$ of "our" feet.[3] But Mersenne's own experiments "repeated more than fifty times" have given quite different results; according to them, in 5″ a heavy body will traverse not 100, but 180 cubits or 300 feet.

Mersenne does not tell us that he has actually dropped heavy bodies from the altitude of 300 feet: it is a conclusion that he draws by applying the "duplicate proportion" to the experimental data at his disposal. Yet as these data "demonstrate" that a heavy body falls three feet in half a second, twelve in a second, forty-eight in two, 108

exprez pour marquer les seules secondes minutes, que la chorde de 2 pieds & demi ou environ faisoit les tours esgaux ausdites secondes. Ce qui n'empesche nullement la vérité ny la iustesse de nos observations, à raison qu'il suffit de scavoir que les secondes dont ie parle, sont esgales à la durée des tours de ma chorde de 3 pieds & demy: de sorte que si quelqu'un peut diviser le jour en 24 parties esgales, il verra aisément si ma seconde dure trop, et de combien est trop longue." For his subsequent experiments reported in *Cogitata Physico-Mathematica, Phenomena Ballistica*, pp. 38 ff, Mersenne used a pendulum of three feet only. He had noticed, indeed, that the three and a half feet one was a bit too long, though the difference was practically imperceptible; see *Cogitata*, p. 44.

[1] "One second of the prime mobile" is the time in which the "prime mobile", i.e. the skies, or the earth, describes a rotation of one second.

[2] See *Harmonie Universelle*, I, p. 138 (Paris: 1636).

[3] As a matter of fact, the foot used by Galileo is shorter – 29·57 cm. – than the "royal" foot used by Mersenne – 32·87 cm.; the difference of their respective data is therefore even much greater than it is assumed by the latter.

in three and 147 in three and a half[1] – figures that are in a perfect accord with the duplicate proportion – Mersenne feels entitled, and even bound, to assert that a heavy body will fall $166\frac{2}{3}$ feet in only $3\frac{1}{2}\frac{8}{5}$ seconds, and not in five. Moreover, he adds, from Galileo's figures, it would follow that a heavy body would fall only one cubit in half a second, and four cubits, that is about $6\frac{2}{3}$ feet, in one second, instead of the twelve feet which it descends in fact.

The results of the Mersennian experiments – the figures obtained by him – of which he is very proud, and of which he makes use for calculating the times with which bodies would fall from all possible altitudes up to the moon and the stars,[2] and the length of all kinds of pendulums with periods up to 30″ – constitute, undoubtedly, a progress in respect to those of Galileo. Yet they imply a rather awkward consequence, opposed not only to common sense and the fundamental teachings of mechanics, but also to Mersenne's own calculations: namely that the descent on the periphery of the circle is quicker than that on the perpendicular.[3]

Mersenne seems not to have noticed this consequence (nor did anybody else) at least for several years. In any case he does not mention it before the *Cogitata Physico-Mathematica* of 1644, where, resuming anew the discussion of the law of the fall and of the properties of the pendulum, he states it, though in somewhat attenuated form, together with that of the non-isochronism of the great and small oscillations.[4]

Thus, having explained how strange it is that a three-foot pendulum (which he uses now instead of the three-and-a-half-foot one which he employed formerly) makes his semi-oscillation in exactly half a second (that is, descends three feet), when free-falling bodies traverse twelve feet in a second (that is equally three feet in a semi-

[1] As a matter of fact, Mersenne obtained 110 and not 108 feet on one hand, and $146\frac{1}{2}$ on the other. But Mersenne does not believe in the possibility of reaching exactitude by experiment – considering the means at his disposal, he is perfectly right – and thus assumes that he is entitled to correct the experimental data in order to fit them to the theory. Once more he is perfectly right, as long, of course, as he remains (and he does) on this side of the margin of the experimental errors. Needless to say that Mersenne's procedure has been followed by science ever since. See Appendix, 2.

[2] See ibid., p. 140. In his calculations, Mersenne assumes – as Galileo – that the value of the acceleration is a universal constant.

[3] The ball descends on the quadrant of the circle as quickly as on the radius, if this radius is equal to 3 feet, or even quicker if the radius is equal to $3\frac{1}{2}$ feet.

[4] See *Cogitata Physico-Mathematica, Phenomena Ballistica*, pp. 38 and 39; see Appendix, 3.

second), whereas according to calculations made already in the *Harmonie Universelle* it should traverse in the time of a semi-oscillation 11/7 of the semi-diameter[1] (i.e., 33/7 or 5 feet), he continues,

> this implies a very great difficulty, because both [these facts] have been confirmed by numerous observations, namely that falling bodies traverse on the perpendicular twelve feet only, and that the three feet pendulum descends from *C* to *B* in half a second; which cannot occur but if the globe [of the pendulum] descends from *C* to *B* on the circumference in the same time as a similar globe [falls] on the perpendicular *AB*. Now as this one should descend 5 feet in the time in which the globe comes from *C* to *D*, I do not see any solution.

One could of course assume that bodies fall quicker than it has been admitted: but this would be against all observations. We have therefore, states Mersenne, either to accept that bodies fall on the perpendicular with the same speed as they descend on the circle, or that the air resists more strongly the motion downwards than the oblique one, or finally, that bodies traverse in free fall more than 12 feet in one second, and more than 48 in two, but that, because of the difficulty of ascertaining exactly, by attending to the sound of the percussion of the body on the pavement, the precise moment of this occurrence, all our observations concerning this question are utterly faulty.[2]

It must have been rather hard for Mersenne to admit the fallaciousness of his, so carefully made, experiments and the meaninglessness of the long calculations and tables based upon them. Yet it was unavoidable. Once more he had to recognize that precision couldn't be achieved in science and that its results were only approximately valid. Thus it is not surprising that in his *Reflexiones Physico-Mathematicae* of 1647 he tries in the same time to perfect his experimental methods – thus by holding the bob of the pendulum and the descending body (similar leaden spheres) in one and the same hand in order to ensure the simultaneity of the beginning of their motions,[3] and by fixing his pendulum to a wall in order to insure the simultaneity of the end of these motions by the merging together of the two sounds produced by the hit of the pendulum upon the wall and by that of the falling

[1] See ibid., p. 41.

[2] It is interesting to note that in his experiments, Mersenne determines the moment of arrival of the falling body to the earth not by sight, but by hearing; the same method will be followed by Huygens, doubtlessly under Mersenne's influence.

[3] See *Reflexiones Physico-Mathematicae* XVIII (Parisiis: 1647), pp. 152 ff.

body upon the ground; and to explain, at a considerable length, the lack of certainty of the results,[1] which, by the way, confirm those of his former investigations: the body seems to fall 48 feet in about 2″, and 12 in 1″. Yet, insists Mersenne, it is impossible to determine exactly the length of the pendulum of which the period would be precisely a second, nor is it possible to perceive, by hearing, the exact coincidence of the two sounds. A couple of inches or even feet more or less does not make any difference. Thus, he concludes, we have to content ourselves with approximations and not ask for more.

Nearly at the same time at which F.M. Mersenne performed his experiments, another experimental research of the laws of fall, linked together with an experimental determination of the value of g, was made in Italy by a team of Jesuit scientists led by the famous author of the *Almagestum Novum*, R.P. Giambattista Riccioli,[2] who, strangely enough, was perfectly independent from, and even wholly ignorant of, the work of Mersenne.

Riccioli has a rather bad reputation with the historians of science – a reputation not quite merited.[3] Yet one must confess that he is not only a much better experimenter than F.M. Mersenne, but even a much more intelligent one, and that he has an infinitely deeper understanding of the value and meaning of precision than the friend of Descartes and Pascal.

It was in 1640, when he was professor of philosophy in the *Studium* of Bologna, that Riccioli started a series of investigations of which I shall give here a brief account,[4] and I would like to stress the

[1] See ibid. XIX, p. 155: *De variis difficultatibus ad funependulum et casum gravium pertinentibus.*

[2] The report on these experiments is included in the *Almagestum Novum, Astronomiam veterem novamque complectens observationibus aliorum et propriis, Novisque Theorematibus, Problematibus ac Tabulis promotam . . . auctore P. Johanne Baptista Riccioli Societatis Jesu . . .* (Bononiae: 1651). The work had to have three volumes, but only the first one, in two parts, has been published. This "first volume" is, indeed, 1504 pages long (in folio).

[3] Riccioli is, of course, an anti-Copernican and, in his great works – *Almagestum Novum* of 1651 and *Astronomia Reformata* of 1665 – he heaps arguments upon arguments in order to refute Copernicus, which is, indeed, regrettable, but after all rather natural for a Jesuit. On the other hand, he does not hide his great admiration for Copernicus and Kepler and gives a surprisingly correct and honest account of the astronomical theories he is criticizing. He is immensely learned and his works, especially the *Almagestum Novum*, are an invaluable source of information. This makes his ignorance of the works of Mersenne so much more surprising.

[4] See *Almagestum Novum*, I (1), bk. II, ch. XX and XXI, pp. 84 ff and I (2), bk.

carefully thought out and methodical way in which he proceeds with his work. He does not want to take anything for granted and, though, as a matter of fact, he is firmly convinced of the value of Galileo's deductions, he first tries to establish, or better to say, to verify, whether the thesis of the isochronism of pendular oscillations is exact; then, whether the relation asserted by Galileo between the length of the pendulum and its period (period proportional to the square root of the length) is confirmed by experience; finally, to determine, as precisely as possible, the period of a given pendulum in order to obtain in this way a time-measuring instrument fit to be used for the experimental research of the speed of fall.

Riccioli starts by preparing a convenient pendulum: a spherical metallic bob suspended from a chain[1] attached to a metallic cylinder turning freely in two, equally metallic, sockets. A first series of experiments aims at the verification of the Galilean assertion of the constancy of the period of the pendulum by counting the number of its oscillations in a given time. The time is measured by the means of a water-glass and Riccioli, revealing a deep understanding of the empirical conditions of experimentations and measurement, explains that it is the double process of running out and filling again of the water-glass that is to be taken as the unit of time. The results of this first series confirm the assertion of Galileo.

A second series of experiments for which Riccioli uses two pendula, of the same weight, but of different length ("height"), namely of one and of two feet, confirms the square root relation established by Galileo: the number of oscillations in the unit of time is, respectively, 85 and 60.[2]

Mersenne would probably stop at this point. Not Riccioli; he understands quite well that even by using his method of turning the water-glass upside down one is still far away from real precision: for this we have still to look elsewhere, that is to the skies, to the only really exact *horologium* existing in this world, to the *organa chronou* provided by nature, the motions of the heavenly bodies and spheres.

Riccioli realizes full well the tremendous importance of the Galilean discovery: the isochronism of the pendulum enables us to achieve a *precise* timekeeper. Indeed, the fact that large and small oscillations

IX, sect. IV, 2, pp. 384 ff. I presented a report on the experiments of Riccioli to the XXII *Congrès international de Philosophie des Sciences* which met in Paris, in 1949.

[1] See *Almagestum Novum*, I (1), bk. II, ch. XX, p. 84.

[2] See ibid., ch. XXI, prop. VIII, p. 86.

are performed in the same time entails the possibility of maintaining its motion as long as we want by counteracting its normal and spontaneous slowing down; for instance, by giving it a new push after a certain amount of beats[1]; thus any number of atoms of time can be accumulated and added together.

It is clear, however, that in order to be able to use the pendulum as a *precise* instrument for measuring time, we have to determine *exactly* the value of its period. This is the task to which, with an unyielding patience, Riccioli will devote himself. His aim is to manufacture a pendulum of which the period would be exactly one second.[2] Alas, in spite of all efforts he will not be able to reach his goal.

To begin with he takes a pendulum weighing about one pound and three feet and four inches (Roman)[3] "high". The comparison with the water-glass has been satisfactory: nine hundred oscillations in a quarter of an hour. Riccioli proceeds, then, to a verification by the means of a sundial. For six consecutive hours, from nine o'clock in the morning to three o'clock in the afternoon, he counts (he is aided by the R.P. Francesco Maria Grimaldi) the oscillations. The result is disastrous: 21,706 oscillations instead of 21,660. Moreover, Riccioli recognizes that for his aim the sundial itself lacks the wanted precision. Another pendulum is prepared and "with the aid of nine Jesuit fathers",[4] he starts counting anew; this time – 2 April 1642 – for twenty-four consecutive hours, from noon to noon: the result is 87,998 oscillations whereas the solar day contains only 86,640 seconds.

Riccioli makes then a third pendulum, lengthening the suspension chain to 3 feet, 4·2 inches. And, in order to increase the precision even more, he decides to take as a unit of time not the solar, but the sidereal day. The count goes on from the passage through the meridian line of the tail of the Lion (12 May 1642) till its next passage on the thirteenth. Once more a failure: 86,999 oscillations instead of 86,400 that there should have been.

Disappointed yet still unbeaten, Riccioli decides to make a fourth trial, with a fourth pendulum, somewhat shorter this time, of 3 feet,

[1] This pushing of the pendulum is by no means easy and implies a long training.

[2] Riccioli, as we shall see, is not as easily satisfied as Mersenne.

[3] A Roman foot is equal to 29·57 cm.

[4] See *Almagestum Novum*, loc. cit., p. 86. The names of these fathers ought to be preserved as examples of devotion to science; here they are (see I (2), p. 386): Stephanus Ghisonus, Camillus Rodengus, Jacobus Maria Palavacinus, Franciscus Maria Grimaldus, Vicentius Maria Grimaldus, Franciscus Zenus, Paulus Casarus, Franciscus Adurnus, Octavius Rubens.

and 2·67 inches only.[1] But he cannot impose upon his nine companions the dreary and wearisome task of counting the swings. Father Zeno and Father F.M. Grimaldi alone remain faithful to him to the end. Three times, three nights, 19 and 28 May and 2 June 1645, they count the vibrations from the passage through the meridian line of the Spica (of Virgo) to that of Arcturus. The numbers are twice 3,212 and the third time 3,214 for 3,192 seconds.[2]

At this point Riccioli seems to have had enough of it. After all, his pendulum, the period of which is equal to 59·36‴, is a perfectly usable instrument. The transformation of the number of oscillations into seconds is easy. Besides, it can be facilitated by precalculated tables.[3]

Still, Riccioli is rather worried about his lack of success. He tries, therefore, to calculate the "height" of a pendulum which would swing in exactly a second: arriving at the result that it should be 3 feet, 3·27 inches.[4] He confesses, however, not having actually made it. On the other hand he has certainly manufactured much shorter pendulums in order to achieve a greater refinement in measuring time intervals: one of 9·76 inches with the period of 30‴; another, still shorter, of 1·15 inches of which the period is only 10‴.

"It is such a pendulum that I employed," says Riccioli, "for measuring the speed of the natural descent of heavy bodies" in the experiments performed in this same year 1645 at the Torre degli Asinelli, in Bologna.[5]

Now it is obviously impossible to use so rapid a pendulum simply by counting its swings; one has to find out some means of summing them up. In other words, one has to construct a clock. Actually it is a clock, the first pendulum clock, that Riccioli has built for his experiments. Yet it would be difficult to consider him a great clockmaker, a forerunner of Huygens and Hooke. His clock, indeed, had neither weight nor spring, nor even hands or dial. As a matter of fact, it was not a mechanical clock, but a human one that he built.

In order to sum up the beats of his pendulum Riccioli imagined a very simple, and a very elegant device: he trained two of his colla-

[1] See ibid., p. 87.

[2] See ibid., p. 85. As the motion of the pendulum is not isochronous the exquisite concordance of the results of Riccioli's experiments can be explained only if we assume that he made his pendulums perform practically equal and *small* oscillations.

[3] Riccioli gives these tables in the *Almagestum Novum*, I (1), bk. 2, ch. XX, prop. XI, p. 387.

[4] See ibid. and I (2), p, 384.

[5] See ibid., I (1), p. 87.

borators and friends, "gifted not only for physics but also for music, to count *un, de, tre* . . . (in the Bolognese dialect in which these words are shorter than in Italian) in a perfectly regular and uniform way, as are wont to do those who direct the execution of musical pieces, in such a way that to the pronunciation of each figure corresponded an oscillation of the pendulum."[1] It is with this "clock" that he performed his observations and experiments.

The first question studied by Riccioli concerned the behaviour of "light" and "heavy" bodies.[2] Do they fall with the same, or with different, speeds? A very important, and very controversial, question, to which, as we know, ancient and modern physics gave different answers. Whereas the Aristotelians maintained that bodies fall so much quicker as they are heavier, Benedetti had taught that all bodies, at least all bodies possessing an identical nature, i.e. specific gravity, fell with the same speed. As for the moderns, such as Galileo and Baliano, followed by the Jesuits Vendelinus and N. Cabeo, they asserted that all bodies, whatever their nature or weight, fell always with the same identical speed (in the *vacuum*).[3]

Riccioli wants to settle this problem once and forever. Thus on 4 August 1645 he proceeds to work. Spheres of equal size but of different weight, made, respectively, of clay and of paper, covered with chalk (this is in order to make their motion along the wall, as well as their bursting when reaching the pavement, easier to observe), were dropped from the summit of the Torre dei Asinelli, particularly convenient for this kind of experiment[4] and sufficiently high – 312 Roman feet – to make such differences in speed perceptible in their effects. The results of the experiments, which Riccioli repeats fifteen times, are indubitable: heavy bodies fall quicker than light ones. Yet their lagging behind, which, depending on the weight and the dimension of the balls, varies from 12 to 40 feet, does not contradict the theory developed by Galileo: it is to be explained by the resistance of the air and has been foreseen by him. On the other hand the observed facts are perfectly incompatible with the teachings of Aristotle.[5]

Riccioli is intensely conscious of the originality and of the value of

[1] See ibid., I (2), p. 384.

[2] Riccioli, a hundred years behind his time, still believes in "lightness" as an independent quality correlated with, and opposed to, "heaviness".

[3] See ibid., p. 387.

[4] The Torre degli Asinelli possesses vertical walls and stands on a rather large and flat platform.

[5] See ibid., p. 388.

his work. Accordingly he pokes fun at the "semi-empiricists" who don't know how to make a really conclusive experiment and who, for instance, assert – or deny – that bodies fall with the same speed because they are unable to determine the precise moment when the body strikes the pavement.[1]

The second problem investigated by Riccioli is even more important. He wants to ascertain the proportion with which the falling body accelerates its motion. Is it, as it is taught by Galileo, a "uniformly difform" (uniformly accelerated) motion, that is a motion in which the spaces traversed are *ut numeri impares ab unitate* or, as Baliano wants it, a motion in which these spaces are a series of natural numbers? As for the speed, is it proportional to the duration of the fall, or to the space traversed.[2]

Aided by R.P. Grimaldi, Riccioli manufactures a number of balls made of chalk, of identical dimensions and weight, and, after having established by direct measurement of the times of their falling from different storeys of the Torre dei Asinelli that they follow the Galilean law,[3] he proceeds to the verification of this result (nothing is more characteristic than this inversion of the procedure) by dropping these balls from previously calculated, determined, altitudes, using to this purpose all the churches and towers of Bologna of which the heights are appropriate, namely, those of St Peter, St Petronio, St James, and St Francis.[4]

The results are concordant in all details. Indeed their accord is so perfect, the spaces traversed by the balls (15, 60, 135, 240 feet) confirm the Galilean law in so rigorous a manner that it is quite obvious that the experimenters have been convinced of its truth before starting. Which, after all, is not surprising, as the experiments with the pendulum have already given to it a full confirmation.

Yet even if we admit – as we must – that the good fathers corrected somewhat the actual results of their measurements, we have nevertheless to acknowledge that these results are of a quite surprising

[1] See ibid. and I (1), p. 87.

[2] See ibid. It is interesting to note that Riccioli uses the old scholastic terminology and, quite correctly, identifies the "uniformly difform" (*uniformiter difformis*) motion with the uniformly accelerated (or retarded) one.

[3] He tells us, indeed, that he thought about the problem since 1629 and adopted the relation 1, 3, 9, 27, before having read Galileo in 1634, having been allowed to do so by his superiors. It is interesting to note that before having read Galileo the very learned Riccioli did not identify the *uniformiter difformis* motion with that of the fall.

[4] See ibid., p. 387. The experiments were continued from 1640 to 1650.

precision. Compared to the rough approximations of Galileo himself and even to those of Mersenne, they constitute a decisive progress. They are certainly the best ones that could be obtained by direct observation and measurement and one cannot but admire the patience, the conscience, the energy, and the passion for truth of the R.P. Zeno, Grimaldi, and Riccioli (as well as of their collaborators) who, without any other instrument for measuring time than the human clock into which they transformed themselves, were able to determine the value of the acceleration, or, more exactly, the length of the space traversed by a heavy body in the first second of its free fall through the air, as being equal to 15 (Roman) feet. A value which Huygens alone, by using the mechanical clock invented by him, or better to say, by applying indirect methods which his mathematical genius enabled him to discover and to use in the very construction of his clock, will be in a position to improve.

It is very interesting, and very instructive, to study the *modi procedendi* of the great Dutch scientist to whom we owe our watches and clocks. Their analysis enables us to witness the transformation of the still empirical or semi-empirical experiences of Mersenne and Riccioli into a truly scientific experiment; it imparts to us, too, a very important lesson, namely, that in scientific investigations the direct approach is by no means the best nor the easiest one, and that empirical facts are to be reached only by using a theoretical circuit.

Huygens starts his work by repeating, in 1659 (21 October), the (last) experiments of Mersenne, as described by the latter in his *Reflexiones* of 1647; and once more we are obliged to stress the appalling poverty of the experimental means at his disposal: a string-pendulum attached to the wall; its bob, a leaden ball, and another, similar, leaden, globe are held in the same hand; the simultaneity of the arrival of the two globes, respectively, to the wall and to the ground is determined by the coincidence of the two sounds produced by the hits. Strangely enough, using exactly the same procedure as Mersenne, Huygens obtains better results; according to him, the body falls 14 feet.[1]

On 23 October 1659 Huygens repeats the experiment, using this time a pendulum the semi-vibration of which is equal not to a half-

[1] See Ch. Huygens, *Œuvres*, XVII, p. 278 (La Haye: M. Nijhof, 1932): "II D. 1. Expertus 21 Oct. 1659. Semisecundo minuto cadit plumbum ex altitudine pedum et dimidij vel 7 pollicum circiter. Ergo unius secundi spatio ex 14 pedem altitudine."

second, but to three-quarters of it. During this time the leaden sphere falls 7 feet, 8 inches. It follows that in one second it would fall about 13 feet, $7\frac{1}{2}$ inches.[1]

On 15 November 1659 Huygens makes a third trial. This time he improves somewhat his procedure by attaching both bob and the leaden sphere to a thread (instead of holding them in the same hand) by the cutting of which they are released. Moreover, he puts parchment on the wall and the ground to make the perception of the sounds more distinct. The result is about 8 feet, $9\frac{1}{2}$ inches. Yet, just as Mersenne before him, Huygens is obliged to admit that his result is valid only as an approximation, because three or even four inches more or less in the height of the fall cannot be distinguished by the means employed by him: the sounds seem to coincide. It follows, therefore, that an exact measure cannot be obtained in this way. But the conclusion he draws therefrom is by no means the same. Quite on the contrary. Whereas Mersenne renounces the very idea of scientific precision, Huygens reduces the role of the experiment to that of verification of theoretically arrived at results. It is enough when it does not contradict them, as in this case where the observed figures are perfectly compatible with those deduced from the analysis of the motion of the circular pendulum, i.e. about 15 feet, $7\frac{1}{2}$ inches per second.[2]

[1] See Huygens, *Œuvres*, XVII, p. 278. "II D. 2. Expertus denuo 23 Oct. 1659. Pendulum adhibui cujus singulae vibrationes 3/2 secundi unius, unde semivibratio qua usus sum erat 3/4." Erat penduli longitudo circiter 6 p. 11 unc. Sed vibrationes non ex hac longitudine sed conferendo eas cum pendulo horologij colligebam. Illius itaque semivibratione cadebat aliud plumbum simul e digitis demissum, ex altitudine 7 pedum 8 unc. Ergo colligitur hinc uno secundo casurum ex altitudine 13 ped. 7 1/2 unc, ferè.

"Ergo in priori experimento debuissent fuisse non toti 3 ped. 5 poll.

"Sumam autem uno secundo descendere plumbum pedibus 13. unc. 8. Mersenne 12 ped, paris. uno secundo confici scribit. 12 ped. Paris. conficiunt circiter 12 ped. 8 unc. Rhijnland. Ergo Mersenni spatium justo brevius est uno pede Rhijnl."

A Rheinland foot is equal to 31·39 cm.

[2] See ibid., p. 281: "II D. 4. 15 Nov. 1659. Pendulum AB semivibrationi impendebat 3/4 unius secundi; filum idem BDC plumbum B et glandem C retinebat, deinde forficubus filum incidebatur, unde necessario eodem temporis articulo globulus C et pendulum moveri incipiebant. plumbum B in F palimsesto impingebatur, ut clarum sonum excitaret. globulus in fundum capsae GH decidebat. simul autem sonabant, cum CE altitudo erat 8 pedum et 9 1/2 unciarum circiter. Sed etsi 3 quatuorve uncijs augeretur vel diminueretur altitudo CE nihilo minus simul sonare videbantur. adeo ut exacta mensura hoc pacto obtineri nequeat. At ex motu conico penduli debebant esse ipsi 8 pedes et 9 1/2 uniciae. unde uno secundo debebunt peragi a plumbo cadente pedes 15. unc. 7 1/2 proxime. Sufficit

As a matter of fact, the analysis of the pendular motion gives, as we shall see, even better results.

I have already mentioned the paradoxical situation of modern science at the time of its birth: possession of exact mathematical laws combined with the impossibility of their application because of the inability of performing a precise measurement of the fundamental magnitude of dynamics, that is, of time.

Nobody seems to have felt it more strongly than Huygens, and it is certainly for that reason, and not for practical considerations such as the necessity of good clocks for navigation – though he by no means neglected the practical aspect of the question[1] – that, at the very beginning of his scientific career, he applied himself to the solution of this fundamental and preliminary problem: the perfecting, or better, the building of a perfect timekeeper.

It is in the year 1659, the same year in which he made the measurements I have just reported, that he reached his goal by constructing an improved pendulum clock[2]; a clock which he used in order to determine the exact value of the oscillation of the pendulum that he had employed in his experiments.

In the history of scientific instruments Huygens' clock occupies a very important position: it is the first apparatus that embodies in its construction the laws of the new dynamics; it is the result not of empirical trial and error, but of careful and subtle theoretical in-

quod experientia huic mensurae non repugnet, sed quatenus potest eam comprobet. Si plumbum B et globulum C inter digitos simul contineas ijsque apertis simul dimittere coneris, nequaquam hoc assequeris, ideoque tali experimento ne credas. Mihi semper hac ratione minus inveniebatur spatium CE, adeo ut totius interdum pedis differentia esset. At cum filum secatur nullus potest error esse, dummodo forfices ante sectionem immotae teneantur. Penduli AB oscillationes ante exploraveram quanti temporis essent ope horologij nostri. Experimentum crebro repetebam. Ricciolus Almag. 1. 9 secundo scrupulo 15 pedes transire gravia statuit ex suis experimentis. Romanos nimirum antiquos a Rhenolandicis non differre Snellius probat."

[1] Member of a maritime nation, Huygens was fully aware of the value and importance of a good timekeeper for navigation, as well as of the financial possibilities of the invention of a marine clock. It is well known that he tried to have his clock patented in England. See L. Defossez, op. cit., pp. 115 ff.

[2] The first pendulum clock was constructed by Huygens in 1657; it contains already curved jaws ensuring the isochronism of the (flexible) pendulum. Yet these jaws were not yet mathematically determined, but formed only on the basis of empirical trial and error procedure. It was only in 1659 that Huygens discovered the isochronism of the cycloid and the means of making the bob of the pendulum move along a cycloid.

vestigation of the mathematical structure of circular and oscillatory motions. Thus the very history of the pendulum-clock gives us a good example of the value of the roundabout way in preference to the direct one.

Huygens, indeed, is perfectly aware that, as already discovered by Mersenne, small and large oscillations of the pendulum are not performed in the same time. In order to construct a perfect timekeeper one has, therefore, (*a*) to determine the truly isochronous curve, and (*b*) to find out the means to make the bob of the pendulum move along this line and not along the periphery of the circle. It is well known that Huygens succeeded in solving both problems (though for doing it he had to devise a completely new geometrical theory),[1] and to achieve a perfectly isochronous motion, the motion along the cycloid; moreover, that he succeeded in fitting his cycloidal pendulum into a clock.[2]

He was now in position to proceed, with an infinitely better equipment – a mechanical clock instead of a human one – and therefore a much better chance of reaching precision, to experiments on the line of those of Riccioli. Yet he never tried to perform them. This because the construction of the pendulum-clock put into his hands a much better procedure.

As a matter of fact, he had not only discovered the isochronism of the motion along the cycloid, but also – something that Mersenne had tried (but failed) to find out for the circle – the relation between the time of descent of a body along the cycloid to that of its fall along the diameter of its generating circle: these times are to each other as the semi-circumference is to the diameter.[3]

Thus, if we could make a (cycloidal) pendulum, swinging in precisely one second, we would be able to determine the exact time in which a heavy body would descend along its diameter, and therefrom – the spaces traversed being proportional to the squares of time – to calculate the distance of its fall in one second.

The length of such a pendulum – which, besides, needn't be a cycloidal one, because, as Huygens will point it out to Moray,[4] small

[1] That of the evolutes of geometrical curves.

[2] See L. Defossez, op. cit., p. 65; on the contemporary attempts of R. Hooke, see Louise D. Patterson, "Pendulums of Wren and Hooke", *Osiris*, X, pp. 277-322 (1952).

[3] See Ch. Huygens, *De vi centrifuga* of 1659, *Œuvres*, XVI, p. 276 (La Haye: M. Nijhof, 1929).

[4] See Christiaan Huygens, Lettre à R. Moray 30 decembre 1661, *Œuvres*

oscillations of a common (perpendicular) pendulum are performed in practically the same amount of time as those of the cycloidal one – can be easily calculated as soon as we have succeeded in determining the period of a given cycloidal one.

But, as a matter of fact, we do not need to bother ourselves with the actual fabrication of such a pendulum. This because the formula devised by Huygens,

$$g=(4\pi^2r^2l)/3600^2 \quad \text{or} \quad T=\pi\sqrt{(l/g)}$$

has a general value and determines the value of g as a function of the length and the speed of whatever pendulum we may be using. Indeed, it is a rather short and quick pendulum that Huygens has used, a pendulum only 6·18 inches long, and making 4,464 double oscillations per hour. Accordingly, Huygens concluded that the value of g is 31·25 feet, i.e. 981 cm., which is the value that has been accepted ever since.[1]

The moral of this history of the determination of the acceleration constant is thus rather curious. We have seen Galileo, Mersenne, Riccioli endeavouring to construct a timekeeper in order to be able to make an experimental measure of the speed of the fall. We have seen Huygens succeed, where his predecessors had failed, and by his very success, dispense with making the actual measurement. This, because his timekeeper is, so to say, a measurement in itself; the determination of its exact period is already a much more precise and

complètes, publiées par la Société hollandaise des sciences, III, p. 438 (La Haye: M. Nijhof, 1890): "Je ne trouve pas qu'il soit nécessaire d'égaler le mouvement du pendule par les portions de la Cycloide pour determiner cette mesure, mais qu'il suffit de le faire mouvoir par des vibrations fort petites, lesquelles gardent assez pres l'eglitè des temps, et chercher ainsi quelle longueur il faut pour marquer, par exemple, une demie seconde par le moyen d'une horologe qui soit desia en train de bien aller, et ajustee avec la Cycloide."

[1] See Huygens, *Œuvres*, XVII, p. 100: "Het getal van de dobbele slaegen die het pendulum in een uyr doen moet, gegeven sijnde, quadreert het selve, en met het quadraat divideert daer mede 12312000000. ende de quotiens sal aenwijsen de lenghde van het pendulum. te weten als men de twee laetste cijffers daer af snijt, soo is het resterende het getal der duijmen die het pendulum moet hebben; de 2 afgesnedene cijffers beteijckenen, het een, de tienden deelen van een duijm die daer noch bij moeten gedaen werden, het ander, de 100ste deelen van een duym van gelijcken daer bijte doen. Rhynlandse maet.

"bij exempel Een horologe to maecken sijnde diens pendulum 4464 dobbele slagen in een uijr doen sal, het quadraet van 4464 is 19927296, waer mede gedeelt sijnde 12312000000, komt 6/18 ontrent. dat is 6 duijm 1/10 en 8/100 van een duijm. Indien het getal van de heele duijmen meer is als 12 soo moet het door 12 gedeelt werden om te weten hoe veel heele voeten daer in sijn."

refined experiment than all those that Mersenne and Riccioli have ever thought of. The meaning and value of the Huygensian circuit – which finally revealed itself as a shortcut – is therefore clear: not only are good experiments based upon theory, but even the means to perform them are nothing else than theory incarnate.

APPENDIX

1. M. Mersenne, *Harmonie Universelle* (Paris: 1636), pp. 111 ff:

Or il faut icy mettre les experiences que nous avons faites très exactement sur ce suiet, afin que l'on puisse suivre ce qu'elles donnent. Ayant donc choisi une hauteur de cinq pieds de Roy, et ayant fait creuser, et polir un plan, nous luy avons donné plusieurs sortes d'inclinations, afin de laisser rouler une boule de plomb, et de bois fort ronde tout au long du plan: ce que nous avons fait de plusieurs endroits différents suivant les différentes inclinations, tandis qu'une autre boule de mesme figure, et pesanteur tombait de cinq pieds de haut dans l'air; et nous avons trouvé que tandis qu'elle tombe perpendiculairement de cinq pieds de haut, elle tombe seulement d'un pied sur le plan incliné de quinze degrez, au lieu qu'elle devroit tomber seize poulces.

Sur le plan incliné de vingt cinq degrez le boulet tombe un pied 5 demi, il devroit tomber deux pieds, un pouce un tiers: sur celuy de trente degrez il tombe deux pieds: il devroit tomber deuż pieds et 1/23 car il feroit six pieds dans l'air, tandis qu'il tombe deux pieds 1/2 sur le plan, au lieu qu'il ne devroit tomber que cinq pieds. Sur le plan incliné de 40 degrez, il devroit tomber trois pieds deux pouces 1/2: et l'experience très exacte ne donne que deux pieds, neuf pouces, car lorsqu'on met le boulet à deux pieds dix pouces loin de l'extrémité du plan le boulet qui se meut perpendiculairement chet le premier; et quand on l'éloigne de deux pieds huit pouces sur le plan, il tombe le dernier: et lorsqu'on l'éloigne de deux pieds neuf pouces, ils tombent instement en mesme temps, sans que l'on puisse distinguer leur bruits.

Sur le plan de quarante cinq degrez il devroit tomber trois pieds et 1/2 un peu davantage, mais il ne tombe que trois pieds, et ne tombera point trois pieds 1/2, si l'autre ne tombe cinq pieds 3/4 par l'air.

Sur le plan de cinquante degrez il devroit faire trois pieds dix pouces, il n'en fait que deux et neuf pouces: ce que nous avons repeté plusieurs fois très exactement, de peur d'avoir failly, à raison qu'il tombe en mesme temps de 3 pieds, c'est à dire de 3 pouces davantage sur le plan incliné de 45 degrez: ce qui semble fort estrange, puisqu'il doit tomber dautant plus viste que le plan est plus incliné: Et néanmoins il ne va pas plus viste sur le plan de 50 degrez que sur celuy de 40: où il faut remarquer que ces deux inclinations sont également éloignées de celle de 45 degrez, laquelle tient le milieu entre les deux extremes, à sçavoir entre l'inclination infinie faite dans la ligne perpendiculaire et celle de l'horizontale: toutefois si l'on considère

cet effet prodigieux, l'on peut dire qu'il arrive à cause que le mouvement du boulet estant trop violent dans l'inclination de 50 degrez, ne peut rouler et couler sur le plan, qui le fait sauter plusieurs fois: dont il s'ensuit autant de repos que de sauts, pendant lesquels le boulet qui chet perpendiculairement, avance toujours son chemin: mais ces sauts n'arrivent pas dans l'inclination de 40, et ne commencent qu'après celle de 45, insques à laquelle la vitesse du boulet s'augmente toujours de telle sorte qu'il peut toujours rouler sans sauter: or tandis qu'il fait trois pieds dix pouces sur le plan incliné de cinquante degrez, il en fait six 1/2 dans l'air au lieu qu'il n'en devroit faire que cinq.

Nous avons aussi experimenté que tandis que la boule fait 3 pieds 10 pouces sur le plan incliné de 50 degrez, elle fait 6 pieds 1/2 par l'air, combien qu'elle ne denst faire que cinq pieds. A l'inclination de 40, elle fait quasi 7 pieds dans l'air, pendant qu'elle fait 3 pieds 2 pouces 1/2 sur le plan; mais l'expérience reiteree à l'inclination de 50, elle fait 3 pieds sur le plan, quoy que la mesme chose arrive à 2 pieds 9 pouces: ce qui monstre la grande difficulté des experiences: car il est très difficile d'appercevoir lequel tombe le premier des deux boulets dont l'un tombe perpendiculairement, et l'autre sur le plan incliné. J'ajoute néanmoins le reste de nos experiences sur les plan inclinez de 60 et de 65 degrez: le boulet éloigné de l'extremité du plan de 2 pieds, 9 pouces, ou de 3 pieds, tombe en mesme temps que celuy qui chet de cinq pieds de haut perpendiculairement, et néanmoins il devroit cheoir 4 pieds 1/3 sur le plan de 60, et 4 pieds 1/2 sur celuy de 65. Sur le plan de 75 il devroit faire 4 pieds 10 pouces, et l'experience ne donne que 3 pieds 1/2.

Peut estre que si les plans ne donnoient point plus d'empeschement aux mobiles que l'air, qu'ils ne tomberoient suivant les proportions que nous avons expliqué: mais les experiences ne nous donnent rien d'asseuré, particulièrement aux inclinations qui passent 45 degrez, parce que le chemin que fait le boulet à cette inclination, est quasi égal à celuy qu'il fait sur les plans de 50, 60, et 65; et sur celuy de 75 il ne fait que demi pied davantage.

F. Mersenne allows himself even to doubt the actual performance by Galileo of some of the experiments mentioned by the great scientist. Thus, referring to the experiments on the inclined plane described by Galileo in his *Dialogo* (not to those described in the *Discorsi*, which I have quoted), he writes (*Harmonie Universelle*, 112, corr. I):

Je doute que le sieur Galilée ayt fait les experiences des cheutes sur le plan puisqu'il n'en parle nullement, et que la proportion qui donne contredit souvent l'experience: et desire que plusieurs esprouvent la mesme chose sur des plans differens avec toutes les précautions dont ils pourront s'aviser, afin qu'ils voyent si leurs experiences respondront aux notres, et si l'on en pourra tirer assez de lumiere pour faire un Theoreme en faveur de la vitesse de ces cheutes obliques, dont les vitresses pourroient estre mesurees par les differens effets du poids, qui frappera dautant plus fort

que le plan sera moins incliné sur l'horizon, et qu'il approchera davantage de la ligne perpendiculaire.

2. Ibid., p. 138:

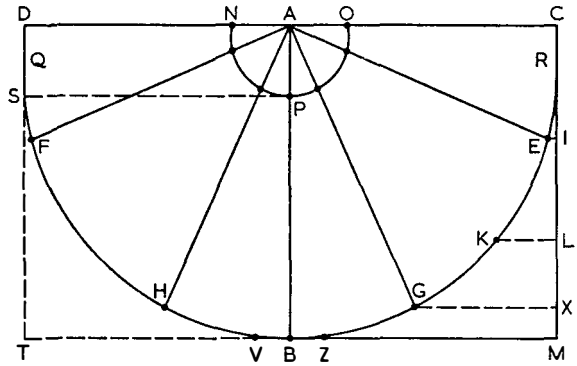
Mais quant à l'expérience de Galilée, on ne peut ni imaginer d'où vient la grande différence qui se trouve icy à Paris et aux environs, touchant le tems des cheutes, qui nous a toujours paru beaucoup moindre que le sien: ce n'est pas que je veuille reprendre un si grand homme de peu de soin en ses expériences, mais on les a faites plusieurs fois de différentes hauteurs, en présence de plusieurs personnes, et elles ont toujours succédé de la mesme sorte. C'est pourquoy si la brasse dont Galilée s'est servy n'a qu'un pied et deux tiers, c'est à dire vingt poulces de pied du Roy dont on use à Paris, il est certain que le boulet descend plus de cent brasses en 55″.

Cecy étant posé, les cent brasses de Galilée font 166 2/3 de nos pieds, mais nos expériences répétées plus de cinquante fois, jointes à la raison doublée, nous contraignent de dire que le boulet fait 300 pieds en 5″, c'est à dire 180 brasses, ou quasi deux fois davantage qu'il ne met: de sorte qu'il doit faire les cent brasses, ou 166 pieds 2/3 en 3″ et 18/25, qui font 3″, 43‴, 20⁗, et non pas 5″; car nous avons prouvé qu'un globe de plomb pesant environ une demie livre, et que celuy de bois pesant environ une once tombent de 48 pieds en 2″, de 108 en 3″, et de 147 pieds en 3″ et ½. Or les 147 pieds reviennent à 88 et 1/5 brasses; et s'il se trouve du mesconte, il vient plustot de ce que nous donnons trop peu d'espace aux dits temps, qu'au contraire, car ayant laissé cheoir le poids de 110 pieds, il est iustement tombé en 3″, mais nous prenons 108 pour régler la proportion; et les hommes ne peuvent observer la différence du temps auquel il tombe de 110, ou de 108 pieds. Quant à la hauteur de 147 pieds, il s'en fallait un demi-pied, ce qui rend la raison double très-iuste, d'autant que le poids doit faire 3 pieds en une demie seconde, suivant cette vistesse, 12 pieds dans une seconde minute; et conséquemment, 27 pieds en 1″ et ½, 48 pieds en 2″, 75 en 2″ et ½, 108 pieds en 3″ et 147 pieds en 3″ et ½, ce qui revient fort bien à nos experiences, suivant lesquelles il tombera 192 pieds en 4″ et 300 en 5″, pendant lequel Galilée ne met que 166 pieds ou 100 brasses, selon lesquelles il doit faire une brasse en une demie seconde, 4 en 1″, ce qui font près de 6 pieds 2/3, au lieu de 12 que le poids descend en effet.

3. F. M. Mersennus, *Cogitata physico-mathematica*, Phenomena ballistica (Parisii: 1644). Propositio XV: Grauium cadentium velocitatem in ratione duplicata temporum augeri probatur ex pendulis circulariter motis, ipsorúmque pendulorum multifarius usus explicatur, 38-44:

Certum est secundò filum à puncto C ad B cadens temporis insumere tantundem in illo casu, quantum insumit in ascensu à B ad D per circumferentiam BHFD; sit enim filum AB 12 pedum, docet experientia globum B tractum ad C, inde ad B spatio secundi minuti recidere, & alterius secundi spatio à B versus D ascendere. Si verò AB trium pedum fuerit, hoc

est praecedentis subquadruplum, spatio dimidij secundi à C descendet ad B, & aequali tempore à B ad D vel S perueniet; ad D si filum & aër nullum afferant impedimentum, cùm impetus ex casu C in B impressus sufficiat ad promouendum globum pendulum ad D punctum.



Globus igitur spatio secundi percurret dimidiam circumferentiam CBD, & aequali tempore à D per B versus C recurret; donec hinc inde vibratus tandem in puncto B quiescat, siue ab aëris & fili resistentiam vnicuique cursui & recursui aliquid detrahentem, siue ob ipsius impetus naturam, quae sensim minuatur, qua de re postea. Nota verò globum plumbeum vnius vnciae filo tripedali appensum, non priùs quiescere postquam ex puncto C moueri coepit, quàm trecenties sexagies per illam semicircumferentiam ierit; cuius postremae vibrationes à B ad V sunt adeo insensibiles, vt illis nullus ad obseruationes vti debeat, sed alijs maioribus, quales sunt ab F, vel ab H ad B. . . .

Certum est tertiò filum AP fili AB subquadruplum vibrationes suas habere celeriores vibrationibus fili BA; esséque filum AB ad PA in ratione duplicata temporum quibus illorum vibrationes perficiuntur, atque adeo tempora habere se ad filorum longitudines vt radices ad quadrata; quapropter ipsae vibrationes sunt in eadem ac tempora ratione. . . .

Sextò, filum tripedale potest alicui iustò videri longius ad secundum minutum qualibet vibratione notandum, cùm enim in linea perpendulari AB graue cadens citiùs ad punctum B perueniat, quam vbi ex C vel D per circumferentiae quadrantem mouetur, quandoquidem AB linea breuissimè ducit ad centrum grauium, & tamen ex observationibus grauia cadentia tripedale duntaxat interuallum ab A ad B semisecundo, & 12 pedes secundo conficiant, illud filum tripedale minus esse debere videtur: Iamque lib. 2. de causis sonorum, corollario 3. prop. 27, monueram eo tempore quo pendulum descendit ab A, vel C and B per CGB, posita perpendiculari AB 7 partium, graue per planum horizonti perpendiculare partes vndecim descendere.

Quod quidem difficultatem insignem continet, cùm vtrúmque multis obseruationibus comprobatum fuerit, nempe grauia perpendiculari motu

duodecim solummodo pedes spatio secundi, globum etiam circumferentiae quadrantem, cuius radius tripedalis, à D ad B semisecundo percurrere; quae fieri tamen nequeunt nisi globus à C ad B per circumferentiae quadrantem descendat eodem tempore quo globus aequalis per AB: qui cùm pedes 5 perpendiculariter descendat eo tempore quo globus à C ad D peruenit, nulla mihi solutio videtur; nisi maius spatium à graui perpendiculariter cadente percurri dicatur quam illud quod hactenus notaueram, quod cùm ab vnoquóque possit obseruari, nec vlla velim mentis anticipatione veritati praeiudicare, nolui dissimulare nodum, quem alius, si potis est, soluat. Vt vt sit obseruatio pluries iterata docet tripedale filum nongentesies spatio quadrantis horae vibrari, ac consequenter horae spatio 3600: quapropter si per lineam perpendicularem graue 48 pedes spatio 2 secundorum exactè percurrat, vel fatendum est graue aequali tempore ab eandem altitudine per circuli quadrantem, ac per ipsam perpendicularem cadere, vel aërem magis obsistere grauibus perpendiculariter, quam obliquè per circumferentiae quadrantem descendentibus, vel graue plures quàm 12 pedes secundi spatio, aut plusquam 48 duobus secundi descendere, in eo fefellisse obseruationes, quòd allisio grauium ad pauimentum aut solum ex audito sono indicata fuerit, qui cùm tempus aliquod in percurrendis 48 pedibus insumat, quo tamen graue non ampliùs descendit, augendum videtur spatium à grauibus perpendiculariter confectum. . . .

Septimò, globus B ex C in B cadens paulò plus temporis quàm ab E, & ab E quàm à G insumit, adeout fila duo aequalia, quorum vnum à C, aliud à G suas vibrationes incipiat, quod à G incipit, 36 propemodum uibretur, dum quod à C incipit 35 duntaxat vibratur, hoc est vnam vibrationem lucretur quod à G cadit, à quo si quamlibet vibrationem inciperet, & aliud suam quamlibet a puncto C, longè cituis illam vibrationem lucraretur. Quantò verò breuiori tempore globus leuoir, verbi gratia suberis, suas vibrationes faciat, quantòque citiùs vibrationum suarum periodum absoluat, lib. 2. de causis sonorum prop. 27 & alijs harmonicorum nostrorum locis reperies. . . .

Duodecimò, pendulorum istorum vibrationes pluribus vsibus adhiberi possunt, vt tractatu de horologio vniversali, & harmonicorum tum Gallicorum 1. 2. de motibus, & alijs pluribus locis, tum Latinorum etiam 1. 2. de causis sonorum à prop. 26. ad 30. dictum est . . . Tantùm addo me postea deprehendisse fili tripedalem longitudinem sufficere, quae sua qualibet vibratione minutum secundum notet, cùm praedictis locis pedibus 3 1/2 vsus fuerim: sed cùm vnusquisque debeat experiri, cum horologio minutorum secundorum exactissimo, filum quo deinceps in suis vtatur obseruationibus, non est quod hac de re pluribus moneam: adde quòd in mechanicis filum illud siue tripedale, siue pedum 3 1/2 satis exactè secunda repraesentet, vt experientia conuictus fateberis: hinc in soni velocitate reperienda, quae secundo 230 hexapedas tribuit, hoc filo vsus sum, quo medici possint exporare varios singulis diebus aegrotorum, sanorúmque pulsus.

V

Gassendi and Science in his Time

To speak of Gassendi's relationship with the science of his time would seem, at first sight, to be an impossible task; and even an injustice. Gassendi, in fact, is not a great *savant*; and in the history of science, in the strict meaning of the term, he is not allotted a very important position. He clearly cannot be compared with great minds such as Descartes, Fermat, Pascal, nor even with Roberval or Mersenne, all of whom gave lustre to his period. He invented nothing; he discovered nothing; and, as B. Rochet (who cannot be suspected of anti-gassendism) remarked on one occasion, there is no 'Law of Gassendi'; not even a wrong one.

The matter is even more involved than that. For, however strange it might seem (or indeed may be), this rabid opponent of Aristotle, this resolute supporter of Galileo, remained a stranger to the spirit of modern science, especially to the spirit of mathematization by which it is actuated. He was not a mathematician; and, because of that, did not always understand the exact import of Galilean reasonings (such as the deduction of the law of falling bodies). Furthermore, his materialistic empiricism seemed to prevent him from understanding the pre-eminent role in science that is played by theory, particularly mathematical theory. His physics, too, being deliberately anti-Aristotelian, remained just as qualitative, and hardly ever went beyond the level of a rough trial to rise to that of experimentation.

However, let us not be too severe, and let us try to avoid anachronism. For, if Gassendi is not a great *savant* in *our* estimation, he was for his contemporaries, and a very great one at that – the equal and rival of Descartes.[1]

Now, an historian should always take account of contemporary opinion, even if posterity reverses its judgement. Undoubtedly, con-

[1] The influence of Descartes on his contemporaries was, in fact, not very great. "L'académie parisienne" grouped around Mersenne was mainly composed of opponents of Descartes. See R. Lenoble, *Mersenne ou la naissance du mécanisme* (Paris: 1946).

temporaries are sometimes deceived; but, on the other hand, they see many things that escape our notice. Moreover, as regards Gassendi, his contemporaries were only half deceived. He was, in actual fact, a rival, and even, in certain respects, a victorious rival of Descartes; and in the second half of the century exerted a very considerable influence,[1] even on minds of very much greater range, from the scientific point of view, than himself – such as Boyle and Newton.

Though he contributed very little (save for one or two exceptions of which I shall speak later) to the effective development of modern science, he did something much more important; he introduced the ontology, or, more correctly, the complement of ontology, of which it was in need. In fact, if, as I have said previously, modern science is a resurgence of Platonic ideas, this victorious resurgence has not been achieved by Plato alone. It was an alliance (undoubtedly an alliance against nature, but history has seen many others) of Plato with Democritos that overthrew the empire of Aristotle. It was precisely Democritean (or Epicurean) ontology, that he modified by doing away with the *clinamen* and unwieldiness, but of which he retained the essential feature, namely, atoms and vacuum. It was this that Gassendi brought to bear on the seventeenth century, and set in battle array against the Stagyrite. The example of Gassendi shows us exactly how, in the history of scientific thought, especially in its creative and critical periods such as the seventeenth century, and such as our own, it is impossible to separate philosophic thought from scientific thought. The one influences the other; to isolate them, is to condemn oneself to understanding nothing of historical reality.

Indeed, the scientific revolution of the seventeenth century inaugurated by Galileo, the profound significance of which consisted in the mathematization of reality, had gone beyond its legitimate purpose at the hands of Descartes – a not infrequent happening in history. It had become involved in what I have formerly called "geometrization to the limit", and had tried to reduce physics to pure geometry by denying every specific quality peculiar to material reality. Also, as a result of considering matter and space as identical, it led to an impossible kind of physics. It could not explain the elasticity of bodies, nor their specific densities, nor the dynamic

[1] Thanks to Bernier and his *Abrégé de la philosophie de Gassendi* (Lyons: 1678, 1684), it seems to me certain that at the close of the seventeenth century knowledgeable persons were much more frequently supporters of Gassendi than of Descartes.

nature of impact. Descartes certainly did; but at what a price! There is a still more serious matter. Physics of this kind, as Newton was to show, which allowed only extension and movement in the world, was not even able to impart these properties to the bodies in its much too tightly bound Universe without departing from its own principles.

Now, it is precisely against this identification of matter with space in Cartesian "extension" that Gassendi, as soon as he became aware of it, made a stand. To be sure, he did not start the violent polemic against Cartesian physics that he had undertaken against its metaphysics and epistemology. In 1645, that is to say, shortly after Descartes had published his *Principia Philosophiae*, he wrote to André Rivet saying that he was going to disappoint those people who attributed that intention to him, or incited him thereto, because he was not in the habit of attacking those who did not attack him.[1] But in that letter, as well as in many others, he showed very plainly his opposition to the essential thesis of Cartesianism; namely, his opposition to the identification of physical matter with geometrical extension. For example, in the same letter to Rivet, he wrote[2]

> There is no need to mention particular points; for, considering only the first principles, is there anyone who cannot see how many difficulties and contradictions they entail? Thus, the material world is infinite, or, undefined, as he nicely distinguishes; in itself it is completely filled, and is indistinguishable from extension; it may be ground into small fragments capable of changing their position locally in various ways without the intervention of vacuum; and there are other matters of the same kind. Not that the author does not succeed, or at least try to succeed, in creating an illusion and in escaping by his subtle distinctions; but if dunces and empty minds allow themselves to be taken in by words, then serious persons devoted to truth certainly do not hesitate on that score, but leaving empty words aside, they pay attention in their researches only to actual things.

Against Cartesian "plenism" Gassendi resolutely set up the existence of "atoms" and "vacuum". But he did not stop there. From 1646 onwards he attacked the very basis of the traditional ontology that Descartes, possibly without being aware of it, had inherited from Aristotle, and which led them both to a denial of vacuum identified with nothingness. The traditional ontology "distinguishes" a body's substance and attributes. But, in his *Animadversiones in decimum librum Diogenis Laertii*[3] – a work which almost certainly

[1] See R. Descartes, *Œuvres*, edited by Adam and Tannery, IV, p. 153.
[2] Ibid.
[3] The *Animadversiones* were not printed until 1649, though they had been

inspired Pascal's famous reproach of Père Noël – Gassendi had already questioned whether or not this distinction was legitimate. In fact, "Place no more than Time is neither Substance nor Accident; nevertheless they are both something, and not nothing; they are precisely the place and time of all substances and all accidents".[1]

Cartesian reasoning which results in a denial of vacuum is, in fact, only equivalent in terms of Aristotelian ontology to saying: empty space being neither substance nor accident cannot be anything else but nothingness; and nothingness, clearly being unable to have attributes, cannot be the subject of measurement; dimensions must be the dimensions of something, that is to say, of a substance and not of nothingness.

In the *Syntagma*, where he elaborates and develops the themes briefly treated in his *Animadversiones*, Gassendi makes it clear that we come up against difficulties as a result of a preconception with which the Peripatetic School has filled our mind; namely, that every thing is either substance or accident, and that

> all that is not Substance or Accident is non-entity (*non-ens*), non-physical (*non-res*), or nothing at all (*nihil*). Well then, since . . . Substance and Accident apart, place or space, and time or duration are entities and real things (*res*), it is clear . . . that the one and the other are nothing (*nihil*) only in the Peripatetic sense [of the term], but not in the true sense. These two entities [time and space] constitute kinds of things distinct from the others; so Place and Time are as little able to be Substance or Accident as Substance and Accident can be Place or Time.[2]

It follows that the geometrization of space in no way involves that of matter. On the contrary, it obliges us to make a careful distinction between the latter and the *space in which* it finds itself, and to endow it with its own characteristics. These characteristics are: mobility, which cannot be an attribute of space, for it is itself necessarily motionless; impenetrability, which cannot be deduced from extension pure and simple (Descartes notwithstanding, for space *qua* space offers no resistance to penetration by bodies); and finally, discontinuity, which imposes limits to the division of bodies, whereas there is no such thing in space, for it is necessarily continuous.

written before 1646. When the manuscript was taken to Lyons in 1646, a copy of it remained at Paris.

[1] See *Animadversiones*, p. 614 of the 1649 edition.

[2] See *Syntagma Philosophicum* (*Opera Omnia*, Lyons: 1658, I, p. 184a). Gassendi, rather unkindly, explicitly says that the reasoning of Descartes is worthy only of an Aristotelian (ibid., p. 219b).

Gassendi's ontology is doubtless neither new nor original: it is, as already noted, that of an atomist of antiquity. However, it not only enabled him on occasion to adopt ideas that were to have much success later on, as for example the concept of the corpuscular nature of light, of which, it must be confessed, he made no use (Newton was to do that), but even enabled him to surpass Galileo in formulating the principle of inertia, and Pascal in interpreting barometric phenomena.

It may be said that I am too severe in my judgement of Pierre Gassendi's strictly scientific work. Attention may be directed to his work as an astronomer; to the experiments he made or repeated, and the inferences he was able to draw therefore; to the ideas he put forward (such as the distinction between atoms, corpuscles and molecules), ideas which he himself was unable to develop, but from which others were later to profit.

This cannot be denied: it is a severe judgement; but, unfortunately, it is the judgement of history. Having said this, it is undeniable that Gassendi did not limit himself to writing interesting and useful biographies of the great astronomers; and to teaching astronomy at the *Collège Royal*, where, moreover, he held an equal balance between the two or three great systems of Ptolemy, Copernicus and Tycho Brahe, between which the scientific conscience was still hesitating. He was a true astronomer, a *professional*, one might say; and we must do justice to the patience with which he studied the heavens and accumulated observations on celestial phenomena throughout his whole life. Here are some examples, which represent only a very small part of his work. He observed the eclipses of the sun at Aix in 1621, at Paris in 1630, at Aix again in 1639, at Paris in 1645, at Digne in 1652, at Paris in 1654; and those of the moon at Digne in 1623, at Aix in 1628, at Digne again in 1633, 1634, 1636, 1638, at Paris in 1642, 1645, 1647, and for the last time at Digne in 1649. He observed the planets, particularly Saturn, a star in which he was specially interested on account of what he believed to be its satellites; he observed the occultation of Mars by the Moon; and so on. He even succeeded in observing on 7 November 1631 the passage of Mercury across the solar disc,[1] as predicted in 1629 by

[1] See *Mercurius in Sole visus et Venus invisa Parisiis anno 1631* (Paris: 1632), *Opera Omnia*, IV, pp. 499 ff. For the astronomical work of Gassendi, see Delambre, *Histoire de l'Astronomie moderne* (Paris: 1821), II, pp. 335 ff, and Pierre Humbert, *L'Œuvre astronomique de Gassendi* (Paris: 1936), from which I quote the following (p. 4): "No-one made observations with so much eagerness and

Kepler[1]; apart from Harriot, he was almost the only one to do so in a scientific manner.

He made experiments, too; even experiments involving measurement. Thus, again following Mersenne, he measured the velocity of propagation of sound, which he found to be 1,473 feet per second. Now, though this figure is much too high – the correct figure is 1,038 feet – the error is not excessive, when we remember the difficulty in making exact measurements in a period that had no accurate clocks, and could not measure time properly.[2] Gassendi's experiments led him to affirm that low and high pitched sounds were propagated with the same velocity. On the other hand, he completely misunderstood its physical nature, having assigned to it, as to all qualities, a particular atomic structure, instead of vibrations in the air. Moreover, he taught that sound was not carried along by the air, and that its propagation, like that of light, was not affected by the wind.[3]

perseverance. No occurrence in the sky, and nothing that could be disclosed there escaped his notice. His eye was ever at a telescope to observe sunspots, mountains on the moon, the satellites of Jupiter, eclipses, occultations, and transits. He remained at his timepiece to determine the positions of planets, longitudes and latitudes, and the correct time. As a matter of fact, he discovered nothing. Though a diligent observer of Jupiter, he failed to notice its bands. His careful drawings of Saturn did not reveal to him the true nature of the ring. With regard to solar rotation or lunar libration he did no more than confirm earlier discoveries. Yet all his observations give evidence of a methodical mind, a care for precision, a quest for elegance that set him above his contemporaries."

[1] Gassendi's merit is all the greater as Kepler's work seems to have been almost completely neglected in France. It was not till 1645 that Ismael Boullaud dealt with it in his *Astronomia Philolaica* (Paris: 1645). In this work he rejected Kepler's celestial dynamics, and adopted, with rather unfortunate modifications, the Keplerian doctrine of the elliptical orbit of the planets. As for Gassendi, he gave an account of it in his *Syntagma Philosophicum* (Lyons: 1658). (See *Opera Onmia*, I, pp. 639 ff.) Or rather, he described the mechanism of magnetic attraction and repulsion adopted by Kepler to explain the ellipticity of planetary orbits, whilst neglecting the mathematical structure of Kepler's astrophysics, the innovating character of which he does not seem to have grasped. He accepted Kepler's predictions without concerning himself unduly with the laws on which they were based; and possibly did not realize that his observation of the transit of Mercury provided decisive confirmation of Kepler's concept.

[2] See "An experiment in measurement", above. Gassendi, moreover, did not seem to ascribe any great value to exactness in measurement. For example, in the *Syntagma* (I, p. 351a) he records the value for the acceleration of a falling body obtained by Galileo (180 feet in 5 seconds) and by Mersenne (300 feet), without expressing a preference for one or the other.

[3] Here, too, experimental conditions must be taken into account; and it must be noted, in exoneration of Gassendi, that Borelli and Viviani came to the same result. Borelli and Viviani were true *savants* and outstanding experimenters, who

In order to provide experimental confirmation of the laws of motion established by Galileo, and at the same time to invalidate those that Michel Varron claimed to have proved, he conceived, and even carried into effect, a most elegant experiment. We know, according to Galileo, that the velocity of a falling body is proportional to the time elapsed: and, according to Varron, that it is proportional to the distance travelled. Now, among the conclusions drawn by Galileo from his dynamics, there is a particularly striking one, which it is impossible to draw from that of Varron; and it is, that bodies falling along the diameter and chords of a vertical circle take the same time to arrive at the end point of the fall. Undoubtedly, it was impossible to measure directly the times taken to travel the various distances that were covered. But, as Gassendi quite well understood, it was possible to avoid measurements. Galileo's theorem implied, in effect, that bodies leaving the points A, B and C at *the same time* arrive at *the same moment* at the point D (AD being a diameter, BD and CD being chords inclined to the vertical). Gassendi therefore made a circle in wood about two toises (12 feet) in diameter, fitted it with glass tubes, and let little balls fall in them. The results fully confirmed Galileo's doctrine, and invalidated that of Varron, by showing that it was greatly at variance with experimental fact.[1]

In 1640 Gassendi undertook a series of experiments on the conservation of motion. These resulted in an experiment involving the release of a ball from the top of the mast of a moving ship – an experiment that had been discussed for centuries, and was generally advanced as an argument against the movement of the Earth.[2] In fact,

obtained an almost correct value of 1,077 feet per second for the velocity of propagation of sound.

[1] See *Syntagma*, I, p. 350b.

[2] In my *Études galiléennes* (Paris: 1939), p. 215 I said that Gassendi was the first to make this experiment. As a matter of fact, the experiment in question had been carried out several times previously. It may have already been tried by Thomas Digges, who in his *Perfit Description of the Celestiall Orbes* published in 1576 as an appendix to the *Prognostication everlastinge of righte good effecte* by his father Leonard Digges, said that falling bodies, or those thrown in the air down to the ground which is moving, seem to move in a straight line. In the same manner, a shot dropped from the top of the mast of a ship in motion falls the length of the mast to the foot of it, and appears to move in a straight line, though in fact it describes a curve. The *Prognostication everlastinge* as well as the *Perfit Description* have been reissued by F. Johnson and S. Larkey, "Thomas Digges, The Copernican System and the Idea of the Infinity of Universe in 1576", *Huntingdon Library Bulletin* (1935): see also F. R. Johnson, *Astronomical Thought in Renaissance England* (Baltimore: 1937), p. 164. In any case, it should be noted that Thomas Digges did not say that he had carried out the experiment himself;

as was said over and over again from the time of Aristotle and Ptolemy, a body thrown vertically into the air could not fall back on to the place whence it had been thrown; and a ball released from the top of a tower could never fall to the foot of that tower, but "would lag behind", behaving like a ball released from the top of a ship's mast, falling at the foot if the vessel were stationary, but "lagging behind" and falling on the poop, if the vessel were moving, and even in the water if the ship were moving too rapidly. To this argument, revived by Tycho Brahe, the Copernicans, in the person of Kepler, replied by postulating a difference in character between the case of the ship and that of the Earth. The Earth, it was said, carries heavy bodies (terrestrial) along with it, whereas the ship does nothing of the kind. Furthermore, a ball released from the top of a tower will fall at the base because it is attracted by the Earth through a *quasi* magnetic attraction, whereas the same ball released from the top of the mast of a ship in motion will deviate, because it is not attracted by the ship. Bruno and, of course, Galileo were alone in having the boldness to affirm that a ball falling from the top of the mast of a

he mentioned it as something that stood to reason. Secondly, Galileo, as I have said, positively stated to Ingoli that he had made the experiment. He did not say where or when; and he contradicted himself in the *Dialogo*; so we are entitled to have doubts. On the other hand, experiments carried out by the French engineer Gallé at an uncertain date, though before 1629, must be accepted as having taken place, as well as those of Morin in 1634. The experiments of Gallé were described and discussed by Froidemont (Fromondus) in his *Ant-Aristarchus, sive Orbis Terrae immobilis liber unicus* (Antverpiae: 1631); and *Vesta sive Ant-Aristarchus Vindex* (Antverpiae: 1634). According to C. de Waard, from whom I have taken this information (see *Correspondance du P. Marin Mersenne*, Paris: 1945 II, p. 74), Gallé's experiments were carried out in the Adriatic. He "dropped a mass of lead from the tall mast of a Venetian galley. It did not fall at the foot of the mast, but was deviated towards the poop, thereby providing the disciples of Ptolemy with an apparent confirmation of their doctrine". As for Morin (see *Correspondance du P. Marin Mersenne*, Paris: 1946, III, pp. 359 ff), he recorded in his *Responsio pro Telluris quiete* . . . (Parisiis: 1634) that he had carried out this experiment on the Seine, and had found that Galileo's assertions were confirmed, "the first time with amazement, the second with admiration, the third with laughter". For, as Morin said, the experiment proved nothing in favour of the Copernicans. In fact, the man at the top of the mast with a stone in his hands imparted to it his own proper movement, and the more so when the ship was moving rapidly. The stone, therefore, was really projected forwards, and that was why it did not lag behind. But if the ship were passing under a bridge, and another stone were dropped from the bridge at the same moment as one from the mast, it would behave quite differently, and would fall on the poop. So, by reasoning copied literally from Bruno (see *La Cena de la Ceneri*, III, 5, *Opere italiane*, Lipsiae: 1830, I, p. 171, quoted by me in my *Études galiléennes*, III, pp. 14 ff), but which he obviously did not understand, Morin was able to confirm his belief in the geocentric theory.

ship, whether stationary or moving, would always fall at the foot of the mast. Now, Galileo in his *Lettera a Francesco Ingoli* (1624) claimed a twofold superiority over Ingoli, as well as over Aristotelian physicists in general. His claim was that he had carried out the experiment they had never made; and that he had done so only *after* having foreseen the result. But, in his *Dialogo sopra i due massimi sistemi del mondo*, and precisely at that point where he discusses the argument in question, he tells us that he had never tried the experiment. Furthermore, he adds that he had no need to make it, because he was such a good physicist, that without any experiment he could determine how the ball would behave, should the necessity arise.

It is clear that Galileo was right. This experiment was quite unnecessary for anyone who understood the concept of motion in modern physics. But what about the others? Those who have not yet understood, and who must be brought to an understanding? For them, the experiment may play a decisive role. It is difficult to say if it was for himself or for others that Gassendi undertook, in 1640, the experiments to which I have referred. Probably it was for the "others" – those for whom it was necessary to provide an experimental proof of the principle of inertia. Yet, perhaps for himself too, in order to be assured that this principle was valid not only *in abstracto*, in the vacuum of imaginary spaces, but also *in concreto*, on our world, *in hoc vero aere*, as Galileo said.

Be that as it may, the experiments succeeded perfectly. With the help of the Comte d'Alais he organized a public demonstration at Marseilles, which excited great interest at the time. It was described thus[1]: "M. Gassendi having been always interested in seeking to justify experimentally the truth of speculations put to him by philosophy, and being at Marseilles in the year 1641, demonstrated on a galley sent to sea at the command of this prince (who is more renowned for love and knowledge of good things than for the splendour of his birth), that a stone released from the highest point of the mast whilst the galley moved at the greatest possible speed, did not fall anywhere else but where it would fall if the same galley were stopped and stationary. The stone always falls the length of the mast to the foot, and on the same side, whether the galley moves or is

[1] See *Recueil de Lettres des sieurs Morin, De la Roche, De Nevre et Gassend et suite de l'apologie du sieur Gassend touchant la question* DE MOTU IMPRESSO A MOTORE TRANSLATO, Paris: 1650, Preface; see my *Études galiléennes*, pp. 215 ff. The date 1641 should be put forward one year.

stationary. This experiment performed in the presence of Monseigneur le Comte d'Alais and a large assembly seemed to be something of a paradox to many who did not witness it; and for this reason M. Gassendi in the same year wrote a treatise *De motu impresso a motore translato*, which came forth as a letter addressed to M. du Puy."

Now, in this "letter", *De motu impresso a motore translato*,[1] Gassendi did not restrict himself to setting forth Galileo's arguments, adding a description of the experiments at Marseilles, and applying to an analysis of the latter the Galilean principles of relative motion and conservation of velocity. He succeeded in surpassing Galileo; and, by freeing himself from both obsession with circularity and obsession with gravity, gave a correct formulation of the law of inertia. In fact, the Galilean restriction of this law to horizontal movement was pointless. In principle, all directions are of equal worth; and in imaginary spaces – empty spaces outside the world where doubtless there is nothing, but where something could nevertheless exist – "motion, in whatever direction it occurs, will be similar to that which is horizontal, and will neither accelerate nor retard; and hence will never cease".[2] Gassendi very sensibly deduced from this that the same holds true on Earth – that motion *qua* motion is maintained in direction and speed. If in fact, the state of affairs is otherwise, it is because the bodies encounter resistance (e.g. from the air), and are deviated by attraction from the Earth.

Imaginary spaces outside the world are obviously not subject to experiment, any more than the bodies that God may put there. Gassendi realized this; and it redounds to his credit. Yet it would be rather hard to dwell on this, and to emphasize the flagrant incompatibility of Gassendi's argument with the empirical epistemology that he professed and, it must be added, had inherited from Epicuros together with his concepts of atoms and vacuum. Furthermore, it was not his epistemology – which only spoiled and vitiated his thought – but the intelligent employment of atomism, that enabled Gassendi to anticipate Robert Boyle in the interpretation of Torricelli's and Pascal's barometric experiments.

These experiments, including that at Puy-de-Dôme of which he was informed by Auzout, were reported at length in an Appendix to his *Animadversiones*. Having repeated them with Bernier on a hill

[1] Paris, 1642; or *Opera Omnia*, Lyons: 1658, III, pp. 478 ff.

[2] See my *Études galiléennes*, pp. 294-309; and *Opera Omnia*, III (1658), p. 495b.

near Toulon (in 1650), he restated and rediscussed them in the *Syntagma.*[1]

The experimental fact revealed by the barometric experiment is, in itself, simple enough. Essentially, it established the variation in height of a column of mercury in a Torricellian tube as a function of the altitude at which it was placed; but its correct interpretation is anything but simple. It implied, in fact, a distinction between the action of two factors in producing the effect; and, hence, the elaboration of two distinct ideas, namely, that of *weight*, and that of *elastic pressure* of the column of air balancing the mercury. Now, even if these two ideas were, at the outset, in the minds of the experimenters (Torricelli speaks of the compression of air by comparing it with a ball of wool), the action of the two factors is far from being clearly analysed. Besides, it was not very easy to do so, as was well shown by the case of Roberval, who was baffled by the fact that a quite small quantity of air – a drop, to all intents and purposes weighing nothing – when introduced into the vacuum of a Torricellian tube caused the level of mercury to fall noticeably. Even Pascal, seduced and led into error by thinking of air as a liquid (this was usual at the time), explained the production of a vacuum in the mercury tube by concepts drawn from hydrostatics, that is to say, by reference to an equilibrium of weights. If, in the interpretation of barometric experiments (expansion of a bladder carried to the top of a mountain, etc.) recorded in his *Traité de l'équilibre des liqueurs* and the *Traité de la pesanteur de la masse de l'air*, the *compression* of air at ground level and its *rarefaction* at the top of a mountain are clearly stated, it remains nonetheless true, that the two treatises (as their very titles indicate) were definitely conceived in the spirit of hydrostatics; and the conceptual analysis of the phenomena studied does not rise above the level already reached by Torricelli.

It was at this point that atomistic ontology allowed Gassendi to take a step forward. The phenomena of dilation (expansion) and condensation (compression) of air, and the fact that the same quantity of air (same number of corpuscles, and hence the same weight) could exert extremely variable *pressures* depending on the state of compression or dilation, became readily understandable to him. In this compression and the resulting pressure, he saw the essential factor of the phenomenon revealed by barometric experi-

[1] See *Animadversiones in decimum librum Diogenis Laertii*, Lyons: 1649; and *Syntagma Philosophicum*, in *Opera Omnia*, I, pp. 180 ff.

ment; and he advanced aerodynamic analogies (the pressure of compressed air in a bombard, or the pump of Ctesibios) in order to explain it. The weight of a column of air, he tells us, compresses the lower layers, and it is this *pressure* that causes the mercury to rise in a tube. The action of *weight* is thus put in its proper place; the direct cause comes from the *pressure*.[1]

All this was by no means unimportant, of course. Still, compared with the effort expended by Gassendi, the role he played, and the influence he exercised, it is very little. But that is what I said at the outset. It is not as a *savant* that he became influential, and conquered a place in the history of scientific thought, it is as a philosopher, by resuscitating Greek atomism and completing thereby the ontology that was needed by science in the seventeenth century.[2] He was unquestionably not the first to do it – Bérigard, Basson and others did it before him – and we could say that atomism was so well adapted to the physics and mechanics of the seventeenth century, that the direct influence of Lucretius and Epicuros would have sufficed for it to be accepted. Even those who, like Descartes, rejected atoms and vacuum, and sought to establish a physics of the continuum, were indeed obliged to make use of corpuscular concepts. Nonetheless it remains true that no-one had presented the atomic concept with so much force, and no-one had defended the existence of a vacuum in all its forms (inside as well as outside the world) with so much perseverance and persistence as Gassendi. Consequently, no-one contributed so much to the ruin of classical ontology based on notions of substance and attribute, of potentiality and actuality. In fact, by proclaiming the existence of a vacuum, that is to say, the reality of something which was "neither substance, nor attribute", Gassendi made a breach in the traditional system of categories: a breach in which he was finally to be engulfed.

In that way therefore, he contributed more than anyone to a reduction of physical being to pure mechanism, with all its implications; namely, making the world infinite as a result of making it autonomous, and making space and time infinite, and making sentient qualities subjective. This is somewhat paradoxical, for in truth, Gassendi himself did not believe in one or the other. Infinity of *space*, for him, did not involve infinity of the real world, seeing that the

[1] See *Syntagma Philosophicum*, pp. 207-12.

[2] See B. Rochot, *Les Travaux de Gassendi sur Epicure et sur l'atomisme*, Paris: 1944.

total number of atoms entering into its composition could not be infinite. The reduction of the properties of atoms to "weight, number, measure", did not prevent him from trying to develop a quantitative physics with an atomic basis by postulating atoms specifically adapted to the production of perceptible qualities; – luminous atoms, resonant atoms, atoms of heat and atoms of cold, and so on. In the case of atoms of light, this led him to anticipate, though remotely and for bad reasons, the Newtonian concept of light (corpuscular theory); and in the case of sound, to deny the existence of sound waves.

What I have said may be summarized in few words. Gassendi tried to establish a system of physics that was still qualitative, basing it on the atomism of antiquity. By renewing, or resurrecting, the atomism of antiquity he was enabled to provide a philosophical basis, an ontological basis, for modern science, which has united what he did not know how to unite, namely the atomism of Democritos with the mathematical outlook of Plato, introduced by the Galilean and Cartesian revolution. It was the union of these two streams that produced the Newtonian synthesis of mathematical physics.

VI

Pascal Savant

It is difficult, if not impossible, to form a proper conception of Pascal's personality and scientific work. In fact, much of his work has been lost; more particularly the great *Traité des coniques*, the excellent qualities of which Mersenne commended to Huygens,[1] and which he mentioned in his *Cogitata Physico-Mathematica*; nor do we possess the *Traité du vide*, of which only the preface together with some fragments have survived,[2] and nothing remains of the *Traité de mécanique*.

As for Pascal's personality, it has been so distorted by Pascalian hagiography that it is extremely difficult to consider the matter without bias. However, that is what I am going to attempt, even at the risk of being accounted anti-Pascalian.

It is obvious that I shall be able to give no more than a rapid, brief and superficial survey. The extant work of Pascal the physicist has been carefully collected together, and comprises no more than some experiments, including the famous one at the Puy-de-Dôme, together with short treatises devoted to elaborating, or more precisely, systematizing, hydrostatics. On the other hand, the work of Pascal the mathematician,[3] even in the depleted amount which has survived, is still vast and varied enough, because it consists mainly of the study

[1] See Marin Mersenne, *Cogitata Physico-Mathematica*, Paris: 1644, preface: *Unica propositione universalissima, 400 corollariis armata, integrum Apollonium complexus est.* Letter to Constantyn Huygens, père, 17 March 1648, *Œuvres complètes de Christiaan Huygens*, La Haye: 1888, I, p. 83: "If your Archimedes comes with you, we shall let him see one of the finest treatises on geometry that he has ever seen, and which has just been completed by the young Pascal." In his *Adresse à l'Académie Parisienne* (1654) Pascal gives notice of: "*Conicorum opus completum, et conica Apollonii et alia innumera unica fere propositione amplectens; quod quidem nondum sexdecimum aetatis annum assecutus excogitavi, et deinde in ordinem congessi.*"

[2] This *Traité du vide*, notice of which is given in the *Expériences nouvelles touchant le vide* (1647), seems to have been completed in 1651. In his letter to M. de Ribeyre dated 12 July 1651 Pascal says that he is completing a treatise which will explain "the true cause of all the effects that are attributed to the horror of a vacuum".

[3] Pascal's scientific works are readily accessible in the *Œuvres complètes de Pascal*, Paris: Bibliothèque de la Pléiade, 1954 2nd Edition.

and solution of specific problems. A detailed analysis of it would be time consuming and rather difficult, at least at present. It would certainly have been much less so for Pascal's contemporaries, because they, like Pascal himself, had the advantage of us in understanding geometry in a way that we no longer do. On the other hand, we know many other things that are perhaps more important, more fruitful and more powerful, such as algebra and the infinitesimal calculus, which they were then only beginning to elaborate. On this score, we are superior to them in being able to solve easily problems which involved them in much work and trouble. Alas! this superiority is of no avail – indeed quite the contrary – when it becomes a matter of recording history and understanding their thought. We are not able, as they were, to reason "in the manner of the ancient philosophers", i.e. the Greeks; nor "in the manner of the modern philosophers", i.e. *grosso modo* in the manner of Cavalieri or Fermat. We do not understand, for example, why in 1658 Pascal considered it necessary to prove "in the manner of the ancient philosophers" the equivalence of the parabola and the spiral. This proposition was attributed by Pascal to Roberval, although it had already been worked out some time previously by Cavalieri, admittedly in a rather laborious way; it had been worked out also by Torricelli in a most elegant manner: neither of them is mentioned by Pascal. Perhaps he purposely wanted – *sit venia verbo* – to outdo Torricelli (the *bête noire* of Roberval, who was both teacher and friend of Pascal), and to prove once again the legitimacy of the methods of the geometry of indivisibles,[1] which he used in another connection.

Indeed, for Pascal, as well as for Cavalieri and Torricelli, the only true and great geometry was that of the Greeks. This is no longer the case as far as we are concerned. So, when we undertake a study of the geometers of the seventeenth century, Pascal amongst others, what do we do? We translate Pascalian arguments into our own language; we write some algebraic formulae together with one or two integrals, and then gain the impression that we understand.[2] In

[1] The expression "geometry of indivisibles" is ambiguous. The title of Bonaventura Cavalieri's work is in fact *Geometria indivisibilibus continuorum nova ratione promota*, Bononiae: 1635, which means: *The geometry of continuous magnitudes treated by means of indivisibles*, and not: *The geometry of indivisibles.* As the expression is used by Pascal I have done the same. See my article "Bonaventura Cavalieri et la géométrie des continus" in *Éventail de l'histoire vivante*, Hommage à Lucien Febvre, Paris: 1953, I, pp. 319 ff.

[2] Pascal lends himself particularly well to this kind of treatment, as has been remarked by Nicolas Bourbaki, who to his mathematical accomplishments adds

fact, it is not so; for by translating Pascal into formulae we distort and even completely misrepresent his thought, which is characterized essentially by the *rejection* of formulae; a rejection which cost him dear, in that Pascal himself failed to make two great discoveries, namely the binomial theorem (subsequently stated by Newton), and the differential calculus (subsequently developed by Leibniz); though both these discoveries doubtless owed something to him.

How are we to explain this rejection of formulae? In the last analysis it undoubtedly depends on the very structure of the Pascalian genius. Historians of mathematics tell us, in effect, that there are, roughly speaking, two types of mathematical mind: the geometers and the algebraists. On the one hand we have those who have the gift of seeing into space "by greatly stretching their imagination", as Leibniz has said, and who are able to trace therein a multitude of lines, and perceive their relevance and relationship without confusion[1]; on the other hand we have those, such as Descartes, who find this effort of imagination, in fact any effort of imagination, tiring, and who prefer the diaphanous purity of algebraic formulae. For the former, every problem is to be solved by a [geometrical] construction; for the latter, by a system of equations. Desargues and Pascal belong to the first type; Descartes and Leibniz to the second. For the former, a conic section is an event in space, whilst an equation is no more than an abstract, remote representation; for the latter, the essence of a curve is precisely its equation, whilst its spatial form is only a projection – something which is quite secondary and at times even useless.

Léon Brunschvicg has written some masterly pages on the contrast between Descartes the algebraist and Pascal the geometer. He contrasts Descartes, the man of *La méthode*, the method of universal validity, which should be applicable everywhere and to everything, with Pascal, the man of *methods*, particular and special methods, peculiar to each particular, concrete example. These writings of Brunschvicg are universally known; so I shall not dwell on them.[2]

a very deep knowledge of the history of this science. See Nicolas Bourbaki, *Éléments de mathématiques* (Paris: 1949), IX, p. 148, note xx: "Thanks to the prestige derived from matchless language, Pascal has succeeded in creating the illusion of perfect clarity."

[1] In his letter to Constantyn Huygens (see p. 131, n. 1 above), when speaking of the solution obtained by Pascal to the problem "of the locus of Pappos *ad* 3, 4 *lineas* which they say here has not been completely solved by M. des Cartes", Mersenne says that "it was necessary to use red, green and black lines in order to distinguish the multitude of details under consideration".

[2] See Léon Brunschvicg, *Blaise Pascal*, Paris: 1953, pp. 127 ff, 158.

The Pascalian attitude may seem strange to us; though it is probably less rare than one would imagine. Thus, Paul Montel[1] has reminded us most opportunely of a remark made by Henri Poincaré, who wrote with regard to Descartes: "A method that reduces discovery to the application of uniform rules, which make a patient man into a great geometer, is not truly creative."

I should like to add that the Pascalian attitude, namely, that of a geometer proper, was much more general and customary than the Cartesian in the seventeenth century.[2] The latter represents a much greater innovation and a much more radical break with respect to tradition than do the innovations of Cavalieri, or even those of Desargues. In the seventeenth century it was Descartes, it was algebra, especially algebraic geometry, that was difficult, unusual, incomprehensible.

As for Pascal, his innate geometrical outlook was certainly strengthened by his mathematical education; and his anti-algebraism by his constant hostility towards Descartes.

To tell the truth, we do not know a great deal about Pascal's mathematical education. The hagiographic account by Mme Périer is not to be taken seriously. We can accept the information provided by Tallemant des Réaux to the effect that Pascal when he was twelve years old was able to read Euclid for pleasure, and that he rapidly mastered the first six books. This is sufficiently commendable and unusual to make it unnecessary to elaborate thereon.

We can be sure, without fear of deceiving ourselves, that Pascal did not stop at Euclid, and that he had already acquired in his youth that deep knowledge of Greek geometry, of Archimedes, of Apollonios, of Pappos, which breaks forth in his work; and rightly so in the proof of the equivalence of the parabola and the spiral. His acquaintance with Greek geometry is all the more likely because his father, Étienne Pascal, was a connoisseur of this kind of geometry. From Greek geometry he passed on to Desargues.

I am inclined to think that the influence of Desargues was exerted through personal correspondence. Indeed, I do not believe that anyone, even a genius like Pascal, could understand and assimilate the ideas and methods of the great geometer from Lyons by simply

[1] See Paul Montel, *Pascal mathématicien*, Palais de la Découverte, Paris: 1950, pp. 127 ff, 158.

[2] See Nicolas Bourbaki, op. cit., p. 153.

reading the *Brouillon projet d'une atteinte aux événements des rencontres du cône avec un plan*, which in the seventeenth century was not unjustifiably called *Leçons des ténèbres*; and, what is more, I do not believe that he could have done so sufficiently quickly to be able to present to the Parisian Academy of Mersenne in 1640 the *Essay pour les coniques*, which is not only obviously inspired by Desargues, but also loudly proclaimed to be so by Pascal himself.[1] Consequently, I believe that in Pascal we can see a true pupil of Desargues. This is as much an honour for the one as for the other.

Let us return to the *Essay*. Side by side with matters which are directly derived from Desargues, we find in Lemmas I and III the equivalent of the famous "Pascal's Theorem", which states that the points of intersection of the opposite sides of a hexagon inscribed in a conic lie on a straight line. Undoubtedly, we have there the unique proposition which was the starting-point from which Pascal developed a complete theory of these lines in his lost *Traité* – at least, that is what Mersenne tells us without, however, quoting this proposition.

The inscribed hexagon was then called the *mystic hexagram*, and Pascal asserted that every conic section has its corresponding specific "mystic hexagram"; and conversely, every hexagram has its specific conic section.

This was a very fine discovery, which has been preserved for us by pure chance in a copy made by Leibniz, who had Pascal's papers in his possession in 1675. He made an inventory of them; copied some few sheets; and unfortunately for us returned the originals to their legitimate owner, Étienne Périer. These papers contained the whole of Pascal's geometrical work, already promised in the *Essay pour les coniques* and promised anew in the *Adresse à l'Académie Parisienne* of 1654.[2]

This body of work was certainly not the *Traité des coniques* spoken of by Mersenne, though it was broadly the equivalent. In the opinion of Leibniz, confirmed moreover by some pages of the treatise entitled *Generatio Conisectionum*, which he has preserved for us, they were treatises inspired by Desargues. Leibniz recommended that they be printed, and insisted on immediate publication.[3] He said that he had

[1] See R. Taton, "*L'Essay pour les Coniques* de Pascal", *Revue d'histoire des sciences*, VIII, fasc. 1 (1955).

[2] See *Lettre de Leibniz à Étienne Périer* dated 20 August 1676 in *Œuvres complètes de Pascal*, Paris: Bibliothèque de la Pléiade, 1954, pp. 63 ff: *Adresse à l'Académie Parisienne*, ibid., pp. 71 ff.

[3] Ibid., pp. 66 ff.

seen certain works (undoubtedly those of de La Hire) bearing evidence of the same inspiration, which could deprive Pascal's work of its novelty.

Leibniz has thereby given formal judgement: Pascal was the disciple and continuator of Desargues. Now, the historians of Pascal either habitually neglect this relationship between the two geometers, or they present it in a totally incorrect manner. Thus, Emile Picard (who is quoted without comment by Jacques Chevalier in his edition of Pascal's *Œuvres complètes*)[1] makes Pascal appear as the inventor of the projective methods "that were to be followed so brilliantly by Poncelet and Chasles" in the nineteenth century; and Pierre Humbert in his latest work devoted to Pascal the *savant*[2] tells us that Pascal was the continuator of Desargues, but with the addition of genius. For my part, I believe that it is preferable to say that Pascal is Desargues with the addition of clarity and systematization, for Pascal is clear whilst Desargues is not; but the great creative genius, the inventor of a new form of geometry, is Desargues, and not Pascal.

The second period of Pascal's mathematical work occurs about the years 1652-4, and is centred round the arithmetical triangle. It was then that Pascal laid the basis of probability calculations, concurrently with Fermat and independently of Galileo, who preceded both the former in this connection. Pascal seems to have abandoned geometry for a while.

With regard to the arithmetical triangle, the invention of which is sometimes attributed to Pascal, we have here something that is quite old. According to Moritz Cantor[3] it has come down to us from the Arabs. It is given in a somewhat similar form by Stifel (1544), Tartaglia (1556), and, closer to the time of Pascal, by Stevin (1625) and Hérigone (1632).[4]

Paradoxically, it is to Pascal's very great credit that he turned the triangle about its apex; and by so doing changed it, at least in principle, into an infinite square – a square subdivided into an infinite number of "cells" by parallel, horizontal and vertical lines. As for

[1] Ibid., p. 58.

[2] See Pierre Humbert, "Cet effrayant génie", *L'Œuvre scientifique de Blaise Pascal*, Paris: 1947, pp. 19, 34, 47.

[3] See Moritz Cantor, *Vorlesungen über Geschichte der Mathematik*, Leipzig: 1900, II, pp. 434, 445.

[4] See Pierre Boutroux, "Introduction au *Traité du triangle arithmétique*" in *Œuvres de Blaise Pascal*, edited by L. Brunschvicg and Pierre Boutroux, Paris: 1908 III, pp. 438 ff.

the triangles themselves, they were formed by the diagonals joining the points corresponding to the above subdivisions; these diagonals formed the "bases" of successive triangles.

In the square formed in this manner the cells in the first "row" contain only the number 1; those of the second, the cardinal numbers; those of the third, the triangular numbers; those of the fourth, the pyramidal numbers; and so on. Pascal discovered a whole series of extremely interesting relationships between the numbers inscribed in the cells, depending on the position occupied by them in the "bases" and "rows" both "parallel" (horizontal) and "perpendicular" (vertical) of the diagram. In his hands the "arithmetical triangle" became an ingenious and powerful instrument for solving problems of grouping and probability. Amongst other things, Pascal showed (though later than Hérigone and Tartaglia) that the "bases" provide the coefficients of integral powers in the binomial expression.

There remained only one more step to take: namely, to discover the underlying structure and internal connection between the numbers forming the basis, and hence to deduce the general formula. Pascal did not take this step. His anti-algebraism, his aversion from formulae, which I have already mentioned, caused him to miss this great discovery. He did not make it because he did not seek it.[1]

On the other hand, when he did seek it, he found the general formula, or more exactly, *the rule* allowing the number of combinations of m things taken p at a time to be deduced.[2]

Finally, because it belongs to this same period as the *Traité du triangle arithmétique* – or perhaps a little earlier – let us mention the most interesting little treatise *Potestatum numericarum summa*[3] in which by comparing, as did Fermat and Roberval, the summation of the powers in an arithmetical progression with the "summation" of lines or areas as performed in the geometry of indivisibles, Pascal directly transferred the results obtained in the realm of discontinuous arithmetic to the realm of continuous geometry.

He wrote as follows: "Those who are somewhat versed in the doctrine of indivisibles will easily recognize how useful this concept is for determining curvilineal areas. In fact, parabolas of all kinds may be immediately squared, and an infinity of other curves easily

[1] Nor did he attempt to use the "triangle" in his geometrical calculations, as did Wallis in his *Arithmetica infinitorum.*

[2] Ibid., pp. 442 ff.

[3] *Potestatum Numericarum Summa, Œuvres complètes,* pp. 166 ff.

measured. If, then, it is required to apply to a continuous quantity what we have found for numbers by this method, we can establish the following rules. . . ." These "rules", which I refrain from quoting, conclude with the following general rule: "The sum of the powers of a certain number of lines each raised to the same degree is to the power of the greatest of them raised to the next higher degree as unity is to the exponent of this higher power."[1]

Apart from this ingenious and fruitful rapprochement – it is less original than is usually claimed – between two great disciplines, namely, arithmetic and geometry, which classical tradition persisted in keeping apart, we find in this little treatise the famous passage on the relationship between different orders of magnitude. It is in this passage that attempts have occasionally been made to find the deep-rooted intuition of Pascalian thought, an intuition which underlies his mathematical thought just as much as his philosophical, and even theological, thought. Here is the passage which forms the conclusion to the treatise *Potestatum numericarum summa*, and which immediately follows the rule on integration quoted above[2]:

I shall say nothing about the remaining cases, because there is no need to consider them here; it suffices to have stated the rules given above. The others will be found without difficulty, bearing in mind this principle: that, *in the case of continuous magnitudes, any desired number of magnitudes of any kind whatsoever added to a magnitude of a superior degree adds nothing to it.* Thus, points add nothing to lines; lines nothing to surfaces; surfaces nothing to solids: or, to express oneself in numbers, as is proper in a treatise on arithmetic, square roots add nothing to their squares; squares nothing to cubes; cubes nothing to fourth powers; and so on. Hence, quantities of a lower order [of magnitude] should be neglected as being of no account. I wanted to make a point of adding these few remarks, which are familiar to those who have studied indivisibles, in order to emphasize the connection, which is inadequately admired, that nature, the lover of unity, has established between the most remote things. It is evident in this example, where we find the computation of continuous magnitudes linked with the summation of the powers of numbers.

This is certainly an admirable passage; but you will notice that

[1] Ibid., pp. 170, 171.

(*Translator's note.* In the original this reads: *Summa omnium [linearum] in quolibet gradu est ad maximam in proximè superiori gradu, ut unitas ad exponentem superioris gradus.*)

That is, $\frac{1^2+2^2+3^2\ldots+n^2}{n^3}=\frac{(1+1/n)(2+1/n)}{6}=1/3$ when n is very large.

[2] Ibid., p. 171.

Pascal says: "I wanted to make a point of adding these few remarks, which are familiar to those who have studied indivisibles." In fact, these remarks are nothing more than the expression of something that was rather commonplace and well known to all mathematicians, whether they dealt with indivisibles or not. The fact that a line is not increased by adding a point, nor an area by adding a line, nor a solid by adding an area, is implied in the formal principles of geometry[1] known from time immemorial; and the fact provides no stimulating impulse for a geometer, unless he raises the general problem of the continuum.[2] As for the connection between the sum of numerical powers (of numbers) and that of indivisibles (of continuous magnitudes), that was undoubtedly something not so well known, but more novel; yet it was something that formed the very basis of the work of Fermat and Roberval, whose influence on Pascal seems to have supplanted that of Desargues. Once again, it is the cleverness of his discoveries, and the clarity of his exposition, not the invention of fresh principles, that stamps the genius of Pascal.

His mathematical genius shone forth in all its brilliance for the last time in the series of works devoted to the roulette (cycloid). The story of this revival of interest on the part of Pascal, who since his "night of fire" (23 November 1654) had resolved to forsake the world (and science), and had renounced all except God, is well known. Marguerite Périer tells us[3] that in 1657 Pascal, when suffering from a violent toothache,

> decided to relieve the pain by devoting himself to something which by its powerful influence would so strongly attract thoughts to his brain that it would divert them from his pain. To that end, he thought about the proposition concerning the roulette, previously considered by Mersenne, which no-one had yet solved, and to which he had not previously given attention.

[1] The analogy with the relationships between various orders of magnitude and those set forth by Pascal between the degree of persons, minds, and charity is not our concern here.

[2] In this instance the principle put forward by Pascal is not to be taken literally, because it is quite certain that by removing a point from a line, or even a space, something is taken from the former, and a hole is made in the latter. We could quite well transpose this comparison and apply it to the relationship between God and created thing, and attribute to the latter, which is incapable of adding something to Divine action, either the capacity to preserve it entire, or on the contrary to rob it of something.

[3] See *Mémoire sur la vie de M. Pascal écrit par Mademoiselle Marguerite Périer, sa nièce*, *Œuvres complètes*, p. 40: also *La Vie de Monsieur Pascal écrite par Madame Périer, sa sœur*, ibid., pp. 19 ff: and the anonymous note in the *Recueil Guerrier* quoted ibid., p. 174.

He pondered so well on the matter that he found the solution and all the proofs. This intense effort of concentration stopped his toothache; and when he ceased thinking about the problem, after having solved it, he found himself cured.

Nonetheless, "he recorded nothing about it, and set little store by the discovery, for he regarded it as vain and useless; and he was unwilling to forgo the time devoted to his work on religion". It was only on the insistence of the Duc de Roannez, that Pascal decided to edit his discoveries and to make them the subject of a competition. The Duke remarked to him that in order to combat atheists and libertines it was "good to show that one knew more than all of them about the concerns of geometry and what is a matter of proof", and that if one submitted to revelation of faith, it was not through ignorance, but, on the contrary, it was because one knew better than others the limits of reason and the value of proofs.

In June 1658, under the pseudonym of Amos Dettonville, Pascal sent a circular letter to European mathematicians challenging them to find the solutions to six rather difficult problems relating to the area of a segment of the cycloid, the centre of gravity of this segment, and the volumes and centres of gravity of the solids of revolution formed by turning this segment about its base and its axis respectively. Two prizes, one of 40 and the other of 20 pistoles, were offered to competitors. Another circular letter laid down the conditions of award of the prizes. The amount of the prizes was deposited with Carcavi, to whom competitors were required to send their memoirs.

Marguerite Périer's story is a good one. Unfortunately, it is rather unlikely. In fact, even if we admit the episode of the toothache, it would still be quite inconceivable that Pascal, twenty years later, should suddenly remember the question propounded by Mersenne in 1636, and also that he had never before pondered on the properties of the cycloid, which was a very popular curve at the time, and which had been considered by Descartes, Fermat, Torricelli, and particularly by his teacher and friend Roberval.[1] Furthermore, Marguerite Périer's story contains another very serious inaccuracy. She says, that Pascal "had fixed the final date at eighteen months". In fact, Pascal, who on his own admission had worked for several months in solving the problems which he put out to competition,

[1] It had even provoked a polemic between Torricelli and Roberval, who had unjustly accused the Italian *savant* of plagiarism. This charge was renewed by Pascal in his *Histoire de la roulette* in 1658.

and who had sent out his first circular letter in June 1658,[1] had fixed the last day for receiving replies at *the first of October in the same year*. Making allowance for postal delays, this date gave competitors three months at the outside. It is not surprising, therefore, that John Wallis, who sent his first reply to Carcavi dated 18 August 1658, should have asked for an extension of time, or at least that the first of October should be regarded as the date of despatch and not the date of receipt of the replies, because these conditions of the competition unduly favoured the French mathematicians, especially those at Paris. Pascal refused. In his *Réflexions sur les conditions des prix attachés à la solution des problèmes concernant la cycloide* (circular letter dated 7 October 1658 announcing the closing of the competition), which is rather haughty and disagreeable in tone, he justifies his refusal by the quite specious reason that if he had done otherwise

> even those who might have won the prizes by being amongst the first whose solutions were received by the first of October, would never have been sure of being able to enjoy them, because they could always have been contested by other solutions arriving every day and bearing earlier dates whereby they would have been excluded on the word of burgomasters and officials of some almost unknown town in the depths of Muscovy, Cochin-China and Japan.[2]

It is easy to see that Pascal had no mind to risk the loss of his 60 pistoles, and had definitely decided to win his own competition.[3]

Notwithstanding the unfavourable conditions, the competition provoked great interest. Sluse wrote to Pascal (6 July 1658) to say that he had solved the first problem a long time before; however, the others seemed to him to be too difficult. Huygens, too, found the

[1] See *Problemata de cycloide proposita mense Junii* 1658: *Œuvres complètes*, p. 180: "*Quum ab aliquot mensibus, quaedam circa cycloidem, ejusque centra gravitatis, meditaremur, in propositiones satis arduas ac difficilis, ut nobis visum est, incidimus.*"

[2] See *Réflexions sur les conditions des prix attachés à la solution des problèmes concernant la cycloide*: *Œuvres complètes*, p. 185. Pascal adds (ibid.): "Glory is not at my disposal; it is given by merit; I have nothing to do with that; I only administer the award of the prizes; as they are provided by my own liberality, I have complete freedom to settle the conditions. They were established accordingly; no-one has cause to complain of it; I owe nothing to the Germans, or the Muscovites; I could have offered them only to the French; I can offer some others only for the Flemish, or for anyone else that I please."

[3] The first three and the sixth. He did not solve the remaining two, and did not solicit a prize. As far as he was concerned, the competition had important results. It attracted his attention to the cycloid, which he proved in 1659 to be a curve having the property of tautochronism.

problems difficult; yet he solved four of them. Christopher Wren solved none. On the other hand, he rectified the cycloid (it was, therefore, the second curve to be rectified), and found its length equal to four times the diameter of the circle by which it is generated. Wallis sent a rather long memoir in which he attempted all the problems propounded by Pascal, dealing with them in a very ingenious manner. Unfortunately for him, through hasty working, he made several mistakes in calculation, and even in method, which he corrected in part, but not entirely.[1] Finally, a Jesuit, Father Lalouère, professor at the Collège de Toulouse, sent in a memoir which he claimed, quite wrongly, to deserve the prize.

Immediately after the *Réflexions sur les conditions des prix*, Pascal published three accounts giving the history of the competition, and explaining his reasons for not awarding the prizes.[2] Then, in December 1658, appeared a *Lettre à M. de Carcavi* in which he made known his results and the methods by which they were obtained. In January 1659 the *Lettres de A. Dettonville, contenant quelques-unes de ses inventions en géométrie* were published. These contain, amongst others, (*a*) the famous *Traité des sinus du quart de cercle*, which gave Leibniz the inspiration for his discovery of differential calculus; (*b*) the proof "in the manner of the ancient philosophers" *de l'égalité des lignes spirale et parabolique*; and (*c*), in a *Lettre à M. Huygens de Zulichem*, a proof in the manner of the modern philosophers that "roulette curves were by their very nature always equal to ellipses"; true ellipses in the case of prolate, or curtate, cycloids; and ellipses flattened into straight lines in the case of the simplest cycloid.[3] The

[1] Wallis revised his memoir and published *Tractatus de cycloide* in 1659. He never forgave Pascal.

[2] *L'Histoire de la roulette*, 10 October 1658; *Récit de l'examen et du jugement des écrits envoyés pour les prix proposés publiquement sur le sujet de la roulette, où l'on voit que ces prix n'ont point été gagnés, parceque personne n'a donné la véritable solution des problèmes*, 25 November 1658; *Suite de l'histoire de la roulette, où l'on 'voit le procédé d'une personne qui s'était voulu attribuer l'invention des problèmes proposés sur ce sujet*, 12 December 1658; *Addition à la suite de l'histoire de la roulette*, 20 January 1659. The last two were directed against the Rev. Father Lalouère, who was accused by Pascal in *l'Histoire de la roulette* of having plagiarized Roberval. *Œuvres complètes*, pp. 194, 208, 211, 216.

[3] See *Dimensions des lignes courbes de toutes les roulettes, Lettre de M. Dettonville à M. Huyghens de Zulichem, Œuvres complètes*, p. 340. It is worth while quoting Pascal in full: "We see . . . that as the base line of the roulette becomes more nearly equal to the circumference of the circle by which it is generated, so the minor axis of the ellipse to which it is equal decreases with respect to the major axis; and when the base is equal to the circumference, i.e. when the roulette is simple, the minor axis of the ellipse is completely eliminated; and then the

subtlety, ingenuity and virtuosity displayed by Pascal in his treatises is dazzling. With unrivalled skill he handles the methods of both the Ancients and the Moderns. He compels one's admiration. Huygens, who was an adept in the methods of the "ancients", and never relished those of the "moderns" (that is to say, the use of indivisibles), nevertheless reproached him for "a rather too daring method, which departs too far from geometric exactness"; and wrote, "that he is anxious to be able to call himself his pupil in a science where he so greatly excels". However, we should be wrong to call these works of Pascal "the first treatise on integral calculus", as is often done, for example by Émile Picard. It is undoubtedly true that in Pascal's work on the roulette we find "fundamental results relating to what geometers nowadays call curvilinear integrals and double integrals presented in extremely ingenious geometrical guise"; and that "it suffices for the purpose of showing the power of these methods to recall the beautiful theorem on the equivalence of the arc of a prolate, or curtate, cycloid to the arc of an ellipse". It is quite true, as I have already said, that it is very easy to translate Pascal's reasonings into the language of the infinitesimal calculus. It is equally true, that in doing so we obtain only a translation, and that Pascal's reasoning remains essentially geometric. The "case" of the "characteristic triangle" is most significant in this respect. It was "characteristic" for Leibniz, but not in the least for Pascal; because the latter did not think of *ratio*, he thought of *object*; and it was for this reason that he failed to make Leibniz's discovery, in the same way that some years before he failed to make Newton's discovery.

I have said that Pascal handled the methods of the modern philosophers, that is to say, the geometry of indivisibles, with unrivalled virtuosity and originality. On the other hand, his *interpretation* of this method seems to me rather disappointing. Pascal seems not to

curve of the ellipse (which is completely flattened) is the same as a straight line, namely, its major axis. Hence, in this case the curve of the roulette is also equal to a straight line. It was for this reason that I informed those to whom I sent this calculation that the curves of roulettes were always by their very nature equal to ellipses; and that the admirable equivalence of the simple roulette to a straight line, as discovered by Mr Wren, was, so to speak, only an accidental equivalence, resulting from the fact that in this case the ellipse is reduced to a straight line. To which M. de Sluze added the appropriate comment, that we should on this account admire even more the orderliness of nature that allows us to discover that a straight line equals a curve, only after we have already allowed a straight line to be equal to a curve. So, in the case of the simple roulette, where the base is presupposed equal to the circumference of the circle by which it is generated, it happens that the curve of the roulette is equal to a straight line."

have understood the true meaning of Cavalieri's concepts, for whom the 'indivisible' elements of a geometric object have a dimension one less than the object.[1] For Roberval the indivisible elements have the same dimensions as the object; and it is Roberval's concepts, which are not a correct interpretation of those of Cavalieri, that Pascal puts before us in a famous passage in the *Lettre à M. de Carcavi*,[2] "in order to show that everything which is proved by the true rules of indivisibles can be proved rigorously also in the manner of the ancient philosophers; and hence that the one method does not differ from the other except in the manner of exposition. This cannot be prejudicial to reasonable people once they have been informed of what is meant thereby.

That is why [Pascal continues] I shall not hesitate in the sequel to resort to the language of indivisibles, and to use the expressions *the sum of lines* or *the sum of areas . . . the sum of ordinates*, which use seems to be ungeometrical to those who do not understand the doctrine of indivisibles, and who imagine that it is a crime against geometry to express an area by an infinite number of lines. This view derives solely from their lack of intelligence, for it means nothing more than the sum of an infinite number of rectangles formed by each ordinate with each of the small equal portions of the diameter, the sum of which is certainly an area. . . .

Briefly, then, Pascal was a mathematician of very great talent, who had the good fortune in the prime of his youth to have been moulded by, or at least to have come under the strong influence of, Desargues; and the ill fortune in his maturity to have been strongly influenced by Roberval.[3] He was, most definitely, one of the foremost geometers of his time, though we cannot place him on the same plane as the three mathematical geniuses of the seventeenth century, namely Descartes, Desargues and Fermat, of whom France has every right to be so proud.

Let us now turn to Pascal the physicist. He is much better known as such, than as a mathematician; and for good reason. Whereas Pascal's mathematical works are now rather difficult for us to follow,

[1] Concerning this see my article mentioned on p. 132, n. 1 above.

[2] See Lettre de *Monsieur Dettonville à Monsieur de Carcavi*, *Œuvres complètes*, pp. 232 ff.

[3] It is difficult to make an objective judgement on the work of Roberval, which is inadequately known, and partly unpublished (or lost). In any case, it seems certain that he did not reach the first rank, in spite of his undeniable talent. Pascal is definitely far superior to him.

the works of Pascal the physicist are not. Furthermore, they have been frequently published and republished. Every well-informed person knows the fascinating accounts in the *Expériences nouvelles touchant le vide* and *La grande expérience de l'equilibre des liqueurs* (the experiment at the Puy-de-Dôme). It has been frequently said, and rightly so, that they are gems of scientific literature, wherein it is impossible not to admire the marvellous clarity of exposition, the power of thought, and the skill with which the experiments are brought one after the other to the attention of the reader.

There is something magical in Pascal's style; and the same ideas found in other authors assume a different significance when read in his works. Three obscure pages of Mersenne, or one of Roberval, are reduced by Pascal to ten lines, which leave the impression that we have something quite different. We are tempted to invoke the Boyle-Mariotte Law, and to say that the density of thought is inversely proportional to the volume or extent of the written word.

I fear, however, that this magic of style somewhat lessens our critical faculties, and prevents us from examining Pascal's accounts in virtue of their content. Let us try to do so without bias. We all know the text of the *Expériences nouvelles touchant le vide.* Nevertheless, I shall take the liberty of quoting some fragments without entering into the history of the circumstances responsible for their publication.[1]

The occasion of these experiments – wrote Pascal – was the following. About four years ago in Italy a trial was made with a glass tube four feet long, open at one end and hermetically sealed at the other. It was filled with quicksilver, the open end was closed with the finger, or in some other way, and the closed end was immersed perpendicularly to a depth of two or three fingers into more quicksilver contained in a vessel filled half with quicksilver and half with water. The open end was then uncovered under the quicksilver in the vessel, whereupon the quicksilver in the tube fell to some extent, leaving an apparently empty space at the top of the tube, whilst the lower part of the same tube remained filled with quicksilver up to a certain height. On raising the tube until the opening, which formerly dipped under the quicksilver in the vessel, emerged from this quicksilver into the water above, the quicksilver in the tube rose to the top together with some water, and these two liquids mingled together in the tube; but finally all the quicksilver fell, and the tube was left full of water.

This experiment being reported from Rome to the Rev. Father Mersenne

[1] See *Œuvres complètes*, pp. 363 ff. For the history of vacuum, see the excellent study by Corneliys de Waard, *L'Expérience barométrique, ses antécédents et ses applications*, Thouars: 1936.

of the Order of Minims at Paris, he revealed it in France in the year 1644. It was admired by all the *savants* and curious, through whom it became generally known. I learned of it from M. Petit, Intendant des fortifications, a person much versed in belles-lettres, who had learned of it from Mersenne himself. We, le Sieur Petit and I, carried out the experiment together at Rouen in the same way that it had been done in Italy, and confirmed in every particular what had been reported from that country, without having noticed anything further.

There are two omissions in Pascal's account. He does not tell us as a fact, that the "*savants* and curious" of Paris, who tried to repeat Torricelli's experiments at Paris, did not succeed for the rather important reason that the glassmakers of Paris were unable to supply glass tubes sufficiently strong to support the pressure of four feet of mercury. The glassmakers of Rouen being superior to those of Paris, the "long blow-pipe" (tube) ordered from them by Pierre Petit was strong enough. For this reason, Pierre Petit (with Pascal) was the first in France to succeed in producing the "Torricellian" vacuum. He does not say, subsequently, that the experiment which he made with Pierre Petit, and on which he had modelled his own, had the illustrious Italian *savant* for its author.

It is rather difficult to account for this twofold silence. We might suppose that Pascal did not wish to wound, or upset, his friends at Paris by publicly proclaiming their failure – a failure for which they were, moreover, in no way responsible. We might also suppose, that he was of the opinion that, having invented and succeeded in enough original and novel experiments, he had no need to boast of having been *the first* (with Petit) to succeed with an old experiment. But why conceal the name of Torricelli? Pascal would no doubt tell us, as he told M. de Ribeyre (16 July 1651), that at the time, namely in 1646 and 1647, he did not know that the author in question was Torricelli; but that having afterwards learned it, he never failed to acknowledge it. However, it must be admitted that this pretended ignorance is, to say the least, rather surprising, seeing that Petit in his letter to Chanut distinctly refers to the experiment "of Torricelli"; and that Roberval, too, in his first *Narration* to Desnoyers written in October 1647 to defend the (relative) priority of Pascal against the claims of Magni to absolute priority, named him quite plainly.[1]

But let us leave that in order to continue and complete the story.

[1] *Œuvres de Blaise Pascal*, edited by Brunschvicg-Boutroux, I, pp. 323 ff (Letter of P. Petit to A. Chanut); and II, pp. 21 ff (First *Narration* from Roberval to Desnoyers).

The experiments made by Petit in collaboration with him were in themselves amply sufficient to refute the traditional doctrine of the impossibility or "horror" of a vacuum. They did not succeed, however, in persuading the champions of tradition. After the departure of Petit, Pascal decided to carry out a series of fresh experiments, this time by himself, in order to convince the most incredulous individuals, and finally to destroy the old, persistent prejudice.

The experiments of Petit and, even more so, those of Pascal excited great interest, and brought well-deserved fame to the latter. In the autumn of 1647 Mersenne received a letter from Warsaw bearing the date 24 July in which Pierre Desnoyers, a Frenchman who had followed Marie de Gonzague thither, broke the news of the experiments "of a Capuchin by the name of Valeriano Magni, who was printing a work on philosophy, wherein he proves that a vacuum can be found in nature". The receipt of this letter, together with the "philosophy" of Magni[1] in which he claimed for himself the glory of having been the first to prove the existence of a vacuum, and to have seen with his own eyes *Locum sine locato*, *Corpus motum successive in vacuo*, *Lumen nulli corporis inhaerens*, forced Pascal to publish his *Expériences nouvelles*. For his part, Roberval sent Desnoyers a *Narration* in which, besides protesting against the claims of Magni, whom he accused of simply plagiarizing Torricelli, he gave an account of the work of his young friend.[2]

In the title of his short treatise Pascal states that the experiments were made "in tubes, syringes, bellows and siphons of various lengths and shapes; with divers liquids, such as quicksilver, water, wine, oil, air, etc. . . ." He states, likewise, that his little treatise is only an "abridgement", put forth in advance "of a larger treatise on the same subject". The author's *Letter to the Reader* informs us that "circumstances preventing him for the time being from giving a full Treatise in which he has recorded many fresh experiments concern-

[1] *Demonstratio ocularis loci sine locato, corporis successive moti in vacuo, luminis nulli corpori inhaerentis*, etc. Varsaviae: [N.D.] (the "Approbation" is dated 16 July 1647). Magni completed his work with the issue of *Altera pars Demonstrationis ocularis de possibilitate vacui*. The two works were then combined under the title *Admiranda de vacuo*, Varsaviae: [N.D.] (1647).

[2] The charge of plagiarism put forward by Roberval is anything but justified. As for that made by Pascal, in his turn (in his letter to M. de Ribeyre dated 16 July 1651), claiming that he had been plagiarized by Magni, it is quite fantastic. For all that, Magni in his reply to the charge made by Roberval acknowledged the rights of Torricelli, but maintained his own claim to originality (5 September 1648); see Corneliys de Waard, op. cit., pp. 125 ff.

ing vacuum, and the conclusions he has drawn therefrom",[1] he therefore wishes to give an account of the principal ones in this abridgement "which is a preview of the design of the complete work".

As a matter of fact "the design of the complete work" nowhere appears in the *Expériences nouvelles*. Undoubtedly the purpose of the *Traité* was to prove that the effects attributed to the horror of a vacuum were in reality to be ascribed to the pressure (or weight) of the ambient air. Now, the *Expériences nouvelles* completely ignore this subject, and are solely devoted to proving that a vacuum can exist. This proof is made in two parts. First of all "an apparently empty space" is produced; then it is shown that "the apparently empty space is not filled with any matter known in nature, or that can be perceived by any of the senses". The conclusion is that the space is indeed empty and "destitute of all matter . . . until the existence of that material which does fill the space has been proved".

The main experiments recorded by Pascal are eight in number: 1. experiments with a syringe; 2. experiments with a pair of bellows; 3. experiments with a glass tube 46 feet long; 4. experiments with a scalene siphon having a longer limb of 50 feet and a shorter limb of 45 feet; 5. experiments with a tube 15 feet long filled with water in which a string is placed, and which stands in a vessel full of mercury; 6. another experiment with a syringe; 7. and 8. two experiments with a siphon having a longer limb of 10 feet and a shorter limb of $9\frac{1}{2}$ feet, the ends standing in two vessels containing mercury. These extremely ingenious experiments demonstrated: *a.* that nature, far from opposing an insuperable resistance to the production of a vacuum, offers only a limited resistance; *b.* that a force, slightly greater than that with which a column of water 31 feet high tends to flow down, is sufficient to produce a vacuum; and, moreover, that nature offers no more resistance to the production of a high vacuum than a low one; *c.* that the latter, once produced, may be increased at will without any opposition. We shall select two of the experiments, namely, the third and fourth, which are the most famous, where Pascal tells us that he used glass tubes 46 and even 50 feet in length. Here are the descriptions.

[1] What prevented Pascal from publishing his *Traité* was the fact that he had not yet written it. It was in fact not completed till 1651 (see p. 131, n. 2 above), nor was it published. According to Florin Périer "this treatise has been lost, or rather, because he was so fond of brevity, he himself reduced it into two small treatises" on *L'équilibre des liqueurs* and *La pesanteur de la masse de l'air*.

3. A glass tube 46 feet long, with one end open and the other hermetically sealed, is filled with water, or better still with dark red wine so as to be more easily seen; it is then closed and raised so as to be perpendicular with the closed end at the bottom, and immersed to a depth of about one foot in a vessel full of water. If the opening is unstopped, the wine falls in the tube to a height of about 32 feet above the surface of the water in the vessel, empties itself into the vessel containing the water which it mingles with and slowly tinges. The wine detaches itself from the top of the tube, leaving an apparently empty space about 13 feet long, or, what comes to the same thing, no substance appears to have replaced it. If the tube is inclined, the height of the wine in the tube becomes less as a result of being tilted, and then rises again until it has reached the height of 32 feet. Finally, if the tube is inclined to a height of 32 feet, it becomes completely filled by sucking in water equal in amount to the wine which had been cast out; so the tube now appears full of wine from the top to about 13 feet from the bottom, and full of slightly coloured water in the lower remaining 13 feet.

4. A scalene siphon, with a longer limb of 50 feet and a shorter limb of 45 feet, is filled with water, the two open ends are stopped and immersed to a depth of about one foot in two vessels full of water in such a way that the siphon is perpendicular, and the water level in one vessel is five feet higher than that in the other. If the two openings are unstopped whilst the siphon is in this position, the longer limb draws no water from the shorter limb, nor, consequently, from the vessel in which it stands (contrary to the opinion of all philosophers and artisans); but the water falls in both limbs standing in the two vessels until it reaches the same height as in the previous tube, measured from the water level in each vessel. When the siphon is inclined so as to bring it below the height of about 31 feet, the longer limb draws water from the vessel containing the shorter limb. On raising the siphon above this height, the flow of water ceases, and both limbs discharge, each into its own vessel. When the siphon is relowered, the water in the longer limb draws water from the shorter limb as before.

The description is worthy of Pascal. For the moment, however, let us forget that we are dealing with Pascal. Suppose we are dealing with an anonymous text, or one written under an unknown name. Would we not ask ourselves whether the author in question really did carry out the experiments he describes? And whether, having done them, he described them *exactly* and *completely*? Let us put these questions to Pascal.

Glass tubes 46 feet long! – they are very difficult to make, even today. Although Roberval assures us that they were made with consummate skill (Roberval, anyhow, says 40 feet), it is very unlikely that seventeenth-century glassmakers, even those of Rouen, were capable of producing such a thing. Furthermore, it is not an easy matter to handle a tube 15 metres long, even if (once again, the infor-

mation is provided by Roberval) they are attached to poles.[1] In order to carry out the movements implied in the experiments of Pascal, scaffolding and hoisting tackle are needed; in short, an industrial installation much more powerful and more complicated than that normally used in shipyards, for it is much easier and simpler to step a ship's mast than to move, in the manner required by Pascal, a scalene siphon with a longer limb of 50 feet . . . . It is rather surprising that Pascal gave neither a description nor a drawing of the devices. We are not satisfied to learn from Pascal that these experiments cost him much trouble and expense; nor from Roberval, that Pascal constructed very ingenious equipment. We should have preferred some details of this equipment, as well as of the way in which the tubes and the great 50-foot siphon were successfully made.

Let it be understood, I do not want to insinuate that Pascal did not carry out the experiments he describes (or that Roberval records), even though the scientific literature of the seventeenth century is full of experiments that were never made. Mersenne, less credulous in these matters than most historians of the nineteenth and twentieth centuries, very rightly cast doubt on the famous experiments of Galileo relating to the free fall of bodies and their motion on an inclined plane. Viviani has related the experiment (a made-up story) that Galileo is alleged to have made by dropping cannon-balls from the leaning tower at Pisa. Borelli in his polemic with Stefano d'Angeli coldly invoked the experiments, the results of which, if he had performed them, would have been to his confusion. As for Pascal himself, the *Traité de l'équilibre des liqueurs* contains a series of experiments the imaginary nature of which has already been rightly stressed by Robert Boyle.[2]

There is nothing abnormal in all this. As I have already said, the scientific literature of the seventeenth century – and not only of the seventeenth century – is full of these fictitious experiments; and we could write a very instructive book on the part played in science by experiments that have not been made, and are even impossible to make.

Once more, I do not wish to assert that Pascal did not carry out the experiments that he claims to have made. On the other hand, I do

[1] First *Narration à Des Noyers.* Roberval's *Narration* is frequently richer in details, and even in facts, than the *Expériences nouvelles.*

[2] For example, the experiment where a man supports a tube on his thigh whilst holding himself twenty feet above the surface of the water.

believe I am able to assert that he has not described them *in the way in which he performed them*, and has not presented their results *in the way that they appeared*. He has very definitely concealed something from us.

In fact, when Gasparo Berti, inspired by Galileo's *Discorsi*, carried out the first experiment on vacuum at Rome[1] – Berti used a lead tube 10 metres long terminated by a large glass head-piece, the whole being fixed to the façade of his house – it was established, as predicted by Galileo, that the water stopped at a certain limiting height. Something else, too, was established, namely, that the water started to boil. It was quite natural for it to do so, because the air dissolved in the water was escaping in the form of bubbles. This phenomenon was rather embarrassing for the supporters of vacuum, such as Berti himself; whilst those who denied a vacuum could, with some semblance of reason, claim that the space over the water was empty only in appearance, whereas in fact it was full of air and water vapour.

The phenomenon of boiling could not have failed to occur in Pascal's tubes. It would have been inevitable. In 1950, when Pascal's experiment was reproduced at the Palais de la Découverte, it was found that the water boiled, and rather violently at that. This was the occasion when the difficulty of obtaining a glass tube 15 metres long was realized; the attempt was finally abandoned, and an assembly of tubes each 255 cm long was employed instead.

Could this phenomenon have escaped Pascal's notice? I think not. Moreover, to admit it would be to pass sentence on Pascal the experimenter. The phenomenon of boiling is not the only remarkable phenomenon that occurs in the tube. As a result of the pressure set up by the air (and the water vapour) the column of water falls, and this fall reaches $1\frac{1}{2}$ metres in 24 hours.[2]

But there is something even better than this. In 1647 Roberval had not only enthusiastically taken the part of Pascal against Magni, but also had espoused all Pascal's conclusions, which he gave in his first *Narration* to Desnoyers (October 1647) where he proffered some details and amplification of Pascal's experiments that were not given by Pascal himself: in 1648 he suddenly altered his mind. The fact is, that in 1647 he himself had made very few experiments (with mercury).

[1] See Corneliys de Waard, op. cit., pp. 101 ff.

[2] These phenomena – boiling, and the drop in level of the liquid – ought to be much more pronounced in the case of wine than water. As for the siphon, an air-lock would inevitably be formed at the top.

Since then, he had made others; and he noticed small air bubbles rising the whole length of the column of mercury. Did they come from the air adhering to the walls of the tube, or was it air contained in a compressed state in the mercury itself? No matter! It was clear, in any case, that the apparent empty space could not be admitted to be identical with a real vacuum. Roberval, then, in his *Seconde Narration* (May 1648), when describing Pascal's experiments with water and wine, added that those who were present (Roberval himself was not present at Rouen) had not failed to notice the small bubbles of air rising the length of the tube, and getting larger during the rise. The phenomenon implies compressibility, and, *vice versa*, an expansion, of air surpassing anything that could possibly be imagined.[1]

It seems to me that we are forced to the conclusion that Pascal has not given us a complete and exact account of the experiments that he made or imagined. This conclusion throws a singular light on his polemic with the Rev. Father Noël; and, what is more, considerably modifies the traditional view of Pascal as a shrewd and careful experimenter, which historical convention contrasts with the impenitent *a priori* reasoner Descartes. No! Pascal is not a faithful disciple of Bacon, nor is he a first edition of Boyle.

So! there are air bubbles in the water, and even the mercury. Well, is that all? It was of no consequence as far as Pascal was concerned. He had so well, so clearly, *imagined* the experiments, which he did (or did not) carry out, that he was able with great profoundness to seize their essentiality, namely, the interaction of liquids mutually in equilibrium, for Pascal regarded air as a liquid.[2] It was a great pity that the liquids he used – wine, water, oil, mercury – were not perfect, continuous, homogeneous liquids; that they contained air; and that this same air adhered to the sides of the tube. So! the expanded air filled the "apparent empty space"? True! it was very inconvenient; but if we could use liquids that do not contain any of it, *then* the experiment would proclaim the identity of the apparent empty space with a true vacuum. For, though Pascal in his conclusions did not

[1] See *Deuxième Narration*, *Œuvres de Blaise Pascal* edited by Brunschvicg-Boutroux, II, p. 328. This is an atrociously ill-natured remark on the part of Roberval.

[2] Pascal was already in full possession of this doctrine in 1647. Proof is found in the *Lettre à Florin Périer* dated 15 November 1647 dealing with the barometric experiment to be carried out at the Puy-de-Dôme; and the fact that he had at this same period conceived the experiment of a vacuum within a vacuum.

formally affirm its existence (he did so in his letters to the Rev. Father Noël and M. Le Pailleur), it is clear that he was fully convinced of it. The very definition given in his *Lettre au R.P. Noël* is sufficient proof; although doubtless he had reason to point out that a definition is not an opinion: and to say, "I give such a name to such a thing", does not in principle imply assertion of its existence. If we do not believe that it really exists, we do not say, "That which we call empty space is a space having length, breadth and depth, is motionless, and capable of receiving and containing a body of similar length and shape: that is what is called *solid* in geometry, where only abstract and immaterial things are considered": and the Rev. Father Noël, though guilty of a formal error, was not deceived thereby. Pascal simply did not want to expose his batteries prematurely; in fact, he kept in reserve a complete *Traité* which would provide the required proof, and at the same time explain, in accordance with the theory of the equilibrium of liquids, the reason why a vacuum is produced in the tubes. In the meantime, he did not want to sow doubt in the mind of the ingenuous, who must, on the contrary, be prepared to accept future proofs; nor did he want to give weapons to his adversaries.

The most famous, the most pathetically famous, of his adversaries was incontestably the Rev. Father Noël, S.J., who, after having read the *Expériences nouvelles*, sent Pascal a letter in which he defended the traditional doctrine by a mixture of old arguments and Cartesian ideas. He pointed out that light is transmitted through the apparently empty space; and he suggested that "the apparently empty space in Torricelli's tubes was filled with a refined air which enters through the small pores of the glass". It was unfortunate for him. Pascal's reply, a masterpiece of polished, cutting irony (a precursor of the *Provinciales*), administered a lesson both in method and physics to the Vice-provincial of La Flèche. Pascal, amongst other things, protested to the unlucky Jesuit that we are not acquainted with the nature of light, and that the definition of it given by the Rev. Father – "Light is a luminary movement of rays composed of lucid, that is to say, luminous bodies" – being circular, means nothing at all; and, further, we have no right to assert that it can be propagated only in a plenum and not in a vacuum; and, still further, because an hypothesis explains a phenomenon, we cannot conclude that the hypothesis is true, for the same phenomena are quite able to receive a multiplicity of explanations, besides being produced by the most diverse causes. For

example, celestial phenomena are explained equally well by the Ptolemaic, the Copernican, or the Tychonian hypotheses.

The Rev. Father ought to have kept quiet. Unfortunately for him, but fortunately for us, he replied; and it is to this reply that we are indebted for the dazzling *Lettre de Pascal à M. Le Pailleur*,[1] an unexcelled masterpiece of pitiless, ferocious polemic. The unlucky Rev. Father was literally put on the grill, turned over and inside out, and made to appear perfectly ridiculous. The reader cannot refrain from laughing, and he ends his reading with the impression that Pascal is a genius, whilst the Rev. Father Noël is a complete fool, and that the metaphysical objections he raised against the notion of a vacuum are just as valueless as his definition of light, or his explanation of the rise of mercury (or of water) in a tube through the action of "mobile lightness".

Pascal was most certainly a genius, and the Rev. Father Noël equally certainly was far from being one. There is no doubt about that, any more than there is about the superiority of Pascal's physics over that of this unfortunate, behind-the-times, scholastic. Nevertheless, when the latter wrote: "*This space, which is neither God, created being, body, spirit, substance, or event*; *which transmits light without being transparent*; *which resists without resistance*; *which is immoveable and yet betakes itself with the tube*; *which is everywhere and nowhere*; *which does everything and does nothing*, etc . . ." – are we sure that he is unquestionably ridiculous and stupid? Pascal in his reply evaded "*neither God, nor created being*" on the pretext that "mysteries concerning the Divinity are too sacred to be profaned in our disputes", as though it were a case of a question of dogma instead of a problem of pure metaphysics. He continued:

> *Neither body, nor spirit.* It is true that space is neither body nor spirit, but is space. In like manner time is neither body nor spirit, but is time; and as time nevertheless exists, even though it be none of these things, so empty space can certainly exist, without being for that reason neither body, nor spirit. *Neither substance, nor event.* That is true, if by the word *substance* we mean that which is either body or spirit; for, in this sense, space will be neither substance nor event; but it will be space, in the same way that time being neither substance nor event, is time; because in order to exist, it is not necessary to be substance, or event.

Is this really a particularly wonderful reply? Isn't Pascal treating important metaphysical problems, which preoccupied the greatest

[1] In 1648. *Œuvres complètes*, pp 377-91.

minds of his time, rather cavalierly, and without due consideration? In any case, when we read all that in Gassendi, from whom Pascal borrowed it, we unquestionably admire it less; and possibly not at all.

On the other hand, when we find the Rev. Father Noël's objection in the works of other writers, they do not seem at all absurd. The Rev. Father says exactly the same things as Descartes, Spinoza and Leibniz, all of whom agreed in denying a vacuum; and who very seriously propounded (as did Newton also) the problem of possible relationships between God and space (as understood by Pascal), which cannot be a created thing. All of them, of course, gave different answers to the problem, which they treated most seriously.

Even the objection, that the passage of light through an "apparently empty space" excludes the possibility of a "true vacuum", is hardly likely to be derided by us, when we find it under the name of Huygens. Nor do we consider the physicists of the nineteenth century, starting with Young and Fresnel, to be absurd, when they postulate a luminiferous aether in order to explain the transmission of light through "apparently empty space", as they did for reasons similar to those of the Rev. Father Noël, who in this respect may be regarded as their forerunner. The magic of the Pascalian word is a dangerous thing, which it is difficult, but therefore all the more necessary, to resist at all costs, for it beguiles us into errors of history, and leads us to injustice and indiscretion.

I have gone on far too long, and must close without being able to discuss *La grande expérience de l'équilibre des liqueurs* (the experiment at the Puy-de-Dôme), the meticulous and exact organization of which remains without question worthy of Pascal, and indisputable evidence of his experimental genius, even though the idea of this experiment had been suggested to him by others, in particular by Descartes, who forecast a positive result; or Mersenne, who doubted it. Nor can I say anything about the *Traités de l'équilibre des liqueurs, et de la pesanteur de la masse de l'air*, which summarize, no doubt completely, the lost *Traité du vide*,[1] which show Pascal in a fresh light as an arranger and systematizer.

These *Traités*, in fact, contain few really new ideas; possibly none at all. In reading them it is easy to detect (as Pierre Boutroux has done) the sources in which he delved, or from which he derived inspiration: Stevin, Mersenne, Torricelli. However, the multiplicity and variety of the experiments described, such as that of "a vacuum with-

[1] See p. 131, n. 2 and p. 148, n. 1 above.

in a vacuum"; and, indeed, the admirable order in which the facts, real as well as imaginary, are set forth and arranged as a function of one single idea, constitute a work of brilliant originality worthy of a place in the classics of science. In support of this statement we may mention more particularly the *Traité de l'équilibre des liqueurs* which is based on the principle of virtual work; nor must we forget the invention of the hydraulic press, a fine example of Pascal's technological ingenuity.

Nevertheless, the spirit of systematization, which is so well exemplified by Pascal in these *Traités*, is not without some danger. In fact, because Pascal considered air as a liquid, he was unable to make a clear distinction between the *pressure* of air and its *weight*; or, what is the same thing, a clear distinction between the elastic pressure of a gas and the non-elastic pressure of a liquid; and hence to explain by the *weight* of the air the phenomena produced by its *pressure*. This is a rather difficult matter, and the credit of supplying the explanation goes to Robert Boyle. It was usual at the time to treat air as a liquid (Descartes regarded air as a very tenuous liquid), thereby assimilating pneumatics with hydrostatics. Nevertheless, Pascal did suggest that air was compressible to a greater or less degree in explanation of the fact that the expansion of a bladder when carried up a mountain varies with the altitude.

But the discussion of all this would require space – of which there is no more.

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