The Science of Mechani Critical and Historical Account of Its Developmey Ernst Mach

TRANSLATED BY THOMAS J. McCORMACEW INTRODUCTION BY KARL MENGER

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INTRODUCTION TO THE SIXTH AMERICAN EDITION, 1960

Ernst Mach's book *Die Mechanik in Ihrer Entwicklung Historisch-Kritisch Dargestellt*, one of the great scientific achievements of the last century, remains a model for the presentation of the development of ideas in any field. In its own domain, the work is still full of vitality. It is an inspiration to philosophers of science, a valuable source of information for historians of physics, and a splendid help to teachers of mechanics. Its first half is a most stimulating introduction of unsurpassed clarity and depth for beginners.

The book follows the development of mechanics up to the turn of the century.[1] As the title indicates, the work is historical and critical.

That the *historical* presentation of a branch of science is the most penetrating approach to the subject matter and leads to the deepest insight was one of Mach's general methodological ideas. Nor is anything more conducive to creative thinking than an exposition of ideas as they have developed, of notions abandoned long since, and of the role that historical accidents have played in the genesis of current concepts—techniques that Mach introduced and superbly developed in his *Science of Mechanics*. Mach also applied the historical method of presentation to the theory of heat and to

[1]Nine German editions of this book have been published. Seven of them appeared during Mach's lifetime (1838-1916), in 1883, 1888, 1897, 1901, 1904, 1908, and 1912. The eighth and ninth German editions appeared in 1921 and 1933. English translations were published by the Open Court Publishing Company in 1893, 1902, 1915, 1919, 1942, and 1960.

parts of optics and the theory of electricity.[2] But the method has further potentialities. Other parts of mathematics, especially algebra, would profit from a similar treatment; and, most of all, the science of mechanics itself might gain if its development since 1900, including the theory of relativity, wave mechanics, and quantum mechanics, were presented *à la* Mach.

The *critical* parts of Mach's book culminate in his analysis of the concept of mass and his examination of Newton's ideas of absolute space and absolute time. The latter critique is quoted in almost every presentation of the theory of relativity. "This book," Einstein wrote in his autobiography[3] about Mach's *Science of Mechanics*, "exercised a profound influence upon me . . . while I was a student."

* * *

In this century, the analysis of the mass concept and, even more, physicists' views on space and time, have advanced beyond Mach. Yet his original discussions remain classics not only of physics but also of *philosophy of science.*

Against Newton's definition of mass as a quantity of matter, Mach raised the objection that it was of no help in actual operations with masses; and he formulated a new definition, based on Newton's Third Law—a definition that made it possible to measure masses. Mach's treatment initiated a method that was later greatly elaborated and applied to the philosophy of

[2] E. Mach, *Prinzipien der Wärmelehre* (Leipzig: 1896); ———, *Principles of Physical Optics* (Leipzig: J. A. Barth, 1921; London: Methuen Co., 1926; New York: Dover Press, 1953.) ———, "On the Fundamental Concepts of Electrostatics," in *Popular Scientific Lectures* (5th ed., La Salle, Illinois: Open Court, 1943), pp. 107-136.

[3] "Albert Einstein: Philosopher-Scientist," in *Library of Living Philosophers*, ed. P. A. Schilpp (Evanston: 1949), pp. 20-21.

physics by P. W. Bridgman[4] under the name of *operationalism.*

Mach rejected absolute space and time because they are unobservable. More generally, he proposed to eliminate from science notions which lack counterparts that are actually or at least potentially observable. He thereby became one of the initiators of *antimetaphysical positivism.*

A third point that Mach stressed over and over again was his view that science had the purpose of saving mental effort. General laws are shorter and easier to grasp than enumerations of specific instances. Simpler theories are preferable to more complicated ones. His theory of "economy of thought" is Mach's main point of contact with R. Avenarius who, in his *empirio-criticism,*[5] regarded philosophy as thinking about the world with minimum effort.

A fourth point of philosophical importance in Mach's program was the replacement of causal explanations by functional connections. In this respect, Newton had been a shining model. Without entering into the question that was uppermost in the minds of his contemporaries—the question as to *why* bodies attract each other—Newton was satisfied with formulating the specific connection of the attractive force between two bodies with their masses and with the distance between them. In the 18th and 19th centuries, countless attempts, now all but forgotten, were made to *explain* gravitation. Physicists hypostasized vortices, or tensions in media, or bombardments of the bodies by particles traversing space at random and driving, for instance, a stone toward the earth because the latter shields the stone against the particles coming from

[4] P. W. Bridgman, *The Nature of Physical Theories* (Princeton: 1936; New York: Dover Press, 1936).

[5] R. Avenarius, *Philosophie als Denken der Welt gemäss dem Princip des kleinsten Kraftmasses* (Leipzig, 1876).

below. But the real triumphs of human insight into gravitation—Newton's deduction of Kepler's Laws, the prediction of new planets which were subsequently observed, and, in the present age of experimental astronomy, the control of the motion of artificial satellites—these triumphs are independent of any explanation of gravitation and are entirely anchored in Newton's law that the attractive force between two bodies is proportional to their masses and inversely proportional to the square of the distance between them. Similarly, Hertz' prediction of electromagnetic waves is based on Maxwell's equations connecting the fundamental electric and magnetic quantities with each other, without aiming at an explanation of phenomena.

Mathematics has the tremendous creative power of evolving hidden consequences from assumptions about the observable universe and thus prompting predictions of previously unobserved phenomena. Not that the universe is under any obligation to conform to those predictions! But if these consequences are verified, then mathematics has led to new discoveries; and if they are not borne out by observations, then mathematics has necessitated a revision of the underlying general assumptions.

Mach's emphasis on functional connection raises two questions: What are functions? And what is it that functions connect?

Once the logarithm of any positive number has been explained as the exponent to which 10 must be raised to give that number, mathematicians define the logarithmic function by pairing, to every positive number, its logarithm. So, e.g., to 10, they pair 1; to 100, 2; to .1, —1. The *logarithmic function* (the result of this definition) is the class of all pairs of numbers thus obtained—a class including in particular the pairs (10, 1), (100, 2), (.1, —1). More generally, mathematicians say that a function has been defined if, to every number or to every number of a certain kind, a number somehow has been paired. The *function* (the result of this definition) is the class of all pairs of numbers thus obtained.

The traditional answer to the second question is: Functions connect variable quantities or, briefly, *variables*. A thorough analysis has revealed that the term variable is used in several totally discrepant meanings.[6] For instance, it is applied to the letters x and y in the mathematical statement:

(1) $x + \log y = \log y + x$ for any number x and any positive number y. Variables in this sense are used according to the following rules: (a) In the formula, according to the accompanying legend, the letters x and y may be replaced with numerals, say, —3 and 10, each such replacement yielding a specific formula, e.g., $-3 + \log 10 = \log 10 - 3$. (b) Without any change in the meaning of the statement (1), any two unlike letters may be used as variables; e.g., one may write:

$x + \log b = \log b + x$ for any number x and any positive number b; or x and y may even be interchanged as in:

$y + \log x = \log x + y$ for any positive number x and any number y.

The so-called variables that are functionally connected in physical laws, however, are of a completely different nature. Suppose, e.g., that, in the course of a process of a certain type, the work w in joules done by changing the pressure p in atmospheres of a gas

[6] K. Menger, "On Variables in Mathematics and in Natural Science," *British Journal for Philosophy of Science*, V., 1954, pp. 134-142. An elementary presentation of the concepts of variable, function, and fluent is contained in K. Menger, *Calculus. A Modern Approach:* (Boston: Ginn & Co., 1955). See particularly chapters IV and VII.

from its initial value p_0 is connected with p/p_0 by the logarithmic function—in a formula:

(2) $w = \overline{w} \log p/p_0$.

The contrast between p and w in (2) on the one hand, and x and y in (1), on the other, could hardly be greater than it is. (a) The letters p and w must not be replaced with just any two values of pressure and work. For instance, the work done by compressing the helium in one tank on Monday is not in general the logarithm of the pressure of oxygen in another tank on Wednesday. Nor is the formula (2) accompanied by any legend authorizing any replacements. (b) The meaning of (2) changes completely if, instead of w, say, the designation v of the gas volume is introduced in the formula or if p and w are interchanged. (In fact, the formulae thus obtained are, in general, false.) For, whereas x and y in (1) stand for *any* number and do not designate anything specific, p and w do; they designate pressure and work.

But exactly what is gas pressure in atmospheres? One way of defining it is by pairing, to each state S of a gas, the pressure in atmospheres $p(S)$ of the gas in the state S. *Gas pressure* (the result of this definition) is the class of all pairs $(S, p(S))$ thus obtained. Similarly, work in joules is a class of pairs $(S, w(S))$. The traditional formula (2) is nothing but an abbreviation of the following law:

(2_s) $w(S) = \overline{w} \log p(S)/p_0$ for any state S of any gas undergoing a process of the type under consideration.

In a strictly positivistic and operationalist spirit, one would take a step beyond the preceding definition of pressure in atmospheres and define *observed pressure in atmospheres* by pairing, to each act A of reading a pressure gauge calibrated in atmospheres, the number $p^*(A)$ that is read as the result of the act. The result of this definition is the class of all pairs $(A, p^*(A))$ thus obtained.[7] Similarly, one can define w^*, the *observed work in joules* as a class of pairs $(B, w^*(B))$ for any act B of measurement of a certain other kind. The formula (2) then is an abbreviation of the following more articulate formulation of the physical law: (2^*) $w^*(B) = \log p^*(A)/p_0$ for any two acts of reading pressure gauges and work meters, respectively—acts simultaneously directed to the same gas sample undergoing a process of the type under consideration.

According to their definitions, p^*, w^*, p and w are classes of pairs, yet not only, as such, fundamentally different from the variables x and y in (1) but also basically different from functions. For while the latter, e.g., the logarithmic functions, are purely logico-mathematical objects, the definitions of pressure, observed pressure, and the like include references to physical states or observations. The work, which is the logarithm of the pressure, bears to the logarithmic function a relation similar to the relation that a yard, which is equal to three feet, bears to the number three.[8] In order to distinguish objects of scientific studies such as p and p^* both from variables and from functions, the writings quoted in [6] and [8] have revived the term coined by Newton for time, distance traveled, gas pressure, work and the like—the term *fluents*. During the 18th century, not only did this term fall into almost

[7]An extreme positivist might question whether, beyond the *observed* pressure p^*, there is any (so to speak *objective*) pressure p. How, indeed, is the pressure $p(S)$ of a gas in the state S ascertained? It is derived (by more or less arbitrary averaging processes) from values of the observed pressure, namely, by somehow averaging numbers $p^*(A_1), p^*(A_2), \ldots$ that result from acts $A_1, A_2, \ldots$ of reading various guages—acts that various observers direct to the gas in the state S. In the case of helium in the corona of the sun or the terrestial atmosphere a million years ago, the pressure is ascertained by what Bridgman calls pencil-and-paper-operations.

[8]Detailed discussions of these and related points are contained

complete oblivion, being replaced by the word *variable,* but the underlying concept was confused with that of a variable in the logico-mathematical sense of a letter meant to be replaced with any numeral or, more generally, with the designation of any element of a certain class of objects.

The questions raised by Mach's emphasis on functional connections thus can be answered as follows: Functions are certain classes of pairs of numbers. The objects that, in physical laws, are connected by functions are fluents—classes of pairs, each of which results from pairing a number to a physical state or an act of observation.

The fifth and last philosophical point to be mentioned here (which is only briefly touched on at the end of the present book) is Mach's emphasis on the immediate sense data. He called them *elements* and used them as building blocks in constituting *complexes* such as the idea of the various things surrounding us as well as the idea of ourselves. He refused to look for objective causes of the phenomena or data.

"As several ideas imprinted on the senses are observed to accompany each other, they become marked by one name and so to be reputed as one thing. Thus, for example, a certain color, taste, smell, figure and consistence having been observed to go together, are accounted one distinct thing, signified by the name apple. Other collections of ideas constitute a stone, a tree, a book and the like sensible things." This passage, which precisely renders the first part of the contention,

in the book quoted in footnote 6, and in the following articles by the same author: K. Menger, "Mensuration and Other Mathematical Connections of Observable Material," in *Measurement: Definitions and Theories,* ed. C. W. Churchman and P. Ratoosh (New York: Wiley & Sons, 1959), pp. 97-128; and ———, "An Axiomatic Theory of Functions and Fluents," in *The Axiomatic Method* ed. Henkin, Suppes, and Tarski (Amsterdam: North-Holland Publishing Co., 1959), pp. 454-473.

might well have been written by Mach; actually, however, it is a quotation from the very first section of the *Treatise concerning the Principles of Human Knowledge* written by Bishop Berkeley in 1710. In developing these thoughts further, however, Mach diverged altogether from Berkeley, who assumed extra-physical, spiritual causes of sense data in which, because he was a theologian, he was greatly interested. Mach shunned a search for causes of data, in particular, for extra-physical and spiritual causes, and confined himself to the phenomena. His fear of being identified with Berkeley's spiritualism probably explains why Mach did not mention the development of what he called the theory of elements and complexes in the first part of Berkeley's classical *Treatise*—a book that can hardly have escaped Mach's attention.[9] In contrast to Berkeley's idealistic metaphysics, Mach's philosophy of science has often been called *phenomenalism.*

* * *

Mach's operationalist, antimetaphysical, anticausal views and his ideas on economy of thought pervade his presentation of the science of mechanics. Yet an introduction to the present book seems to call less for a discussion of some moot points of Mach's philosophy[10] than for an appraisal of its author as a scientist.

Mach repeatedly emphasized that admiration for a great physicist of the past should not keep historians from discussing the master's limitations. In keeping

[9]On the occasion of the 200th anniversary of Berkeley's death, K. R. Popper published "A Note on Berkeley as a Precursor of Mach," *British Journal for Philosophy of Science,* IV, 1953, including an amazing list of quotations from lesser known writings by Berkeley wherein also some of Mach's scientific ideas (connected with the concept of force, the critique of absolute space, time, and motion and with the economy of thought) are clearly anticipated.

[10]Debates have centered on the question whether references to basically unobservable entities can be, and should be, eliminated

with this admonition, our respect for Mach should not prevent us from observing that, in the course of this century, three limitations of Mach himself as a scientist have become apparent.

Even though Mach is generally recognized as one of the principal precursors of the theory of relativity, he himself not only ignored that theory in the editions of the present book[1] that he published after the appearance of Einstein's first paper in 1905, but actually underlined his aloofness. Remarks to that effect are included in the Preface to his book *Principles of Physical Optics*;[2] and his son, Ludwig Mach, quoted the following passage from papers left by his father: "I do not consider the Newtonian principles as completed and perfect; yet, in my old age, I can accept the theory of relativity just as little as I can accept the existence of atoms and other such dogma."

This leads to the second point where the actual development of physics has completely diverged from Mach's views. Mach seems to have been unimpressed by Boltzmann's triumphs in the kinetic theory of gases as well as by Perrin's experiments on Brownian motion; at least, it appears from the quoted passage, he was not sufficiently impressed to attribute physical significance to the assumption of atoms. We can only speculate as to how he would react to science of the mid-twentieth century, which is completely dominated by atomic physics. Would he admit that the phenomena in a Wilson cloud chamber make a granular structure

only from the final statements of a theory or also from all intermediate steps and from the basic assumptions. Other discussions have dealt with the concept of the simplicity of a theory. On questions of this type there is not only disagreement between various scientists, but some of them, in the course of their lives, have changed their own opinions. For instance, Einstein mentions in his autobiography[3] that, while always admiring Mach as a physicist, at a later age he abandoned many of Mach's philosophical views which had impressed and influenced him greatly in his youth.

of matter and electricity almost visible? Would he perhaps, while admitting granular structure, question the precise equality of all grains of like type and point to galaxies, which also have a granular structure without all grains of like type (e.g., all red giants) having precisely equal masses or sizes? How would he react to the discovery of ever more types of elementary particles?

Strangely, Mach, who had such a sharp eye for the difficulties of atomism, did not seem to appreciate that the idea of matter continuously filling space leads to other conceptual problems and perhaps to even greater difficulties. One can imagine advances in the technique of experimentation and observation that will make statements about hitherto unobservable particles verifiable. Certain statements about the behavior of matter continuously filling space, however, are fundamentally unverifiable.

The writer of this Introduction strongly believes that some of the difficulties common to *all* theories of microphysical phenomena have a common cause—the lack of an adequate microgeometry. The current views on geometry are still essentially Euclid's. The various non-Euclidean geometries developed in the 19th century differ from Euclid's geometry only with regard to assumptions such as the parallel postulate which, when applied to nature, are reflected in properties of space *in the large*, wherefore the main domain of application of those geometries is astronomy or cosmology. *In the small*, all the 19th century non-Euclidean geometries are indistinguishable from Euclid's geometry. Any two points are assumed to have an exact distance from one another and (at least if they are not too far apart) to be joined by exactly one straightest line. And even the sum of the angles in any small non-Euclidean triangle is, according to the assumptions, indistinguishable from two right angles. Only rather recent work has abandoned the conception that the same laws are valid or even the same general notions are applicable in the very small that everyone knows from the geometry of larger regions. In fact, the very ideas of numerical distance and of points have been challenged.[11] And this is what microphysics probably needs: a microgeometry built on assumptions that are completely different from Euclid's; a theory of lumps rather than of points, and of distance distributions rather than of exact numerical distances; that is to say, given any two lumps and any interval of numbers, all that can be determined is the probability that the distance between the two lumps belongs to the said interval.

Mach's third limitation lies in his neglect of logic and of the critique of language—even the prelogical critique developed by his contemporary F. Mauthner[12] who, unfortunately, has been sadly neglected not only in his own day but by his successors as well. The way in which a man combines immediate sense data or elements in constituting complexes is profoundly influenced by others: mainly by those who, long since, taught him to speak; then by his teachers and educators; and finally by people with whom he exchanges information and views. He is, in other words, strongly influenced by language and all the wisdom and all the folly which, since time immemorial, his ancestors have stored in that means of communication.

Connected with Mach's alogical orientation are two

[11]See K. Menger, "Theory of Relativity and Geometry," in *Albert Einstein: Philosopher-Scientist*,[3] especially pp. 472-74; ———, "Statistical Metric," *Proceedings National Academy of Science*, XXVIII, 1942, p. 535 et seq.
———, "Probabilistic Geometry," *ibid*, XXXVII, 1951, p. 226.
B. Schweizer and A. Sklar, "Statistical Metric Spaces," *Pacific Journal of Mathematics*, X, 1960, p. 313 et seq.

[12]F. Mauthner, *Beiträge zu einer Kritik der Sprache* (Stuttgart: 1901-2), I, II, III.

other aspects of his work: a certain lack of precision in the formulation of some philosophical ideas and strictly empiristic views on mathematics. Mach considers even arithmetic as entirely based on experience. While in recent times empiristic views in mathematics have been decidedly underemphasized and, therefore, still seem to present unexplored potentialities, they unquestionably are one-sided and in definite need of a complementation by logic, by the logical analysis of language and, perhaps, by some of the ideas advanced by H. Poincaré.[13]

* * *

Of the many scientists who have been influenced by Mach, only Einstein and Bridgman have been mentioned on the preceding pages; of the kindred philosophers, only Avenarius and Poincaré. The 1920's witnessed the constitution of a group of philosophers of science who may be considered as direct successors and continuators of Mach—even in a geographical sense: they taught at the two schools with which Mach had been connected: the Universities of Vienna and Prague.[14] This group has become widely known under the name of *Wiener Kreis or Vienna Circle*.*

In its beginnings, the Vienna Circle was altogether Mach-oriented. The philosopher M. Schlick emphasized[15] that the postulates of a theory are implicit definitions of its basic concepts—an idea which, in the

[13]H. Poincaré, *Science and Hypothesis* (New York: Dover Press, 1952). First French edition, 1912.

[14]Mach, who was born in Turas, Moravia, was professor at the University of Prague from 1867 to 1895, and at the University of Vienna from 1895 until his retirement in 1901. He died near Munich in 1916.

*The writer of this Introduction was a member of the Vienna Circle (remark by the editor of the 6th American Edition, 1960).

[15]M. Schlick, *Allgemeine Erkenntnislehre* (2d ed.; Berlin: Springer, 1923).

field of mechanics, goes back to Mach's use of Newton's Third Law as a definition of mass. In 1927, O. Neurath founded an Ernst Mach Society in Vienna; and the first meeting of the philosophers of science in Prague stood, as P. Frank emphasized, in the sign of Mach.

It was the mathematician H. Hahn who first directed the interest of the Vienna Circle to logic by his detailed presentation of the ideas of B. Russell and the *Principia Mathematica*.[16] In this way, L. Wittgenstein's *Tractatus*[17] became a topic—and in the years 1925-27 the dominant topic—of the discussions in the Circle. The interest shifted from Mach's elements and complexes to the ways of talking about observations and of formulating laws; from the analysis of sensations to the analysis of language.[18]

Even in this respect, and in spite of his limited interest in logic, Mach was a precursor. His anti-metaphysical attitude, exemplified in his views on absolute space and time, anticipated the statement that only verifiable propositions are meaningful or, to put it somewhat less dogmatically, the positivistic postulate that extra-logical propositions should be verifiable. Moreover, long before Wittgenstein and Carnap, Mach had used, if only on the base of common sense and unsystematically, the terms *Scheinprobleme* (pseudo—or apparent problems) and *meaningless questions*. "Refraining from answering questions that have been recognized as meaningless," Mach wrote in the *Analysis of Sensations*, "is by no means resignation. It is, in the presence of the enormous material that may be meaningfully investigated, the only reasonable attitude of scientific investigators." Later, on the basis of linguistic analysis, R. Carnap tried to eradicate pseudo-problems systematically. The philosophy of the Vienna Circle developed into *logical positivism*.

[16] B. Russell, *Introduction to Mathematical Philosophy* (London: 1919); and A. N. Whitehead and B. Russell, *Principia Mathematica* (2d ed.; Cambridge: Cambridge University Press, 1925-27), I, II, III.

[17] L. Wittgenstein, *Tractatus Logico-Philosophicus* (London: 1922).

[18] A more detailed account of this transition is given in R. von Mises, *Positivism. A Study in Human Understanding* (Cambridge: Harvard University Press, 1951).

A description of Mach's influence would be incomplete without a mention of the impact of his ideas in the first decade of this century on philosophers in Russia. That development seems to have culminated in 1908, when an outline of philosophy with empiriocritical contributions by A. Bodgdanov and A. Lunacharsky was published in St. Petersburg. The man who was to shape the future of Russia, however, was opposed to Mach. Even in letters from his exile in Siberia in the first years of this century, Lenin had been critical of "Machism." In 1908, he went to London for extensive studies of the philosophical literature and, as their result, in the following year, he published a violent attack on Mach, Avenarius, Poincaré, and related thinkers in a book *Materialism and Empirio-Criticism. Critical Notes on a Reactionary Philosophy.*[19] Lenin begins by emphasizing the strong similarity of Mach's constitution of the idea of objects with Berkeley's. He then quotes several passages from Mach (e.g., references to the world of which we form pictures) which actually are at variance with Mach's own general views, whose presentation here and there lacks precision. Lenin further criticizes some statements that were also abandoned by logical positivists—two decades later, but on the basis of an analysis of language, whereas Lenin's critique is based on dialectical ma-

[19] An English translation with a Foreword by A. Deborin describing the book's background was published as volume 13 of Lenin's *Collected Works* (New York: International Publishers, 1927).

terialism. Lenin asserts, for instance, that "there is nothing in the world but matter in motion; and matter can not move save in space and time"; and "on the basis of relative conceptions we arrive at the absolute truth"—statements that logical positivists analyzing language find just about as unacceptable as the (it goes without saying, un-Machian) opposite statements that one used to hear from idealistic and relativistic philosophers: "there is nothing but ideas and minds wherein they exist; and ideas cannot exist without having a cause"; and "all truth is relative; there is no absolute truth." Lenin then proceeds to identify Mach's philosophy with Berkeley's idealistic and theological views, from which Mach definitely kept aloof; and he finally condemns the author of the *Science of Mechanics* because of the pietistic utterances of some of Mach's minor followers. In the Soviet Union, Lenin's views on Mach's philosophy became authoritative.

* * *

Mach's life, his son Ludwig once wrote, was dominated by a fundamental impulse toward personal clarity. He was a champion of mass education and progress and always a fearless advocate of truth as he saw it. "I see Mach's greatness," Einstein wrote,[3] "in his incorruptible skepticism and independence." In the prosperous but nationalistic and militaristic atmosphere of Central Europe in the late Victorian and Edwardian era, Mach seems to have felt a strong affinity to the English speaking world. Special ties connected him with Paul Carus and the Edward Hegeler family, who founded the Open Court Publishing Company in La Salle, Illinois. To them Mach dedicated his last book, *The Principles of Physical Optics,* in gratitude for their help in disseminating his ideas. In that dedication Mach expressed the wish that in discussions of

his work mention should be made of their names.

The first English translation of Mach's *Science of Mechanics* was made and published by Open Court in 1893, and it is indeed appropriate that the first book to appear in a new series of quality paperbound books now being published by Open Court is this new edition of Mach's great work on *The Science of Mechanics.*

Karl Menger

Illinois Institute of Technology
Chicago, March, 1960

PREFACE TO THE FIRST GERMAN EDITION

The present volume is not a treatise upon the application of the principles of mechanics. Its aim is to clear up ideas, expose the real significance of the matter, and get rid of metaphysical obscurities. The little mathematics it contains is merely secondary to this purpose.

Mechanics will here be treated, not as a branch of mathematics, but as one of the physical sciences. If the reader's interest is in that side of the subject, if he is curious to know how the principles of mechanics have been ascertained, from what sources they take their origin, and how far they can be regarded as permanent acquisitions, he will find, I hope, in these pages some enlightenment. All this, the positive and physical essence of mechanics, which is of greatest and most general interest for a student of nature, is in existing treatises completely buried and concealed beneath a mass of technical considerations.

The gist and kernel of mechanical ideas has in almost every case grown up in the investigation of very simple and special cases of mechanical processes; and the analysis of the history of the discussions concerning these cases must ever remain the method at once the most effective and the most natural for laying this gist and kernel bare. Indeed, it is not too much to say that it is the only way in which a real comprehen-

sion of the general upshot of mechanics is to be attained.

I have framed my exposition of the subject agreeably to these views. It is perhaps a little long, but, on the other hand, I trust that it is clear. I have not in every case been able to avoid the use of the abbreviated and precise terminology of mathematics. To do so would have been to sacrifice matter to form; for the language of everyday life has not yet grown to be sufficiently accurate for the purposes of so exact a science as mechanics.

The elucidations which I here offer are, in part, substantially contained in my treatise, *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit* (Prague, Calve, 1872). At a later date nearly the same views were expressed by KIRCHHOFF (*Vorlesungen über mathematische Physik: Mechanik*, Leipzig, 1874) and by HELMHOLTZ (*Die Thatsachen in der Wahrnehmung*, Berlin, 1879), and have since become commonplace enough. Still the matter, as I conceive it, does not seem to have been exhausted, and I cannot deem my exposition to be at all superfluous.

In my fundamental conception of the nature of science as Economy of Thought,—a view which I indicated both in the treatise above cited and in my pamphlet, *Die Gestalten der Flüssigkeit* (Prague, Calve, 1872), and which I somewhat more extensively developed in my academical memorial address, *Die ökonomische Natur der physikalischen Forschung* (Vienna, Gerold, 1882)—I no longer stand alone. I have been much gratified to find closely allied ideas developed, in an original manner, by DR. R. AVENARIUS (*Philosophie als Denken der Welt gemäss dem Princip des kleinsten Kraftmasses*, Leipzig, 1876). Regard for the true endeavor of philosophy, that of guiding into one common stream the many rills of knowledge, will not be found wanting in my work, although it takes a determined stand against the encroachments of speculative methods.

The questions here dealt with have occupied me since my earliest youth, when my interest for them was powerfully stimulated by the beautiful introductions of LAGRANGE to the chapters of his *Analytic Mechanics,* as well as by the lucid and lively tract of JOLLY, *Principien der Mechanik* (Stuttgart, 1852). If DÜHRING's estimable work, *Kritische Geschichte der Principien der Mechanik* (Berlin, 1873), did not particularly influence me, it was that at the time of its appearance, my ideas had been not only substantially worked out, but actually published. Nevertheless, the reader will, at least on the negative side, find many points of agreement between Dühring's criticisms and those here expressed.

The new apparatus for the illustration of the subject, here figured and described, were designed entirely by me and constructed by Mr. F. Hajek, the mechanician of the physical institute under my control.

In less immediate connection with the text stand the facsimile reproductions of old originals in my possession. The quaint and naïve traits of the great inquirers, which find in them their expression, have always exerted upon me a refreshing influence in my studies, and I have desired that my readers should share this pleasure with me.

E. MACH.

PRAGUE, May, 1883.

AUTHOR'S PREFACE TO THE TRANSLATION

Having read the proofs of the present translation of my work, *Die Mechanik in ihrer Entwickelung,* I can testify that the publishers have supplied an excellent, accurate, and faithful rendering of it, as their previous translations of essays of mine gave me every reason to expect. My thanks are due to all concerned, and especially to Mr. McCormack, whose intelligent care in the conduct of the translation has led to the discovery of many errors, heretofore overlooked. I may, thus, confidently hope, that the rise and growth of the ideas of the great inquirers, which it was my task to portray, will appear to my new public in distinct and sharp outlines.

E. MACH.

PRAGUE, April 8th, 1893.

PREFACE TO THE SECOND EDITION

In consequence of the kind reception which this book has met with, a very large edition has been exhausted in less than five years. This circumstance and the treatises that have since then appeared of E. Wohlwill, H. Streintz, L. Lange, J. Epstein, F. A. Müller, J. Popper, G. Helm, M. Planck, F. Poske, and others are evidence of the gratifying fact that at the present day questions relating to the theory of cognition are pursued with interest, which twenty years ago scarcely anybody noticed.

As a thoroughgoing revision of my work did not yet seem to me to be expedient, I have restricted myself, so far as the text is concerned, to the correction of typographical errors, and have referred to the works that have appeared since its original publication, as far as possible, in a few appendices.

E. Mach.

Prague, June, 1888.

PREFACE TO THE SEVENTH GERMAN EDITION

When, forty years ago, I first expressed the ideas explained in this book, they found small sympathy, and indeed were often contradicted. Only a few friends, especially Josef Popper, the engineer, were actively interested in these thoughts and encouraged the author. When, two years later, Kirchhoff published his well-known and often-quoted dictum, which even today is hardly correctly interpreted by the majority of physicists, people liked to think that the author of the present work had misunderstood Kirchhoff. I must decline with thanks this, as it were, prophetical misunderstanding as not corresponding either to my faculty of presentiment or to my powers of understanding.

However, the book has reached a seventh German edition, and by means of excellent English, French, Italian, and Russian translations has spread over almost all the world. Gradually some of those who work at this subject, like J. Cox, Hertz, Love, MacGregor, Maggi, H. von Seeliger, and others, gave voice to their agreement. For them, of course, only details in a book meant for a general introduction could be of interest.

In this subject, I could hardly avoid touching upon philosophical, historical, and epistemological questions; and by this the attention of various critics was aroused. I took special joy in the recognition which I found with the philosophers, R. Avenarius, J. Petzoldt, H. Cornelius, and, later, W. Schuppe. The apparently small concessions which philosophers of another tendency, like G. Heymans, P. Natorp, and Aloys Müller, have granted to my characterization of absolute space and absolute time as misconceptions suffice for me; indeed, I do not wish for anything more. I thank Messrs. L. Lange and J. Petzoldt not only for their agreement in certain details, but also for their active and fruitful collaboration. In an historical respect, the criticisms of Emil Wohlwill, whose death, I regret to say, has just been announced to me, were valuable and enlightening, especially on the period of Galileo's youthful work; further, critical remarks of P. Duhem and G. Vailati have also been valuable. I am very grateful to Mr. Philip E. B. Jourdain of Cambridge for his critical notes that unfortunately, for the most part, came too late for inclusion in *this* edition, which was already nearly finished. P. Duhem, O. Hölder, G. Vailati, and P. Volkmann have taken part in the epistemological discussions with vigor, and their remarks have been helpful to me.

At the end of the last century my disquisitions on mechanics fared well as a rule; it may have been felt that the empirico-critical side of this science was the most neglected. But now the Kantian traditions have gained power once more, and again we have the demand for an *a priori* foundation of mechanics. Now, I am indeed of the opinion that all that can be known *a priori* of an empirical domain must become evident to mere logical circumspection only after frequent surveys of this domain, but I do not believe that investigations like those of G. Hamel* do any harm to the subject. Both sides of mechanics, the empirical and the logical side, require investigation. I think that this is expressed clearly enough in my book, although my work is for good reasons turned especially to the empirical side.

I myself—seventy-four years old, and struck down by a grave malady—shall not cause any more revolutions. But I hope for important progress from a young mathematician, Dr. Hugo Dingler, who, judging from his publications,† has proved that he has attained to a free and unprejudiced survey of *both* sides of science.

This edition will be found somewhat more homogeneous than the former ones. Many an ancient dispute which today interests nobody any more is left out and many new things are added. The character of the book has remained the same. With respect to the monstrous conceptions of absolute space and absolute time I can retract nothing. Here I have only shown more clearly than hitherto that Newton indeed spoke much about these things, but throughout made no serious application of them. His fifth corollary‡ contains the only practically usable (probably approximate) *inertial system.*

* "Über Raum, Zeit, und Kraft als apriorische Formen der Mechanik," *Jahresber. der deutschen Mathematiker-Vereinigung,* xviii, 1909; "Über die Grundlagen der Mechanik," *Math. Ann.,* lxvi, 1908.

† *Grenzen und Ziele der Wissenschaft,* 1910; *Die Grundlagen der angewandten Geometrie,* 1911.

‡ *Principia,* 1687, p. 19.

ERNST MACH.

VIENNA, February 5th, 1912.

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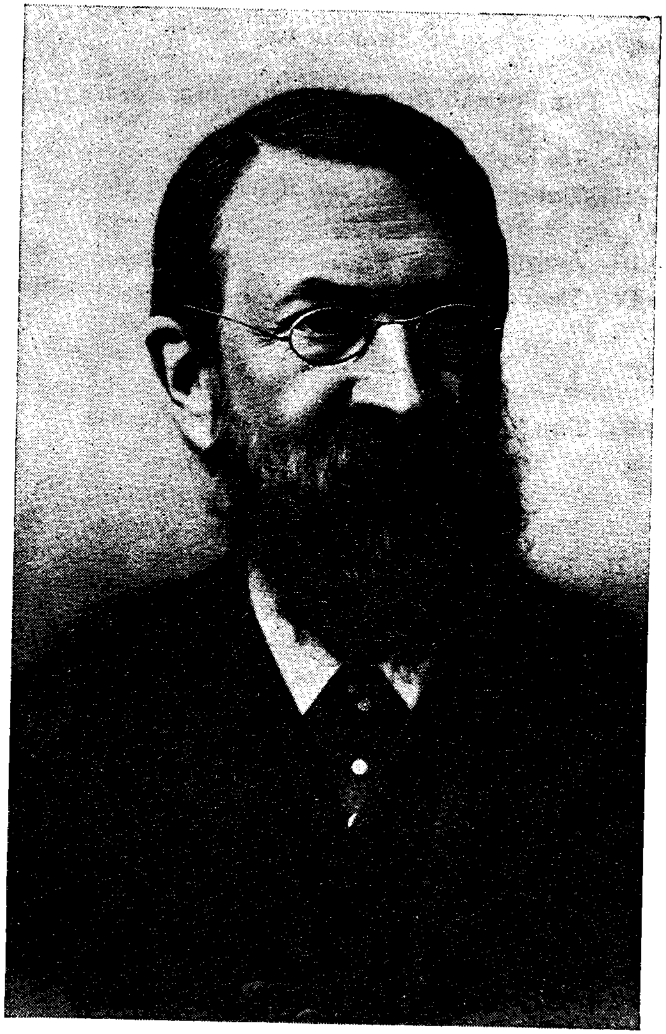

Ernst Mach

INTRODUCTION

1. That branch of physics which is at once the oldest and the simplest and which is therefore treated as introductory to other departments of this science, is concerned with the motions and equilibrium of masses. It bears the name of mechanics.

2. The history of the development of mechanics is quite indispensable to a full comprehension of the science in its present condition. It also affords a simple and instructive example of the processes by which natural science generally is developed.

An *instinctive,* irreflective knowledge of the processes of nature will doubtless always precede the scientific, conscious apprehension, or *investigation,* of phenomena. The former is the outcome of the relation in which the processes of nature stand to the satisfaction of our wants. The acquisition of the most elementary truth does not devolve upon the individual alone: it is pre-effected in the development of the race.

In point of fact, it is necessary to make a distinction between mechanical experience and mechanical science, in the sense in which the latter term is at present employed. Mechanical experiences are, unquestionably, very old. If we carefully examine the ancient Egyptian and Assyrian monuments, we shall find there pictorial representations of many kinds of implements and mechanical contrivances; but accounts of the scientific knowledge of these peoples are either totally lacking, or point conclusively to a very inferior grade of attainment. By the side of highly ingenious appliances, we behold the crudest and roughest expedients employed—as the use of sleds, for instance, for the transportation of enormous blocks of stone. All bears an instinctive, unperfected, accidental character.

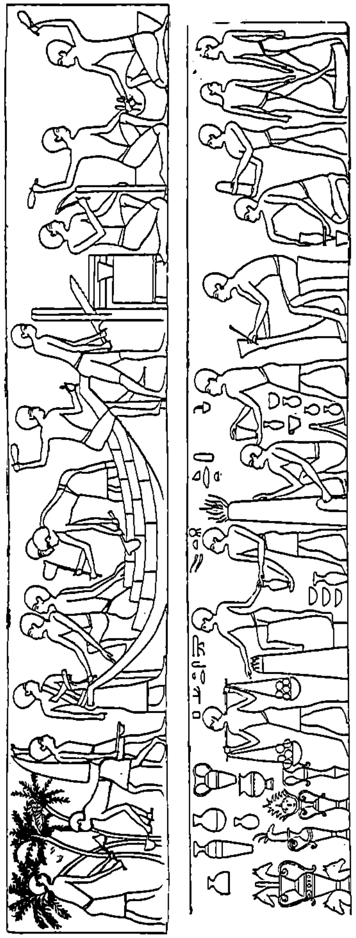

Fig. 1.

So, too, prehistoric graves contain implements whose construction and employment imply no little skill and much mechanical experience. Thus, long before theory was dreamed of, implements, machines, mechanical experiences, and mechanical knowledge were abundant.

3. The idea often suggests itself that perhaps the incomplete accounts we possess have led us to underrate the science of the ancient world. Passages occur in ancient authors which seem to indicate a pro

founder knowledge than we are wont to ascribe to those nations. Take, for instance, the following passage from Vitruvius, *De Architectura,* Lib. V, Cap. III, 6:

"The voice is a flowing breath, made sensible to the organ of hearing by the movements it produces in the air. It is propagated in infinite numbers of circular zones, exactly as when a stone is thrown into a pool of standing water countless circular undulations are generated therein, which, increasing as they recede from the center, spread out over a great distance, unless the narrowness of the locality or some obstacle prevent their reaching their termination; for the first line of waves, when impeded by obstructions, throw by their backward swell the succeeding circular lines of waves into confusion. Conformably to the very same law, the voice also generates circular motions; but with this distinction, that in water the circles remaining upon the surface, are propagated horizontally only, while the voice is propagated both horizontally and vertically."

Does not this sound like the imperfect exposition of a popular author, drawn from more accurate disquisitions now lost? In what a strange light should we ourselves appear, centuries hence, if our popular literature, which by reason of its quantity is less easily destructible, should alone outlive the productions of science? This too favorable view, however, is very rudely shaken by the multitude of other passages containing such crude and patent errors as cannot be conceived to exist in any high stage of scientific culture.

Recent research has contributed greatly to our knowledge of the scientific literature of antiquity, and our opinion of the achievements of the ancient world in science has been correspondingly increased. Schiaparelli has done much to place the work of the Greeks in astronomy in its right light, and Govi has disclosed many precious treasures in his edition of the *Optics* of Ptolemy. The view that the Greeks were especially neglectful of experiment can no longer be maintained unqualifiedly. The most ancient experiments are doubtless those of the Pythagoreans, who employed a monochord with movable bridge for determining the lengths of strings emitting harmonic notes. Anaxagoras's demonstration of the corporeality of the air by means of closed inflated tubes, and that of Empedocles by means of a vessel having its orifice inverted in water (Aristotle, *Physics*) are both primitive experiments. Ptolemy instituted systematic experiments on the refraction of light, while his observations in physiological optics are still full of interest today. Aristotle (*Meteorology*) describes phenomena that go to explain the rainbow. The absurd stories which tend to arouse our mistrust, like that of Pythagoras and the anvil which emitted harmonic notes when struck by hammers of different weights, probably sprang from the fanciful brains of ignorant reporters. Pliny abounds in such vagaries. But they are not, as a matter of fact, a whit more incorrect or nonsensical than the stories of Newton's falling apple and of Watts's tea-kettle. The situation is, moreover, rendered quite intelligible when we consider the difficulties and the expense attending the production of ancient books and their consequent limited circulation. The conditions here involved are concisely discussed by J. Mueller in his paper, "Ueber das Experiment in den physikalischen Studien der Griechen," Naturwiss. Verein zu Innsbruck, XXIII, 1896-1897.

4. When, where, and in what manner the development of science actually began, is at this day difficult historically to determine. It appears reasonable to assume, however, that the instinctive gathering of experiential facts preceded the scientific classification of them. Traces of this process may still be detected in the science of today; indeed, they are to be met with, now and then, in ourselves. The experiments that man heedlessly and instinctively makes in his struggles to satisfy his wants, are just as thoughtlessly and unconsciously applied. Here, for instance, belong the primitive experiments concerning the application of the lever in all its manifold forms. But the things that are thus unthinkingly and instinctively discovered, can never appear as peculiar, can never strike us as surprising, and as a rule therefore will never supply an impetus to further thought.

The transition from this stage to the classified, scientific knowledge and apprehension of facts, first becomes possible on the rise of special classes and professions who make the satisfaction of definite social wants their lifelong vocation. A class of this sort occupies itself with particular kinds of natural processes. The individuals of the class change; old members drop out, and new ones come in. Thus arises a need of imparting to those who are newly come in, the stock of experience and knowledge already possessed; a need of acquainting them with the conditions of the attainment of a definite end so that the result may be determined beforehand. The communication of knowledge is thus the first occasion that compels distinct reflection, as everybody can still observe in himself. Further, that which the old members of a guild mechanically pursue, strikes a new member as unusual and strange, and thus an impulse is given to fresh reflection and investigation.

When we wish to bring to the knowledge of a person any phenomena or processes of nature, we have the choice of two methods: we may allow the person to observe matters for himself, when instruction comes to an end; or, we may describe to him the phenomena in some way, so as to save him the trouble of personally making anew each experiment. Description, however, is only possible of events that constantly recur, or of events that are made up of component parts that constantly recur. That only can be described, and conceptually represented, which is uniform and conformable to law; for description presupposes the employment of names by which to designate its elements; and names can acquire meanings only when applied to elements that constantly reappear.

5. In the infinite variety of nature many ordinary events occur; while others appear uncommon, perplexing, astonishing, or even contradictory to the ordinary run of things. As long as this is the case we do not possess a well-settled and unitary conception of nature. Thence is imposed the task of everywhere seeking out in the natural phenomena those elements that are the same, and that amid all multiplicity are ever present. By this means, on the one hand, the most economical and briefest description and communication are rendered possible; and on the other, when once a person has acquired the skill of recognizing these permanent elements throughout the great-

est range and variety of phenomena, of seeing them in the same, this ability leads to a *comprehensive, compact, consistent*, and *facile conception of the facts*. When once we have reached the point where we are everywhere able to detect the *same* few simple elements, combining in the ordinary manner, then they appear to us as things that are familiar; we are no longer surprised, there is nothing new or strange to us in the phenomena, we feel at home with them, they no longer perplex us, they are *explained*. It is a process of adaptation of thoughts to facts with which we are here concerned.

6. Economy of communication and of apprehension is of the very essence of science. Herein lies its pacificatory, its enlightening, its refining element. Herein, too, we possess an unerring guide to the historical origin of science. In the beginning, all economy had in immediate view the satisfaction simply of bodily wants. With the artisan, and still more so with the investigator, the concisest and simplest possible knowledge of a given province of natural phenomena—a knowledge that is attained with the least intellectual expenditure—naturally becomes in itself an economical aim; but though it was at first a means to an end, when the mental motives connected therewith are once developed and demand their satisfaction, all thought of its original purpose, the personal need, disappears.

To find, then, what remains unaltered in the phenomena of nature, to discover the elements thereof and the mode of their interconnection and interdependence—this is the business of physical science. It endeavors, by comprehensive and thorough description, to make the waiting for new experiences unnecessary; it seeks to save us the trouble of experimentation, by making use, for example, of the known interdependence of phenomena, according to which, if one kind of event occurs, we may be sure beforehand that a certain other event will occur. Even in the description itself labor may be saved, by discovering methods of describing the greatest possible number of different objects at once and in the concisest manner. All this will be made clearer by the examination of points of detail than can be done by a general discussion. It is fitting, however, to prepare the way, at this stage, for the most important points of outlook which in the course of our work we shall have occasion to occupy.

7. We now propose to enter more minutely into the subject of our inquiries, and, at the same time, without making the history of mechanics the chief topic of discussion, to consider its historical development so far as this is requisite to an understanding of the present state of mechanical science, and so far as it does not conflict with the unity of treatment of our main subject. Apart from the consideration that we cannot afford to neglect the great incentives that it is in our power to derive from the foremost intellects of all epochs, incentives which taken as a whole are more fruitful than the greatest men of the present day are able to offer, there is no grander, no more intellectually elevating spectacle than that of the utterances of the fundamental investigators in their gigantic power. Possessed as yet of no methods, for these were first created by their labors, and are only rendered comprehensible to us by their performances, they grapple with and subjugate the object of their inquiry, and imprint upon it the forms of conceptual thought. They that know the entire course of the development of science,

will, as a matter of course, judge more freely and more correctly of the significance of any present scientific movement than they, who, limited in their views to the age in which their own lives have been spent, contemplate merely the momentary trend that the course of intellectual events takes at the present moment.

CHAPTER I.

THE DEVELOPMENT OF THE PRINCIPLES OF STATICS.

I.

THE PRINCIPLE OF THE LEVER.

1. The earliest investigations concerning mechanics of which we have any account, the investigations of the ancient Greeks, related to statics, or to the doctrine of equilibrium. After the conquest of Constantinople by the Turks in 1453, when a fresh impulse was imparted to the thought of the Occident by the ancient writings that the fugitive Greeks brought with them, it was likewise investigations in statics, principally evoked by the works of Archimedes, that occupied the foremost investigators of the period.

Researches in mechanics by the Greeks do not begin until a late date, and in no wise keep pace with the rapid advancement of the race in the domain of mathematics, notably in geometry. Reports of mechanical inventions, so far as they relate to the early inquirers, are extremely meager. Archytas, a distinguished citizen of Tarentum (*circa* 400 B. C.), famed as a geometer and for his employment with the problem of the duplication of the cube, devised mechanical instruments for the description of various curves. As an astronomer he taught that the earth was spherical and that it rotated upon its axis once a day. As a mechanician he founded the theory of pulleys. He is also said to have applied geometry to mechanics in a treatise on this latter science, but all information as to details is lacking. We are told, though, by Aulus Gellius (X. 12) that Archytas constructed an automaton consisting of a flying dove of wood and presumably operated by compressed air, which created a great sensation. It is, in fact, characteristic of the early history of mechanics that attention should have been first directed to its practical advantages and to the construction of automata designed to excite wonder in ignorant people.

Even in the days of Ctesibius (285-247 B. C.) and Hero (first century A. D.) the situation had not materially changed. So, too, during the decadence of civilization in the Middle Ages, the same tendency asserts itself. The artificial automata and clocks of this period, the construction of which popular fancy ascribed to the machinations of the Devil, are well known. It was hoped, by imitating life outwardly, to apprehend it from its inward side also. In intimate connection with the resultant misconception of life stands also the singular belief in the possibility of perpetual motion. Only gradually and slowly, and in indistinct forms, did the genuine problems of mechanics loom up before the minds of inquirers. Aristotle's tract, *Mechanical Problems* (German trans. by Poselger, Hanover, 1881) is characteristic in this regard. Aristotle is quite adept in detecting and in formulating problems; he perceived the principle of the parallelogram of motions, and was on the verge of discovering centrifugal force; but in the actual solution of problems he was infelicitous. The entire tract partakes more of the character of a dialectic than of a scientific treatise and rests content with enunciating the "apories," or contradictions, involved in the problems. But the tract upon the whole very well illustrates the intellectual situation that is characteristic of the beginnings of scientific investigation.

"If a thing take place whereof the cause be not apparent, even though it be in accordance with nature, it appears wonderful. . . . Such are the instances in which small things overcome great things, small weights heavy weights, and incidentally all the problems that go by the name of 'mechanical.' . . . To the apories (contradictions) of this character belong those that appertain to the lever. For it appears contrary to reason that a large weight should be set in motion by a small force, particularly when that weight is in addition combined with a larger weight. A weight that cannot be moved without the aid of a lever can be moved easily with that of a lever added. The primordial cause of all this is inherent in the nature of the circle, which is as one should naturally expect: for it is not contrary to reason that something wonderful should proceed out of something else that is wonderful. The combination of contradictory properties, however, into a single unitary product is the most wonderful of all things. Now, the circle is actually composed of just such contradictory properties. For it is generated by a thing that is in motion and by a thing that is stationary at a fixed point."

In a subsequent passage of the same treatise there is a very dim presentiment of the principle of virtual velocities.

Considerations of the kind here adduced give evidence of a capacity for detecting and enunciating problems, but are far from conducting the investigator to their solution.

2. ARCHIMEDES of Syracuse (287-212 B. C.) left

behind him a number of writings, of which several have come down to us in complete form. We will first employ ourselves a moment with his treatise *De Æquiponderantibus,* which contains propositions respecting the lever and the center of gravity.

In this treatise Archimedes starts from the following assumptions, which he regards as self-evident:

a. Magnitudes of equal weight acting at equal distances (from their point of support) are in equilibrium.

b. Magnitudes of equal weight acting at unequal distances (from their point of support) are not in equilibrium, but the one acting at the greater distance sinks.

From these assumptions he deduces the following proposition:

"Commensurable magnitudes are in equilibrium when they are inversely proportional to their distances (from the point of support)."

It would seem as if analysis could hardly go behind these assumptions. However, when we carefully look into the matter, this is not the case.

Imagine (Fig. 2) a bar, the weight of which is neglected. The bar rests on a fulcrum. At equal distances from the fulcrum we append two equal weights. That the two weights, thus circumstanced, are in equilibrium, is the assumption from which Archimedes starts. We might suppose that this was self-evident entirely apart from any experience, according to the so-called principle of sufficient reason; that in view of the symmetry of the entire arrangement there is no reason why rotation should occur in the one direction

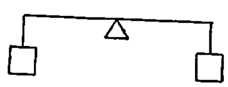

Fig. 2.

rather than in the other. But we forget, in this, that a great multitude of negative and positive experiences is implicitly contained in our assumption; the negative, for instance, that dissimilar colors of the lever-arms, the position of the spectator, an occurrence in the vicinity, and the like, exercise no influence; the positive, on the other hand, (as it appears in the second assumption,) that not only the weights but also their distances from the supporting point are decisive factors in the disturbance of equilibrium, that they also are circumstances determinative of motion. By the aid of these experiences we do indeed perceive that rest (no motion) is the only motion which can be uniquely* determined, or defined, by the determinative conditions of the case.†

Now we are entitled to regard our knowledge of the decisive conditions of any phenomenon as sufficient only in the event that such conditions determine the phenomenon precisely and uniquely. Assuming the fact of experience referred to, that the weights and their distances *alone* are decisive, the first proposition of Archimedes really possesses a high degree of evidence and is eminently qualified to be made the foundation of further investigations. If the spectator place himself in the plane of symmetry of the arrangement in question, the first proposition manifests itself, moreover, as a highly imperative *instinctive* perception—a result determined by the symmetry of our own body. The pursuit of propositions of this character is, furthermore, an excellent means of accustoming ourselves

* So as to leave only a single possibility open.

† If, for example, we were to assume that the weight at the right descended, then rotation in the opposite direction also would be determined by the spectator, whose person exerts no influence on the phenomenon, taking up his position on the opposite side.

in thought to the precision that nature reveals in her processes.

3. We will now reproduce in general outlines the train of thought by which Archimedes endeavors to reduce the general proposition of the lever to the particular and apparently self-evident case. The two equal weights 1 suspended at a and b (Fig. 3) are, if the bar ab be free to rotate about its middle point c, in equilibrium. If the whole be suspended by a cord at c, the cord, leaving out of account the weight of the bar, will have to support the weight 2. The equal weights at the extremities of the bar accordingly replace the double weight at the center.

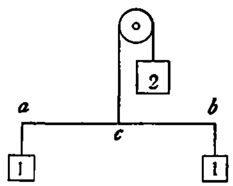

Fig. 3.

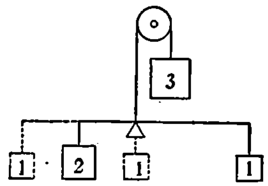

Fig. 4.

On a lever (Fig. 4), the arms of which are in the proportion of 1 to 2, weights are suspended in the proportion of 2 to 1. The weight 2 we imagine replaced by two weights 1, attached on either side at a distance 1 from the point of suspension. Now again we have complete symmetry about the point of suspension, and consequently equilibrium.

On the lever-arms 3 and 4 (Fig. 5) are suspended

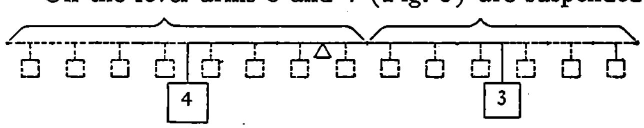

Fig. 5

the weights 4 and 3. The lever-arm 3 is prolonged

the distance 4, the arm 4 is prolonged the distance 3, and the weights 4 and 3 are replaced respectively by 4 and 3 pairs of symmetrically attached weights ½, in the manner indicated in the figure. Now again we have perfect symmetry. The preceding reasoning, which we have here developed with specific figures, is easily generalized.

4. It is of interest to see how Archimedes's mode of view was modified by Stevinus and Galileo.

Galileo imagines (Fig. 6) a heavy horizontal prism, homogeneous in material composition, suspended by its extremities from a homogeneous bar of the same length. The bar is provided at its middle point with a suspensory attachment. In this case equilibrium will obtain; this we perceive at once. But in this case is contained every other case. Galileo shows this in the following manner. Let us suppose the whole length of the bar or the prism to be $2(m + n)$. Cut the prism in two, in such a manner that one portion shall have the length $2m$ and the other the length $2n$. We can effect this without disturbing the equilibrium by previously fastening to the bar by threads, close to the point of proposed section, the inside extremities of the two portions. We may then remove all the threads, if the two portions of the prism be antecedently attached to the bar by their centers. Since the whole length of the bar is $2\ (m + n)$, the length of each half

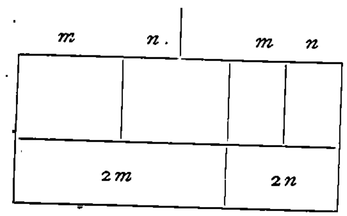

Fig. 6.

is $m + n$. The distance of the point of suspension of the right-hand portion of the prism from the point of suspension of the bar is therefore m, and that of the left-hand portion n. The experience that we have here to deal with the weight, and not with the form, of the bodies, is easily made. It is thus manifest, that equilibrium will still subsist if *any* weight of the magnitude $2m$ be suspended at the distance n on the one side and *any* weight of the magnitude $2n$ be suspended at the distance m on the other. The instinctive elements of our perception of this phenomenon are even more prominently displayed in this form of the deduction than in that of Archimedes.

We may discover, moreover, in this beautiful presentation, a remnant of the ponderousness which was particularly characteristic of the investigators of antiquity.

How a modern physicist conceived the same problem, may be learned from the following presentation of LAGRANGE. He says: Imagine a horizontal homogeneous prism suspended at its center. Let this prism (Fig. 7) be conceived divided into two prisms of the lengths $2m$ and $2n$. If now we consider the

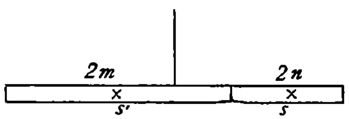

Fig. 7.

centers of gravity of these two parts, at which we may imagine weights to act proportional to $2m$ and $2n$, the two centers thus considered will have the distances n and m from the point of support. This concise disposal of the problem is only possible to the practised mathematical perception.

5. The object that Archimedes and his successors sought to accomplish in the considerations we have here presented, consists in the endeavor to reduce the more complicated case of the lever to the simpler and apparently self-evident case, to *discern* the simpler in the more complicated, or *vice versa*. In fact, we regard a phenomenon as explained, when we discover in it known simpler phenomena.

But surprising as the achievement of Archimedes and his successors may at the first glance appear to us, doubts as to its correctness, on further reflection, nevertheless spring up. From the mere assumption of the equilibrium of equal weights at equal distances is derived the inverse proportionality of weight and lever-arm! How is that possible? If we were unable philosophically and *a priori* to excogitate the simple fact of the dependence of equilibrium on weight and distance, but were obliged to go for *that* result to experience, in how much less a degree shall we be able, by speculative methods, to discover the *form* of this dependence, the proportionality!

As a matter of fact, the assumption that the equilibrium-disturbing effect of a weight P at the distance L from the axis of rotation is measured by the product $P \cdot L$ (the so-called statical moment), is more or less covertly or tacitly introduced by Archimedes and all his successors.

First it is obvious that if the arrangement is absolutely symmetrical in every respect, equilibrium obtains on the assumption of *any* form of dependence whatever of the disturbing factor on L, or, generally, on the assumption $P \cdot f(L)$; and that consequently the *particular* form of dependence PL cannot possibly be inferred from the equilibrium. The fallacy of the deduction must accordingly be sought in the transformation to which the arrangement is subjected. Archimedes makes the action of two equal weights to be the same under all circumstances as that of the combined weights acting at the middle point of their line of junction. But, seeing that he both knows and assumes that distance from the fulcrum is determinative, this procedure is by the premises unpermissible, if the two weights are situated at unequal distances from the fulcrum. If a weight situated at a distance from the fulcrum is divided into two equal parts, and these parts are moved in contrary directions symmetrically to their original point of support; one of the equal weights will be carried as near to the fulcrum as the other weight is carried from it. If it is assumed that the action remains constant during such procedure, then the particular form of dependence of the moment on L is implicitly determined by what has been done, inasmuch as the result is only possible provided the form be PL, or be *proportional* to L. But in such an event all further deduction is superfluous. The entire deduction contains the proposition to be demonstrated, by assumption if not explicitly.

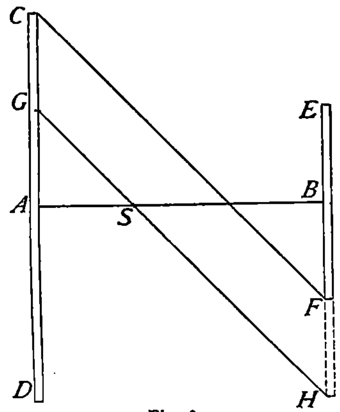

Fig. 8.

6. Huygens, indeed, reprehends this method, and gives a different deduction, in which he believes he

has avoided the error. If in the presentation of Lagrange we imagine the two portions into which the prism is divided turned ninety degrees about two vertical axes passing through the centers of gravity s, s' of the prism-portions (see Fig. 9), and it be shown that under these circumstances equilibrium still continues to subsist, we shall obtain the Huygenian deduction. Abridged and simplified, it is as follows: In a rigid weightless plane (Fig. 9)

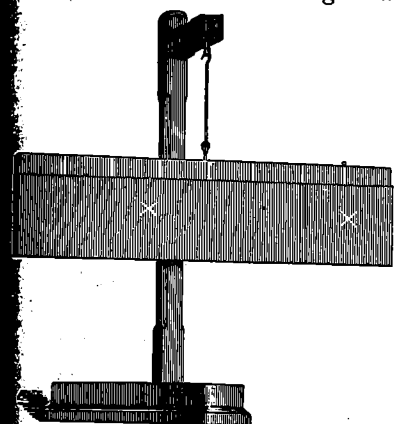
Fig. 9

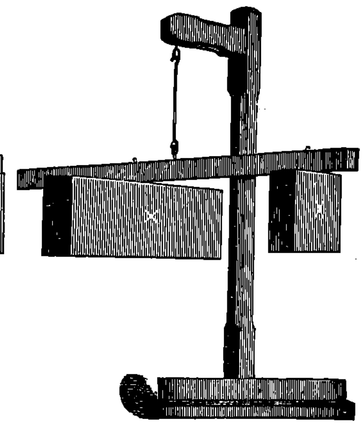
Fig. 9

through the point S we draw a straight line, on which we cut off on the one side the length 1 and on the other the length 2, at A and B respectively. On the extremities, at right angles to this straight line, we place, with the centers as points of contact, the heavy, thin, homogeneous prisms CD and EF, of the lengths and weights 4 and 2. Drawing the straight line HSG (where $AG = \frac{1}{2}AC$) and, parallel to it, the line CF, and translating the prism-portion CG by parallel displacement to FH, everything about the axis GH is symmetrical and equilibrium obtains. But equilibrium also obtains for the axis AB; obtains consequently for every axis through S, and therefore also for that at right angles to AB: wherewith the new case of the lever is given.

Apparently, nothing else is assumed here than that equal weights p, p (Fig. 10) in the same plane and at equal distances, l, l from an axis AA' (in this plane) equilibrate one another. If we place ourselves in the plane passing through AA' perpendicularly to l, l, say

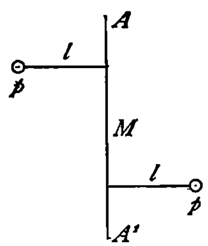

Fig. 10.

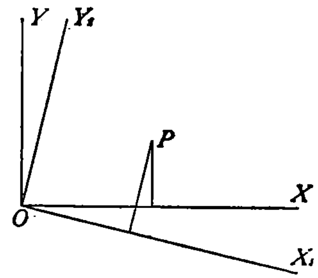

Fig. 11.

at the point M, and look now towards A and now towards A', we shall accord to this proposition the same evidentness as to the first Archimedean proposition. The relation of things is, moreover, not altered if we institute with the weights parallel displacements with respect to the axis, as Huygens in fact does.

The error first arises in the inference: if equilibrium obtains for two axes of the plane, it also obtains for every other axis passing through the point of intersection of the first two. This inference (if it is not to be regarded as a purely instinctive one) can be drawn only upon the condition that disturbant effects are ascribed to the weights *proportional* to their distances from the axis. But in this is contained the very kernel of the doctrine of the lever and the center of gravity.

Let the heavy points of a plane be referred to a system of rectangular coördinates (Fig. 11). The coördinates of the center of gravity of a system of masses $m\, m'\, m'' \ldots$ having the coördinates $x\, x'\, x'' \ldots y\, y'\, y'' \ldots$ are, as we know,

$$\xi = \frac{\Sigma mx}{\Sigma m}, \quad \eta = \frac{\Sigma my}{\Sigma m}.$$

If we turn the system through the angle α, the new coördinates of the masses will be

$$x_1 = x \cos\alpha - y \sin\alpha, \quad y_1 = y \cos\alpha + x \sin\alpha$$

and consequently the coördinates of the center of gravity

$$\xi_1 = \frac{\Sigma m\,(x \cos\alpha - y \sin\alpha)}{\Sigma m} = \cos\alpha \frac{\Sigma mx}{\Sigma m} - \sin\alpha \frac{\Sigma my}{\Sigma m}$$
$$= \xi \cos\alpha - \eta \sin\alpha$$

and, similarly,

$$\eta_1 = \eta \cos\alpha + \xi \sin\alpha.$$

We accordingly obtain the coördinates of the new center of gravity, by simply transforming the coördinates of the first center of the new axes. The center of gravity remains therefore *the self-same* point. If we select the center of gravity itself as origin, then $\Sigma\, m\, x = \Sigma\, m\, y = 0$. On turning the system of axes, this relation continues to subsist. If, accordingly, equilibrium obtains for two axes of a plane that are perpendicular to each other, it also obtains, and obtains then only, for every other axis through their point of intersection. Hence, if equilibrium obtains for any two axes of a plane, it will also obtain for every other axis of the plane that passes through the point of intersection of the two.

These conclusions, however, are not deducible if the coördinates of the center of gravity are determined by some other, more general equation, say

$$\xi = \frac{mf(x) + m'f(x') + m''f(x'') + \ldots}{m + m' + m'' + \ldots}$$

The Huygenian mode of inference, therefore, is inadmissible and contains the very same error that we remarked in the case of Archimedes.

Archimedes's self-deception in this, his endeavor to reduce the complicated case of the lever to the case instinctively grasped, probably consisted in his unconscious employment of studies previously made on the center of gravity *by the help of the very proposition he sought to prove*. It is characteristic, that he will not trust on his own authority, nor perhaps even on that of others, the easily presented observation of the import of the product $P \cdot L$, but searches after a further verification of it.

Now as a matter of fact we shall not, at least at this stage of our progress, attain to any comprehension whatever of the lever unless we directly *discern* in the phenomena the product $P \cdot L$ as the factor decisive of the disturbance of equilibrium. In so far as Archimedes, in his Grecian mania for demonstration, strives to get around this, his deduction is defective. But regarding the import of $P \cdot L$ as given, the Archimedean deductions still retain considerable value, in so far as the modes of conception of different cases are supported the one on the other, in so far as it is shown that one simple case contains all others, in so far as the same mode of conception is established for all cases. Imagine (Fig. 12) a homogeneous prism, whose axis is AB, supported at its center C. To give a graphical representation of the sum of the products of the weights and distances, the sum decisive of the disturbance of

equilibrium, let us erect upon the elements of the axis, which are proportional to the elements of the weight, the distances as ordinates; the ordinates to the right

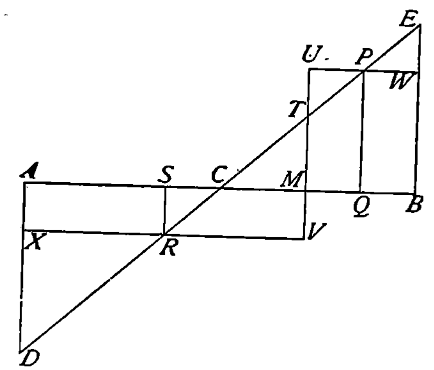

Fig. 12.

of C (as positive) being drawn upwards, and to the left of C (as negative) downwards. The sum of the areas of the two triangles, $ACD + CBE = 0$, illustrates here the subsistence of equilibrium. If we divide the prism into two parts at M, we may substitute the rectangle $MUWB$ for $MTEB$, and the rectangle $MVXA$ for $TMCAD$, where $TP = \frac{1}{2}TE$ and $TR = \frac{1}{2}TD$, and the prism-sections MB, MA are to be regarded as placed at right angles to AB by rotation about Q and S.

In the direction here indicated the Archimedean view certainly remained a serviceable one even after no one longer entertained any doubt of the significance of the product $P \cdot L$, and after opinion on this point had been established historically and by abundant verification.

Experiments are never absolutely exact, but they at least may lead the inquiring mind to *conjecture* that the key which will clear up the connection of all the facts is contained in the exact metrical expression *PL*. On no other hypothesis are the deductions of Archimedes, Galileo, and the rest intelligible. The required transformations, extensions, and compressions of the prisms may now be carried out with perfect certainty.

A knife edge may be introduced at any point under a prism suspended from its center without disturbing the equilibrium (see Fig. 12a), and several such arrangements may be rigidly combined together so as to form apparently new cases of equilibrium. The conversion and disintegration of the case of equilibrium into several other cases (Galileo) is possible only by taking into account the value of *PL*. I cannot agree with O. Hölder who upholds the correctness of the Archimedean deductions against my criticisms in his essay *Denken und Anschauung in der Geometrie*, although I am greatly pleased with the extent of our agreement as to the nature of the exact sciences and their foundations. It would seem as if Archimedes (*De æquiponderantibus*) regarded it as a general experience that two equal weights may under all circumstances be replaced by one equal to their combined weight at the center (Theorem 5, Corollary 2). In such an event, his long deduction (Theorem 6) would be necessary, for the reason sought follows immediately (see pp. 19, 20). Archimedes's mode of expression is not in favor of this view.

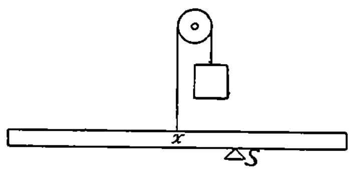

Fig. 12a.

Nevertheless, a theorem of this kind cannot be regarded as *a priori* evident; and the views advanced on pp. 19-20 appear to me to be still uncontroverted.

I must here draw my readers' attention to a beautiful paper by G. Vailati[1], in which the author takes sides with Hölder against my criticism of Archimedes' deduction of the law of the lever but at the same time he criticizes Hölder somewhat. I believe that everyone may read Vailati's exposition with profit and, by comparison with what I have said will be in a position himself to form a judgment upon the points at issue. Vailati shows that Archimedes derives the law of the lever on the basis of general experiences about the center of gravity. I have never disputed the view that such a process is possible and permissible and even very fruitful at a certain stage of investigation, and further, is perhaps the only correct one at that stage. On the contrary, by the manner in which I have exposed the derivations of Stevinus and Galileo, which were made after the example of Archimedes, I have expressly recognized this. But the aim of my whole book is to convince the reader that we cannot make up properties of nature with the help of self-evident suppositions, but that these suppositions must be taken from experience. I would have been false to this aim if I had not striven to disturb the impression that the general law of the lever could be deduced from the equilibrium of equal weights on equal arms. I had, then, to show where the experience, that already contains the general law of the lever, is introduced. Now this experience lies in the supposition emphasized on p. 19, and in the same way it lies in every one of the

[1] La dimonstrazione del principio delle leva data "la Archimede," *Bollettino di bibliografia storia delle scienze matematiche*, May and June 1904.

general and undoubtedly correct theorems on the center of gravity brought forward by Vailati. Now, because the fact that the value of a load is proportional to the arms of the lever is not directly and in the simplest way apparent in such an experience, but is found in an artificial and roundabout way, and is then offered to the surprised reader, the modern reader has to object to the deduction of Archimedes. This deduction from simple and almost self-evident theorems may charm a mathematician who either has an affection for Euclid's method, or who puts himself into the appropriate mood. But in other moods and with other aims we have all the reason in the world to distinguish in value between getting from one proposition to another, and conviction, and between surprise and insight. If the reader has derived some usefulness out of this discussion, I am not very particular about maintaining every word I have used.

7. The manner in which the laws of the lever, as handed down to us from Archimedes in their original simple form, were further generalized and treated by modern physicists, is very interesting and instructive. LEONARDO DA VINCI (1452-1519), the famous painter

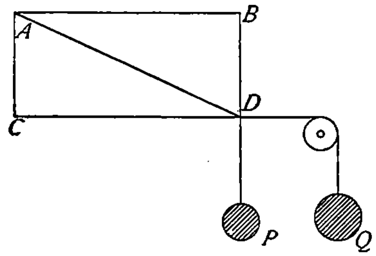

Fig. 13.

and investigator, appears to have been the first to recognize the importance of the general notion of the so-

called statical moments. In the manuscripts he has left us, several passages are found from which this clearly appears. He says, for example: We have a bar *AD* (Fig. 13) free to rotate about *A*, and suspended from the bar, a weight *P*, and suspended from a string which passes over a pulley, a second weight *Q*. What must be the ratio of the forces that equilibrium may obtain? The lever-arm for the weight *P* is not *AD*, but the "potential" lever *AB*. The lever-arm for the weight *Q* is not AD, but the "potential" lever *AC*. The method by which Leonardo arrived at this view is difficult to discover. But it is clear that he recognized the essential circumstances by which the effect of the weight is determined.

Considerations similar to those of Leonardo da Vinci are also found in the writings of GUIDO UBALDI.

8. We will now endeavor to obtain some idea of the way in which the notion of statical moment, by which as we know, is understood the product of a force into the perpendicular let fall from the axis of rotation upon the line of direction of the force, could have been arrived at—although the way that really led to this idea is not now fully ascertainable. That equilibrium exists (Fig. 14) if we lay a cord, subjected at both sides to equal tensions, over a pulley, is perceived without difficulty. We shall always find a plane of symmetry for the apparatus—the plane which stands at right angles to the plane of the cord and bisects (*EE*) the angle made by its two parts. The motion that might be supposed

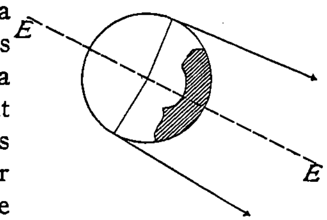

Fig. 14.

possible cannot in this case be precisely determined or defined by any rule whatsoever: no motion will therefore take place. If we note, now, further, that the material of which the pulley is made is essential only to the extent of determining the form of motion of the points of application of the strings, we shall likewise readily perceive that almost any portion of the pulley may be removed without disturbing the equilibrium of the machine. The rigid radii that lead out to the tangential points of the string, are alone essential. We see, thus, that the rigid radii (or the perpendiculars on the linear directions of the strings) play here a part similar to the lever-arms in the lever of Archimedes.

Let us examine a so-called wheel and axle (Fig. 15) of wheel-radius 2 and axle-radius 1, provided respectively with the cord-hung loads 1 and 2; an apparatus which corresponds in every respect to the lever of Archimedes. If now we place about the axle, in any manner we may choose, a second cord, which we subject at each side to the tension of a weight 2, the second cord will not disturb the equilibrium. It is plain, however, that we are also permitted to regard

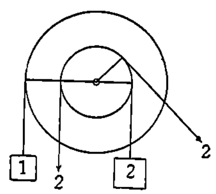

Fig. 15.

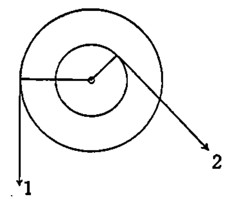

Fig. 16.

the two pulls marked in Fig. 16 as being in equilibrium, by leaving the two others, as mutually destructive, out of account. But we arrive in so doing, dismissing from consideration all unessential features, at

the perception that not only the pulls exerted by the weights but also the perpendiculars let fall from the axis on the lines of the pulls, are conditions determinative of motion. The decisive factors are, then, the products of the weights into the respective perpendiculars let fall from the axis on the directions of the pulls; in other words, the so-called statical moments.

9. What we have so far considered, is the development of our knowledge of the principle of the lever. Quite independently of this was developed the knowledge of the principle of the inclined plane. It is not necessary, however, for the comprehension of the machines, to search after a new principle beyond that of the lever; for the latter is sufficient by itself. Galileo, for example, explains the inclined plane from the lever in the following manner.

We have before us (Fig. 17) an inclined plane, on which rests the weight *Q*, held in equilibrium by the weight *P*. Galileo, now, points out the fact, that it is not requisite that *Q* should lie directly upon the inclined plane, but that the essential point is rather the form, or character, of the motion of *Q*. We may, consequently, conceive the weight attached to the bar *AC*, perpendicular to the inclined plane, and rotatable about *C*. If then we institute a very slight rotation about the point C, the weight will move in the element of an arc coincident with the inclined plane. That the path assumes a curve if the motion be continued is of no consequence here, since this further movement does not in the case of equilib-

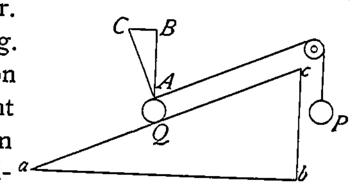

Fig. 17.

rium take place, and the movement of the instant alone is decisive. Reverting, however, to the observation of Leonardo da Vinci, mentioned before, we readily perceive the validity of the theorem $Q \cdot CB = P \cdot CA$ or $Q/P = CA/CB = ca/cb$, and thus reach the law of equilibrium on the inclined plane. Once we have reached the principle of the lever, we may, then, easily apply that principle to the comprehension of the other machines.

II.

THE PRINCIPLE OF THE INCLINED PLANE.

1. Stevinus, or Stevin, (1548-1620) who investigated the mechanical properties of the inclined plane, did so in an eminently original manner. If a weight lie (Fig. 18) on a horizontal table, we perceive at once, since the pressure is directly perpendicular to the plane of the table, by the principle of symmetry, that equilibrium subsists. On a vertical wall, on the other hand, a weight is not at all obstructed in its motion of descent. The inclined plane accordingly will present an intermediate case between these two limiting suppositions. Equilibrium will not exist of itself, as it does on the horizontal support, but it will be maintained by a weight less than that necessary to preserve it on the vertical wall. The ascertainment of the statical law that obtains in this case, caused the earlier inquirers considerable difficulty.

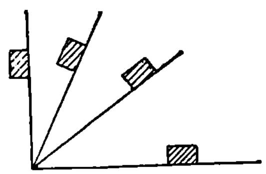

Fig. 18.

Stevinus's manner of procedure is in substance as follows. He imagines a triangular prism with horizontally placed edges, a cross-section of which ABC is

represented in Fig. 19. For the sake of illustration we will say that $AB = 2BC$; also that AC is horizontal. Over this prism Stevinus lays an endless string on which 14 balls of equal weight are strung and tied at equal distances apart. We can advantageously replace this string by an endless uniform chain or cord. The chain will either be in equilibrium or it will not. If we assume the latter to be the case, the chain, since

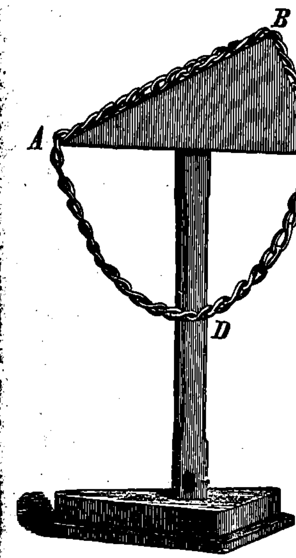

Fig. 19.

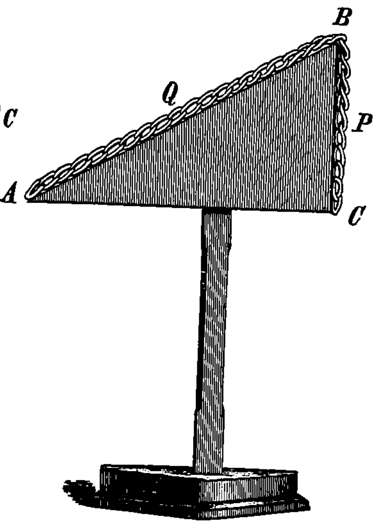

Fig. 20.

the conditions of the event are not altered by its motion, must, when once actually in motion, continue to move forever, that is, it must present a perpetual motion, which Stevinus deems absurd. Consequently only the first case is conceivable. The chain remains in equilibrium. The symmetrical portion ADC may, therefore, without disturbing the equilibrium, be removed.

The portion AB of the chain consequently balances the portion BC. Hence: on inclined planes of equal heights equal weights act in the inverse proportion of the lengths of the planes.

In the cross-section of the prism in Fig. 20 let us imagine AC horizontal, BC vertical, and $AB = 2BC$; furthermore, the chain-weights Q and P on AB and BC proportional to the lengths; it will follow then that $Q/P = AB/BC = 2$. The generalization is self-evident.

2. Unquestionably in the assumption from which Stevinus starts, that the endless chain does not move, there is contained primarily only a *purely instinctive* cognition. He feels at once, and we with him, that we have never observed anything like a motion of the kind referred to, that a thing of such a character does not exist. This conviction has so much logical cogency that we accept the conclusion drawn from it respecting the law of equilibrium on the inclined plane without the thought of an objection, although the law if presented as the simple result of experiment, or otherwise put, would appear dubious. We cannot be surprised at this when we reflect that all results of experiment are obscured by adventitious circumstances (as friction, etc.), and that every conjecture as to the conditions which are determinative in a given case is liable to error. That Stevinus ascribes to instinctive knowledge of this sort a higher authority than to simple, manifest, direct observation might excite in us astonishment if we did not ourselves possess the same inclination. The question accordingly forces itself upon us: Whence does this higher authority come? If we remember that scientific demonstration, and scientific criticism generally can

only have sprung from the consciousness of the individual fallibility of investigators, the explanation is not far to seek. We feel clearly, that we ourselves have contributed *nothing* to the creation of instinctive knowledge, that we have added to it nothing arbitrarily, but that it exists in absolute independence of our participation. Our mistrust of our own subjective interpretation of the facts observed, is thus dissipated.

Stevinus's deduction is one of the rarest fossil indications that we possess in the primitive history of mechanics, and throws a wonderful light on the process of the formation of science generally, on its rise from instinctive knowledge. We will recall to mind that Archimedes pursued exactly the same tendency as Stevinus, only with much less good fortune. In later times, also, instinctive knowledge is very frequently taken as the starting-point of investigations. Every experimenter can daily observe in his own person the guidance that instinctive knowledge furnishes him. If he succeeds in abstractly formulating what is contained in it, he will as a rule have made an important advance in science.

Stevinus's procedure is no error. If an error were contained in it, we should all share it. Indeed, it is perfectly certain, that the union of the strongest instinct with the greatest power of abstract formulation alone constitutes the great natural inquirer. This by no means compels us, however, to create a new mysticism out of the instinctive in science and to regard this factor as infallible. That it is not infallible, we very easily discover. Even instinctive knowledge of so great logical force as the principle of symmetry employed by Archimedes, may lead us astray. Many of my readers will recall, perhaps, the intellectual shock they experienced when they heard for the first time that a magnetic needle lying in the magnetic meridian is deflected in a definite direction away from the meridian by a wire conducting a current being carried along in a parallel direction above it. The instinctive is just as fallible as the distinctly conscious. Its only value is in provinces with which we are very familiar.

Let us rather put to ourselves, in preference to pursuing mystical speculations on this subject, the question: How does instinctive knowledge originate and what are its contents? Everything which we observe in nature imprints itself *uncomprehended* and *unanalyzed* in our percepts and ideas, which, then, in their turn, mimic the processes of nature in their most general and most striking features. In these accumulated experiences we possess a treasure-store which is ever close at hand and of which only the smallest portion is embodied in clear articulate thought. The circumstance that it is far easier to resort to these experiences than it is to nature herself, and that they are, notwithstanding this, free, in the sense indicated, from all subjectivity, invests them with a high value. It is a peculiar property of instinctive knowledge that it is predominantly of a negative nature. We cannot so well say what must happen as we can what cannot happen, since the latter alone stands in glaring contrast to the obscure mass of experience in us in which single characters are not distinguished.

Still, great as the importance of instinctive knowledge may be, for discovery, we must not, from our point of view, rest content with the recognition of its authority. We must inquire, on the contrary: Under what conditions could the instinctive knowledge in

question have originated? We then ordinarily find that the very principle to establish which we had recourse to instinctive knowledge, constitutes in its turn the fundamental condition of the origin of that knowledge. And this is quite obvious and natural. Our instinctive knowledge leads us to the principle which explains that knowledge itself, and which is in its turn also corroborated by the existence of that knowledge, which is a separate fact by itself. This we will find on close examination is the state of things in Stevinus's case.

3. The reasoning of Stevinus impresses us as so highly ingenious because the result at which he arrives apparently contains more than the assumption from which he starts. While on the one hand, to avoid contradictions, we are constrained to let the result pass, on the other, an incentive remains which impels us to seek further insight. If Stevinus had distinctly set forth the entire fact in all its aspects, as Galileo subsequently did, his reasoning would no longer strike us as ingenious; but we should have obtained a much more satisfactory and clear insight into the matter. In the endless chain which does not glide upon the prism, is contained, in fact, everything. We might say, the chain does not glide because no sinking of heavy bodies takes place here. This would not be accurate, however, for when the chain moves many of its links really do descend, while others rise in their place. We must say, therefore, more accurately, the chain does not glide because for every body that could possibly descend an equally heavy body would have to ascend equally high, or a body of double the weight half the height, and so on. This fact was familiar to Stevinus, who likewise presented it, in his theory of pulleys; but he was plainly too distrustful of himself to lay

down the law, without additional support, as also valid for the inclined plane. But if such a law did not exist universally, our instinctive knowledge respecting the endless chain could never have originated. With this our minds are completely enlightened.—The fact that Stevinus did not go as far as this in his reasoning and rested content with bringing his (indirectly discovered) ideas into agreement with his instinctive thought, need not further disturb us.

Stevinus's procedure may be looked at from still another point of view. If it is a fact, for our mechanical instinct, that a heavy endless chain will not rotate, then the individual simple cases of equilibrium on an inclined plane which Stevinus devised and which are readily controlled quantitatively, may be regarded as so many special experiences. For it is not essential that the experiments should have been actually carried out, if the result is beyond question of doubt. As a matter of fact, Stevinus experiments mentally. Stevinus's result could actually have been deduced from the corresponding physical experiments, with friction reduced to a minimum. In an analogous manner, Archimedes's considerations with respect to the lever might be conceived after the fashion of Galileo's procedure. If the various mental experiments had been executed physically, the linear dependence of the static moment on the distance of the weight from the axis could be deduced with perfect rigor. We shall have still many instances to adduce, among the foremost inquirers in the domain of mechanics of this tentative adaptation of special quantitative conceptions to general instinctive impressions. The same phenomena are presented in

other domains also. I may be permitted to refer in this connection to the expositions which I have given in my *Principles of Heat,* page 151. It may be said that the most significant and most important advances in science have been made in this manner. The habit which great inquirers have of bringing their single conceptions into agreement with the general conception or ideal of an entire province of phenomena, their constant consideration of the whole in their treatment of parts, may be characterized as a genuinely philosophical procedure. A truly philosophical treatment of any special science will always consist in bringing the results into relationship and harmony with the established knowledge of the whole. The fanciful extravagances of philosophy, as well as infelicitous and abortive special theories, will be eliminated in this manner.

It will be worth while to review again the points of agreement and difference in the mental procedures of Stevinus and Archimedes. Stevinus reached the very general view that a mobile, heavy, endless chain of any form stays at rest. He is able to deduce from this general view, without difficulty, special cases, which are quantitatively easily controlled. The case from which Archimedes starts, on the other hand, is the most special conceivable. He cannot possibly deduce from his special case in an unassailable manner the behavior which may be expected under more general conditions. If he apparently succeeds in so doing, the reason is that he already knows the result which he is seeking, whilst Stevinus, although he too doubtless knows, approximately at least, what he is in search of, nevertheless could have found it directly by his manner of procedure, even if he had not known it. When the static relationship is rediscovered in such a manner it has a higher value than the result of a metrical experiment would have, which always deviates somewhat from the theoretical truth. The deviation increases with the disturbing circumstances, as with friction, and decreases with the diminution of these difficulties. The exact static relationship is reached by idealization and disregard of these disturbing elements. It appears in the Archimedean and Stevinian procedures as an *hypothesis* without which the individual facts of experience would at once become involved in logical contradictions. Not until we have possessed this hypothesis can we by operating with the exact concepts reconstruct the facts and acquire a scientific and logical mastery of them. The lever and the inclined plane are self-created ideal objects of mechanics. These objects alone completely satisfy the logical demands which we make of them; the physical lever satisfies these conditions only in measure in which it approaches the ideal lever. The natural inquirer strives to *adapt* his *ideals* to reality.

The service which Stevinus renders himself and his readers, consists, therefore, in contrasting and comparing knowledge that is instinctive with knowledge that is clear, in bringing the two into connection and accord with one another, and in supporting one upon the other. The strengthening of perception which Stevinus acquired by this procedure, we learn from the fact that a picture of the endless chain upon the prism graces as vignette, the title-page of his work *Hypomnemata Mathematica* (Leyden, 1605)* with the inscription "Wonder en is gheen wonder." As a fact,

* The title given is that of Willebrord Snell's Latin translation (1608) of Simon Stevin's *Wisconstige Gedachtenissen*, Leyden, 1605—*Trans.*

every enlightening progress made in science is accompanied with a certain feeling of disillusionment. We discover that that which appeared wonderful to us is no more wonderful than other things which we know

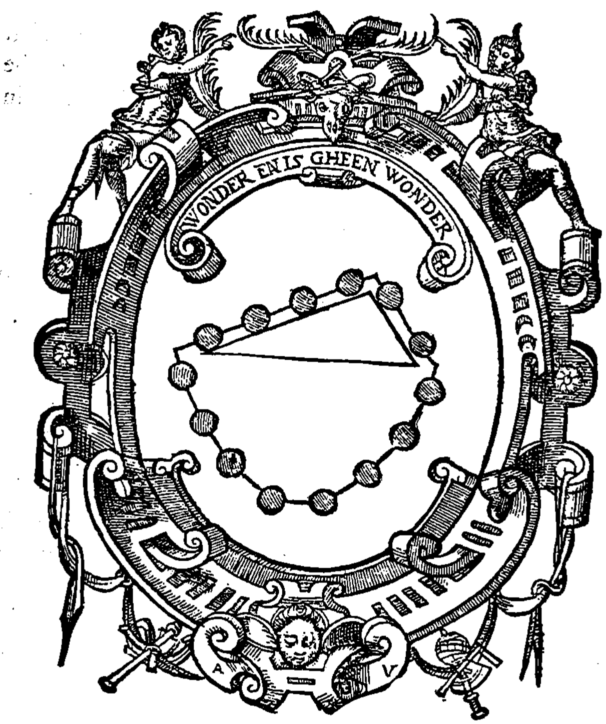

Fig. 21.

instinctively and regard as self-evident; nay, that the contrary would be much more wonderful; that everywhere the same fact expresses itself. Our puzzle turns out then to be a puzzle no more; it vanishes into nothingness, and takes its place among the shadows of history.

4. After he had arrived at the principle of the inclined plane, it was easy for Stevinus to apply that principle to the other machines and thereby to explain their action. He makes, for example, the following application.

We have, let us suppose, an inclined plane (Fig. 22) and on it a load Q. We pass a string over the pulley A at the summit and imagine the load Q held in equilibrium by the load P. Stevinus, now, proceeds by a method similar to that later taken by Galileo. He remarks that it is not necessary that the load Q should lie directly on the inclined plane. Provided

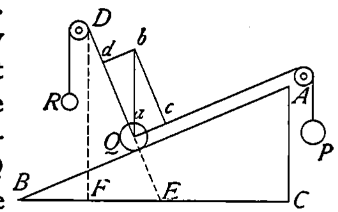

Fig. 22

only the form of the machine's motion be preserved, the proportion between force and load will in all cases remain the same. We may therefore equally well conceive the load Q to be attached to a properly weighted string passing over a pulley D: which string is normal to the inclined plane. If we carry out this alteration, we shall have a so-called funicular machine. We now perceive that we can ascertain very easily the portion of weight with which the body on the inclined plane tends downwards. We have only to draw a vertical line and to cut off on it a portion ab corresponding to the load Q. Then drawing on aA the perpendicular bc, we have $P/Q = AC/AB = ac/ab$. Therefore, ac represents the tension of the string aA. Nothing prevents us, now, from making the two strings change functions and from imagining the load Q to lie on the dotted inclined plane EDF. Similarly, here, we ob-

tain ad for the tension of the second string. In this manner, accordingly, Stevinus indirectly arrives at a knowledge of the statical relations of the funicular machine and of the so-called parallelogram of forces; at first, of course, only for the particular case of strings (or forces) ac, ad at right angles to one another.

Subsequently, indeed, Stevinus employs the principle of the composition and resolution of forces in a more general form; yet the method by which he

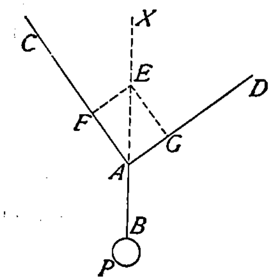

Fig. 23.

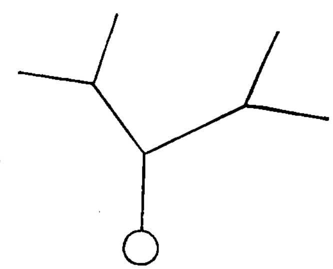
Fig. 24.

reached the principle, is not very clear, or at least is not obvious. He remarks, for example, that if we have three strings AB, AC, AD, stretched at any given angles, and the weight P is suspended from the first, the tensions may be determined in the following manner. We produce (Fig. 23) AB to X and cut off on it a portion AE. Drawing from the point E, EF parallel to AD and EG parallel to AC, the tensions of AB, AC, AD are respectively proportional to AE, AF, AG.

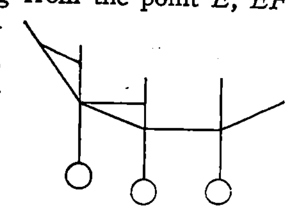
Fig. 25.

With the assistance of this principle of construction Stevinus solves highly complicated problems. He determines, for instance, the tensions of a system of ramifying strings like that illustrated in Fig. 24; in doing which of course he starts from the given tension of the vertical string.

The relations of the tensions of a funicular polygon are likewise ascertained by construction, in the manner indicated in Fig. 25.

We may therefore, by means of the principle of the inclined plane, seek to elucidate the conditions of operation of the other simple machines, in a manner similar to that which we employed in the case of the principle of the lever.

III.

THE PRINCIPLE OF THE COMPOSITION OF FORCES.

1. The principle of the parallelogram of forces, at which STEVINUS arrived and employed, (yet without expressly formulating it) consists, as we know, of the following truth. If a body *A* (Fig. 26) is acted upon by two forces whose directions coincide with the lines *AB* and *AC*, and whose magnitudes are proportional to the lengths *AB* and *AC*, these two forces produce the same effect as a single force, which acts in the direction of the diagonal *AD* of the parallelogram *ABCD* and is proportional to that diagonal. For instance, if on the strings *AB*, *AC* weights exactly proportional to the lengths *AB*, *AC* be supposed to act, a single weight acting on the string *AD* exactly proportional to the length *AD* will produce the same effect as the first two. The forces *AB* and *AC* are called the compo-

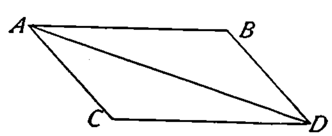

Fig. 26.

nents, the force *AD* the resultant. It is furthermore obvious, that conversely, a *single* force is replaceable by two or several other forces.

2. We shall now endeavor, in connection with the investigations of Stevinus, to give ourselves some idea of the manner in which the general proposition of the parallelogram of forces might have been reached. The relation, discovered by Stevinus, that exists between two mutually perpendicular forces and a third force that equilibrates them, we shall assume as (indirectly) given. We suppose now (Fig. 27) that there act on three strings *OX*, *OY*, *OZ*, pulls which balance each other. Let us endeavor to determine the nature of these pulls. Each pull holds the two remaining ones in equilibrium. The pull *OY* we will replace (following Stevinus's principle) by two new rectangular pulls, one in the direction *Ou* (the prolongation of *OX*), and one at right angles thereto in the direction *Ov*. And let us similarly resolve the pull *OZ* in the directions *Ou* and *Ow*. The sum of the pulls in the direction *Ou*, then, must balance the pull *OX*, and the two pulls in the directions *Ov* and *Ow* must mutually destroy each other. Taking the two latter as equal and opposite, and representing them by *Om* and *On*, we determine coincidently with the operation the com-

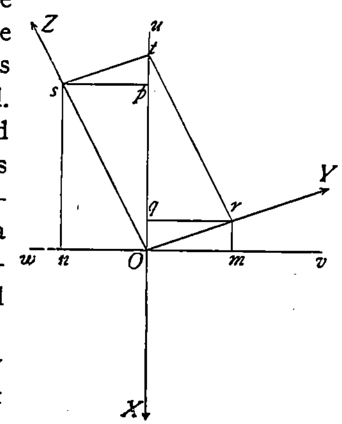

Fig. 27.