SCIENTIFIC BOOKS.

POINCARÉ'S 'SCIENCE AND HYPOTHESIS.'

La Science et l'Hypothèse. Par H. POINCARÉ, Membre de l'Institut. Paris, 1903. Pp. 284.

Wissenschaft und Hypothese. H. POINCARÉ. Autorisierte deutsche Ausgabe, mit erläuterenden Anmerkungen, von F. und L. LINDEMANN. Leipzig, 1904. Pp. xvi + 342; the notes, pp. 245–333.

A work from the pen of one of the distinguished savants who have so recently been the guests of the American scientific public is doubly interesting at the present time. Among the several domains of pure and applied mathematics which M. Poincaré has enriched by his researches, not the least important is that of the fundamental concepts and logical development of various branches of science. Like its predecessors, the work under consideration here is remarkable for the clear, incisive and succinct fashion in which it deals with the difficult and elusive problems lying at the foundation of mathematical knowledge.

The work is divided into four parts, preceded by a short introduction, *viz.:* First Part: 'Number and Magnitude,' pp. 9–48. Second Part: 'Space,' pp. 49–109. Third Part: 'Force,' pp. 110–166. Fourth Part: 'Nature,' pp. 167–281.

The first chapter is entitled, 'On the Nature of the Reasoning of Mathematics.' At the very outset, even the existence of the science of mathematics seems to present an irreconcilable contradiction. If mathematics is deductive, drawing all its conclusions strictly from their antecedent premises, how can it be more than a huge tautology? How are all the ponderous tomes of mathematical theory aught else than devious ways of saying A is A? If, on the other hand, the conclusions of mathematics say more than their antecedent premises, how is the unquestioned perfect rigor of mathematics maintained?

M. Poincaré finds the answer to these questions in the so-called 'mathematical induction' which proceeds from the particular to the more general, but at the same time does so by steps of the highest degree of certitude. In this process he sees the creative force of mathematics, which leads to real proofs and not mere sterile verifications. The illustrations used to make the thought clear are taken from the beginnings of arithmetic, where mathematical thought has remained least elaborated and uncomplicated by the difficult questions related to the notion of space. In successive instances it is shown how more general results are obtained from fundamental definitions and from previous results by means of mathematical induction. In each case the advance is made by virtue of that "power of the mind which knows that it can conceive of the indefinite repetition of the same act as soon as this act is at all possible. The mind has a direct intuition of this power and experience gives only the opportunity to use it and to become conscious of it" (pp. 23–4).

The conviction that the method of mathematical induction is valid our author regards as truly an *à priori* synthetic judgment; the mind can not tolerate nor conceive its contradictory and could not even draw any theoretic consequences from the assumption of the contradictory. No arithmetic could be built up, rejecting the axiom of mathematical induction, as the non-Euclidean geometries have been built up, rejecting the postulate of Euclid.

The second chapter terminates the first part and is entitled, 'Mathematical Magnitude and Experience.' It deals with irrational numbers and the creation of the mathematical continuum, concluding that 'this notion has been created by the mind, but that experience furnished the occasion' (p. 35). "The mind has the power of creating symbols, and by this means it has constructed the mathematical continuum which is merely a particular system of symbols. This power is limited only by the necessity of avoiding contradiction, but the mind makes use of it only when experience furnishes the warrant" (p. 40).

The second part, devoted to 'Space,' consists of chapters on 'The non-Euclidean Geometries,' 'Space and Geometry' and 'Experience and Geometry.'

In this part the fundamental question is: What is the nature of the axioms of geometry? Our author's views may be seen in the following quotations:

The axioms of geometry are neither synthetic judgments à priori, *nor experimental facts.* They are *conventions:* our choice among all possible conventions is *guided* by experimental facts, but it remains *free* and is limited only by the necessity of avoiding all contradiction. Hence the postulates can remain *rigorously* true even though the experimental laws which have determined their adoption are only approximative.

In other words, *the axioms of geometry* (I am not speaking of those of arithmetic) *are merely disguised definitions.* Consequently the question: 'Is Euclidean geometry true?' has no meaning. As well ask whether the metric system is true and the old measures false, whether Cartesian coordinates are true and polar coordinates false. One geometry can not be more true than another, it can only be *more convenient.*

Euclidean geometry is and will remain the most convenient:

1. Because it is the simplest; and it is so not only in consequence of our mental habits, or of I know not what direct intuition we may have of Euclidean space, but it is the simplest in itself, just as a polynomial of the first degree is simpler than one of the second.

2. Because it accords well with the properties of natural solids.

Beings with minds and senses like ours, but who had received no previous education, might receive, from an external world suitably chosen, impressions such that they would be led to construct a geometry other than that of Euclid and to localize the phenomena of that external world in a non-Euclidean space, or even in a space of four dimensions.

If, on the other hand, we whose education has been received in our actual world were suddenly transported into this new world, we should have no difficulty in relating its phenomena to our Euclidean space (pp. 66–8).

If the geometry of Lobatscheffsky is true, the parallax of a very distant star would be finite; if that of Riemann is true, it would be negative. These are results which seem within the reach of experiment, and there have been hopes that astronomical observations might enable us to decide between the three geometries.

But in astronomy 'straight line' means simply 'path of a luminous ray.' If, to suppose the impossible, negative parallaxes were found, or if it were demonstrated that all parallaxes are superior to a certain limit, two courses would be open; either we could renounce Euclidean geometry, or we could modify the laws of optics and admit that light does not travel rigorously in a straight line. It is useless to add, that every one would regard the latter as the more advantageous, Euclidean geometry has nothing to fear from new experiments (p. 93). "No experience will ever contradict the postulate of Euclid, nor will any ever contradict that of Lobatscheffsky" (p. 95).

The third part, devoted to force, consists of chapters dealing with 'Classic mechanics,' 'Relative movement and absolute movement' and 'Energy and thermodynamics.'

Here, as in geometry, our author finds that the fundamental principles are neither *à priori* truths nor experimental facts but convenient definitions or conventions.

If the principle of inertia, for example, were an *à priori* truth, how could the Greeks believe that movement ceases as soon as the cause which originated it ceases to act? How could they believe that every body free from constraint would move in a circle, the noblest of all motions?

Is there any more warrant to say that the velocity of a body can not change without cause for the change, than that it can not change its position or the curvature of its path except under the influence of an exterior cause?

Have any experiments ever been made on a body subject to no force, and if so how was it known that no force was acting? A sphere rolling on a marble table for a very long time is a usual example, but has the force of gravity ceased to act?

Can the law that *the acceleration of a body equals the force acting on it divided by its mass* be verified experimentally? To do so the acceleration, the force and the mass must be measured. If we overlook the difficulties connected with the measurement of time, it may be granted that the acceleration can be measured, but there are inextricable difficulties in the definition of *mass* and *force.* Useful definitions must teach how to *measure* mass and force, and require definition of the *equality* of two forces, and this implies the principle of the equality of action and reaction. "Hence, this principle should no longer be regarded as an experimental law, but as a definition" (p. 122). The result reached is that the 'law of Newton' as to acceleration must be regarded as a definition, in which *mass* is still undefined. "We are driven to the following definition, which is simply an avowal of impotence: *Masses are coefficients which it is convenient to introduce into calculations*" (p. 127).

While the principles of dynamics are definitions, they can be approximately verified by experiment. A more precise experiment would show simply that the law was only approximately true in that case; which we knew already. Thus we see how experience has served as basis for the principles of mechanics and still can never contradict it.

The analogy between geometry and mechanics would at first glance seem complete. In each the fundamental principles are merely conventions which experience has led us to set up as convenient. But there is a difference. The laws of geometry are set up in consequence of experiments in mechanics, in optics, in physiology; they are in no sense experiments in geometry; they do not relate to space (which geometry studies), but to material objects. On the other hand, the fundamental conventions of mechanics and the experiences which show that they are convenient, relate to the very same objects or to analogous objects. This is not an artificial barrier between sciences but a real distinction. The teaching of mechanics should, therefore, remain objective, experimental.

The fourth part, devoted to 'Force,' contains chapters on: 'Hypotheses in physics'; 'The theories of modern physics'; 'The theory of probabilities, optics and electricity,' and 'Thermodynamics.' In this part the relation of observation to hypotheses and generalization is taken up. Experience is the sole source of truth, but one must use his observations; he must generalize. A mere accumulation of facts is no more a science than a pile of stones is a house. Above all, the scientist must foresee. A good experiment teaches more than an isolated fact; it permits us to foresee, *i. e.*, it permits us to generalize. Interpolation is necessary. Experiments give us only a certain number of isolated points; generalization traces a curve. This curve does not pass exactly through all the points given by experiment. We not merely generalize experience, but correct it. Experimental physics furnishes the facts; mathematical physics orders them, makes the generalizations and points out the needs. In this generalization the unity of nature and the simplicity of its laws is presupposed. The curve does not follow all the zigzags indicated by the points given by experiment. Nevertheless, it is not certain that nature is simple, but generalization, and with it science, could not exist if the hypothesis of simplicity were entirely abandoned.

Generalization requires hypotheses. There are three categories of hypothesis: (1) Those which are natural and which can hardly be avoided, as that the influence of very distant bodies is negligible; (2) those that are indifferent, as that matter is continuous or that it is composed of atoms. These indifferent hypotheses are never dangerous, provided their true character is recognized. The hypotheses of the *third* category are true generalizations which experience should either confirm or invalidate.

The hypotheses of physics lead to physical theories which, though apparently well established, are in turn displaced by others. Various examples are discussed.

"No theory seemed more solid than that of Fresnel which attributed light to movements of ether. But now that of Maxwell is preferred. Does this mean that the work of Fresnel was in vain? No, because the real aim of Fresnel was not to find out whether there really is ether, whether it is or is not formed of atoms, whether these atoms really move in this or that sense; his object was to foresee optical phenomena.

Now the theory of Fresnel always permits this, to-day as well as before Maxwell. The differential equations are always true; they can always be integrated by the same procedure and the results always retain their value.

Let no one say that thus we reduce physical theories to the rôle of mere practical recipes; these equations express relations, and if the equations remain true it is because these relations preserve their reality. They teach us, now as then, that there is such a relation between such a thing and such another thing; only this something which formerly we called *movement* we now call *electric current.* But these appellations were only images substituted for the real objects which nature will eternally hide from us. The veritable relations between these real objects are the only reality that we can attain, and the only condition is that the same relations exist between the objects as between the images by which we are forced to replace them. If these relations are known to us, what matter if we deem it convenient to replace one image by another.

That some periodic phenomena (an electric oscillation, for example) is really due to the vibration of some atom which, acting like a pendulum really moves in this or that sense, is neither certain nor interesting. But that between electric oscillation, the movement of the pendulum and all periodic phenomena there exists a close relationship which corresponds to a profound reality; that this relationship, this similitude or rather this parallelism extends into details; that it is a consequence of more general principles, that of energy and that of least action, this is what we can affirm; this is the truth which will always remain the same under all the garbs in which we may deem it useful to deck it out" (pp. 189–191).

Our author has thus discussed the question of the degree of reality in various branches of science from four points of view. In arithmetic we have necessary truth developed *à priori* in the mind; in geometry we have to do with conventions, conveniently related to the material world, but not themselves amenable to direct experimental treatment; in mechanics we have likewise to do with conventions, but they are amenable to direct experiments; while in physical sciences we seek under various images to express relations which are profound realities.

It is impossible to give a summary of a work which is itself so summary. What precedes is an inadequate attempt to present a few characteristic views which may serve to indicate the general spirit of the work and the style of treatment. The larger part of the rich mass of material has necessarily remained untouched.

The work is characterized throughout by masterly clearness and by the skill with which the overgrowth of unessentials and consequences is stripped off and the fundamental idea presented in a few phrases. In its tone, the work addresses the non-scientist. Little technical knowledge is requisite to read it, but still it will hardly prove inviting to those who have not in some way attained a certain facility in following strict reasoning. To these it will furnish an excellent and stimulating discussion of some fundamental principles of modern science apart from the technicalities, while the scientist will welcome this presentation in connected form of carefully thought out views which have already aroused much interest in their earlier publication in various journals.

The work is also remarkable for the ease and directness of its style and for the genial manner in which the illustrative examples are chosen and treated. M. Poincaré is a past master of that most difficult art of giving the central thought of a large theory in a few words without sacrificing lucidity.

It is to be hoped that the work will receive in America that wide and thoughtful reading which it deserves equally on account of the subjects treated and the stimulating originality of the treatment. An English translation of the book and of the notes of Lindemann is a desideratum.

Of the German translation little need be said. It is faithful and quite close, and acquits itself remarkably well of the difficult task of conveying the delicate and precise thoughts of the author into the German tongue. The task was of course much facilitated by the remarkable clearness of the original, in which there is seldom opportunity to question just what is meant, though the domain is one where few can avoid involved ideas and entangling phraseology. The imperative requirement that every shade of meaning be faithfully reproduced effectually restrains the translator from any of those paraphrases which must be permitted if the translation is to conform itself, unhampered, to the genius of the language. In view of these restrictions, the translation seems good, but of course, other things being equal, preference will be given to the original.

A few points of detail may be mentioned: Page 9, lines 3 and 4 should read: '. . . dass er auch für $a = \alpha + 1$ gilt, wenn er für $a = \alpha$ richtig ist.' Lines 8 and 9 analogously.

Page 91, the essential phrase, 'ce qui est expérience, ce qui est raisonnement mathématique' (p. 111 of original) has not been translated.

Page 92, line 2, read 'ist' instead of 'wäre.'

The original, pp. 31 *et seq.*, ascribes to Kronecker that definition of number (as a partition of all rational number into two sets) which is commonly known as Dedekind's. The translation renders all these passages impersonally, and a note calls the presentation of the text Dedekind's, as modified by Tannery.

The notes added to the translation have decided value of their own, and make it desirable either to own both editions or on their account to give the translation the preference. They are to a considerable extent bibliographic, giving excellent lists of references to other works, many of them classic, on the numerous topics which come up. In this respect alone, the notes constitute a welcome and useful supplement to the original work, which makes citations only in the most general way with almost no specific references. But they also develop in many instances mathematical treatment of points touched on in the original, which contains practically no such matter. Frequently the notes state briefly the views of others on the topic in hand, or sketch its historical development, usually with detailed references.

A good index and a fuller table of contents have been added in the German edition.

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THE NEW SEISMOLOGY.*

IN the old seismology the only earthquake tremors studied were those of sensible magnitude, and the records related chiefly to destructive effects. The earliest philosophy of the subject regarded the tremor chiefly as a cause, ascribing to it various geologic results, such as the uplifting of coasts and the eruption of volcanoes; and only by slow degrees did it come to be recognized as an effect, the jar communicated by subterranean rending. The new seismology employs instruments of the most delicate and sensitive character, and by their aid not only detects tremors far too faint for direct perception, but undertakes to measure in absolute terms the amplitude, period and speed of the waves and the intensity of the shocks. Its analysis discriminates earth waves of four different kinds, classifies shocks according to origin as volcanic or tectonic, and by means of its data discusses the physical condition of the earth's interior. In a volume recently issued Dutton sets forth the present condition of the science, sketching its history in outline, describing its instruments and characterizing its progress toward the solution of its more important problems. The treatise is well balanced, compact and as comprehensive as consists with adaptation to the needs of the general reader. Technicalities are avoided so far as practicable, and details are introduced only for the purpose of illustrating principles. While it does not neglect that aspect of the subject which falls within the domain of mechanics, and properly gives a major share of space to the treatment of tremors as elastic waves, it is especially strong in its discussion of the bearing of seismology on geophysics. Fortunately for the geologic as well as the general reader, the author brought to his task not only the experience acquired in monographing the Charleston earthquake, but the mental equipment resulting from prolonged study of volcanism and the greater problems of the inner earth.

The discovered blemishes of the book consist of occasional lapses, either of statement or of correlation between text and illustration. For example, the symbol a (page 175), which stands for the intensity of a shock at unit

* 'Earthquakes in the Light of the New Seismology,' by Clarence Edward Dutton, Major U. S. A. [No. 14 of The Science Series.] New York, G. P. Putman's Sons; London, John Murray, 1904.