

SCIENCE AND HYPOTHESIS

BY

H. POINCARÉ

MEMBER OF THE INSTITUTE OF FRANCE

AUTHORIZED TRANSLATION BY

GEORGE BRUCE HALSTED, PH.D., F.R.A.S.

KENYON COLLEGE, GAMBIER, OHIO

WITH A SPECIAL PREFACE BY

M. POINCARÉ

AND AN INTRODUCTION BY

PROFESSOR JOSIAH ROYCE

HARVARD UNIVERSITY

NEW YORK
THE SCIENCE PRESS

1905

CONTENTS.

	PAGE
Author's Preface to the Translation.....	ix
Introduction by Royce.....	xiv
 Introduction	1
PART I. NUMBER AND MAGNITUDE.	
CHAPTER I.—On the Nature of Mathematical Reasoning	5
Syllogistic Deduction	5
Verification and Proof	6
Elements of Arithmetic	7
Mathematical Induction	10
Reasoning by Recurrence	11
Induction	13
Mathematical Construction	14
 CHAPTER II.—Mathematical Magnitude and Experience	17
Definition of Incommensurables	18
The Physical Continuum	20
Creation of the Mathematical Continuum	20
Measurable Magnitude	23
Various Remarks (Curves without Tangents)	24
The Physical Continuum of Several Dimensions	25
The Mathematical Continuum of Several Dimensions	27
PART II. SPACE.	
 CHAPTER III.—The Non-Euclidean Geometries	29
The Bolyai-Lobachevski Geometry.....	30
Riemann's Geometry	31
The Surfaces of Constant Curvature	32
Interpretation of Non-Euclidean Geometries	33
The Implicit Axioms	34
The Fourth Geometry	36
Lie's Theorem	36
Riemann's Geometries	37
On the Nature of Axioms	37
 CHAPTER IV.—Space and Geometry	40
Geometric Space and Perceptual Space	40
Visual Space	41
Tactile Space and Motor Space	42
Characteristics of Perceptual Space	43
Change of State and Change of Position	44
Conditions of Compensation	46

INTRODUCTION.

BY PROFESSOR JOSIAH ROYCE,

HARVARD UNIVERSITY.

THE treatise of a master needs no commendation through the words of a mere learner. But, since my friend and former fellow student, the translator of this volume, has joined with another of my colleagues, Professor Cattell, in asking me to undertake the task of calling the attention of my fellow students to the importance and to the scope of M. Poincaré's volume, I accept the office, not as one competent to pass judgment upon the book, but simply as a learner, desirous to increase the number of those amongst us who are already interested in the type of researches to which M. Poincaré has so notably contributed.

I.

The branches of inquiry collectively known as the Philosophy of Science have undergone great changes since the appearance of Herbert Spencer's *First Principles*, that volume which a large part of the general public in this country used to regard as the representative compend of all modern wisdom relating to the foundations of scientific knowledge. The summary which M. Poincaré gives, at the outset of his own introduction to the present work, where he states the view which the 'superficial observer' takes of scientific truth, suggests, not indeed Spencer's own most characteristic theories, but something of the spirit in which many disciples of Spencer interpreting their master's formulas used to conceive the position which science occupies in dealing with experience. It was well known to them, indeed, that experience is a constant guide, and an inexhaustible source both of novel scientific results and of unsolved problems; but the fundamental Spencerian principles of science, such as 'the persistence of force,' the 'rhythm of motion' and the rest, were treated by Spencer himself as demonstrably objective, although indeed 'relative' truths, capable of being tested once for all by the 'inconceivability

of the opposite,' and certain to hold true for the whole 'knowable' universe. Thus, whether one dwelt upon the results of such a mathematical procedure as that to which M. Poincaré refers in his opening paragraphs, or whether, like Spencer himself, one applied the 'first principles' to regions of less exact science, this confidence that a certain orthodoxy regarding the principles of science was established forever was characteristic of the followers of the movement in question. Experience, lighted up by reason, seemed to them to have predetermined for all future time certain great theoretical results regarding the real constitution of the 'knowable' cosmos. Whoever doubted this doubted 'the verdict of science.'

Some of us well remember how, when Stallo's 'Principles and Theories of Modern Physics' first appeared, this sense of scientific orthodoxy was shocked amongst many of our American readers and teachers of science. I myself can recall to mind some highly authoritative reviews of that work in which the author was more or less sharply taken to task for his ignorant presumption in speaking with the freedom that he there used regarding such sacred possessions of humanity as the fundamental concepts of physics. That very book, however, has quite lately been translated into German as a valuable contribution to some of the most recent efforts to reconstitute a modern 'philosophy of nature.' And whatever may be otherwise thought of Stallo's critical methods, or of his results, there can be no doubt that, at the present moment, if his book were to appear for the first time, nobody would attempt to discredit the work merely on account of its disposition to be agnostic regarding the objective reality of the concepts of the kinetic theory of gases, or on account of its call for a logical rearrangement of the fundamental concepts of the theory of energy. We are no longer able so easily to know heretics at first sight.

For we now appear to stand in this position: The control of natural phenomena, which through the sciences, men have attained, grows daily vaster and more detailed, and in its details more assured. Phenomena men know and predict better than ever. But regarding the most general theories, and the most fundamental, of science, there is no longer any notable scien-

tific orthodoxy. Thus, as knowledge grows firmer and wider, conceptual construction becomes less rigid. The field of the theoretical philosophy of nature—yes the field of the logic of science—this whole region is to-day an open one. Whoever will work there must indeed accept the verdict of experience regarding what happens in the natural world. So far he is indeed bound. But he may undertake without hindrance from mere tradition the task of trying afresh to reduce what happens to conceptual unity. The circle-squarers and the inventors of devices for perpetual motion are indeed still as unwelcome in scientific company as they were in the days when scientific orthodoxy was more rigidly defined; but that is not because the foundations of geometry are now viewed as completely settled, beyond controversy, nor yet because the ‘persistence of force’ has been finally so defined as to make the ‘opposite inconceivable’ and the doctrine of energy beyond the reach of novel formulations. No, the circle-squarers and the inventors of devices for perpetual motion are to-day discredited, not because of any unorthodoxy of their general philosophy of nature, but because their views regarding special facts and processes stand in conflict with certain equally special results of science which themselves admit of very various general theoretical interpretations. Certain properties of the irrational number π are known, in sufficient multitude to justify the mathematician in declining to listen to the arguments of the circle-squarer; but, despite great advances, and despite the assured results of Dedekind, of Cantor, of Weierstrass and of various others, the general theory of the logic of the numbers, rational and irrational, still presents several important features of great obscurity; and the philosophy of the concepts of geometry yet remains, in several very notable respects, unconquered territory, despite the work of Hilbert and of Pieri, and of our author himself. The ordinary inventors of the perpetual motion machines still stand in conflict with accepted generalizations; but nobody knows as yet what the final form of the theory of energy will be, nor can any one say precisely what place the phenomena of the radioactive bodies will occupy in that theory. The alchemists would not be welcome workers in modern laboratories; yet some sorts of transformation and of evolution of the elements are to-day

matters which theory can find it convenient, upon occasion, to treat as more or less exactly definable possibilities; while some newly observed phenomena tend to indicate, not indeed that the ancient hopes of the alchemists were well founded, but that the ultimate constitution of matter is something more fluent, less invariant, than the theoretical orthodoxy of a recent period supposed. Again, regarding the foundations of biology, a theoretical orthodoxy grows less possible, less definable, less conceivable (even as a hope) the more knowledge advances. Once 'mechanism' and 'vitalism' were mutually contradictory theories regarding the ultimate constitution of living bodies. Now they are obviously becoming more and more 'points of view,' diverse but not necessarily conflicting. So far as you find it convenient to limit your study of vital processes to those phenomena which distinguish living matter from all other natural objects, you may assume, in the modern 'pragmatic' sense, the attitude of a 'neo-vitalist.' So far, however, as you are able to lay stress, with good results, upon the many ways in which the life processes can be assimilated to those studied in physics and in chemistry, you work as if you were a partisan of 'mechanics.' In any case, your special science prospers by reason of the empirical discoveries that you make. And your theories, whatever they are, must not run counter to any positive empirical results. But otherwise, scientific orthodoxy no longer predetermines what alone it is respectable for you to think about the nature of living substance.

This gain in the freedom of theory, coming, as it does, side by side with a constant increase of a positive knowledge of nature, lends itself to various interpretations, and raises various obvious questions.

II.

One of the most natural of these interpretations, one of the most obvious of these questions, may be readily stated. Is not the lesson of all these recent discussions simply this, that general theories are simply vain, that a philosophy of nature is an idle dream, and that the results of science are coextensive with the range of actual empirical observation and of successful prediction? If this is indeed the lesson, then the decline of theoretical orthodoxy in science is—like the eclipse of dogma in religion—

merely a further lesson in pure positivism, another proof that man does best when he limits himself to thinking about what can be found in human experience, and in trying to plan what can be done to make human life more controllable and more reasonable. What we are free to do as we please—is it any longer a serious business? What we are free to think as we please—is it of any further interest to one who is in search of truth? If certain general theories are mere conceptual constructions, which to-day are, and to-morrow are cast into the oven, why dignify them by the name of philosophy? Has science any place for such theories? Why be a ‘neo-vitalist,’ or an ‘evolutionist,’ or an ‘atomist,’ or an ‘Energetiker’? Why not say, plainly: “Such and such phenomena, thus and thus described, have been observed; such and such experiences are to be expected, since the hypotheses by the terms of which we are required to expect them have been verified too often to let us regard the agreement with experience as due merely to chance; so much then with reasonable assurance we know; all else is silence—or else is some matter to be tested by another experiment?” Why not limit our philosophy of science strictly to such a counsel of resignation? Why not substitute, for the old scientific orthodoxy, simply a confession of ignorance, and a resolution to devote ourselves to the business of enlarging the bounds of actual empirical knowledge?

Such comments upon the situation just characterized are frequently made. Unfortunately, they seem not to content the very age whose revolt from the orthodoxy of traditional theory, whose uncertainty about all theoretical formulations, and whose vast wealth of empirical discoveries and of rapidly advancing special researches, would seem most to justify these very comments. Never has there been better reason than there is to-day to be content, if rational man could be content, with a pure positivism. The splendid triumphs of special research in the most various fields, the constant increase in our practical control over nature—these, our positive and growing possessions, stand in glaring contrast to the failure of the scientific orthodoxy of a former period to fix the outlines of an ultimate creed about the nature of the knowable universe. Why not ‘take the cash and let the credit go’? Why pursue the elusive theoretical ‘unification’ any further, when

what we daily get from our sciences is an increasing wealth of detailed information and of practical guidance?

As a fact, however, the known answer of our own age to these very obvious comments is a constant multiplication of new efforts towards large and unifying theories. If theoretical orthodoxy is no longer clearly definable, theoretical construction was never more rife. The history of the doctrine of evolution, even in its most recent phases, when the theoretical uncertainties regarding the 'factors of evolution' are most insisted upon, is full of illustrations of this remarkable union of scepticism in critical work with courage regarding the use of the scientific imagination. The history of those controversies regarding theoretical physics, some of whose principal phases M. Poincaré, in his book, sketches with the hand of the master, is another illustration of the consciousness of the time. Men have their freedom of thought in these regions; and they feel the need of making constant and constructive use of this freedom. And the men who most feel this need are by no means in the majority of cases professional metaphysicians—or students who, like myself, have to view all these controversies amongst the scientific theoreticians from without as learners. These large theoretical constructions are due, on the contrary, in a great many cases to special workers, who have been driven to the freedom of philosophy by the oppression of experience, and who have learned in the conflict with special problems the lesson that they now teach in the form of general ideas regarding the philosophical aspects of science.

Why, then, does science actually need general theories, despite the fact that these theories inevitably alter and pass away? What is the service of a philosophy of science, when it is certain that the philosophy of science which is best suited to the needs of one generation must be superseded by the advancing insight of the next generation? Why must that which endlessly grows, namely, man's knowledge of the phenomenal order of nature, be constantly united in men's minds with that which is certain to decay, namely, the theoretical formulation of special knowledge in more or less completely unified systems of doctrine?

I understand our author's volume to be in the main an answer to this question. To be sure, the compact and manifold teachings

which this text contains relate to a great many different special issues. A student interested in the problems of the philosophy of mathematics, or in the theory of probabilities, or in the nature and office of mathematical physics, or in still other problems belonging to the wide field here discussed, may find what he wants here and there in the text, even in case the general issues which give the volume its unity mean little to him, or even if he differs from the author's views regarding the principal issues of the book. But in the main, this volume must be regarded as what its title indicates—a critique of the nature and place of hypothesis in the work of science and a study of the logical relations of theory and fact. The result of the book is a substantial justification of the scientific utility of theoretical construction—an abandonment of dogma, but a vindication of the rights of the constructive reason.

III.

The most notable of the results of our author's investigation of the logic of scientific theories relates, as I understand his work, to a topic which the present state of logical investigation, just summarized, makes especially important, but which has thus far been very inadequately treated in the text-books of inductive logic. The useful hypotheses of science are of two kinds:.

1. The hypotheses which are valuable *precisely* because they are either verifiable or else refutable through a definite appeal to the tests furnished by experience; and

2. The hypotheses which, despite the fact that experience suggests them, are valuable *despite*, or even *because*, of the fact that experience can *neither* confirm nor refute them. The contrast between these two kinds of hypotheses is a prominent topic of our author's discussion.

Hypotheses of the general type which I have here placed first in order are the ones which the text-books of inductive logic and those summaries of scientific method which are customary in the course of the elementary treatises upon physical science are already accustomed to recognize and to characterize. The value of such hypotheses is indeed undoubted. But hypotheses of the type which I have here named in the second place are far less frequently recognized in a perfectly explicit way as useful aids in

the work of special science. One usually either fails to admit their presence in scientific work, or else remains silent as to the reasons of their usefulness. Our author's treatment of the work of science is therefore especially marked by the fact that he explicitly makes prominent both the existence and the scientific importance of hypotheses of this second type. They occupy in his discussion a place somewhat analogous to each of the two distinct positions occupied by the 'categories' and the 'forms of sensibility,' on the one hand, and by the 'regulative principles of the reason,' on the other hand, in the Kantian theory of our knowledge of nature. That is, these hypotheses which can neither be confirmed nor refuted by experience appear, in M. Poincaré's account, partly (like the conception of 'continuous quantity') as devices of the understanding whereby we give conceptual unity and an invisible connectedness to certain types of phenomenal facts which come to us in a discrete form and in a confused variety; and partly (like the larger organizing concepts of science) as principles regarding the structure of the world in its wholeness; *i. e.*, as principles in the light of which we try to interpret our experience, so as to give to it a totality and an inclusive unity such as Euclidean space, or such as the world of the theory of energy is conceived to possess. Thus viewed, M. Poincaré's logical theory of this second class of hypotheses undertakes to accomplish, with modern means and in the light of to-day's issues, a part of what Kant endeavored to accomplish in his theory of scientific knowledge with the limited means which were at his disposal. Those aspects of science which are determined by the use of the hypotheses of this second kind appear in our author's account as constituting an essential human way of viewing nature, an interpretation rather than a portrayal or a prediction of the objective facts of nature, an adjustment of our conceptions of things to the internal needs of our intelligence, rather than a grasping of things as they are in themselves.

To be sure, M. Poincaré's view, in this portion of his work, obviously differs, meanwhile, from that of Kant, as well as this agrees, in a measure, with the spirit of the Kantian epistemology. I do not mean therefore to class our author as a Kantian. For Kant, the interpretations imposed by the 'forms of sensibility,'

and by the 'categories of the understanding,' upon our doctrine of nature are rigidly predetermined by the unalterable 'form' of our intellectual powers. We 'must' thus view facts, whatever the data of sense must be. This, of course, is not M. Poincaré's view. A similarly rigid predetermination also limits the Kantian 'ideas of the reason' to a certain set of principles whose guidance of the course of our theoretical investigations is indeed only 'regulative,' but is 'a priori,' and so unchangeable. For M. Poincaré, on the contrary, all this adjustment of our interpretations of experience to the needs of our intellect is something far less rigid and unalterable, and is constantly subject to the suggestions of experience. We must indeed interpret in our own way; but our way is itself only relatively determinate; it is essentially more or less plastic; other interpretations of experience are conceivable. Those that we use are merely the ones found to be most convenient. But this convenience is not absolute necessity. Unverifiable and irrefutable hypotheses in science are indeed, in general, indispensable aids to the organization and to the guidance of our interpretation of experience. But it is experience itself which points out to us what lines of interpretation will prove most convenient. Instead of Kant's rigid list of *a priori* 'forms,' we consequently have in M. Poincaré's account a set of conventions, neither wholly subjective and arbitrary, nor yet imposed upon us unambiguously by the external compulsion of experience. The organization of science, so far as this organization is due to hypotheses of the kind here in question, thus resembles that of a constitutional government—neither absolutely necessary, nor yet determined apart from the will of the subjects, nor yet accidental—a free, yet not a capricious establishment of good order, in conformity with empirical needs.

Characteristic remains, however, for our author, as, in his decidedly contrasting way, for Kant, the thought that *without principles which at every stage transcend precise confirmation through such experience as is then accessible the organization of experience is impossible*. Whether one views these principles as conventions or as *a priori* 'forms,' they may therefore be described as hypotheses, but as hypotheses that, while lying at the basis of our actual physical sciences, at once refer to experience and help

us in dealing with experience, and are yet neither confirmed nor refuted by the experiences which we possess or which we can hope to attain.

Three special instances or classes of instances, according to our author's account, may be used as illustrations of this general type of hypotheses. They are: (1) The hypothesis of the existence of continuous extensive *quanta* in nature; (2) The principles of geometry; (3) The principles of mechanics and of the general theory of energy. In case of each of these special types of hypotheses we are at first disposed, apart from reflection, to say that we *find* the world to be thus or thus, so that, for instance, we can confirm the thesis according to which nature contains continuous magnitudes; or can prove or disprove the physical truth of the postulates of Euclidean geometry; or can confirm by definite experience the objective validity of the principles of mechanics. A closer examination reveals, according to our author, the incorrectness of all such opinions. Hypotheses of these various special types are needed; and their usefulness can be empirically shown. They are in touch with experience; and that they are not merely arbitrary conventions is also verifiable. They are not *a priori* necessities; and we can easily conceive intelligent beings whose experience could be best interpreted without using these hypotheses. Yet these hypotheses are *not* subject to direct confirmation or refutation by experience. They stand then in sharp contrast to the scientific hypotheses of the other, and more frequently recognized, type, *i. e.*, to the hypotheses which *can* be tested by a definite appeal to experience. To these other hypotheses our author attaches, of course, great importance. His treatment of them is full of a living appreciation of the significance of empirical investigation. But the central problem of the logic of science thus becomes the problem of the relation between the two fundamentally distinct types of hypotheses, *i. e.*, between those which can not be verified or refuted through experience, and those which can be empirically tested.

IV.

The detailed treatment which M. Poincaré gives to the problem thus defined must be learned from his text. It is no part of my purpose to expound, to defend or to traverse any of his special

conclusions regarding this matter. Yet I can not avoid observing that, while M. Poincaré strictly confines his illustrations and his expressions of opinion to those regions of science wherein, as special investigator, he is himself most at home, the issues which he thus raises regarding the logic of science are of even more critical importance and of more impressive interest when one applies M. Poincaré's methods to the study of the concepts and presuppositions of the organic and of the historical and social sciences, than when one confines one's attention, as our author here does, to the physical sciences. It belongs to the province of an introduction like the present to point out, however briefly and inadequately, that the significance of our author's ideas extends far beyond the scope to which he chooses to confine their discussion.

The historical sciences, and in fact all those sciences such as geology, and such as the evolutionary sciences in general, undertake theoretical constructions which relate to past time. Hypotheses relating to the more or less remote past stand, however, in a position which is very interesting from the point of view of the logic of science. Directly speaking, no such hypothesis is capable of confirmation or of refutation, because we can not return into the past to verify by our own experience what then happened. Yet indirectly, such hypotheses may lead to predictions of coming experience. These latter will be subject to control. Thus, Schliemann's confidence that the legend of Troy had a definite historical foundation led to predictions regarding what certain excavations would reveal. In a sense somewhat different from that which filled Schliemann's enthusiastic mind, these predictions proved verifiable. The result has been a considerable change in the attitude of historians toward the legend of Troy. Geological investigation leads to predictions regarding the order of the strata or the course of mineral veins in a district, regarding the fossils which may be discovered in given formations, and so on. These hypotheses are subject to the control of experience. The various theories of evolutionary doctrine include many hypotheses capable of confirmation and of refutation by empirical tests. Yet, despite all such empirical control, it still remains true that whenever a science is mainly concerned with the remote past, whether this science be archeology, or geology, or anthro-

pology, or Old Testament history, the principal theoretical constructions always include features which no appeal to present or to accessible future experience can ever definitely test. Hence the suspicion with which students of experimental science often regard the theoretical constructions of their confrères of the sciences that deal with the past. The origin of the races of men, of man himself, of life, of species, of the planet; the hypotheses of anthropologists, of archeologists, of students of 'higher criticism'—all these are matters which the men of the laboratory often regard with a general incredulity as belonging not at all to the domain of true science. Yet no one can doubt the importance and the inevitableness of endeavoring to apply scientific method to these regions also. Science needs theories regarding the past history of the world. And no one who looks closer into the methods of these sciences of past time can doubt that verifiable and unverifiable hypotheses are in all these regions inevitably interwoven; so that, while experience is always the guide, the attitude of the investigator towards experience is determined by interests which have to be partially due to what I should call that 'internal meaning,' that human interest in rational theoretical construction which inspires the scientific inquiry; and the theoretical constructions which prevail in such sciences are neither unbiased reports of the actual constitution of an external reality, nor yet arbitrary constructions of fancy. These constructions in fact resemble in a measure those which M. Poincaré in this book has analyzed in the case of geometry. They are constructions molded, but *not* predetermined in their details, by experience. We report facts; we let the facts speak; but we, as we investigate, in the popular phrase, 'talk back' to the facts. We interpret as well as report. Man is not merely made for science, but science is made for man. It expresses his deepest intellectual needs, as well as his careful observations. It is an effort to bring internal meanings into harmony with external verifications. It attempts therefore to control, as well as to submit, to conceive with rational unity, as well as to accept data. Its arts are those directed towards self-possession as well as towards an imitation of the outer reality which we find. It seeks therefore a disciplined freedom of thought. The discipline is as essential as the freedom; but

the latter has also its place. The theories of science are human, as well as objective, internally rational, as well as (when that is possible) subject to external tests.

In a field very different from that of the historical sciences, namely, in a science of observation and of experiment, which is at the same time an organic science, I have been led in the course of some study of the history of certain researches to notice the existence of a theoretical conception which has proved extremely fruitful in guiding research, but which apparently resembles in a measure the type of hypotheses of which M. Poincaré speaks when he characterizes the principles of mechanics and of the theory of energy. I venture to call attention here to this conception, which seems to me to illustrate M. Poincaré's view of the functions of hypothesis in scientific work.

The modern science of pathology is usually regarded as dating from the earlier researches of Virchow, whose 'Cellular Pathology' was the outcome of a very careful and elaborate induction. Virchow, himself, felt a strong aversion to mere speculation. He endeavored to keep close to observation, and to relieve medical science from the control of fantastic theories, such as those of the *Naturphilosophen* had been. Yet Virchow's researches were, as early as 1847, or still earlier, already under the guidance of a theoretical presupposition which he himself states as follows: "We have learned to recognize," he says, "that diseases are not autonomous organisms, that they are no entities that have entered into the body, that they are no parasites which take root in the body, but that *they merely show us the course of the vital processes under altered conditions*" ('dass sie nur Ablauf der Lebenserscheinungen unter veränderten Bedingungen darstellen').

The enormous importance of this theoretical presupposition for all the early successes of modern pathological investigation is generally recognized by the experts. I do not doubt this opinion. It appears to be a commonplace of the history of this science. But in Virchow's later years, this very presupposition seemed to some of his contemporaries to be called in question by the successes of recent bacteriology. The question arose whether the theoretical foundations of Virchow's pathology had not been set aside. And in fact, the theory of the parasitical origin

of a vast number of diseased conditions has indeed come upon an empirical basis to be generally recognized. Yet to the end of his own career, Virchow stoutly maintained that in all its essential significance his own fundamental principle remained quite untouched by the newer discoveries. And, as a fact, this view could indeed be maintained. For if diseases proved to be the consequences of the presence of parasites, the diseases themselves, so far as they belonged to the diseased organism, were still not the parasites, but were, as before, the reaction of the organism to the *veränderte Bedingungen* which the presence of the parasites entailed. So Virchow could well insist. And if the famous principle in question is only stated with sufficient generality, it amounts simply to saying that if a disease involves a change in an organism, and if this change is subject to law at all, then the nature of the organism and the reaction of the organism to whatever it is which causes the disease must be understood in case the disease is to be understood.

For this very reason, however, Virchow's theoretical principle in its most general form *could be neither confirmed nor refuted by experience*. It would remain empirically irrefutable, so far as I can see, even if we should learn that the devil was the true cause of all diseases. For the devil himself would then simply predetermine the *veränderte Bedingungen* to which the diseased organism would be reacting. Let bullets or bacteria, poisons or compressed air, or the devil be the *Bedingungen* to which a diseased organism reacts, the postulate that Virchow states in the passage just quoted will remain irrefutable, if only this postulate be interpreted to meet the case. For the principle in question merely says that whatever entity it may be, bullet, or poison, or devil, that affects the organism, the disease is not that entity, but is the resulting alteration in the process of the organism.

I insist, then, that this principle of Virchow's is no trial supposition, no scientific hypothesis in the narrower sense—capable of being submitted to precise empirical tests. It is, on the contrary, a very precious *leading idea*, a theoretical interpretation of phenomena, in the light of which observations are to be made—'a regulative principle' of research. It is equivalent to a resolution to search for those detailed connections which link

the processes of disease to the normal process of the organism. Such a search undertakes to find the true unity, whatever that may prove to be, wherein the pathological and the normal processes are linked. Now without some such leading idea, the cellular pathology itself could never have been reached; because the empirical facts in question would never have been observed. Hence this principle of Virchow's was indispensable to the growth of his science. Yet it was not a verifiable and not a refutable hypothesis. One value of unverifiable and irrefutable hypotheses of this type lies, then, in the sort of empirical inquiries which they initiate, inspire, organize and guide. In these inquiries hypotheses in the narrower sense, that is, trial propositions which are to be submitted to definite empirical control, are indeed everywhere present. And the use of the other sort of principles lies wholly in their application to experience. Yet without what I have just proposed to call the 'leading ideas' of a science, that is, its principles of an unverifiable and irrefutable character, suggested, but not to be finally tested, by experience, the hypotheses in the narrower sense would lack that guidance which, as M. Poincaré has shown, the larger ideas of science give to empirical investigation.

V.

I have dwelt, no doubt, at too great length upon one aspect only of our author's varied and well-balanced discussion of the problems and concepts of scientific theory. Of the hypotheses in the narrower sense and of the value of direct empirical control, he has also spoken with the authority and the originality which belong to his position. And in dealing with the foundations of mathematics, he has raised one or two questions of great philosophical import into which I have no time, even if I had the right, to enter here. In particular, in speaking of the essence of mathematical reasoning, and of the difficult problem of what makes possible novel results in the field of pure mathematics, M. Poincaré defends a thesis regarding the office of 'demonstration by recurrence'—a thesis which is indeed disputable, which has been disputed and which I myself should be disposed, so far as I at present understand the matter, to modify in some respects, even in accepting the spirit of our author's assertion. Yet there

can be no doubt of the importance of this thesis, and of the fact that it defines a characteristic that is indeed fundamental in a wide range of mathematical research. The philosophical problems that lie at the basis of recurrent proofs and processes are, as I have elsewhere argued, of the most fundamental importance.

These, then, are a few hints relating to the significance of our author's discussion, and a few reasons for hoping that our own students will profit by the reading of the book as those of other nations have already done.

Of the person and of the life-work of our author a few words are here, in conclusion, still in place, addressed, not, to the students of his own science, to whom his position is well known, but to the general reader who may seek guidance in these pages.

Jules Henri Poincaré was born at Nancy, in 1854, the son of a professor in the Faculty of Medicine at Nancy. He studied at the *École Polytechnique* and at the *École des Mines*, became first an engineer, and later received his doctorate in mathematics in 1879. In 1883 he began courses of instruction in mathematics at the *École Polytechnique*; in 1886 received a professorship of mathematical physics in the Faculty of Sciences at Paris; then became member of the Academy of Sciences at Paris, in 1887, and has devoted his life to instruction and investigation in the regions of pure mathematics, of mathematical physics and of celestial mechanics. His list of published treatises relating to various branches of his chosen sciences is long; and his original memoirs have included several momentous investigations, which have gone far to transform more than one branch of research. His presence recently at the International Congress of Arts and Science in St. Louis was one of the most noticeable features of that remarkable gathering of distinguished foreign guests. In Poincaré the reader meets, then, not one who is primarily a speculative student of general problems for their own sake, but an original investigator of the highest rank in several distinct, although interrelated, branches of modern research. The theory of functions—a highly recondite region of pure mathematics—owes to him advances of the first importance, for instance, the definition of a new type of functions. The 'problem of the three bodies,' a famous and fundamental problem of celestial mechanics,

has received from his studies a treatment whose significance has been recognized by the highest authorities. His international reputation has been confirmed by the conferring of more than one important prize for his researches. His membership in the most eminent learned societies of various nations is widely extended; his volumes bearing upon various branches of mathematics and of mathematical physics are used by special students in all parts of the learned world; in brief, he is, as geometer, as analyst and as a theoretical physicist, a leader of his age.

Meanwhile, as contributor to the philosophical discussion of the bases and methods of science, M. Poincaré has long been active. When, in 1893, the admirable *Revue de Métaphysique et de Morale* began to appear, M. Poincaré was soon found amongst the most satisfactory of the contributors to the work of that journal, whose office it has especially been to bring philosophy and the various special sciences (both natural and moral) into a closer mutual understanding. The discussions brought together in the present volume are in large part the outcome of M. Poincaré's contributions to the *Revue de Métaphysique et de Morale*. The reader of M. Poincaré's book is in presence, then, of a great special investigator who is also a philosopher.