

A background image showing a grid of white circular wells, likely from a microplate. Each well contains a dark liquid with several small, glowing spots in various colors (blue, red, green, purple), suggesting a biological or chemical assay. The lighting is dim, highlighting the fluorescence of the spots.

***HENRI
POINCARÉ***

***THE FOUNDATIONS
OF SCIENCE:
SCIENCE AND
HYPOTHESIS,
THE VALUE
OF SCIENCE,
SCIENCE AND
METHOD***

A background image showing a top-down view of several white petri dishes. Each dish contains a dark liquid medium with numerous small, glowing cells. The cells are illuminated with various colors, including blue, green, red, and purple, suggesting fluorescence microscopy. The dishes are arranged in a grid-like pattern, with some in the foreground and others receding into the background.

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POINCARÉ***

***THE FOUNDATIONS
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Henri Poincaré

The Foundations of Science: Science and Hypothesis, The Value of Science, Science and Method

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TABLE OF CONTENTS

SCIENCE AND HYPOTHESIS

AUTHOR'S PREFACE TO THE TRANSLATION

INTRODUCTION

SCIENCE AND HYPOTHESIS

INTRODUCTION

PART I NUMBER AND MAGNITUDE

CHAPTER I

On the Nature of Mathematical Reasoning

CHAPTER II

Mathematical Magnitude and Experience

PART II SPACE

CHAPTER III

The Non-euclidean Geometries

CHAPTER IV

Space and Geometry

CHAPTER V

Experience and Geometry

Ancestral Experience

PART III FORCE

CHAPTER VI

The Classic Mechanics

CHAPTER VII

Relative Motion and Absolute Motion

CHAPTER VIII

Energy and Thermodynamics

General Conclusions on Part Third

PART IV NATURE

CHAPTER IX

Hypotheses in Physics

CHAPTER X

The Theories of Modern Physics

CHAPTER XI

The Calculus of Probabilities

CHAPTER XII

Optics and Electricity

CHAPTER XIII

Electrodynamics

THE VALUE OF SCIENCE

TRANSLATOR'S INTRODUCTION

INTRODUCTION

PART I THE MATHEMATICAL SCIENCES

CHAPTER I

Intuition and Logic in Mathematics

CHAPTER II

The Measure of Time

CHAPTER III

The Notion of Space

CHAPTER IV

Space and its Three Dimensions

PART II THE PHYSICAL SCIENCES

CHAPTER V

Analysis and Physics

CHAPTER VI

Astronomy

CHAPTER VII

The History of Mathematical Physics

CHAPTER VIII

The Present Crisis of Mathematical Physics

CHAPTER IX

The Future of Mathematical Physics

PART III THE OBJECTIVE VALUE OF SCIENCE

CHAPTER X

Is Science Artificial?

CHAPTER XI

Science and Reality

SCIENCE AND METHOD

INTRODUCTION

BOOK I SCIENCE AND THE SCIENTIST

CHAPTER I

The Choice of Facts

CHAPTER II

The Future of Mathematics

Arithmetic

Algebra

Geometry

Cantorism

The Investigation of the Postulates

CHAPTER III

Mathematical Creation

CHAPTER IV

Chance

BOOK II MATHEMATICAL REASONING

CHAPTER I

The Relativity of Space

CHAPTER II

Mathematical Definitions and Teaching

Arithmetic

Geometry

Mechanics

CHAPTER III

Mathematics and Logic

CHAPTER IV

The New Logics

CHAPTER V

The Latest Efforts of the Logisticians

BOOK III THE NEW MECHANICS

CHAPTER I

Mechanics and Radium

CHAPTER II

Mechanics and Optics

CHAPTER III

The New Mechanics and Astronomy

BOOK IV ASTRONOMIC SCIENCE

CHAPTER I

The Milky Way and the Theory of Gases

CHAPTER II

French Geodesy

GENERAL CONCLUSIONS

INDEX

SIR GEORGE DARWIN, worthy son of an immortal father, said, referring to what Poincaré was to him and to his work: "He must be regarded as the presiding genius—or, shall I say, my patron saint?"

Henri Poincaré was born April 29, 1854, at Nancy, where his father was a physician highly respected. His schooling was broken into by the war of 1870-71, to get news of which he learned to read the German newspapers. He outclassed the other boys of his age in all subjects and in 1873 passed highest into the École Polytechnique, where, like John Bolyai at Maros Vásárhely, he followed the courses in mathematics without taking a note and without the syllabus. He proceeded in 1875 to the School of Mines, and was *Nommé*, March 26, 1879. But he won his doctorate in the University of Paris, August 1, 1879, and was appointed to teach in the Faculté des Sciences de Caen, December 1, 1879, whence he was quickly called to the University of Paris, teaching there from October 21, 1881, until his death, July 17, 1912. So it is an error to say he started as an engineer. At the early age of thirty-two he became a member of l'Académie des Sciences, and, March 5, 1908, was chosen Membre de l'Académie Française. July 1, 1909, the number of his writings was 436.

His earliest publication was in 1878, and was not important. Afterward came an essay submitted in competition for the Grand Prix offered in 1880, but it did not win. Suddenly there came a change, a striking fire, a bursting forth, in February, 1881, and Poincaré tells us the very minute it happened. Mounting an omnibus, "at the moment when I put my foot upon the step, the idea came to

me, without anything in my previous thoughts seeming to foreshadow it, that the transformations I had used to define the Fuchsian functions were identical with those of non-Euclidean geometry." Thereby was opened a perspective new and immense. Moreover, the magic wand of his whole life-work had been grasped, the Aladdin's lamp had been rubbed, non-Euclidean geometry, whose necromancy was to open up a new theory of our universe, whose brilliant exposition was commenced in his book *Science and Hypothesis*, which has been translated into six languages and has already had a circulation of over 20,000. The non-Euclidean notion is that of the possibility of alternative laws of nature, which in the Introduction to the *Électricité et Optique*, 1901, is thus put: "If therefore a phenomenon admits of a complete mechanical explanation, it will admit of an infinity of Others which will account equally well for all the peculiarities disclosed by experiment."

The scheme of laws of nature so largely due to Newton is merely one of an infinite number of conceivable rational schemes for helping us master and make experience; it is *commode*, convenient; but perhaps another may be vastly more advantageous. The old conception of *true* has been revised. The first expression of the new idea occurs on the title page of John Bolyai's marvelous *Science Absolute of Space*, in the phrase "haud unquam a priori decidenda."

With bearing on the history of the earth and moon system and the origin of double stars, in formulating the geometric criterion of stability, Poincaré proved the existence of a previously unknown pear-shaped figure, with the possibility that the progressive deformation of this figure

with increasing angular velocity might result in the breaking up of the rotating body into two detached masses. Of his treatise *Les Méthodes nouvelles de la Mécanique céleste*, Sir George Darwin says: "It is probable that for half a century to come it will be the mine from which humbler investigators will excavate their materials." Brilliant was his appreciation of Poincaré in presenting the gold medal of the Royal Astronomical Society. The three others most akin in genius are linked with him by the Sylvester medal of the Royal Society, the Lobachevski medal of the Physico-Mathematical Society of Kazan, and the Bolyai prize of the Hungarian Academy of Sciences. His work must be reckoned with the greatest mathematical achievements of mankind.

The kernel of Poincaré's power lies in an oracle Sylvester often quoted to me as from Hesiod: The whole is less than its part.

He penetrates at once the divine simplicity of the perfectly general case, and thence descends, as from Olympus, to the special concrete earthly particulars.

A combination of seemingly extremely simple analytic and geometric concepts gave necessary general conclusions of immense scope from which sprang a disconcerting wilderness of possible deductions. And so he leaves a noble, fruitful heritage.

Says Love: "His right is recognized now, and it is not likely that future generations will revise the judgment, to rank among the greatest mathematicians of all time."

GEORGE BRUCE HALSTED.

SCIENCE AND HYPOTHESIS

[Table of Contents](#)

AUTHOR'S PREFACE TO THE TRANSLATION

[Table of Contents](#)

I am exceedingly grateful to Dr. Halsted, who has been so good as to present my book to American readers in a translation, clear and faithful.

Every one knows that this savant has already taken the trouble to translate many European treatises and thus has powerfully contributed to make the new continent understand the thought of the old.

Some people love to repeat that Anglo-Saxons have not the same way of thinking as the Latins or as the Germans; that they have quite another way of understanding mathematics or of understanding physics; that this way seems to them superior to all others; that they feel no need of changing it, nor even of knowing the ways of other peoples.

In that they would beyond question be wrong, but I do not believe that is true, or, at least, that is true no longer. For some time the English and Americans have been devoting themselves much more than formerly to the better understanding of what is thought and said on the continent of Europe.

To be sure, each people will preserve its characteristic genius, and it would be a pity if it were otherwise, supposing such a thing possible. If the Anglo-Saxons wished to become Latins, they would never be more than bad Latins; just as the French, in seeking to imitate them, could turn out only pretty poor Anglo-Saxons.

And then the English and Americans have made scientific conquests they alone could have made; they will make still more of which others would be incapable. It would therefore be deplorable if there were no longer Anglo-Saxons.

But continentals have on their part done things an Englishman could not have done, so that there is no need either for wishing all the world Anglo-Saxon.

Each has his characteristic aptitudes, and these aptitudes should be diverse, else would the scientific concert resemble a quartet where every one wanted to play the violin.

And yet it is not bad for the violin to know what the violon-cello is playing, and *vice versa*.

This it is that the English and Americans are comprehending more and more; and from this point of view the translations undertaken by Dr. Halsted are most opportune and timely.

Consider first what concerns the mathematical sciences. It is frequently said the English cultivate them only in view of their applications and even that they despise those who have other aims; that speculations too abstract repel them as savoring of metaphysic.

The English, even in mathematics, are to proceed always from the particular to the general, so that they would never

have an idea of entering mathematics, as do many Germans, by the gate of the theory of aggregates. They are always to hold, so to speak, one foot in the world of the senses, and never burn the bridges keeping them in communication with reality. They thus are to be incapable of comprehending or at least of appreciating certain theories more interesting than utilitarian, such as the non-Euclidean geometries. According to that, the first two parts of this book, on number and space, should seem to them void of all substance and would only baffle them.

But that is not true. And first of all, are they such uncompromising realists as has been said? Are they absolutely refractory, I do not say to metaphysic, but at least to everything metaphysical?

Recall the name of Berkeley, born in Ireland doubtless, but immediately adopted by the English, who marked a natural and necessary stage in the development of English philosophy.

Is this not enough to show they are capable of making ascensions otherwise than in a captive balloon?

And to return to America, is not the *Monist* published at Chicago, that review which even to us seems bold and yet which finds readers?

And in mathematics? Do you think American geometers are concerned only about applications? Far from it. The part of the science they cultivate most devotedly is the theory of groups of substitutions, and under its most abstract form, the farthest removed from the practical.

Moreover, Dr. Halsted gives regularly each year a review of all productions relative to the non-Euclidean geometry,

and he has about him a public deeply interested in his work. He has initiated this public into the ideas of Hilbert, and he has even written an elementary treatise on 'Rational Geometry,' based on the principles of the renowned German savant.

To introduce this principle into teaching is surely this time to burn all bridges of reliance upon sensory intuition, and this is, I confess, a boldness which seems to me almost rashness.

The American public is therefore much better prepared than has been thought for investigating the origin of the notion of space.

Moreover, to analyze this concept is not to sacrifice reality to I know not what phantom. The geometric language is after all only a language. Space is only a word that we have believed a thing. What is the origin of this word and of other words also? What things do they hide? To ask this is permissible; to forbid it would be, on the contrary, to be a dupe of words; it would be to adore a metaphysical idol, like savage peoples who prostrate themselves before a statue of wood without daring to take a look at what is within.

In the study of nature, the contrast between the Anglo-Saxon spirit and the Latin spirit is still greater.

The Latins seek in general to put their thought in mathematical form; the English prefer to express it by a material representation.

Both doubtless rely only on experience for knowing the world; when they happen to go beyond this, they consider their foreknowledge as only provisional, and they hasten to ask its definitive confirmation from nature herself.

But experience is not all, and the savant is not passive; he does not wait for the truth to come and find him, or for a chance meeting to bring him face to face with it. He must go to meet it, and it is for his thinking to reveal to him the way leading thither. For that there is need of an instrument; well, just there begins the difference—the instrument the Latins ordinarily choose is not that preferred by the Anglo-Saxons.

For a Latin, truth can be expressed only by equations; it must obey laws simple, logical, symmetric and fitted to satisfy minds in love with mathematical elegance.

The Anglo-Saxon to depict a phenomenon will first be engrossed in making a *model*, and he will make it with common materials, such as our crude, unaided senses show us them. He also makes a hypothesis, he assumes implicitly that nature, in her finest elements, is the same as in the complicated aggregates which alone are within the reach of our senses. He concludes from the body to the atom.

Both therefore make hypotheses, and this indeed is necessary, since no scientist has ever been able to get on without them. The essential thing is never to make them unconsciously.

From this point of view again, it would be well for these two sorts of physicists to know something of each other; in studying the work of minds so unlike their own, they will immediately recognize that in this work there has been an accumulation of hypotheses.

Doubtless this will not suffice to make them comprehend that they on their part have made just as many; each sees the mote without seeing the beam; but by their criticisms

they will warn their rivals, and it may be supposed these will not fail to render them the same service.

The English procedure often seems to us crude, the analogies they think they discover to us seem at times superficial; they are not sufficiently interlocked, not precise enough; they sometimes permit incoherences, contradictions in terms, which shock a geometric spirit and which the employment of the mathematical method would immediately have put in evidence. But most often it is, on the other hand, very fortunate that they have not perceived these contradictions; else would they have rejected their model and could not have deduced from it the brilliant results they have often made to come out of it.

And then these very contradictions, when they end by perceiving them, have the advantage of showing them the hypothetical character of their conceptions, whereas the mathematical method, by its apparent rigor and inflexible course, often inspires in us a confidence nothing warrants, and prevents our looking about us.

From another point of view, however, the two conceptions are very unlike, and if all must be said, they are very unlike because of a common fault.

The English wish to make the world out of what we see. I mean what we see with the unaided eye, not the microscope, nor that still more subtle microscope, the human head guided by scientific induction.

The Latin wants to make it out of formulas, but these formulas are still the quintessenced expression of what we see. In a word, both would make the unknown out of the

known, and their excuse is that there is no way of doing otherwise.

And yet is this legitimate, if the unknown be the simple and the known the complex?

Shall we not get of the simple a false idea, if we think it like the complex, or worse yet if we strive to make it out of elements which are themselves compounds?

Is not each great advance accomplished precisely the day some one has discovered under the complex aggregate shown by our senses something far more simple, not even resembling it—as when Newton replaced Kepler's three laws by the single law of gravitation, which was something simpler, equivalent, yet unlike?

One is justified in asking if we are not on the eve of just such a revolution or one even more important. Matter seems on the point of losing its mass, its solidest attribute, and resolving itself into electrons. Mechanics must then give place to a broader conception which will explain it, but which it will not explain.

So it was in vain the attempt was made in England to construct the ether by material models, or in France to apply to it the laws of dynamic.

The ether it is, the unknown, which explains matter, the known; matter is incapable of explaining the ether.

POINCARÉ.

INTRODUCTION

[Table of Contents](#)

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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 scientific 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 today 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.



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.



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