KEY WORKS IN THE HISTORY AND PHILOSOPHY
OF LOGIC, MATHEMATICS AND SCIENCE

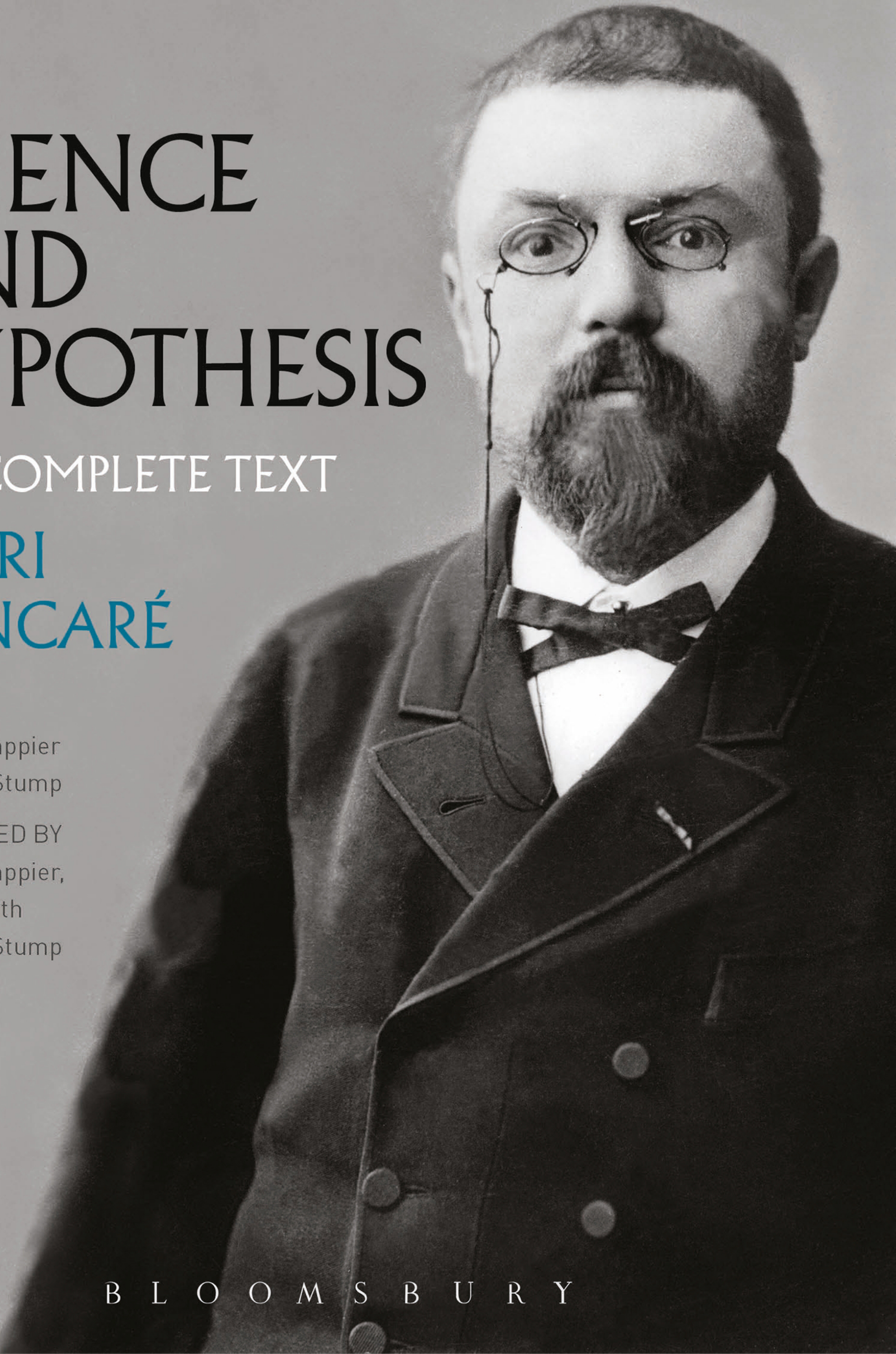
SCIENCE AND HYPOTHESIS

THE COMPLETE TEXT

HENRI
POINCARÉ

EDITED BY
Mélanie Frappier
& David J. Stump

TRANSLATED BY
Mélanie Frappier,
Andrea Smith
& David J. Stump



BLOOMSBURY

Science and Hypothesis

Also available from Bloomsbury

The Bloomsbury Companion to the Philosophy of Science, edited by Steven French and Juha Saatsi

The History and Philosophy of Science: A Reader, edited by Daniel J. McKaughan and Holly VandeWall

Philosophy of Science: Key Concepts, Steven French

Science and Hypothesis

The Complete Text

Henri Poincaré

Translated by Mélanie Frappier, Andrea Smith, and
David J. Stump

Edited by Mélanie Frappier and David J. Stump

BLOOMSBURY ACADEMIC
LONDON • NEW YORK • OXFORD • NEW DELHI • SYDNEY

BLOOMSBURY ACADEMIC
Bloomsbury Publishing Plc
50 Bedford Square, London, WC1B 3DP, UK
1385 Broadway, New York, NY 10018, USA
29 Earlsfort Terrace, Dublin 2, Ireland

BLOOMSBURY, BLOOMSBURY ACADEMIC and the Diana logo
are trademarks of Bloomsbury Publishing Plc

First published 2018

This edition published in 2022

A catalogue record for this book is available from the British Library.

A catalog record for this book is available from the Library of Congress.

ISBN: HB: 978-1-3500-2677-3
PB: 978-1-3503-5557-6
ePDF: 978-1-3500-2675-9
ePub: 978-1-3500-2676-6

Typeset by RefineCatch Limited, Bungay, Suffolk

Contents

Foreword, *David J. Stump* vii
Acknowledgments xxii
Original Sources of the Material in *Science and Hypothesis* xxiii
Author's Preface to the Halsted Translation xxvii

Introduction 1

Part One Number and Magnitude

1 On the Nature of Mathematical Reasoning 7

2 Mathematical Magnitude and Experience 19

Part Two Space

3 Non-Euclidian Geometries 33

4 Space and Geometry 45

5 Experience and Geometry 59

Part Three Force

6 Classical Mechanics 71

7 Relative and Absolute Motion 83

8 Energy and Thermodynamics 91

General Conclusions for Part Three 99

Part Four Nature

9 Hypotheses in Physics 103

10 Theories of Modern Physics 115

11 Probability Calculus 127

12 Optics and Electricity 143

13 Electrodynamics 151

14 The End of Matter 163

Index 167

Foreword

David J. Stump

With this new translation, we would like to reintroduce Poincaré to a general audience. *Science and Hypothesis* was his first work aimed at such an audience and it was an immediate and continuing success. Even though this work is a collection of articles first published elsewhere, drawing them together created a whole with a sustained argument for the importance of hypotheses in mathematics and in physical science. Poincaré saw the recognition of the role of hypotheses in science as an important alternative to both rationalism and empiricism. Of course, Kant had already presented an alternative to, and synthesis of, these two traditions, but Poincaré's view is different from Kant's, even if he seems to side with Kant in his debate with the logicists later in his life. In *Science and Hypothesis*, his aim is to show that both in mathematics and in the physical sciences, scientists rely on hypotheses that are neither necessary first principles, as the rationalists claim, nor learned from experience, as the empiricists claim. These hypotheses fall into distinct classes, as presented in Chapter 9 and (somewhat differently) in the Introduction.[1] There are "natural hypotheses" that seem to be obviously true and which are the last to be abandoned, though we may be forced to consider changing them because of new evidence. "Indifferent hypotheses", by which he means, for example, the mechanical models that we develop to explain observed phenomena are the second kind of hypothesis. Poincaré argues that these hypotheses are not to be taken literally as what is going on at the atomic level, but are rather just aides to our understanding. There are also "real generalizations", which are empirically verified laws. Poincaré includes them in the list of hypotheses in Chapter 9 and also in the Introduction, despite the fact that they are learned from experience. Finally, there are conventions, which Poincaré sometimes describes as being merely linguistic – a choice of language – but which also include his famous thesis of the conventionality of metric geometry, which is more than just linguistic. The conventionality of metric geometry is the prime example of the abandonment of

[1] I follow Heinzmann (2009) and Ly (2008) here. See de Paz (2015) for further discussion of the types of hypotheses in Poincaré.

part of Kant's synthetic *a priori*. Poincaré will argue that metric geometry is not determined *a priori*, given that we know that there are consistent alternatives, nor determined empirically because of its role as a convention in physical theory. The central feature of Poincaré's view of science is that it makes essential use of conventions that are neither *a priori* nor empirical.

Summary of the chapters

We have included the preface that Poincaré wrote for the American edition of *Science and Hypothesis* (1905), translated by the mathematician George Bruce Halsted who was known for his advocacy of non-Euclidean geometry. It is not known if Poincaré wrote the preface directly in English or if he wrote it in French and had it translated. Besides thanking the translator for his work, Poincaré describes differences in national styles of doing science, arguing that while English and continental scientists use hypotheses of different sorts, the central thesis of Poincaré's book remains true – namely that science requires hypotheses in a way that has often not been noticed. The preface to the American edition is also notable because here Poincaré makes his strongest statement against Newton's absolute space, saying that "space is only a word that we have believed a thing" (xxix below). Poincaré clearly had a relational view of space, though the details of his view remain rather unstudied.[2]

Poincaré opens his introduction to *Science and Hypothesis* by contrasting a naïve realist view of science with his own view. He argues that hypotheses play an important role in both mathematics and in the physical sciences. Asking whether this does not lead to skepticism about science, he answers negatively; that a careful evaluation of the role of hypotheses will show that science is still objective, even if it does not fit the image of the naïve realist. He warns against the danger of overgeneralizing the idea of conventions in science, calling those who do so nominalists. Poincaré situates himself between the rationalists and the empiricists, that is, those who would found science on first principles and those who would found science on direct experience.

Poincaré describes some of the frameworks that he will argue we create in order to describe nature; one such framework being the theory of mathematical magnitude and space. While these are created by us and are conventional, they are not arbitrary, given that Poincaré argues that our constructions are guided by

[2] See Stump (1989; 2015) for an argument that relationalism is an element of the argument for the conventionality of metric geometry.

our experience. Furthermore, when we get to the physical sciences the situation changes. While there are still conventional elements in physical science, the principles and laws are founded directly on experiment through inductive argument. Poincaré mentions how he will treat the problem of induction with probability theory, and gives some examples in the physical sciences.

Chapter 1 opens with the question of how mathematics can tell us anything new if it is a deductive science that is based on the principle of identity. Poincaré shows that mathematics is also inductive; that is, it can make universal claims, even though it starts with particulars, by making use of mathematical induction. Through the example of elementary arithmetic, Poincaré shows how we can make assertions about all of the numbers, while starting with a few simple definitions and using the principle of mathematical induction. In making claims about the infinite natural numbers, we go beyond any experience that we could possibly have, so we cannot base the principle of mathematical induction on experience. Therefore, Poincaré claims that it is a genuine synthetic *a priori* judgment that presents itself to the mind as manifestly true.

In Chapter 2, Poincaré introduces the continuum and the definitions of rational and irrational numbers. He notes that in physical cases, we can often distinguish quantity A from quantity C, even while we are unable to distinguish quantity A from quantity B or quantity B from quantity C. Thus, we are led to a contradiction, which we must overcome by inventing the mathematical continuum. As Poincaré points out, we also need the real numbers to account for the intersection of some figures in geometry. He presents Dedekind's account of the real numbers in some detail.

Chapter 3 introduces non-Euclidean geometries and makes the preliminary case for the conventionalism of metric geometry, that is the thesis that each of these geometries is a more or less useful way of describing space and the things in it. None of the geometries are true or false. Ultimately, Poincaré will argue that geometry is neither *a priori* nor empirical and, therefore, it is conventional. In this chapter, he focusses on the argument that the true metric geometry cannot be determined *a priori*, given that alternative geometries are logically consistent. Thus, we have no *a priori* method of favoring one over another. To prove this, Poincaré introduces his famous "dictionary" that gives an interpretation of Lobachevskii's geometry. We would now say that he has given us a model showing the consistency of hyperbolic geometry, which is often called the Poincaré or the Beltrami–Poincaré half plane model.

Chapter 4 presents Poincaré's argument that the space that we experience, in its tripartite form of visual, tactile, and motor space, differs fundamentally from geometric space. While acknowledging that experience plays an indispensable role in the origins of geometry, Poincaré argues that geometry is not at all an experimental science. He shows how we construct our lived space from what we experience, but then abstracts further when we construct pure mathematical space. Poincaré introduces his famous example of a non-Euclidean world to show us how such a world can be imagined. He also claims that it is possible to visualize a four-dimensional world.

Continuing with his geometric theme, Chapter 5 is an extended argument against the idea that experiment can decide between alternative geometries. As mentioned above, Poincaré's basic strategy for showing that metric geometry is conventional is to show that choice of a geometry can be determined neither *a priori* nor experimentally. Therefore, he concludes, the only alternative is to treat choice of metric geometry as a convention. He has shown that we cannot favor Euclidean geometry *a priori* because alternative geometries are consistent and, indeed, it is possible to describe objects in space with them. As for using experiments to determine the metric geometry of space, Poincaré's fundamental point is that we only experiment on bodies, not on space itself nor on the relations of bodies to space. He sees this point as following from the principle of relativity, that is, that the laws of nature are the same whether a frame of reference is at rest or in uniform motion in a straight line. He acknowledges that it is sometimes possible to do an experiment that suggests that a body is moving, such as in the case of the rotation of the earth, remarking that he will take the issue up again in Part Three of the book.

Chapter 6 takes up classical mechanics and the status of Newton's laws. He takes up each law in turn: the law of inertia; the law of acceleration; and the law of action and reaction. In modern terms, we say first: that a body at rest remains at rest and a body in motion remains in uniform motion in a straight line unless some force acts upon it; second: that force is equal to mass times acceleration, or equivalently that acceleration is equal to force divided by mass; and third: that for every action there is an equal and opposite reaction. Poincaré argues that only the first law is really empirical, the others being definitions. He will later argue that it is impossible for all three laws to be treated as definitions since, in that case, classical mechanics would no longer be an empirical science. Furthermore, in this chapter, Poincaré considers the use of definitions in science and the origin of our concepts in our experience. These concepts need to be clarified and made concrete, a process that includes conventions that are, however, not arbitrary since they are guided by experiment. A supplement in Chapter 6 covers the number of dimensions of space, showing how our experience leads us to consider three-dimensional space the most useful.

In Chapter 7, Poincaré discusses relative and absolute motion and returns to Newton's argument for the existence of absolute space, arguing here that it is meaningless to say that the earth rotates in absolute space. Poincaré categorically rejects Newton's arguments, arguing that absolute space is not only unverifiable, but that it is a meaningless concept. He ends the discussion by saying that "the earth rotates" and "it is useful to suppose that the earth rotates" mean the same thing. In his lifetime, these statements on the rotation of the earth were misinterpreted. Some, such as Le Roy, used them as justification for the idea that everything in science is conventional, leading to a sharp rebuke by Poincaré (1902a; reprinted in Poincaré 1905). The right-wing Catholic press picked up the idea to argue that it was meaningless to say that the earth moves (around the sun), and that therefore the church was not wrong to condemn Galileo. Poincaré expressed his exasperation at such lines of argument in his last talk at Göttingen, but even a letter to the editor did little to clear things up (Ginoux and Gerini 2012: 132).

Even though he rejects absolute time as well as absolute space, Poincaré explicitly says in Chapter 7 that he will assume absolute time in order to simplify the discussion, while arguing against absolute space. In his excellent recent intellectual biography of Poincaré, Jeremy Gray suggests that in his entire corpus of writings it is possible that Poincaré goes further in rejecting absolute space than he does in rejecting absolute time, arguing that he is misled by intuitive notions of time when he is developing his views that come close to Einstein's special theory of relativity (Gray 2013: 373–4). I am not aware of any full treatment of this issue, which certainly merits further study.

Chapter 8 presents an introduction to the definitions of energy in thermodynamics. Poincaré opens with a discussion of energetics, which arose from the discovery of the principle of the conservation of energy and was given its definitive form by Helmholtz. However, Poincaré argues that problems remain with this theory and that there is still much more to be done. He then introduces thermodynamics, which is based on two fundamental principles. He discusses the idea of the simplicity of nature, arguing that while this has been assumed in the past it is not actually justified, which is another way in which science depends on hypotheses. Poincaré summarizes the argument of the book so far in a section called General Conclusions for Part Three. Poincaré here argues that science remains empirical, despite the role of conventions in physical theory.

In Chapter 9, Poincaré discusses the role of hypotheses in physics and distinguishes the three types of hypotheses mentioned above: natural hypotheses; indifferent hypotheses; and real generalizations. "Natural hypotheses" are first principles that are assumed *a priori* to be true. "Indifferent hypotheses" refers to the idea of making a representation in a physical sense that provides an explanation of unobserved processes. "Real generalizations" are taken directly from experience.

Chapter 10 opens with a refutation of the charge that science is "bankrupt" because all past theories have proven to be incorrect. In rejecting this argument, which is now known as the pessimistic induction, Poincaré argues for the continuity of science, despite acknowledging revolutionary changes in the past and correctly predicting that major changes in physics were still to come. Poincaré surveys the then contemporary physics (in 1902), including a long discussion of ether theories, especially that of Lorentz.

Chapter 11 is a discussion of probability theory and its application to science, especially a theory of causation. After surveying various interpretations of probability, Poincaré applies the theory to gambling examples, to astronomy, to probabilistic causation, and to the theory of errors. He also provides an answer to the problem of induction, arguing in terms of probability theory that use of induction is analogous to calculating the odds that an event will occur.

Optics and electricity are the topics in Chapter 12, starting with a discussion of Fresnel's wave theory in optics, which Poincaré considers to be the most well-developed theory of physics, and proceeding to consider Maxwell's electrodynamics. Poincaré uses presentation of the theories as an occasion to comment on the use of metaphysical assumptions about the nature of reality in science, given that Fresnel's theory requires the existence of ether and Maxwell's, an explanation of electromagnetic phenomena. Poincaré argues that all scientists need to do is demonstrate the possibility of a mechanical explanation, rather than concerning themselves with actually finding the explanation itself.

In Chapter 13 Poincaré presents a brief history of electrodynamics in order to show the role of changing hypotheses in science. Starting with the work of Ampère, Poincaré shows the hypotheses involved with different kinds of experimental setups and considers the different kinds of currents that they study. His main point is that Ampère uses hypotheses in his work, despite claims to the contrary (the subtitle of Ampère's work claims that his theory is derived from experiment). Poincaré then follows the development of the theories of electrodynamics after Ampère through the work of Helmholtz, Maxwell, and

Rowland, arguing that history of science is important to understand its conceptual foundations (see Darrigol 1995; 2000).

Chapter 14, "The End of Matter" was added to *Science and Hypothesis* in 1907 after Poincaré and Einstein's independent publications of the equations that form the basis of Special Relativity. Poincaré discusses the evidence pointing towards the fact that the length and mass of objects change proportionally to velocity, thus showing that matter is not fixed in the way that was traditionally thought. The chapter offers another example of Poincaré's reflections on cutting-edge physics and shows again how he was open to the changes taking place in physics in his lifetime. Although he strongly advocates the continuity of science, arguing that past theories remain valuable and true in an observational sense, even when they have been superseded, he was nevertheless aware that physics was on the cusp of a revolution.

Major interpretations of Poincaré

Poincaré's work has been widely discussed in philosophy of science from the time of its original publication onward. Early critics of *Science and Hypothesis* include Bertrand Russell, who reviewed the British edition (Russell 1905). Russell had already debated Poincaré over the nature of geometry in *Revue de métaphysique et de morale*. Poincaré started the exchange by commenting on Russell's published doctoral dissertation (Russell 1897) which had been reviewed by Couturat in the same journal (Poincaré 1899) and Russell immediately replied to Poincaré's critique (Russell 1899). Poincaré then responded in turn (1900). Although both had discussed non-Euclidean geometries, Russell and Poincaré presented starkly different views of geometry, especially when it came to the definition of geometric primitives. In his review of *Science and Hypothesis*, Russell argues first, that Poincaré has mischaracterized mathematical induction, and second, that once we are talking about physical bodies, we can make an empirical determination of which geometry is correct. Another important early critique of *Science and Hypothesis* is Federigo Enriques who, like Russell, rejects metric conventionalism, claiming that the geometry of space can be empirically determined (Enriques 1914).

The philosophical literature on Poincaré bifurcates twice. First, there has always been a split between those interested in the philosophy of spacetime and those interested in the philosophy of mathematics. Poincaré can seem to be a radical conventionalist in the first context, given his thesis of the conventionality of metric geometry, but at the same time seems to hold a very traditionalist view of the philosophy of mathematics, maintaining the role of intuition in arithmetic, thereby rejecting logicism and indeed modern formal logic. Poincaré can seem Kantian in the philosophy of mathematics, maintaining that the principle of mathematical induction is a synthetic *a priori* principle, while at the same time rejecting such a status for Euclidean geometry, and indeed holding the anti-Kantian views that we have no intuition of space or of time. Thus, there tend to be very different images of Poincaré in these different philosophical contexts. There are ways, however, of seeing his position as more consistent than it may first appear. Poincaré's conventionalism is restricted in at least two ways. First, it is guided by experience, leading to conventions that are not arbitrary. Second, outside of metric geometry, conventions play an important yet very limited role in science, according to Poincaré. There are some principles that we could take to be conventional, but in large part science remains empirical and is thus constrained by experimental results. Another way that there is less divergence between Poincaré's view on arithmetic and geometry than may first appear is that both require some form of intuition. The principle of mathematical induction is fundamental to arithmetic, he argues, and our knowledge of it is intuitive and *a priori*. In a similar manner, we have intuitive *a priori* knowledge of some of the abstract properties of geometry, such as groups. We have no similar intuition of metric properties, that is, we have no intuitive knowledge of distance, for example, according to Poincaré. A small sampling of the literature in the philosophy of mathematics concerning Poincaré includes works by Deltefsen (1992), Folina (1992), Heinzmann (1990), McLarty (1997), and Mooij (1966).

A second bifurcation occurs within the literature on conventionalism. Some focus primarily on the thesis of the conventionality of metric, as was often the case in the literature on the philosophy of space and time (DiSalle 2006, Earman *et al.* 1977, Earman 1989, Friedman 1983, Grünbaum 1968, Sklar 1974 and 1986). An early exemplar would be Louis Rougier's 1920 book on Poincaré's conventionalism. A little broader, but still focused on metric conventionalism, is Torretti's historical study (1978, 1984). Others, however, embed metric conventionalism in a broader notion of conventionalism and make that the focus, often making comparisons with other philosophers such as Wittgenstein. Ben-Menahem's book *Conventionalism* (2006) is in this tradition, as is Psillos (2014) who helpfully connects conventionalism to structuralism, but Schlick's influential reading of Poincaré (1918, 1925 and 1935) is what defined him in the philosophical literature. The Logical Positivists cited Poincaré frequently and followed Schlick's interpretation of Poincaré as holding a general conventionalist thesis. There are some recent authors who cross the boundaries between the competing interpretations of Poincaré, such as Gray (2013) and Heinzmann (2006 and 2009).

In order to explain the bifurcation in the literature on conventionalism, I have argued elsewhere that there are two kinds of conventions in Poincaré's work, and that these have not been clearly distinguished, neither by Poincaré himself nor by his interpreters (Stump 2015, Chapter 3). Poincaré argues that some elements of empirical science can be "erected" into principles, that is, they are conventions that can be taken to be definitely true and never questioned. However, geometric conventionalism has a separate two-part justification that is quite different from his justification of the conventionality of principles.[3] Milena Ivanova takes this point a step further, insightfully noting that while the source of conventional principles is empirical, the source of geometry is *a priori*, from the concept of a group (Ivanova 2015).

The argument against empirical determination of metric is certainly the most important defense of Poincaré's conventionalism, but Poincaré's argument also includes an *a priori* argument against Kant's theory of geometry, and this argument should in part be understood in the context of a philosophical theory of meaning and the development of a formal conception of geometry. Applying an argument about the meaning of terms to Poincaré's argument for the geometrical conventionalism has led to some of the most serious misinterpretations of his view, so I should emphasize here that I completely reject the view that Poincaré's geometric conventionalism is based on any kind of linguistic conventionalism. However, Poincaré does use some arguments concerning the definition of primitive terms in geometry.

Poincaré quite explicitly distinguishes between the two kinds of conventions as part of the distinction between mathematical theory and physical theory, in the "General Conclusions for Part Three" section of *Science and Hypothesis*. Not only has it been commonly assumed that there is but one kind of convention in Poincaré and one set of arguments for his conventionalism, it has also frequently been said that Poincaré does not distinguish mathematical theory from physical theory and that if he had done so his conventionalism would be undermined. Missing the distinction between the conventionality of principles and the conventionality of metric geometry has led to many errors

[3] Jerzy Giedymin also distinguishes between the conventionality of metric geometry and the conventionality of principles in Poincaré but, in calling the latter generalized conventionalism, he is misleading, since Poincaré thinks that all conventions are limited (Giedymin 1977: 273; 1982).

of interpretation, so it is important to elaborate the difference. It is the conventionality of principles that is most analogous with the idea that some statements of physical theory function as though they were *a priori* or are constitutive, though Poincaré does not use this language, calling them instead "definitions in disguise."

As for the arguments for the conventionality of geometry, I have stated above that Poincaré has a two-stage argument for the conventionality of metric. First, he argues against any *a priori* determination of the metric of space, which is one of the many ways that Poincaré directly opposes Kant. The existence of consistent non-Euclidean geometries shows us that there is no *a priori* method of determining the metric of space. Poincaré also has what looks like a formal view of geometry, which may seem incongruous given his strong anti-formalist stand in arithmetic, but nevertheless we can find this in his writings. Second, he argues against any empirical determination of the metric of space, a point in the argument where Poincaré is often thought to be using something like the Duhem–Quine underdetermination thesis. However, this cannot be the correct interpretation of Poincaré's geometric conventionalism, because all empirical theories, and not just physical geometry, are underdetermined in the Duhemian sense. The major interpretive problem for those who hold this epistemological interpretation of conventionalism is to explain why Poincaré holds that only geometry and a few principles are conventional, not all of science (Stump 1989: 348; Friedman 1999: 73). As we see clearly in Poincaré's critique of Le Roy, and even earlier in *Science and Hypothesis*, he firmly rejects the idea that there is a generalized conventionality of science (1902a; pp. 2 and 100 below, respectively). Instead, his thoroughgoing relational theory of space, and his view that space exhibits only topological properties, leads him to the view that the metric geometry of space cannot be determined empirically.

Poincaré is often read as an instrumentalist, one who holds that sciences should aim at giving an account of what is observable, not at explaining phenomena in terms of unobservable entities and processes. While this seems to be his view in *Science and Hypothesis*, for example arguing that the atomic hypothesis is useful but not the only possible explanation for chemical and other phenomena, Poincaré accepted the reality of atoms after he learned of the work of Perrin (Nye 1976; Demopoulos *et al.* 2012). Furthermore, in at least one case, Poincaré also accepted the refutation of a fundamental principle of physics, what he calls a "natural hypothesis", by empirical theory. When he accepted the quantum theory as presented in the first Solvay conference in 1911, Poincaré gave up the hypothesis of continuity (McCormmach 1967). There is however another way of interpreting Poincaré's view on scientific realism and antirealism. In a very influential article, John Worrall makes a case for interpreting Poincaré as a structural realist (1989). Poincaré takes relations to be real, but withholds judgment on the real existence of the entities that are described in the relations. Worrall's article has spawned a large literature, much of it moving beyond the original inspiration that was found in Poincaré (Ladyman 2016).

Poincaré has often been taken to be presenting a hierarchy of the science in *Science and Hypothesis* and in later work. This interpretation has recently come under criticism in an important pair of articles that present an alternative view (Dunlop 2016 and 2017). I believe that clarifying what is meant by the hierarchy will resolve this debate. While it is true that Poincaré does not use the term "hierarchy" to describe his image of the relation of the sciences and mathematics, he does present the sciences in a specific order. In the introduction, Poincaré says that: "Such is the conclusion we will reach, but to get there, we must first review the sequence of sciences, from arithmetic and geometry to mechanics and experimental physics" (p. 2 below).[4] What is the relationship between arithmetic, geometry and the rest of the sciences? Michael Friedman originated the claim that Poincaré presents the sciences in a hierarchy, and in doing so makes clear that the point is that each lower level must first be in place in order to be able to proceed in the next level:

> But the point perhaps can be made even more clearly if we consider Newton's theory of universal gravitation. For Newton's *Principia* had already shown clearly how we can empirically discover the law of universal gravitation – on the presupposition, that is, of the Newtonian laws of motion and Euclidean geometry. Without these presuppositions, however, we certainly would not have been able to discover the law of gravitation. And the same example also shows clearly how each level in the hierarchy of sciences presupposes all of the preceding levels: we would have no laws of motion if we did not presuppose spatial geometry, no geometry if we did not presuppose the theory of mathematical magnitude, and of course no mathematics at all if we did not presuppose arithmetic.
>
> Friedman 1999: 76

Note that Friedman mentions the theory of mathematical magnitude as well. There is a step between arithmetic and geometry, namely the theory of mathematical magnitude, which is precisely the topic of Chapter 2 below. The

[4] "Telle est la conclusion à laquelle nous parviendrons, mais pour cela il nous faudra parcourir la série des sciences depuis l'arithmétique et la géométrie jusqu'à la mécanique et à la physique expérimentale" (Poincaré 1902, 1968: 25).

theory of mathematical magnitude requires the possibility of indefinite repetition, and therefore something like mathematical induction, which Poincaré presents as central to arithmetic in Chapter 1. I would argue that the only claim being made about the hierarchy is that each level of the sciences requires the previous one. There is direct evidence that Poincaré had something like this in mind. For example, in the opening of Chapter 12, he remarks that: "The purpose of mathematical theories is not to reveal the true nature of things. Such a claim would be unreasonable. Their only goal is to coordinate the physical laws that experiment reveals to us, but that we could not even state without the help of mathematics" (p. 143 below). In other words, we need mathematics in place before we can do any empirical science.

Dunlop, by contrast, seems to have something much stronger in mind with the idea of the hierarchy, which is perhaps why she calls the thesis the dependency hierarchy interpretation. She seems to take the interpretation to be a kind of thesis of reductionism, that geometry reduces to arithmetic, or at least, the intuitive reasoning used in arithmetic is all that we need in geometry.

> Commentators have suggested that the intuition that grounds the use of induction in arithmetic also underlies the conception of a continuum, that the consistency of geometrical axioms must be proved through arithmetical induction, and that arithmetical induction licenses the supposition that certain operations form a group.
>
> Dunlop 2016: 274

Dunlop is completely correct in saying that is not Poincaré's view. Rather, he says that we need arithmetic and further things in order to do things in geometry. All that I take Poincaré to be saying is that we still need to add, subtract, multiply, and divide when we are doing geometry, but we will be doing more than what we do in arithmetic. The reason that we need a theory of magnitude before we can do geometry is because we measure things. The reason we need geometry in classical mechanics is because we are doing things like tracking the elliptical orbits of the planets. Thus, I also agree with Dunlop when she says that "Poincaré's views of arithmetic and geometry cannot be reconciled by supposing geometry to be founded on arithmetical operations or principles" (Dunlop 2016: 306). We need arithmetic plus group theory plus the conventions in order to have metric geometry.

There may be some authors who made reductionist claims, but it is not what I took from Friedman's original presentation. Furthermore, I would say that this interpretation cannot be right when we get to the physical sciences, where there is clearly more content in the higher-level sciences – empirical content that could not possibly be reduced to mathematics.

Poincaré discusses the sciences in a sequence, starting with arithmetic. Mathematical induction is essential in arithmetic, because only by using it can we make assertions about all numbers. Poincaré considers mathematical induction to be a genuine synthetic *a priori*. He next considers magnitude, which requires arithmetic, but goes further. Likewise, geometry extends our knowledge still further, but requires the theory of magnitude to make measurements, and arithmetic to combine numbers. Poincaré then considers classical mechanics, which again extends our knowledge while relying on the mathematics that came before it. Finally, he considers theories of physics, where we have genuine empirical results, but based on the mathematics, hypotheses and conventions that came before. Thus the sciences are laid out like expanding concentric circles, with new content being added to the base at each level. The lower levels are necessary preconditions for higher levels, but they are not sufficient.

Bibliography

Ben-Menahem, Yemima. 2006. *Conventionalism.* New York: Cambridge University Press.

de Paz, María. 2015. "Poincaré's Classification of Hypotheses and Their Role in Natural Science," *International Studies in the Philosophy of Science* 29 (4): 369–82.

Darrigol, Olivier. 1995. "Henri Poincaré's Criticism of Fin de Siècle Electrodynamics," *Studies in History and Philosophy of Modern Physics* 26 (1): 1–44.

Darrigol, Olivier. 2000. *Electrodynamics from Ampère to Einstein*. Oxford and New York: Oxford University Press.

Demopoulos, William, Frappier, Mélanie, and Bub, Jeffrey. 2012. "Poincaré's 'Les conceptions nouvelles de la matière'," *Studies in History and Philosophy of Modern Physics* 43 (4): 221–5.

Detlefsen, Michael. 1992. "Poincaré against the Logicians," *Synthese* 90 (3): 349–78.

DiSalle, Robert. 2006. *Understanding Space-Time: The Philosophical Development of Physics from Newton to Einstein*. Cambridge: Cambridge University Press

Dunlop, Katherine. 2016. "Poincaré on the Foundations of Arithmetic and Geometry. Part 1: Against 'Dependence-Hierarchy' Interpretations," *HOPOS: The Journal of the International Society for the History of Philosophy of Science* 6 (2): 274–308.

Dunlop, Katherine. 2017. "Poincaré on the Foundations of Arithmetic and Geometry. Part 2: Intuition and Unity in Mathematics," *HOPOS: The Journal of the International Society for the History of Philosophy of Science* 7 (1): 88–107.

Earman, John. 1989. *World Enough and Space-Time: Absolute versus Relational Theories of Space and Time*. Cambridge, MA: MIT Press.

Earman, John, Glymour, Clark N., and Stachel, John J. (eds.). 1977. *Foundations of Space–Time Theories*. Minneapolis: University of Minnesota Press.

Enriques, Federigo. 1914. *Problems of Science* (K. Royce, trans.). Chicago, IL: The Open Court Publishing Company.

Folina, Janet. 1992. *Poincaré and the Philosophy of Mathematics*. London: MacMillan.

Friedman, Michael. 1983. *Foundations of Space–Time Theories: Relativistic Physics and Philosophy of Science*. Princeton, NJ: Princeton University Press.

Friedman, Michael. 1999. *Reconsidering Logical Positivism*. Cambridge: Cambridge University Press.

Giedymin, Jerzy. 1977. "On the Origin and Significance of Poincaré's Conventionalism," *Studies in History and Philosophy of Science Part A* 8 (4): 271–301.

Giedymin, Jerzy. 1982. *Science and Convention: Essays on Henri Poincaré's Philosophy of Science and the Conventionalist Tradition*. Oxford: Pergamon Press.

Ginoux, Jean-Marc, and Gerini, Christian. 2012. *Henri Poincaré: Une biographie au(x) quotidien(s)*. Paris: Ellipses.

Gray, Jeremy J. 2013. *Henri Poincaré: A Scientific Biography*. Princeton, NJ: Princeton University Press.

Grünbaum, Adolf. 1968. *Geometry and Chronometry in Philosophical Perspective*. Minneapolis: University of Minnesota Press.

Heinzmann, Gerhard. 1990. *Zwischen Objektkonstruktion und Strukturanalyse: zur Philosophie der Mathematik bei Jules Henri Poincaré*. Göttingen: Vandenhoeck & Ruprecht.

Heinzmann, Gerhard. 2006. "La philosophie des sciences de Henri Poincaré," in M. Bitbol and J. Gayon (eds.), *L'épistémologie française: 1830–1970*. Paris: Presses Universitaires de France, 335–55.

Heinzmann, Gerhard. 2009. "Hypotheses and Conventions in Poincaré," in M. Heidelberger and G. Schiemann (eds.), *The Significance of the Hypothetical in the Natural Sciences*. Berlin and New York: Walter de Gruyter, 169–92.

Ivanova, Milena. 2015. "Conventionalism, Structuralism and neo-Kantianism in Poincaré's Philosophy of Science," *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics* 52: 114–22.

Ladyman, James. 2016. "Structural Realism," *The Stanford Encyclopedia of Philosophy* (Winter edn.), Edward N. Zalta (ed.), available online at https://plato.stanford.edu/archives/win2016/entries/structural-realism/ [accessed April 29, 2017]

Ly, Igor. 2008. *Géométrie et physique dans l'oeuvre de Henri Poincaré*, Thèse Université Nancy 2, Nancy.

McCormmach, Russell. 1967. "Henri Poincaré and the Quantum Theory," *Isis* 58: 37–55.

McLarty, Colin. 1997. "Poincaré: Mathematics and Logic and Intuition," *Philosophia Mathematica* (3) 5: 97–115.

Mooij, J. J. A. 1966. *La philosophie des mathematiques de Henri Poincaré*. Paris: Gauthier-Villars.

Nye, Mary Jo. 1976. "The Nineteenth-Century Atomic Debates and the Dilemma of an 'Indifferent Hypothesis,'" *Studies in History and Philosophy of Science* 7: 245–68.

Poincaré, Henri. 1899. "Des fondements de la géometrie: à propos d'un livre de M. Russell," *Revue de métaphysique et de morale* 7: 251–79.

Poincaré, Henri. 1900. "Sur les principles de la géometrie: réponse à M. Russell," *Revue de métaphysique et de morale* 8: 73–86.

Poincaré, Henri. 1902a. "Sur la valeur objective de la science," *Revue de métaphysique et de morale* 10: 263–93.

Poincaré, Henri. 1902b, 1968. *La science et l'hypothèse*. Paris: Flammarion.

Poincaré, Henri. 1905, 1970. *La valeur de la science*. Paris: Flammarion.

Psillos, Stathis. 2014. "Conventions and Relations in Poincaré's Philosophy of Science," *Methode: Analytic Perspectives* 3 (4): 98–140.

Rougier, Louis A. P. 1920. *La philosophie géométrique de Henri Poincaré*. Paris: Alcan.

Russell, Bertrand. 1897. *An Essay on the Foundations of Geometry*. Cambridge: Cambridge University Press.

Russell, Bertrand. 1899. "Sur les axiomes de la géometrie," *Revue de métaphysique et de morale* 7: 684–707.

Russell, Bertrand. 1905. "Review of H. Poincaré's 'Science and Hypothesis,'" *Mind, n. s.* 14: 412–18.

Schlick, Moritz. 1918, 1925. *Allgemeine Erkenntnislehre* (2nd edn.). Berlin: J. Springer.

Schlick, Moritz. 1935. "Are Natural Laws Conventions?" in H. Feigl and M. Brodbeck (eds.), *Readings in the Philosophy of Science*. New York: Appleton Century Crofts, 181–8.

Sklar, Lawrence. 1974. *Space, Time, and Spacetime*. Berkeley, CA: University of California Press.

Sklar, Lawrence. 1986. *Philosophy and Spacetime Physics*. Berkeley, CA: University of California Press.

Stump, David J. 1989. "Henri Poincaré's Philosophy of Science," *Studies in History and Philosophy of Science* 20 (3): 335–63.

Stump, David J. 2015. *Conceptual Change and the Philosophy of Science: Alternative Interpretations of the A Priori*. New York and London: Routledge.

Torretti, Roberto. 1978, 1984. *Philosophy of Geometry from Riemann to Poincaré*. Dordrecht: D. Reidel.

Worrall, John. 1989. "Structural Realism: The Best of Both Worlds?" *Dialectica* 43: 99–124.

Acknowledgments

We would like to thank John Stillwell for his advice on the translation of mathematical terms, and Olivier Darrigol for his advice on the translation of the chapter on electrodynamics. Any errors are the responsibility of the translators. We would also like to thank the University of King's College for its support of Mélanie Frappier, and the Faculty Development Fund of the University of San Francisco for its contribution.

Original Sources of the Material in *Science and Hypothesis*[1]

Following the French edition of *Science and Hypothesis* and the substantial corrections added in Louis Rougier and Laurent Rollet's *L'opportunisme scientifique*,[2] we can provide the following information on the original sources of the material presented below.[3]

Author's Preface to the Halsted Translation

Poincaré's preface is found in the 1905 English translation by Halsted and reprinted in the collection of Poincaré's work *The Foundations of Science*, trans. George Bruce Halsted (New York and Garrison, 1921), available online at https://archive.org/stream/cu31924012063537 [accessed April 29, 2017]. If there is a French original of the preface, it is unknown.

Introduction

Poincaré's introduction seems to have been written for the book.

Part One: Number and Magnitude

Chapter 1 On the Nature of Mathematical Reasoning

The main part of the book starts with this chapter adapted from "Sur la nature du raisonnement mathématique," *Revue de métaphysique et de morale* 2 (1894): 371–84.

1 From this point forward, numbered footnotes are from the editors. Footnotes with an * are Poincaré's.

2 Henri Poincaré. 2002. *Scientific Opportunism = L'opportunisme scientifique: An Anthology. Compiled by Louis Rougier* (Laurent Rollet, ed.). Basel: Birkhäuser, 169–71.

3 Almost all of Poincaré's original papers have been digitalized and made available online at: http://henripoincarepapers.univ-lorraine.fr/bibliohp/?t [accessed April 29, 2017].

Chapter 2 Mathematical Magnitude and Experience

The chapter was adapted, with substantial modifications, from "Le continu mathématique," *Revue de métaphysique et de morale* 1 (1893): 26–34. Rougier and Rollet note that the section "Remarques diverses" was not in the original article (Rougier and Rollet, 2002: 169). They also suggest that the final section probably found its origin in the article "Correspondance sur les géométries non euclidiennes (lettre à M. Mouret)," *Revue générales des science pures et appliquées* 3 (1892): 74–5.

Part Two: Space

Chapter 3 Non-Euclidian Geometries

Part Two opens with this chapter adapted from "Les géométries non euclidiennes," *Revue générale des sciences pures et appliquées* 2 (1891): 769–74.

Chapter 4 Space and Geometry

The chapter opens with paragraphs from the end of the article "Les géométries non euclidiennes" used as the basis of Chapter 5. The remainder of the chapter is an adaptation of "L'espace et la géométrie," *Revue de métaphysique et de morale* 3 (1895): 631–46.

Chapter 5 Experience and Geometry

Chapter 5 brings together elements from four different essays:

1. A part of the section entitled "La géométrie et l'astronomie" is from the article "Les géométries non euclidiennes," which was used as the basis of Chapter 5;
2. "On the Foundations of Geometry," *The Monist* 9 (1898): 1–43;
3. "Des fondements de la géométrie à propos d'un livre de M. Russell," *Revue de métaphysique et de morale* 7 (1899): 251–79;
4. "Sur les principes de la géométrie. Réponse à M. Russell," *Revue de métaphysique et de morale* 8 (1900): 73–86.

Part Three: Force

Chapter 6 Classical Mechanics

We begin Part Three with the first part of the article "Sur les principes de la mécanique," from *Bibliothèque du Congrès international de philosophie* (Paris: Armand Colin, 1901), III: 457–94 (up to and including the section "L'école du fil"), which was used as the basis of Chapter 6.

Chapter 7 Relative and Absolute Motion

Chapter 7 is based on the second part of the article "Sur les principes de la mécanique," used for Chapter 6.

Chapter 8 Energy and Thermodynamics

We continue with Chapter 8, a heavily edited and simplified version of the Introduction of *La thermodynamique* (Paris: G. Carré & C. Naud, 1891).

General Conclusions for Part Three

These few paragraphs are inspired by a section of "Sur les principes des la mécanique," which was used for Chapters 6 and 7.

Part Four: Nature

Chapter 9 Hypotheses in Physics

As Rougier and Rollet note (2002, 171), this chapter is a slight re-edition of the first part of the article "Les relations entre la physique expérimentale et la physique mathématique", which appeared in the following conference proceedings and journals: *Rapport du Congrès international de physique* (Paris: Gauthier-Villars) I: 1–29; *Revue générale des sciences pures et appliquées* 11 (1900): 1163–75, and *Revue scientifique* (*Revue rose*) 14 (23): 705–16.

Chapter 10 Theories of Modern Physics

Chapter 10 is adapted from the second part of "Les relations entre la physique expérimentale et la physique mathématique" used for Chapter 9.

Chapter 11 Probability Calculus

Chapter 11 is an adaptation of Poincaré's article "Réflexions sur le calcul des probabilités," *Revue générale des sciences pures et appliquée* 10 (1899): 262–9

Chapter 12 Optics and Electricity

This chapter was inspired from the prefaces of *Théorie mathématique de la lumière* I (Paris: G. Carré et C. Naud, 1889) and *Électricité et optique I*: *Les théories de Maxwell et la théorie électromagnétique de la lumière* (Paris: G. Carré and C. Naud, 1890).

Chapter 13 Electrodynamics

Chapter 13 is an edited version of Poincaré's article: "À propos des expériences de M. Crémieu," *Revue générale des sciences pures et appliquées* 12 (1901): 994–1007.

Chapter 14 The End of Matter

We complete the book with Chapter 14, which was first published in the *Athenaeum* 4086 (1906): 201–2 and was added to the French editions of *Science and Hypothesis* in 1907.[4]

[4] Henri Poincaré, *La correspondence entre Henri Poincaré et les physiciens, chimistes et ingénieurs*, Walter Scott (ed.), in collaboration with Étienne Bolmont and André Coret (Basel: Birkhäuser, 2008), 232

Author's Preface to the Halsted Translation

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.

Everyone 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 or which others would be incapable. It would therefore be deplorable if they 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 everyone wanted to play the violin.

And yet it is not bad for the violin to know what the violoncello 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 group 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 someone has discovered under the complex aggregate shown by our senses something far more simple, nor 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 dynamics. The ether it is, the unknown, which explains matter, the known; matter is incapable of explaining the ether.

Poincaré

Introduction

To a superficial observer, scientific truth is not to be doubted, the logic of science is infallible, and if scientists are at times mistaken, it is for having disregarded its rules. Mathematical truths originate from a small number of self-evident propositions through a train of flawless reasoning. They apply not only to us, but to nature itself. They constrain the Creator, as it were, and only allow him to choose between relatively few solutions. Only a few experiments would then be enough to inform us of the choice he has made. Out of each experiment, a multitude of consequences might arise through a series of mathematical deductions and it is in this way that each experiment would reveal to us a part of the universe.

Such is the understanding that many well-educated people, as well as high school students getting their first notions of physics, have of the origins of scientific certainty. That is how they understand the role of experimentation and of mathematics. It was also the understanding of many scientists and philosophers who, one hundred years ago, wished to construct the world while borrowing as little material from experience as possible. Upon further reflection, we recognized the position held by the hypothesis, realizing that the mathematician cannot do without it, and the experimenter even less so. We then questioned whether all these constructions were truly solid and believed that a puff of air would bring them to the ground. To be skeptical in this way is still to be superficial. To doubt everything or to believe everything are two equally convenient solutions: both spare us from thinking.

Rather than delivering a summary condemnation, we must instead carefully examine the role of the hypothesis. We will then recognize not only that this role is necessary, but also that in most cases it is legitimate. We shall also see that there are many kinds of hypotheses, some of which are verifiable and, once experimentally confirmed, become fruitful truths. Others, unable to lead us astray, can be useful to us by focusing our thoughts. Lastly, others are hypotheses in appearance only and amount to definitions or conventions in disguise. The latter are found mostly in mathematics and its related sciences. In fact, it is from these conventions that these sciences draw their rigor. They are the product of the free activity of our mind that, in this field, encounters no obstacles. Here, our mind can make affirmations because it rules by decree. But let us be clear, while these decrees apply to *our* science which would be impossible without them, they do not apply to nature. Are these decrees then arbitrary? No, otherwise they would be unproductive. Experience allows us to choose freely, but it guides our decisions by helping us to discern the most useful path. Our decrees are therefore like those of an absolute but wise prince who would consult his council of state.

Some people have been struck by this character of free convention found in some fundamental principles of science. They tried to overgeneralize, while at the same time forgetting that freedom is not arbitrariness. In so doing, they reached what we call *nominalism*, wondering if scientists are not fooled by their own definitions and if the world they believe they are discovering is not simply created by their whims.* Under such conditions, science would be certain, but insignificant. If this were the case, science would be powerless; yet every day we see it work with our very own eyes and this could not be unless it taught us something about reality. However, contrary to the naïve dogmatists' view, that which science captures are not the things themselves, but simply relationships between them. Beyond these relations, there is no knowable reality.

Such is the conclusion we will reach, but to get there we must first review the sequence of sciences, from arithmetic and geometry through to mechanics and experimental physics.

What is the nature of mathematical reasoning? Is it actually deductive as is commonly supposed? A deeper analysis reveals that it is nothing of the kind, that mathematical reasoning participates to some extent in the nature of inductive reasoning, and it is in that way that it is productive. Nonetheless, it retains its character of absolute rigor, which is what we have to show first.

Once we are better acquainted with one of the tools that mathematics put into the hands of the researcher, we have to analyse another fundamental notion, that of mathematical magnitude. Do we find it in nature or do we insert it there?

* See Le Roy, "Science et philosophie." (*Revue de métaphysique et de morale*, 1901). [Translators' note: Le Roy's article appeared in two parts: "Science et philosophie," *Revue de métaphysique et de morale* 7 (1899): 503–62 and then "Science and philosophie (suite et fin)," *Revue de métaphysique et de morale* 8 (1900): 37–72. Poincaré may have confused this article with Le Roy's 1901 two-part article: "Sur quelques objections adressées à la nouvelle philosophie," *Revue de métaphysique et de morale* 9 (1901): 292–327 and "Sur quelques objections adressées à la nouvelle philosophie (suite et fin)," *Revue de métaphysique et de morale*, 9 (1901): 407–32.]

If the latter case, do we not run the risk of distorting everything? We must acknowledge the existence of a discrepancy when comparing the raw data of our senses with this extremely complex and sophisticated concept mathematicians call magnitude. While it is us who built this framework in which we want to fit everything, we did not build it at random. We have made it to measure, so to speak, which is why we can fit all the facts in it without deforming their essential qualities.

Space is another framework that we impose on the world. Where do geometry's first principles come from? Does logic force us to accept them? Lobachevskii has shown that it does not by creating non-Euclidean geometries. Is space revealed to us by our senses? Again no; since the space that our senses could show us differs completely from the geometer's space. Does geometry stem from experience? A thorough discussion will show that it does not. We will therefore conclude that its principles are only conventions. However, these conventions are not arbitrary and we would have been compelled to adopt different ones had we been transported to another world (which I call non-Euclidean[1] and try to imagine).

We would be led to similar conclusions in mechanics and would see that the principles of this science, while resting more directly on experience, still partake of the conventional character of the geometric postulates. Up to this point, nominalism prevails, but we now come to the physical sciences themselves, where the scene changes. We come across a different kind of hypothesis and fully perceive its productivity. Undoubtedly, the theories seem at first sight fragile and the history of science shows us how fleeting they are. However, they do not entirely die out and something remains of each of them. It is this something that we must try to sort out, for there and only there is true reality.

The method of the physical sciences rests on induction, which leads us to expect the repetition of a phenomenon when the circumstances under which it first arose recur. If *all* these circumstances could reappear at the same time, this principle could be applied without fear, but this will never happen. Some of these circumstances will always be lacking. Are we absolutely sure that they are unimportant? Of course not! While probable, this could never be completely certain, hence the significant role the notion of probability plays in the physical sciences. The probability calculus is not therefore simply a pastime or a guide to

[1] Although Poincaré uses the general term non-Euclidean geometry, he will in fact be discussing only hyperbolic geometries of constant curvature in this latter section.

baccarat players. We must try to deepen its principles. The vague instinct that makes us guess the likelihood of an event is so unamenable to analysis that I have only been able to give quite incomplete results on this issue.

After a study of the working conditions of physicists, I have thought it necessary to show the latter at work. In order to do so, I have taken a few examples from the history of both optics and electricity. We will see where the ideas of Fresnel and Maxwell have originated and what unconscious hypotheses were made by Ampère and the other founders of electrodynamics.

Part One

Number and Magnitude

1

On the Nature of Mathematical Reasoning

I

The very possibility of mathematical science seems an insoluble contradiction. If this science is deductive in appearance only, from where does it get its perfect rigor that no one dares to doubt? If, on the contrary, all the propositions it sets forth can be derived from one another by the rules of formal logic, why is mathematics not reducible to an immense tautology? Syllogism can teach us nothing that is essentially new and, if everything originated in the principle of identity, it should also be possible to reduce everything to it. Are we then to concede that the statements of all those theorems filling so many volumes are merely roundabout ways of saying that A is A?

Admittedly, we can go back to the axioms that are at the source of all the arguments. If we deem that they cannot be reduced to the principle of contradiction, and if moreover we refuse to see them as experimental facts that could not contribute to mathematical necessity, we still have the option of classifying them as synthetic *a priori* judgments. While this gives a name to the difficulty, it does not solve it. Even if the nature of synthetic judgments were no longer a mystery to us, the contradiction would not vanish, it would only recede. Syllogistic reasoning is still unable to add anything to the supplied data. These data are reducible to a few axioms and nothing else should be found in the conclusions. No theorem should be new unless a new axiom was to be used in its proof. Reasoning would only be able to convey to us immediately obvious truths borrowed from direct intuition, making the theorem nothing but a freeloading intermediary. If this were the case, would we not be justified in asking whether the whole syllogistic apparatus serves solely to conceal what we borrowed?

The contradiction will be even more striking if we open any book on mathematics. On every page, the author is likely to announce the intention to generalize some already-known proposition. Does the mathematical method proceed then from the particular to the general and if so, how can it be called deductive?

Finally, if the science of numbers was purely analytic or could emerge analytically from a small number of synthetic judgments, it seems that a sufficiently potent mind could at a glance perceive all its truths. What am I saying! We could even hope to invent someday a rather simple language to express these truths directly to a mind of normal intelligence.

If we refuse to admit these consequences, we must then concede that mathematical reasoning has in itself a kind of creative power and, consequently, that it differs from syllogism. The difference must indeed be a profound one. For instance, we will not find the key to the mystery in the frequent use of the rule according to which a single uniform operation applied to two equal numbers will give identical results. All these modes of reasoning, whether or not they are reducible strictly speaking to syllogisms, preserve an analytic character and are for this very reason powerless.

II

The argument is a long-standing one. Leibniz was already looking to prove that 2 and 2 make 4. Let us briefly examine his proof. I assume as already defined both the number 1 and the operation $x + 1$, which consists in adding a unit to a given number x. Whatever they may be, these definitions will not play a role in the subsequent reasoning. I next define the numbers 2, 3, and 4 by the equalities:

(1) $1 + 1 = 2$; (2) $2 + 1 = 3$; (3) $3 + 1 = 4$.

In the same way, I define the operation $x + 2$ by the relation:

(4) $x + 2 = (x + 1) + 1$.

Taking this as given, we have:

$2 + 2 = (2 + 1) + 1$ (Definition 4)
$(2 + 1) + 1 = 3 + 1$ (Definition 2)
$3 + 1 = 4$ (Definition 3)

Hence,

$2 + 2 = 4$ Q.E.D.

It cannot be denied that this reasoning is purely analytic, but ask any mathematician and the answer will be: "This is not properly speaking a proof, but a verification." We have limited ourselves to bringing closer together two purely conventional definitions and have ascertained their identity, but we have not learned anything new. *Verification* differs from genuine proof precisely because it is purely analytic and because it is unproductive. It is unproductive because the conclusion is merely a translation of the premises into another language. Conversely, genuine proof is productive because the conclusion is, in a sense, more general than the premises. So the equation $2 + 2 = 4$ has only been susceptible to verification because it is particular rather than general. Any particular mathematical statement may always be verified in this way. However, if mathematics were reducible to a series of such verifications, it would not be a science. Likewise, a chess player does not, for instance, create a science by winning a game. Science is necessarily general. We can even say that the very goal of the exact sciences is to spare us these direct verifications.

III

Let us then watch geometers at work to catch a glimpse of their methods. The task is not without difficulty. It is not enough to open a book at random and to analyse one of its proofs. We must first exclude geometry since the question is complicated by difficult problems relating to the role of the postulates as well as to the nature and origin of the notion of space. For similar reasons, we cannot make use of infinitesimal analysis. We must pursue mathematical thought where it has remained pure, that is, in arithmetic. We still have to choose. In the more advanced parts of number theory, primitive mathematical notions have already undergone such a profound elaboration that it has become difficult to analyse them. Therefore, it is at the beginning of arithmetic that we must expect to find the explanation we seek. However, it so happens that it is in the proof of the most elementary theorems that the authors of classic treatises have displayed the least precision and rigor. We should not reproach them for they had no choice. Beginners are not prepared for true mathematical rigor; they would see it as nothing but futile and tedious subtleties. It would be a waste of time to try to make them more exacting too soon. Without skipping steps, they must rapidly retrace the path slowly traveled by the founders of the science.

Why is such a long preparation necessary to get used to this perfect rigor which should, it seems, be a matter of course for all good thinkers? While it is a logical and psychological question worthy of consideration, we shall not reflect on it; it is off topic. All I want to take from it is that, to reach our goal, we must prove the most elementary theorems anew, giving them a form that would satisfy the experienced geometer rather than the crude form in which they must remain so as not to tax beginners.

Definition of addition

I suppose that the operation $x + 1$, consisting in the addition of the number 1 to a given number x, has already been defined. Besides, whatever this definition may be, it will not play any role in the subsequent line of reasoning. We now have to define the operation $x + a$, which consists in adding the number a to a given number x. Suppose that we have defined the operation $x + (a—1)$. The operation $x + a$ will be defined by the equation:

(1) $x + a = [x + (a—1)] + 1.$

We will therefore know what $x + a$ is once we find out what $x + (a—1)$ is and, as I have assumed at the start that we knew what $x—1$ is, we will be able to define successively and "by mathematical induction"[1] the operations $x + 2$, $x + 3$, etc.

This definition deserves a moment's attention. It is of a particular nature that already distinguishes it from the purely logical definition. The equation (1) actually contains an infinite number of distinct definitions, each of which has a meaning only when the preceding definition is known.

Properties of addition

Associativity

I say that

$$a + (b + c) = (a + b) + c.$$

The theorem is in fact true for $c + 1$. In this case, it is written:

$$a + (b + 1) = (a + b) + 1,$$

[1] Poincaré uses the term "par recurrence" (by recurrence); we have opted for the more modern expression "mathematical induction."

which, apart from the notations, is nothing but equation (1), with which I just defined addition. Supposing the theorem is true for $c = \gamma$, I say that it will also be true for $c = \gamma + 1$. Indeed, given

$$(a + b) + \gamma = a + (b + \gamma),$$

we can derive successively:

$$[(a + b) + \gamma] + 1 = [a + (b + \gamma)] + 1,$$

or by definition (1):

$$(a + b) + (\gamma + 1) = a + (b + \gamma + 1) = a + [b + (\gamma + 1)],$$

which shows, through a series of purely analytic deductions, that the theorem is true for $\gamma + 1$. Since it is true for $c = 1$, we can thus see that it is successively true for $c = 2$, for $c = 3$, etc.

Commutativity

1° I say that

$$a + 1 = 1 + a.$$

The theorem is obviously true for $a = 1$. Using purely analytic reasoning, we could *verify* that if it is true for $a = \gamma$, then it will be true for $a = \gamma + 1$. As it is true for $a = 1$, it will also be true for $a = 2$, for $a = 3$, etc., which is what we mean when we say that the given proposition is proved by mathematical induction.
2° I say that

$$a + b = b + a.$$

The theorem has just been proved for $b = 1$. We can *verify* analytically that, if it is true for $b = \beta$, it will be true for $b = \beta + 1$. The proposition is thus established by mathematical induction.

Definition of Multiplication

We define multiplication by the equations:

$$a \times 1 = a$$
$$a \times b = [a \times (b-1)] + a.$$

Like equation (1), equation (2) contains an infinite number of definitions. Having defined $a \times 1$, it enables us to define successively: $a \times 2$, $a \times 3$, etc.

Properties of multiplication

Distributivity:

I say that

$$(a + b) \times c = (a \times c) + (b \times c).$$

We verify analytically that the equation is true for $c = 1$ and then that the theorem will be true for $c = \gamma + 1$, if it is true for $c = \gamma$. Again, the proposition is proved by mathematical induction.

Commutativity

1° I say that

$$a \times 1 = 1 \times a.$$

The theorem is obvious for $a = 1$. We verify analytically that, if it is true for $a = \alpha$, it will be true for $a = \alpha + 1$.
2° I say that

$$a \times b = b \times a.$$

The theorem has just been demonstrated for $b = 1$. We could verify analytically that it will be true for $b = \beta + 1$ if it is true for $b = \beta$.

IV

I will stop this monotonous series of reasoning here. However, this monotony itself has better brought out the uniform process that we re-encounter at each step. This process is proof by mathematical induction. First, we establish a theorem for $n = 1$. Next, we show that if it is true for $n - 1$, then it is true for n and, from there, we conclude that it is true for all the natural numbers. We have just seen how it can be used to prove the rules of addition and multiplication, that is, the rules of algebra. Algebra is an instrument of transformation that lends itself to many more different combinations than the simple syllogism, but it remains a purely analytic instrument, unable of teaching us anything new. If mathematics had nothing else, its development would immediately come to a standstill. However, it again draws on the same process that is on mathematical induction, and can continue its march forward. If we look closely, we find this mode of reasoning at each step, either under the simple form we just gave it, or under a more or less modified form. It is therefore truly the mathematical reasoning par excellence and we must examine it more closely.

V

The essential character of mathematical induction is that it contains an infinite number of syllogisms condensed, so to speak, into a unique formula. In order to see this more clearly, I will state one after another these syllogisms that are, if you will pardon the expression, arranged in a "cascade." They are, of course, hypothetical syllogisms.

The theorem is true of the number 1.

If it is true of 1, it is true of 2.

Therefore, it is true of 2.

If it is true of 2, it is true of 3.

Therefore, it is true of 3, and so on.

We see that the conclusion of each syllogism acts as the minor premise of the next one. Moreover, the major premises of all our syllogisms can be reduced to a single formula: if the theorem is true of $n - 1$, it is true of n. We see then that in mathematical induction, we only state the minor of the first syllogism and the general formula containing as particular cases all of the major premises. This unending series of syllogisms is thus reduced to a phrase of a few lines.

It is now easy to understand why any particular consequence of a theorem can, as I have explained above, be verified by purely analytical procedures. If instead of showing that our theorem is true for all numbers, we only wish to show that it is true, for example, of the number 6, it will be sufficient to establish the first five syllogisms of our cascade. We would need nine of them if we wanted to prove the theorem for the number 10. We would need even more for a larger number. Nevertheless, however large this number may be, we will always manage to reach it, making analytic verification possible. And yet, however far we go in this manner, we will never reach the general theorem, applicable to all numbers, which alone can be the object of science. To do so, we would need an infinite number of syllogisms, we would need to cross an abyss that could never be bridged by the patience of the analyst who is restricted solely to the resources of formal logic.

At the outset, I asked why we could never conceive of a mind sufficiently powerful to perceive at a glance the whole body of mathematical truths. The answer is now simple. A chess player can plan four or five moves in advance but, no matter how extraordinary we take him to be, he will always prepare only a finite number of them. If he applies his mental powers to arithmetic, he will not be able to perceive its general truths with a single, direct intuition. To get to the most insignificant theorem, he will not be able to do without the aid of mathematical induction, since it is an instrument that allows us to go beyond the finite to the infinite. This instrument is always useful as it saves us from long, tedious, and monotonous verifications, which would rapidly become impracticable, by enabling us to skip over as many steps as we wish. However, it becomes indispensable as soon as we aim at the general theorem, which we can approach indefinitely, but never reach, through analytic verification.

In this area of arithmetic, we may think that we are still very far from infinitesimal analysis, and yet, as we have just seen, the idea of the mathematical infinite already plays a preponderant role without which there would be no science, because there would be nothing general.

VI

The judgment on which mathematical induction rests can be formulated in other ways. For example, we can say that in an infinite collection of different natural numbers, there is always one that is smaller than all the others. We may readily go from one statement to the other, thus giving ourselves the illusion of having demonstrated the legitimacy of mathematical induction. However, we will always be brought to a standstill, we will always arrive at an unprovable axiom which, in the end, will be nothing but the proposition to be proven translated in another language. We cannot therefore escape the conclusion that the rule of mathematical induction is irreducible to the principle of contradiction. Nor can this rule come to us from experience. What experience could teach us is that the rule is true of the first ten or the first one hundred numbers, for instance. Experience cannot reach the indefinite series of numbers, only a longer or shorter, but always limited, portion of this series. Now, if this were the only problem, the principle of contradiction would be sufficient. It would always allow us to develop as many syllogisms as we wish. It is only when an infinite number of syllogisms must be contained in a single formula, only in the face of the infinite, that this principle fails. It is also at this point that experience becomes powerless. This rule, inaccessible to either analytic proof or experience, is the genuine kind of synthetic *a priori* judgment. Besides, we would not even think of seeing in it a convention, as is the case for some of the postulates of geometry.

Why does this judgment strike us as so compellingly obvious? It is nothing but the affirmation of the power of a mind that knows itself capable of conceiving the indefinite repetition of a particular action as soon as this action is possible. The mind has a direct intuition of this power and uses experience only as an occasion to make use of this power and thereby to become conscious of it.

Still, some will say, if raw experience cannot legitimize mathematical induction, is it the same for experience assisted by induction? We see consecutively that a theorem is true of the number 1, of the number 2, of the number 3, and so on. We say, *the law is manifest*, and for the same reason that any physical law based on a very large, but limited, number of observations is manifest.

We could not fail to recognize that here there is a striking analogy with the usual processes of induction. However, a fundamental difference remains. When applied to the physical sciences, induction is always uncertain because it rests on the belief in a general order of the Universe, an order that lies outside of us. Conversely, mathematical induction, that is proof by recurrence, inevitably stands out as necessary because it is nothing but the affirmation of a property of the mind itself.

VII

As I said earlier, mathematicians constantly strive to *generalize* the propositions they obtain. To seek no further example, above we proved the equation:

$$a + 1 = 1 + a$$

using it thereafter to establish the equation:

$$a + b = b + a,$$

which is obviously more general. Like the other sciences, mathematics can therefore proceed from the particular to the general. Here is a fact which would have bewildered us at the outset of this study, but is no longer mysterious to us since we observed the analogies between proof by mathematical induction and by ordinary induction. Undoubtedly, mathematical induction and physical inductive reasoning rest on different foundations, but their course is parallel; they advance in the same direction—that is from the particular to the general.

Let us examine the matter at hand a little more closely.

To prove the equation:

$$a + 2 = 2 + a, \tag{1}$$

we only need to apply the rule $a + 1 = 1 + a$ twice and write:

$$(2) \quad a + 2 = a + 1 + 1 = 1 + a + 1 = 1 + 1 + a = 2 + a.$$

While equation (2) is thus deduced in a purely analytic manner from equation (1), it is not a particular case of it: it is something different. We cannot then even say that in the genuinely analytic and deductive part of mathematical reasoning we proceed from the general to the particular, in the ordinary sense of the term. The two sides of equation (2) are simply more complicated combinations than the two sides of equation (1) and analysis only serves to separate the elements entering into these combinations and to elucidate their relations.

Mathematicians proceed then "by construction," constructing more and more complicated combinations of elements. Next, through the analysis of these combinations, of these sets so to speak, to their primitive elements, they perceive the relations between these elements and from them deduce the relations of the sets themselves. This is a purely analytic approach and yet it is not an approach from the general to the particular, for the sets can obviously not be regarded as more particular than their elements.

Much importance has been given, and rightly so, to this "construction" procedure that some have tried to see as the necessary and sufficient condition for the progress of the exact sciences. Necessary, undoubtedly—but sufficient? No. In order for a construction to be useful and not a pointless mental effort, for it to act as a stepping stone for those hoping to rise higher, it must first have some kind of unity that would reveal in it something more than the juxtaposition of its elements. Or more exactly, there must be some advantage in considering the construction rather than its elements themselves.

What can this advantage be? For instance, why discuss a polygon—which can always be decomposed into triangles, rather than on the elementary triangles? It is because there are properties that we can prove for polygons with any number of sides and that we can then directly apply to any particular polygon. However, most often it is only by dint of the most prolonged effort that these properties could be recovered through direct study of the relations of the elementary triangles. The knowledge of the general theorem spares us these efforts. A construction therefore becomes interesting only when it can be placed next to other analogous constructions that make up the species of a single genus. A quadrilateral is something beyond the juxtaposition of two triangles in as much as it belongs to the genus polygon. We must still be able to demonstrate the properties of the genus without having to set out the properties of each of the species respectively. To achieve this, we have to work our way back from the particular to the general, climbing up the ladder. The analytic process "by construction" does not require us to delve down, but rather leaves us at the same level. We can ascend only by mathematical induction, for it alone can teach us something new. Without the help of this induction, different from physical induction in some respects but just as productive, construction would be incapable of creating science.

In closing, let us note that induction is possible only if a given operation can be repeated indefinitely, which is why chess theory will never become a science, since the different moves of a given game do not resemble one another.

2

Mathematical Magnitude and Experience

If we want to know what mathematicians mean by a continuum, we should not turn to geometry for the answer. The geometer always more or less tries to visualize[1] the figures studied, but such representations are only instruments. In geometry, one uses extension as one uses chalk. We must therefore be careful not to give too much importance to non-essential attributes that often have no more significance than the whiteness of the chalk. The pure analyst does not have to fear this pitfall. Having cleared mathematical science of all extraneous elements, the analyst can answer our question: "What exactly is this continuum about which mathematicians argue?" Many of them, who know how to reflect upon their art, have already done so: Tannery, for example, in his *Introduction à la théorie des fonctions d'une variable*.[2]

Let us start with the series of whole numbers. Between two consecutive steps, let us insert one or more intermediary steps, then others again between these new steps, and so on indefinitely. We will thus have an unlimited number of terms, the numbers called fractional, rational, or commensurable. But this is still not enough. In between these terms that are already infinite in number, others, called irrational or incommensurable, still must be inserted.

Before continuing, let us make a preliminary comment. The continuum thus conceived is now nothing more than a collection of individuals arranged in a certain order; infinite in number, it is true, but *separated* from each other. This is not the usual conception whereby some intimate connection is supposed between the elements of the continuum, to form a whole in which the point does not exist before the line, but the line before the point. Of the celebrated formula, "the continuum is unity in multiplicity," multiplicity alone remains, unity has disappeared. Analysts are nevertheless right to define their continuum as they do, since it has always been that continuum that they have been working with,

1 The French "se représenter" is translated by "to visualize" or "to imagine" in the text.

2 Jules Tannery, *Introduction à la théorie des fonctions d'une variable* [Introduction to the Theory of Functions of One Variable] (Paris: A. Hermann, 1886).

ever since they started priding themselves on their rigor. Yet, it is enough to warn us that the true mathematical continuum is altogether different from that of the physicists or that of the metaphysicians.

Some will perhaps also say that the mathematicians who make do with this definition are fooled by words, that it would be necessary to say in a precise manner what each of these intermediary steps are, to explain how they must be inserted, and to prove that it is possible to do so. This would be a mistake. The only property of these steps intervening in the analysts' arguments* is that of being found before or after such and such steps; it must therefore also be the only property included in the definition. We therefore do not need to concern ourselves with the way in which the intermediary terms are inserted. Besides, no one will doubt that this operation is possible, unless they forget that in the geometers' language "possible" simply means to be free from contradiction. Our definition is however not yet complete and I now return to it after this overly long digression.

Definition of incommensurables

The Berlin School mathematicians, Kronecker[3] in particular, have been intent on constructing this continuous scale of the fractional and irrational numbers without using anything other than the whole numbers. From this perspective, the mathematical continuum would be a pure creation of the mind in which experience would play no part. Since the notion of rational number did not seem problematic to them, they have mainly endeavored to define the incommensurable number. However, before presenting their definition, I must make an observation so as to forestall the surprise it would no doubt cause to readers who are not very familiar with the habits of geometers.

* With those contained in the special conventions, discussed later, that serve to define addition.

3 In the 1893 article "Le continu mathématique" where this chapter finds its origin (see notes on text and translation) and in the first edition of *Science and Hypothesis* (1902), Kronecker was the only "Berlin mathematician" named in this section. In the later French editions of *Science and Hypothesis*, the name "Kronecker" has been replaced by "Dedekind" in the following three references. This error has unfortunately been reprinted many times, since it appears in Greenstreet's English translation. The first reference seems to be correctly made to Kronecker. The second could be to Kronecker, even though Poincaré changed it to Dedekind. The third and fourth references, on the definition of the irrational numbers, should clearly be to Dedekind. In the German edition of *Science and Hypothesis*, Lindemann comments that Poincaré is actually following the presentation given in Tannery (Poincaré, *Wissenschaft und Hypothese*, Berlin: Xenomoi [1928] 2003: 197). The original 1893 article is available at http://henripoincarepapers.univ-lorraine.fr/bibliohp/?a=on&art=Le+continuath%C3%A9matique&action=go [accessed May 2, 2017].

Mathematicians do not study objects, but relations between objects. It is immaterial to them whether objects are replaced by others, as long as the relations do not change. Matter is of no concern to them, only form is of interest. If we did not keep this in mind, we would not understand how Dedekind designates by the phrase *incommensurable number* a simple symbol, which is something quite different from the idea that we have of a quantity, something that should be measurable and almost tangible.

Dedekind's definition is as follows: "There is an infinite number of ways to divide commensurable numbers into two classes, subject to the condition that any number of the first class be greater than any number of the second class. It may happen that among the numbers of the first class there is one smaller than all the others. For instance, if we put 2 and all the numbers greater than 2 in the first class and all the numbers smaller than 2 in the second class, it is obvious that 2 will be the smallest of all the numbers in the first class. The number 2 might therefore be chosen as the symbol of this partition. It may happen, on the contrary, that among the numbers of the second class, one will be larger than all the others. It is the case, for example, if the first class contains all the numbers greater than 2, while the second contains all the numbers smaller than 2 and 2 itself. Here again the number 2 might be chosen as the symbol for this partition."

However, it can also happen that a number smaller than the others cannot be found in the first class, nor can a number bigger than the others be found in the second class. Suppose, for instance, that we place all commensurable numbers whose square is greater than 2 in the first class and all those whose square is smaller than 2 in the second class. We know that no commensurable number has a square exactly equal to 2. Obviously, in the first class no number will be smaller than all the others since however near to 2 the square of a number is, it is always possible to find a commensurable number whose square is still closer to 2. According to Dedekind's perspective, the incommensurable number $\sqrt{2}$ is nothing but the symbol of this particular mode of partition of the commensurable numbers and thus, to each mode of partition corresponds a number, commensurable or not, that serves as its symbol.

Settling for this would be, however, to ignore the origin of these symbols too much. It remains to explain how we have been led to attribute a sort of concrete existence to them. Besides, does not the difficulty start with the fractional numbers themselves? Would we have any idea of what these numbers are if we were not already familiar beforehand with physical matter that we consider to be infinitely divisible, that is, a continuum?

The physical continuum

We thus come to wonder whether the concept of the mathematical continuum is not simply drawn from experience. If this were so, the raw empirical data that are our perceptions would be measurable, and it may seem so given that attempts have been made recently to measure these perceptions and that even a law, known as Fechner's law, has been formulated according to which perception would be proportional to the logarithm of the stimulation.

However, if we closely examine the experiments with which we sought to establish this law, we will arrive at the opposite conclusion. For instance, it has been observed that a 10-gram weight A and an 11-gram weight B produce identical sensations, and that moreover the weight B cannot be differentiated from a 12-gram weight C, but that weight A and weight C are easily distinguishable. Thus, the raw empirical results may be expressed by the following relations:

$$A = B, \quad B = C, \quad A < C,$$

which may be seen as the expression of the physical continuum.

Here arises an intolerable discord with the principle of contradiction, compelling us to invent the mathematical continuum to alleviate it. We must therefore conclude that this notion has been entirely created by the mind, even though experience provided the occasion. We cannot believe that two quantities equal to a third one are not equal to one another and we are thus led to suppose that A is different from B, and B from C, but that the imperfection of our senses prevented us from distinguishing them from one another.

Creation of the mathematical continuum

First stage

Up to now, we might manage to account for the facts by inserting a small number of discrete terms between A and B. What happens now if we employ some instrument or other to compensate for our senses' imperfection, if, for example, we use a microscope? Terms that were previously undistinguishable, such as A and B above, now appear distinct. Nevertheless, a new term D, indistinguishable from either A or B, can be inserted between the now distinct A and B. Despite the use of the most sophisticated methods, our raw empirical data will always present the characteristics of the physical continuum with its inherent contradiction. We could escape it only by continually inserting new terms between those previously distinguished, an operation that would have to be carried on indefinitely. We could only conceive of bringing an end to it if we could conceive of an instrument powerful enough to break down the physical continuum into discrete elements, just as the telescope resolves the Milky Way into stars. But we could never imagine this because it is always with our senses that we make use of our instruments. It is with the eye that we observe an image magnified by the microscope and this image must therefore always retain the characteristics of visual sensation and therefore those of the physical continuum.

Nothing distinguishes a directly observed length from half of the length doubled by the microscope. The whole is homogenous to the part, which is a new contradiction or, rather, it would be one if the number of terms were considered to be finite. It is clear that the part containing fewer terms than the whole could not be similar to the whole. The contradiction disappears as soon as the number of terms is considered infinite. For instance, nothing keeps us from considering the set of whole numbers to be equinumerous with the set of even numbers despite the fact that the former is only a part of the latter and, indeed, for each whole number there is a corresponding even number, which is its double. However, it is not simply to escape this contradiction in the empirical data that the mind is drawn to create the concept of a continuum consisting of an indefinite number of terms. Everything works out as with the whole number series. We have the ability to understand that a unit can be added to a collection of units. It is thanks to experience that we have the chance to exercise this faculty and that we become aware of it. We then immediately feel that our ability is unlimited and that we could count indefinitely, even though we have only had to count a finite number of objects. Likewise, as soon as we insert the intermediaries between two consecutive elements in a series, we feel that this operation could be carried on limitlessly and that there is, so to speak, no intrinsic reason to stop.

For brevity's sake, let me call any set of terms formed according to the same law as the scale of commensurable numbers the first-order mathematical continuum. If we then insert new steps, following the law of formation of the incommensurable numbers, we will obtain what we shall call a second-order continuum.

Second stage

We have still only taken our first step. Having explained the origin of the first-order continuums, it is now necessary to see why they were still insufficient and why the incommensurable numbers had to be invented. If we want to imagine a line, it can only be with the characteristics of the physical continuum, which is to say that we can only imagine it with a certain width. Two lines will therefore appear to us as two narrow bands and, if we settle for this rough image, it is obvious that if the two lines cross, they will have a part in common. The pure geometer, however, makes one further effort: without completely renouncing the help of the senses, the geometer tries to get at the concept of a line without breadth, of the point without extension. This can be achieved only by considering the line as the limit towards which tends an increasingly narrow band, and the point as the limit towards which tends an increasingly small area. Thus, however narrow our two bands may be, they will always have an area in common, all the smaller as they become narrower, and its limit will be what the pure geometer calls a point. This is why we say that two intersecting lines share a common point, a truth that seems intuitive, but would entail a contradiction if lines were conceived of as first-order continuums, that is, if only points having rational numbers for coordinates were found on the lines drawn by the geometer. The contradiction would become obvious as soon as we posited, for example, the existence of straight lines and circles. Clearly, if only the points with commensurable coordinates were considered real, the circle inscribed in a square and the diagonal of this square would not intersect, since the coordinates of the intersection point are incommensurable. It would still be insufficient, since we would then have only certain incommensurable numbers rather than all these numbers.[4]

However, let us imagine a straight line divided into two rays, each of which will appear to our mind as a band of a certain breadth. As there must be no distance between them, these bands will overlap. The part in common will look like a point to us, always persisting as we attempt to imagine our bands becoming ever narrower, so that we will admit as an intuitive truth that when a straight line is divided into two rays, their common boundary is a point. We recognize here Kronecker's formulation,[5] by which an incommensurable number was conceived as the common boundary of two classes of rational numbers. Such is the origin of the second-order continuum, which is the mathematical continuum *per se*.

4 Something appears to be missing here, given that it is unclear which incommensurable numbers have been added.

5 Here Kronecker appears in all the French editions of *Science and Hypothesis*, but not in the original 1893 article from which the section is taken. Halsted changed the reference to Dedekind, which seems correct, while Lindemann simply removes the reference altogether.

Summary

In conclusion, the mind has the ability to create symbols and it is in this way that it has created the mathematical continuum, which is only a particular system of symbols. Its capacity is limited only by the necessity to avoid all contradictions, but the mind makes use of it only when experience provides some reason to do so. In the case that concerns us, this reason was the notion of a physical continuum, derived from the raw data of the senses. However, this notion leads to a series of contradictions which we must avoid one by one. We are thus forced to imagine an ever more complicated system of symbols. The one on which we settle is not only free of internal contradiction—as was the case in each of the steps already surmounted—but it is also not in contradiction with many of the so-called intuitive propositions obtained from more or less sophisticated empirical notions.

Measurable magnitude

The magnitudes we have studied so far were not *measurable*. We can tell whether any of these magnitudes is larger than any other, but we cannot say whether it is two or three times larger. Up to this point, I have focused only on the order in which our terms are arranged. However, this is not sufficient for most applications. We must learn how to compare the interval separating any two terms. It is only on this condition that the continuum becomes a measurable magnitude to which arithmetical operations can be applied.

Only with the help of a new and special *convention* can we achieve this. We will *agree to stipulate* that in a given case, the interval defined by the terms A and B is equal to the interval separating C and D. For example, at the beginning of our study, we started with the series of whole numbers and took as a given that, between two consecutive steps, *n* intermediary ones were inserted. So, these new steps will be regarded as equidistant by convention.

Thus we have a way to define the addition of two magnitudes since, if interval AB is by definition equal to interval CD, then interval AD will be by definition equal to the sum of intervals AB and AC. This definition is in very large measure arbitrary, and yet, not completely so. It is subject to certain conditions and, for example, to the rules of commutativity and associativity of addition. As long as the chosen definition satisfies these rules, the choice is immaterial and it is unnecessary to make it more precise.

Various remarks

We can ask ourselves many important questions:

1° Is the creative capacity of the mind exhausted by the creation of the mathematical continuum? No, as the work of du Bois-Reymond strikingly demonstrates. We know that mathematicians distinguish different orders of infinitesimals, and that those of the second order are not only infinitely small in an absolute manner, but even with respect to those of the first order. It is not difficult to imagine infinitesimals of a fractional or even an irrational order, and thus we re-encounter again the scale of the mathematical continuum discussed in the preceding pages.

Furthermore, some infinitesimals are infinitely small with respect to those of the first order, but are, on the contrary, infinitely large with respect of those of order $1 + \varepsilon$, however small ε may be. Here again new terms are inserted into our series and, if I may be allowed to come back to the language I used earlier, which is rather useful despite not being commonly used, I would say that we have thus created a kind of third-order continuum. It would be easy to go further, but this would be a pointless mental exercise. One would only imagine symbols with no possible application and no one would take on that challenge. The third-order continuum to which the reflections upon the various orders of infinitesimals led is itself of too little use to be fully warranted and geometers only regard it as a mere curiosity. The mind uses its creative faculty only when experience requires it.

2° Once we have the concept of the mathematical continuum, can we steer clear of contradictions analogous to those that gave rise to it? No, and I will give an example. One must be quite astute not to consider it obvious that every curve has a tangent. In fact, when we imagine such a curve and a straight line as two narrow bands, it is always possible to arrange them so that they will share a common part, despite never intersecting. Imagine then the width of these two bands diminishing indefinitely. This common part can always continue to exist and at the limit, so to speak, the two lines will have a point in common without intersecting, that is to say, they will touch. The geometer who would think in this way, whether consciously or not, would not do anything different from what we did above to demonstrate that two intersecting lines share a common point, an intuition that could seem just as legitimate, but would be misleading. We can prove that some

> curved lines have no tangent when the curve is defined as a second-order analytic continuum. Undoubtedly, some trick comparable to those we studied above could have lifted the contradiction. However, as the latter is encountered only in very rare cases, it was of little concern. Rather than trying to reconcile intuition and analysis, it was simpler to sacrifice one of the two and, since analysis must remain infallible, the blame was laid on intuition.

The multidimensional physical continuum

I have examined above the physical continuum as it emerges from our immediate sensory data or, if you like, from the raw results of Fechner's experiments. I showed that these results are summed up in the contradictory formulas:

$$A = B, \quad B = C, \quad A < C.$$

Let us now see how this notion was generalized and how the concept of multidimensional continua may have sprung from it. Consider any two sets of sensations. Either we will be able to tell one from the other or we won't, just as in Fechner's experiments a 10-gram weight could be distinguished from a 12-gram weight, but not from an 11-gram one. I do not need anything else to construct the multi dimensional continuum.

Let us call *factor* one of these groups of sensations. It will be something like the *point* of mathematicians, but not altogether the same thing. We cannot say that our factor is without extension, since we do not know how to distinguish it from the neighboring factors, and it is therefore surrounded by a kind of fog. If I may offer an astronomical analogy, our "factors" would be like nebulae, whereas mathematical points would be like stars.

Supposing the above, a system of factors will form a *continuum* if we can go from any one of them to any other through a series of consecutive factors connected to one another in such a way that none can be distinguished from the preceding one. This *chain*[6] is to the *line* of the mathematician what an isolated *factor* was to the point.

Before going any further, I must explain what a *cut* is. Consider a continuum C and remove from it some of its factors which, for an instant, we will regard as

[6] The term "série linéaire" (linear series) used in the first French edition was later replaced by "chaîne" (chain).

no longer belonging to this continuum. The group of factors thus removed will be called a cut. It may be that, thanks to this cut, C could be *subdivided* into many distinct continuums, the group of the remaining elements ceasing to form a unique continuum. In such a case, there will be on C two factors, A and B, which will have to be regarded as belonging to two distinct continuums, which we will recognize given that it will be impossible to find a *chain* of consecutive factors belonging to C [starting from A and going to B in such a way that each factor will be indiscernible from the previous one][7], *unless one of the factors of this chain is indiscernible from one of the factors of the cuts* [*and consequently has to be excluded*].[8]

Conversely, the cut made may be insufficient to subdivide the continuum C. To classify the physical continua, we will examine precisely which cuts must be made in order to subdivide them. If we can subdivide a physical continuum C by a cut equivalent to a finite number of factors, all discernible from one another (and therefore forming neither one continuum, nor several continua), we will say that C is a *one-dimensional* continuum. If, on the contrary, C can be subdivided only by cuts which are themselves continua, we will say that C has many dimensions. If cuts that are one-dimensional continua are sufficient, we will say that C is two-dimensional. If two-dimensional cuts are sufficient, we will say that C is three-dimensional, and so on. The notion of multidimensional continua is thus defined, thanks to the very simple fact that two groups of sensations can be either discernible or indiscernible.

The multidimensional mathematical continuum

The notion of the *n*-dimensional mathematical continuum sprung quite naturally through a process quite like the one studied at the beginning of this chapter. As we know, a point from such a continuum appears to us as defined by a system of *n* distinct magnitudes, which we call its coordinates. These magnitudes need not always be measurable. For example, there is a branch of geometry in which we ignore the measurement of these magnitudes, where we are concerned only with knowing, for example, whether on a curve ABC, the point B is between the points A and C, but not whether the arc AB is equal to the arc BC, or whether

7 This text replaces the following text in parentheses found in the first French edition: "(each of these elements being indistinguishable from the preceding one, the first being A and the last B) (1902: 46)".

8 This is not in the first edition.

it is twice as long. This is what we call topology,[9] an entire theoretical system that has attracted the attention of the greatest geometers and in which we see a series of remarkable theorems springing from one another. What differentiates these theorems from those of ordinary geometry is that they are purely qualitative and would remain true if the figures were copied by a poor draftsman who would grossly distort their proportions and would replace the straight lines with more or less curved ones. With the introduction of measurement in the continuum defined above, the continuum became space and geometry was born; a discussion that I will reserve for Part Two.

[9] Instead of our modern term "topology," Poincaré uses the term "*analysis situs*." He was instrumental in the development of this branch of geometry with his 1895 seminal article, entitled *Analysis situs* (*Journal de l'École Polytechnique* 1 (1895): 1–121.) and its supplements. John Stillwell's translation of these papers can be found at: http://www.maths.ed.ac.uk/~aar/papers/poincare2009.pdf [accessed May 2, 2017]. The French original can be found at: http://gallica.bnf.fr/ark:/12148/bpt6k4337198/f7.image [accessed May 2, 2017].

Part Two

Space

3

Non-Euclidian Geometries

Every conclusion supposes premises that are themselves either self-evident and do not need to be proven or cannot be grounded without the support of other propositions. Since we cannot go back indefinitely in order to ground premises, all deductive science—and geometry in particular—has to rest on a certain number of unprovable axioms. Therefore, all geometry texts start with the statement of these axioms, but there is an important distinction to make between them. Some axioms, like this one for example: "two quantities that equal a third one are themselves equal," are not geometrical propositions but analytic propositions. I regard them as *a priori* analytic judgments and will refrain from considering them.

I must emphasize, however, other axioms that are unique to geometry. Most texts mention three of them explicitly:

1° Only one straight line can be drawn through two points.
2° A straight line is the shortest path between two points.
3° Through a point, only one parallel to a given line can be drawn.

Even though we generally dispense with the proof of the second of these axioms, it would be possible to derive it from the other two, and from the much more numerous ones that we accept without naming, as I will explain later.

We have tried at length and in vain to prove the third axiom, known as Euclid's postulate. The effort expended on this unattainable goal is truly unimaginable. At last, at the beginning of the century and about at the same time, two scientists, a Russian and a Hungarian, Lobachevskii and Bolyai, established irrefutably that this proof is impossible.[1] They more or less rid us of

[1] That is, the nineteenth century, as this chapter was initially published in 1891 in the *Revue générale des sciences*. See also N. Lobachevskii, *Geometrical Researches on the Theory of Parallels*. Transl. G. B. Halsted (Austin: University of Texas, 1891) (available at: https://archive.org/details/geometricalresea00loba [accessed May 2, 2017]); and J. Boylai, *The Science Absolute of Space Independent of the Truth or Falsity of Euclid's Axiom XI* (*which can never be decided* a priori). Transl. G. B. Halsted (Austin: The Neomon, 1896) (available at: https://archive.org/details/scienceabsolute00bolyrich [accessed May 2, 2017].

the inventors of geometries that attempt to do away with this postulate. Since that time, the Academy of Sciences receives annually no more than one or two new proofs.

The question was not exhausted and it was not long before a great advance was made with the publication of a famous paper by Riemann, entitled: *Über die Hypothesen welche der Geometrie zum Grunde liegen*.[2] This short text has inspired the majority of the recent publications that I will discuss later, among which should be cited, those of Beltrami and Helmholtz.

Lobachevskiian geometry

If it were possible to derive Euclid's postulate from the other axioms, we would run into contradictory consequences when rejecting the postulate while accepting the other axioms. It would then be impossible to base a coherent geometry on such premises. Yet this is exactly what Lobachevskii did. At the outset, he supposes that:

Through a point, we can draw several lines parallel to a given straight line

Otherwise, he retains all of Euclid's other axioms. From these hypotheses, he derives a series of theorems in which it is impossible to find a contradiction and he constructs a geometry the impeccable logic of which is equal to that of Euclidean geometry. The theorems, of course, are very different from those that we are used to and they do unsettle us a bit at first.

The sum of the angles of a triangle is thus always smaller than two right angles and the difference between this sum and two right angles is proportional to the area of the triangle. It is impossible to construct a figure similar to a given figure but with different dimensions. If we divide the circumference of a circle into n equal parts and we draw tangents at each division point, these n tangents will form a polygon if the ray of the circumference is rather small, but if the ray is rather large, the tangents will not intersect.

It is useless to supply more examples. Lobachevskii's propositions do not relate to Euclid's anymore, but they are no less logically connected to one another.

2 Bernhard Riemann, *On the Hypotheses which Lie at the Bases of Geometry*. Ed. J. Jost (Cham: Springer International Publishing: Imprint: Birkhäuser, 2016).

Riemann's geometry

Let us imagine a world populated only by beings with no depth and let us suppose that these "infinitely flat" animals are all in the same plane from which they cannot escape. Let us also suppose that this world is far enough from the other ones to be free of their influence. While we are in the process of making these hypotheses, it is no more trouble for us to endow these beings with the ability to reason and to imagine them to be capable of doing geometry. They will certainly attribute only two dimensions to space in this situation.

Let us now suppose these imaginary animals as having a spherical rather than a flat form while still having no thickness. They are all on a single sphere they cannot leave. What geometry will they be able to construct? It is clear first that they will only attribute two dimensions to space. For them, it is the shortest distance between one point on the sphere and another that will play the part of a straight line, that is an arc of a great circle. In brief, their geometry will be spherical geometry. The inescapable sphere, where all phenomena they can know are to be found, will be what they call space. Their space will then be *limitless*, since on the surface of a sphere one can always go forward without stopping, and yet it will be finite. Even though it is possible to go all the way around this world, it will remain impossible to find its end.

So, Riemannian geometry is spherical geometry extended to three dimensions.[3] In order to construct it, the German mathematician had to throw overboard not only Euclid's postulate but the first axiom as well: *Only one straight line can be drawn through two points.*

On a sphere, one can *in general* draw only one great circle through two given points (keeping in mind that, as we just saw, the great circle would play the role of a straight line for our imaginary beings). There is, however, an exception, which is that if the two given points are diametrically opposed, an infinite number of great circles could be drawn through these two points. Similarly, in Riemann's geometry in general (at least in one of its forms,) only a single straight line can be drawn through two points, although there are exceptional cases where an infinite number of straight lines can be drawn through two points.

A sort of opposition exists between Riemann's geometry and that of Lobachevskii. The sum of the angles of a triangle is equal to two right angles in

[3] So far, Poincaré has discussed only spherical geometry in two dimensions. Riemannian geometry usually refers to elliptical geometry, as here, in three dimensions.

Euclidean geometry, smaller than two right angles in Lobachevskiian geometry, and greater than two right angles in Riemannian geometry.

The number of parallels that can be drawn to a given straight line through a given point is equal to one in Euclidean geometry, equal to zero in Riemannian geometry, and equal to infinity in that of Lobachevskii. Let us add that Riemannian space is finite, although limitless in the sense given above.

Surfaces with a constant curvature

One possible objection remained, however. The theorems of Lobachevskii and of Riemann present no contradictions, but no matter how many conclusions these two geometers drew from their hypotheses, they had to stop before exhausting all of them, for their number would be infinite. Who can tell us then whether they would have ended up finding some contradiction if they had pushed their deductions farther?

This problem does not exist for Riemannian geometry, provided that we limit ourselves to two dimensions. In fact, two-dimensional Riemannian geometry does not differ, as we have seen, from spherical geometry, which is simply a branch of ordinary geometry and which is therefore not under discussion here.

Beltrami refuted the objection for the two-dimensional Lobachevskiian geometry by similarly bringing it back simply to a branch of ordinary geometry. Here is how he managed to do so. Let us consider any figure on a surface and let us imagine that this figure is traced on a flexible and unstretchable cloth applied to this surface, such that when the cloth moves and changes shape, the various lines of the figure can change shape, without changing in length. In general, this flexible and unstretchable figure cannot move without leaving the surface, but there are certain unusual surfaces for which such a movement would be possible. These are the surfaces of constant curvature.

If we take another look at the comparison we were making above and we imagine beings without depth living on one of these surfaces, these beings would consider possible the movement of a figure whose lines all retain a constant length. By contrast, the same movement would seem absurd to animals without depth living on a surface with variable curvature.

These surfaces of constant curvature are of two types. The first surfaces have *positive curvature* and can be deformed in such a way as to be applied on a sphere. The geometry of these surfaces is thus reducible to spherical geometry, that of Riemannian. The other surfaces have *negative curvature*. Beltrami showed that the geometry of these surfaces is none other than that of Lobachevskii. The two-dimensional geometries of Riemann and Lobachevskii are therefore embedded in Euclidean geometry.

Interpretation of non-Euclidean geometries

In this way we lay to rest the objection to two-dimensional geometries. It would be easy to extend Beltrami's arguments to three-dimensional geometries. Thinkers not averse to four-dimensional space will see no problem here, but they are few in number, so I prefer to approach the problem in a different way.

Let us consider a particular plane that I will call a fundamental plane and let us construct a sort of dictionary, matching up in one-to-one correspondence terms from two series, written in two columns just as in ordinary dictionaries, where entries from the two languages having the same meaning are put in correspondence:

Space . . .	Portion of space situated above the fundamental plane.
Plane . . .	Sphere that intersects the fundamental plane at a right angle.
Straight line . . .	Circle that intersects the fundamental plane at a right angle.
Sphere . . .	Sphere.
Circle . . .	Circle.
Angle . . .	Angle.
Distance between two points . . .	Logarithm expressing the cross ratio of these two points and of the intersection of the fundamental plane with the circle going through these two points and intersecting it at a right angle.
Etc. . . .	*Etc. . . .*

Let us then take Lobachevskii's theorems and translate them with the help of this dictionary as we would translate a German text with the help of a German–French dictionary. *In this way we will get the theorems of ordinary geometry.* For example, Lobachevskii's theorem, "The sum of the angles of a triangle is less than two right angles" is translated as follows: "If a curved triangle whose sides are the arcs of circles that when extended would intersect the fundamental plane at right angles, the sum of the angles of this curved triangle will be smaller than two right angles." Thus, no matter how far we push the consequences of Lobachevskii's hypotheses, we will never fall into a contradiction. In fact, if two of Lobachevskii's theorems were contradictory, the translations of these two theorems made with the help of our dictionary of these two theorems would also be contradictory. However, these translations are theorems of ordinary geometry and no one doubts that ordinary geometry is free of contradictions. Where does our certitude come from and is it justified? This is a question that I cannot address here, for it would require some elaboration.[4] So nothing remains of the objection that I formulated above.

There is still more to consider. Amenable to a concrete interpretation, Lobachevskiian geometry is no longer a vain exercise in logic and can have some practical applications. I do not have the time to mention these applications nor how Klein and I put it to good use in the integration of linear equations.

Besides, this interpretation is not unique and we could make several dictionaries comparable to the preceding one that would all allow us by means of a simple "translation" to transform Lobachevskiian theorems into theorems of ordinary geometry.

Implicit axioms

Are axioms explicitly stated in treatises the only basis of geometry? We can be assured of the contrary since even after we abandoned them one by one, we see that some propositions common to the theories of Euclid, Lobachevskii and Riemann are still left standing. These propositions must rest on some premises that geometers accept without explicitly stating. It is interesting to extract them from classical proofs.

John Stuart Mill claimed that every definition contains an axiom, since the existence of the defined object is implicitly affirmed when it is defined. This is taking things too far. It is unusual in mathematics to provide a definition without following it up with a proof of the existence of the defined object, and when the proof is skipped over, it is generally because the reader can easily fill in what is missing. We should not forget that the word existence does not have the same meaning when referring to a mathematical entity as it does when referring to a physical object. A mathematical entity exists, provided that its definition does

4 The text of the first edition reads instead "This is a question that I cannot answer here, but that is quite interesting and, I believe, not unsolvable" (Poincaré 1902: 58).

not imply a contradiction, either intrinsically or with respect to previously accepted propositions.

If Mill's observation cannot be applied to all definitions, it is nevertheless true for some. We sometimes define the plane in the following way: The plane is a surface such that the straight line that joins any two of its points lies entirely on this surface. This definition obviously conceals a new axiom. While it is true it would be better to change this definition, we would then have to state the axiom explicitly.

Other definitions can give rise to equally important considerations as, for example, the question of the equality of two figures. Two figures are equal when we can superimpose them, but to do so we have to move one of them until it coincides with the other. How should it be moved? If we asked the question, our answer would certainly be that it should be moved without deforming it and in the same way as that of a rigid body.[5] The vicious circle would then be obvious.

This definition does not in fact define anything and would make no sense for a being that lived in a world where there were only fluids. If it seems clear to us, it is because we are used to the properties of natural solids that do not differ much from ideal solids whose dimensions are all invariable. However, as imperfect as it may be, this definition implies an axiom. It is not a self-evident truth that an invariable figure can move, at least it is only evident in the way of Euclid's postulates, not in the way of an analytic *a priori* judgment.

Furthermore, in studying the definitions and proofs of geometry, we see the need to accept without proof not only the possibility of this movement, but also some of its properties. We can see this above all in the definition of a straight line. Many inadequate definitions have been given, but the correct one is that implicitly understood in all of the proofs where the straight line plays a role: "It can happen that the movement of an invariable figure is such that all of the points on a line belonging to that figure stay still while all the points outside this line move. Such a line will be called a straight line." In this statement, we have expressly separated the definition from the axiom that it implies.

Many proofs, such as the proofs of the equality of triangles and that of the possibility of dropping a perpendicular line from a point on a straight line, presuppose propositions that we do not state, because they force us to accept that it is possible to move a figure through space in a particular way.

[5] We translate "solide invariable" by "rigid body."

The fourth geometry[6]

Among these implicit axioms, there is one that deserves some attention, because abandoning it would allow us to build a fourth geometry that would be as coherent as those of Euclid, Lobachevskii, and Riemann. To prove that we can always draw a perpendicular to the straight line AB at point A, we consider a line AC movable around point A and originally superimposed on the fixed straight line AB and we rotate it around point A until it lies in the prolongation of AB.

Two propositions are thus assumed: first, that such a rotation is possible, and then that it may be continued until the two straight lines become the prolongation of one another. If we accept the first point and reject the second, we will end up with a series of theorems that are even stranger than those of Lobachevskii and Riemann, although equally free of contradiction. I will cite only one of these theorems and not the oddest of them at that: *A real straight line can be perpendicular to itself.*

Lie's theorem

The number of axioms implicitly introduced in classic proofs is larger than necessary and effort has been made to reduce that number to a minimum. Hilbert seems to have provided the definitive solution to the problem.[7] We could first ask ourselves *a priori* if this reduction is possible, if the number of necessary axioms and the number of imaginable geometries is infinite. A theorem of Sophus Lie is central to this question, and we can state it in this way:

Suppose that we allow the following premises:

1° Space has n dimensions.
2° The movement of an invariable figure is possible.
3° We need p conditions to determine the position of this figure in space.

[6] In the original article, Poincaré mentions the work of Charles Bernard Renouvier, Auguste Calinon, and Georges Lechalas (see Anastasios Brenner, "Géométrie et genèse de l'espace selon Poincaré," *Philosophiques* 31 (1) (2004): 115–30). Poincaré also seems to be referring to his work on hyperbolic geometry on one sheet as developed in his article "Sur les hypothèses fondamentales de la géométrie," *Bulletin de la Société Mathématique de France* 15 (1887): 203–16. This geometry has been identified with a two-dimensional Minkowskian geometry by, for example, John Stachel, "Albert Einstein: A Man for the Millennium?" in *AIP Conference Proceeding 861*, Eds. J.-M. Alimi and A. Füzfa. Melville: NY: American Institute of Physics, 2005, 211–44, but Shlomo Sternberg has argued that Poincaré's "fourth geometry" is in fact a two-dimensional version of the closely related De Sitter space (see Philippe Nabonnand "La 'quatrième géométrie' de Poincaré," *Gazette des Mathématiciens, Société Mathématique de France* (2012):76–86 (https://hal.archives-ouvertes.fr/hal–01081806/ [accessed May 2, 2017]).

[7] This passage replaces the following one from the first French edition: "[...] and it would be interesting to reduce it to a minimum" (1902: 62).

The number of geometries compatible with these premises will be limited. I might even add that if n is given, p can be assigned a higher limit. If we then accept that movement is possible, we can only invent a finite (and even rather limited) number of three-dimensional geometries.

Riemann's geometries

However, Riemann seems to contradict this conclusion, since he develops an infinite number of different geometries of which the one that we refer to normally with his name is only one particular example. According to him, everything depends on the way in which one defines the length of a curve. There is an infinite number of ways to define this length and each one can become the starting point for a new geometry. While this is absolutely correct, most of these definitions are incompatible with the movement of an invariable figure that is considered possible in Lie's theorem. Riemann's geometries, interesting as they are in a number of ways, could only ever be purely analytic and do not lend themselves to proofs similar to Euclid's.

Hilbert's geometries[8]

Finally, Veronese and Hilbert imagined even stranger new geometries that they called *non-Archimedean geometries*. They constructed them after rejecting *Archimedes' axiom* by which any given length multiplied by a sufficiently large whole number will end up exceeding any other length, no matter how long it may be. All points of our ordinary geometry exist on a non-Archimedean straight line, but there are an infinite number of other points that fit in between them, such that between two segments old school geometers would have considered contiguous, we can fit an infinite number of new points. In brief, to use the terminology of the preceding chapter, non-Archimedean space is no longer a second-order continuum but rather a third-order continuum.

On the nature of axioms

Most mathematicians see Lobachevskii's geometry as a mere logical curiosity, although some have gone further. Since many geometries are possible, is it

[8] This section is absent from the early French editions as well as from the previous English translations.

certain that ours is the right one? Admittedly, experiments show us that the sum of the angles of a triangle is equal to two right angles, but that is because we are working on triangles that are too small. The difference, according to Lobachevskii, is proportional to the area of the triangle. Might the difference become detectible if we work on larger triangles or if the measurements become more precise? Euclidean geometry would then be just a provisionary geometry.

In order to consider this view, we must first enquire as to the nature of geometrical axioms. Are they synthetic *a priori* judgments, as Kant said? They would then seem so self-evident that we would not be able to conceive of the opposite proposition, nor could we base a theoretical construct on them. There would be no non-Euclidean geometry. To be convinced, take a synthetic *a priori* judgment, for example this one, whose preponderant role we saw in Chapter 1:

If a theorem is true for the number 1 and if it has been proven that it is true of n + 1, provided that it is true of n, it will be true of all of the positive whole numbers

Next, try to escape and reject this proposition to found a false arithmetic analogous to non-Euclidean geometry; we will never succeed. We would even be tempted in response to consider these judgments as analytic ones.

Besides, let us reconsider our imaginary animals with no depth. We can scarcely accept that these beings, if they have a mind like ours, would adopt Euclidean geometry which would be contradicted by all their experience.

Should we conclude that the geometrical axioms are experimental truths? We do not experiment on ideal straight lines or ideal circumferences, but rather only on physical objects. On what would geometry's founding experiments bear? The answer is simple. We saw above that we always think as if geometrical figures behaved like solids. The properties of these bodies are then what geometry would borrow from experiment.

The properties of light and of its rectilinear propagation have also occasioned the appearance of some of these geometric propositions, especially propositions concerning projective geometry, such that we would be tempted to say that metric geometry is the study of solids while projective geometry is the study of light.

A difficulty remains, however, and it is unsurmountable. If geometry were an experimental science, it would not be an exact science but rather one subject to continual revision. No, it would be sure of being incorrect from now on, since we know that there are no perfectly rigid solids.

Geometric axioms are then neither synthetic a priori *judgments, nor experimental facts*

They are *conventions*. Among all possible conventions, our choice is *guided* by experimental facts, but it remains *free* and is only limited by the need to avoid all contradiction. In this way, the postulates can remain *strictly* true even when the experimental laws contributing to their adoption are only approximate. In other words, *the axioms of geometry* (I am not speaking of those of arithmetic) *are only definitions in disguise.*

At this point, how should we understand this question: Is Euclidean geometry true? The question does not make any sense. One might just as well ask if the metric system is true and the old units of measure false; if the Cartesian coordinates are true and the polar coordinates false. One geometry cannot be truer than another, it can only be *more useful.*

Euclidean geometry is now and will remain the most useful geometry:

1° Because it is the simplest, and it is the simplest not only as a result of our habitual ways of thinking or of some direct intuition that we might have of Euclidean space. It is the simplest in itself in the same way that a first-degree polynomial is simpler than a second-degree polynomial. [The formulas of spherical trigonometry are more complicated than those of plane trigonometry and they would seem even more complicated to an analyst who would not know their geometric meaning.][9]

2° Because it agrees rather well with the properties of natural solids, those bodies akin to our limbs and our eyes and with which we fashion our measuring instruments

[9] The text in brackets was not in the first edition.

4

Space and Geometry

Let us start with a little paradox. Beings with minds like ours and with the same senses as we have, but without any prior education, could receive from a suitably chosen external world such impressions that they would be led to construct a geometry other than that of Euclid and to locate the phenomena of this external world in a non-Euclidean space or even in a four-dimensional one. As for us, whose education has been formed by our actual world, if we were to be transported suddenly to this new world, we would have no difficulty relating its phenomena to our Euclidean space. [Conversely, if these beings were transported here, they would be led to relate our phenomena to non-Euclidean space. Indeed, with some effort we could do it too.][1] Someone who devoted their existence to the task might be able to visualize the fourth dimension.

Geometrical space and representative space

It is often said that the images of external objects are located in space, or even that they can form only under this condition. It is also said that this space, which acts therefore as a ready-made *framework* for our sensations and representations, is identical to the geometers' space, possessing all of its properties. To all clear-minded thinkers who hold this true, the preceding sentence must have appeared quite extraordinary. However, we should make sure that they do not fall victim to some illusion that could be cleared-up by careful analysis. To begin, what are the properties of space itself, by which I mean the space which is the object of geometry and which I will call *geometrical space*? Here are some of the more essential ones:

1. It is continuous.
2. It is infinite.
3. It has three dimensions.

[1] The text in brackets is not in the first French edition.

4. It is homogenous, which is to say that all its points are identical to one another.
5. It is isotropic, which is to say that all straight lines going through the same point are identical to one another.

Let us now compare geometric space to the framework of our representations and sensations, which I could call *representative space*.

Visual space

Let us first consider a purely visual impression caused by an image forming on the back of the retina. A cursory analysis presents this image as continuous, but only two dimensional, which already distinguishes geometrical space from what we could call *pure visual space*. Moreover, this image is confined within a limited framework. Finally, there is another equally important difference: *this pure visual space is not homogenous*. The points of the retina, disregarding images that can form there, do not all play the same role. On no account can the yellow spot be considered identical to a point on the edge of the retina. In fact, not only does the same object create more vivid impressions there, but in any *limited* framework, the point at the center of the framework will not appear to be identical to a point near one of its edges. Without a doubt, a more thorough analysis would reveal that this continuity of visual space and its two dimensions, are nothing more than an illusion, thus separating visual space even more from geometrical space, but let us leave aside this remark [whose consequences have been sufficiently examined in Chapter 2].[2]

Nevertheless, sight allows us to estimate distances and consequently to perceive a third dimension. Everyone knows that this perception of the third dimension can be reduced to the feeling associated with the effort of focusing that must be made, and to the feeling of the convergence that must be given to both eyes, in order to see an object distinctly. These are muscular sensations completely different from the visual sensations that gave us the concept of the first two dimensions. Thus, the third dimension will not appear to us as playing the same role as the other two. What we might call *complete visual space* is not then an isotropic space. True, complete visual space has precisely three dimensions, which means that the elements of our visual sensations (or at least those participating in the formation of the notion of extension) will be completely

[2] This passage is not in the first French edition.

defined when three of them are known. In mathematical language, they will be functions of three independent variables.

Upon examining the question more closely, the third dimension is revealed to us in two different ways: through the effort of focusing and through the eyes' convergence. Undoubtedly, these two sources of information always concur. There is between them a constant relationship or, in mathematical terms, the two variables measuring these two muscular sensations do not appear independent to us. Or again, to avoid any appeal to already rather subtle mathematical notions, we could return to the language of Chapter 2[3] and state the same fact as follows: If two sensations of convergence A and B are indiscernible, the two sensations of accommodation A′ and B′ that will respectively accompany them will also be indiscernible. However, this is, so to speak, an experimental fact. Nothing prevents us from supposing *a priori* the opposite and, if the opposite does occur, if these two muscular sensations vary independently of one another, we will have to take into account an additional independent variable and "complete visual space" will appear to us as a four-dimensional physical continuum. I will add that it is even a fact of *external* experience. Nothing prevents us from imagining that a being with a mind like ours, having the same sensory organs as we do, is placed in a world where light would reach it only after passing through some refractive mediums having complicated shapes. The two sources of information we use to evaluate distances would no longer be linked to one another by a constant relationship. A being training its senses in such a world would undoubtedly attribute four dimensions to complete visual space.

Tactile space and motor space

"Tactile space" is even more complex than visual space and distances itself further from geometric space. The remarks I made on sight need not be repeated relative to the sense of touch. However, apart from information coming from sight and touch, there are other sensations that contribute as much and more to the genesis of the concept of space, sensations known to all, that accompany all of our movements, and that are normally called "muscular sensations." The corresponding framework constitutes what we might call *motor space*. Each muscle gives rise to a particular sensation which may increase or decrease in

[3] In the first edition, Poincaré mistakenly wrote "we could . . . return to the language of the precedent chapter," that is, Chapter 3.

such a way that the whole of our muscular sensations will depend on as many variables as we have muscles. From this perspective, *motor space would have as many dimensions as we have muscles*.

I know that some will say that if muscular sensations contribute to the formation of the concept of space, it is because we feel the *direction* of each movement and this feeling is an integral part of the sensation. If this were the case, if a muscular sensation could arise only if accompanied by this geometric feeling of direction, geometric space would truly be a form imposed upon our sensibility, but I do not perceive this at all when analysing my sensations. What I do see is that sensations corresponding to movements made in the same direction are connected in my mind by a simple *association of ideas*, an association that results in what we call "our feeling of direction." We could therefore never trace this feeling back to a single sensation. Such an association is extremely complex because, depending on the position of the limbs, the contraction of a particular muscle may correspond to movements in very different directions. As with all associations of ideas, this one is clearly acquired as the result of a *habit*, which is itself the result of many, many *experiences*. Undoubtedly, had the education of our senses taken place in a different environment where we should have experienced different impressions, other habits would have arisen and our muscular sensations would have been associated with one another according to other laws.

Characteristics of representative space

Thus, in its tripartite form—visual, tactile, and motor—representative space differs fundamentally from geometric space. It is neither homogenous, nor isotropic. We cannot even say that it is three-dimensional. It is often said that we "project" the objects of our external perception into geometric space, that we "locate" them. Does this mean anything and, if so, what? Does it mean that we visualize external objects in geometric space? Our representations are merely reproductions of our sensations, therefore they can only be placed in the same framework as the latter, that is to say, in representative space. For us it is as impossible to represent external objects in geometric space as it is for a painter to paint objects with their three dimensions on a flat canvas. Representative space is merely an image of geometric space, an image distorted by some sort of perspective, and we can visualize objects only by forcing them to comply with the laws of this perspective. We therefore do not *visualize* external bodies in geometric space, but we *reason* about these bodies as if they were located in geometric space. On the other hand, what does it mean to say that we are "locating" a given object at a given point in space? *It means simply that we are imagining the movements needed to reach this object.* Let it not be said that, in order to imagine these movements, we must project them into space and that, consequently, the notion of space must pre-exist. When I say that we imagine these movements, I mean only that we imagine the muscular sensations accompanying them. Void of geometric character, these sensations do not imply in any way the pre-existence of the concept of space.

Changes of state and changes of position

Still, what can be said if the concept of geometric space is neither dictated to our minds nor furnished by any of our sensations? How did it come into being? Our task is to examine this question at some length, although I can summarize in a few words a provisional explanation to be developed: *None of our sensations could have in isolation led us to the idea of space, to which we are led only through the study of the laws by which these sensations follow one another.* We see first that our impressions are subject to change. However, from the changes that we notice, we are soon led to make a distinction. We say sometimes that the objects causing these impressions have changed state, and sometimes that they have changed position, that they have only moved. Whether an object changes state or merely position is always conveyed to us in the same way: *as a modification in an aggregate set of impressions*. How have we come to differentiate them? [It is easy to notice the difference.][4] If there was simply a change of position, we can restore the original set of impressions by performing a number of movements that put us back in the same position *relative* to the movable object. In this way, we *correct* the modification that has taken place and re-establish the initial state by a reverse modification. In the case of sight for example, if an object moves in front of our eye, we can "keep an eye on it" and by appropriate motions of the eyeball maintain its image at the same point on the retina. We are aware of these movements because they are voluntary and because they are accompanied by muscular sensations, but this does not mean that we are imagining them in geometric space.

[4] This sentence is not in the first edition.

Thus, what characterizes a change of position, what distinguishes it from a change of state, is that it can be *corrected* in this manner.[5] It is therefore possible to go from a set of impressions A to a set B in two different ways: (1) involuntarily and without experiencing any muscular sensations, which is what happens when the object moves; and (2) voluntarily and with muscular sensations, which is what happens when the object is motionless, but we are moving in such a way that the object has a relative movement with respect to us. In this case, the shift from set A to set B is only a change of position. Here it follows, then, that the senses of sight and touch could not have given us the concept of space without the help of the "muscular sense." This notion could not have been derived from a single sensation but, rather, *only from a series of sensations*. Moreover, a *motionless* being would never have been able to acquire this concept. Unable to *correct* through its movements the effects caused by the changes of position of external objects, it would have had no reason to distinguish them from changes of state. It could not have acquired the concept of space either had its movements not been voluntary or if they had not been accompanied by some sensation or other.

Conditions of compensation

How is a compensation possible such that two independent changes can reciprocally correct one another? A mind *already versed in geometry* would thus reason: "For compensation to take place it is obviously necessary that, on the one hand, the different parts of the external object and, on the other hand, our different sense organs, must find themselves in the same *relative* position after the double change. For this to occur, the different parts of the external object must also have retained the same relative position with respect to one another. The same must also be true of the different parts of our body with respect to one another. In other words, during the first change, the external object must move as a rigid body. The same must hold of our whole body during the second change, which corrects the first one. Under these conditions, compensation can take place. However, for us *who do not yet know geometry*, the concept of space is not yet formed, so we cannot reason in this way; we cannot predict *a priori* whether compensation is possible. However, experience teaches us that it sometimes takes place and it is from this experimental fact that we start to distinguish changes of states from changes of position."

5 In the first edition, Poincaré wrote that it could "always be *corrected* in this manner" (1902: 77).

Solid bodies and geometry

Among the objects surrounding us, some often undergo displacements that can be corrected by a *correlative* movement of our own body: these are *solid bodies*. The other objects, whose form is variable, are only in rare cases subject to such displacements (change of position without change of form). If a body moves while *losing its shape*, it is no longer possible for us to return, through appropriate movements, our sense organs to the same position *relative* to this body. Consequently, we can no longer re-establish our original set of impressions. Only later, and after new experiences, do we learn how to break down bodies of variable form into smaller elements, such that each of them moves more or less according to the same laws as solid bodies. We distinguish in this way "deformations" from other changes of state. In these deformations, each element undergoes a simple change of position that can be corrected, but the modification experienced by the whole is more profound and can no longer be corrected by a correlative movement. Such a concept is already very complex and could have appeared only at a relatively late stage. Besides, it could not have arisen had the observations of solid bodies not first taught us how to set apart changes of position. *Therefore, if there were no solid bodies in nature, there would be no geometry.*

Another point also deserves a moment's attention. Let us posit a solid body located initially in position α before passing to position β. In its first position, it produces in us the set of impressions A, in its second position, the set of impressions B. Let there now be a second solid body, having qualities entirely different from those of the first, a different color for instance. Let us again suppose that it passes from position α, where it produces in us the set of impressions A′, to position β, where it produces in us the set of impressions B′. In general, set A will have nothing in common with set A′, nor set B with set B′. The transition from set A to set B and the one from set A′ to set B′ are therefore two changes which *in themselves* have nothing in common. Nevertheless, we view both of these changes as displacements. Better still, we consider them as the *same* displacement. How can that be? It is simply because both can be corrected by the *same* correlative movement of our body. It is therefore the "correlative movement" that constitutes the *sole connection* between two phenomena that otherwise we would never have thought of associating. Moreover, our body, due to the number of its articulations and muscles, can make a vast number of different movements, although not all of them are capable of "correcting" a modification in external objects. The only ones capable of doing so are those where our body moves as a whole, or at least where all of our sense organs that come into play move as a whole, that is, change place without varying their relative positions, as in the case of a solid body.

To sum up:

1° We are led initially to distinguish between two categories of phenomena. We attribute the first, which are neither voluntary nor accompanied by muscular sensations, to external objects, calling the phenomena external changes. The rest, with opposing characteristics and which we attribute to the movement of our own bodies, are internal changes.

2° We notice that some of the changes in each of these categories may be corrected by a correlative change taking place in the other category.

3° We distinguish among the external changes those that have a correlative in the other category, calling them displacements. Similarly, we distinguish among the internal changes those that have a correlative in the first category. This reciprocity allows us to define a particular class of phenomena that we call displacements. *The laws of these phenomena are the object of geometry.*

The law of homogeneity

The first of these laws is the law of homogeneity. Let us suppose that through an external change α, we pass from the set of impressions A to the set B and that this change α is then corrected by a voluntary correlative movement β, so that we are brought back to set A. Now, let us suppose that another external change α' makes us pass once again from set A to set B. Experience then teaches us that, like α, this change α' can be corrected by a voluntary correlative movement β', and that this movement β' corresponds to the same muscular sensations as movement β which corrected α. It is this fact that is usually expressed by the claim that *space is homogenous and isotropic*. We can also say that a movement that has happened once can be repeated a second time, a third time, and so forth, without its proprieties changing. In the first chapter, when we studied the nature of mathematical reasoning, we saw the importance that should be attributed to the possibility of indefinitely repeating the same operation. Mathematical reasoning owes its power to this repetition, so it is thanks to the law of homogeneity that mathematical reasoning can make use of geometric facts. To be complete, we would have to include a host of laws analogous to the law of homogeneity, the details of which I do not want to discuss here, but which mathematicians sum up with a single word, by saying that displacements form a "group."

The non-Euclidean world[6]

If geometric space were a framework required of *each* of our representations considered individually, it would be impossible to visualize an image in the absence of this framework, and we would be unable to change anything about our geometry. This is not the case, however, since geometry is nothing but the sum total of the laws according to which these images *follow one another*. Nothing prevents us then from imagining a series of representations in all respects similar to our ordinary representations, but following one another according to laws different from those to which we are accustomed. We see then that beings educated in an environment where these laws were drastically changed might have a geometry very different from ours.

Let us suppose, for example, that there is a world enclosed in a large sphere and subject to the following laws:

1. Its temperature is not uniform. It is highest at the center and gradually decreases away from the center, tailing off to absolute zero at the sphere enclosing this world. Defining the law by which this temperature varies, let R be the radius of the limit sphere and let r be the distance of the point under consideration from the center of this sphere. The absolute temperature will be proportional to $R^2 - r^2$.
2. Moreover, I suppose that, in this world, all bodies have the same coefficient of thermal expansion, so that the length of any ruler will be proportional to its absolute temperature.
3. Finally, I suppose that an object transported from one point to another point of a different temperature attains thermal equilibrium[7] with its new environment immediately.

Nothing in these hypotheses is either contradictory or unimaginable. A moving object would then become ever smaller as it would draw closer to the

6 Although Poincaré uses the general term non-Euclidean geometry, he is here as in Chapter 3 only discussing hyperbolic geometries of constant curvature.

7 Poincaré uses the term "équilibre calorifique." We have brought his language up to date by using "thermal equilibrium."

limit sphere. Note first that, while this world is limited from the perspective of our usual geometry, it will appear infinite to its inhabitants. Indeed, when the latter want to get closer to the limit sphere, they get colder and become smaller and smaller. The steps they take are also therefore progressively smaller, so that they never can reach the limit sphere. While for us geometry is nothing but the study of the laws according to which rigid bodies move, for these imaginary beings, it will be the study of the laws of motion of solids *distorted by the temperature differences* just discussed.

Undoubtedly, in our world, natural solids also experience variations in form and volume caused by heating or cooling. However, we neglect these variations in laying the foundations of geometry since they are irregular and seemingly incidental to us, besides being very small. The same would not apply in this hypothetical world, and these variations would follow regular and very simple laws. Furthermore, the various rigid elements composing the bodies of its inhabitants would undergo the same variations of form and volume. I will make a further hypothesis, that light passes through variously refracting mediums in such a way that the refractive index is inversely proportional to $R^2 - r^2$. It is easy to see that, under these conditions, the rays of light would not be rectilinear, but circular.

To justify the preceding claims, it remains for me to show that certain changes in the position of external objects may be *corrected* by correlative movements on the part of the sentient beings inhabiting this imaginary world in such a way as to restore the original set of impressions experienced by these sentient beings. Let us suppose that an object is distorted as it moves, not in the manner of a rigid body, but as a solid experiencing unequal expansions in exact conformity with the law of temperature posited above. For brevity, allow me to call such a movement a *non-Euclidean displacement*. If a sentient being is nearby, its impressions will be modified by the object's displacement, but it will be able to re-establish the initial impressions by moving in a suitable manner. It is sufficient that in the end the ensemble consisting of the object and sentient being, considered as forming a single body, undergoes one of these particular displacements that I just called non-Euclidean, which is possible if we suppose that the limbs of these beings expand following the same laws as the other bodies of the world they inhabit. Even though from the perspective of our usual geometry the bodies were distorted during this displacement and their different parts are no longer in the same relative position, we will see that the sentient being's impressions are the same again. Indeed, while the mutual distances of the different parts may have varied, the parts originally in contact come back into contact again. Therefore, the tactile impressions have not changed. Taking into account the hypothesis adopted above in regard to the refraction and curvature of light rays, the visual impressions will also have remained the same.

Like us, these imaginary beings will therefore be led to classify the phenomena they witness and to distinguish among them the "changes of positions" that can be corrected by a correlative voluntary movement. If they establish a geometry, it will not be, like ours, the study of the movement of our rigid bodies. It will be the study of the changes of position which they will have perceived and which are precisely "non-Euclidean displacements." *It will be non-Euclidean geometry*. Therefore, beings like ourselves, who were educated in such a world, would not have the same geometry as we do.

The four-dimensional world

We can visualize a four-dimensional world as well as a non-Euclidean one. Sight combined with muscular sensations connected to movements of the eyeball would be sufficient to convey three-dimensional space, even with only a single eye. The images of external objects cast onto the retina, a two-dimensional surface, are *perspectives*. However, since these objects and our eye are both mobile, we see successively different perspectives of the same body, taken from several different points of view. At the same time, we notice that the transition from one perspective to another is often accompanied by muscular sensations. If the transition from perspective A to perspective B and that from perspective A′ to perspective B′ are accompanied by the same muscular sensations, we associate them as operations of the same nature. Studying the laws according to which these operations combine, we recognize that they form a group that has the same structure as that of the movements of rigid bodies. Now, as we saw, it is from the properties of this group that we have derived the concept of geometric space and that of three dimensions. In this way, we understand how the idea of a three-dimensional space could have arisen from the viewing of these perspectives, despite the fact that each of them is only two-dimensional, *for they follow one another according to certain laws*.

Just as we can draw the perspective of a three-dimensional figure on a plane, we can trace that of a four-dimensional figure on a three- (or two-) dimensional surface. To the geometer, this is but child's play. We can even trace many perspectives of the same figure from many different points of view and we can easily visualize these perspectives, since they only have three dimensions.

Imagine that the different perspectives of the same object line successively replace one another, that the transition from one to the other is accompanied by muscular sensations. Clearly, we will consider two of these transitions as operations of the same type each time they are associated with the same muscular sensations. Nothing then prevents us from imagining that these operations combine following any law we please, so as to form, for instance, a group with the same structure as that of the movements of a four-dimensional rigid body. There is nothing here that we cannot visualize, even though these sensations are precisely those that would be experienced by a being with a two-dimensional retina who could move in four-dimensional space. In this sense we may say that it is possible to visualize the fourth dimension. [It would not be possible to visualize a non-Archimedean space, which we have discussed in the preceding chapter, in this way, because this space is no longer a second-order continuum and consequently differs much too significantly from our ordinary space.][8]

Conclusions

We see that experience plays an indispensable role in the origin of geometry; however, it would be a mistake to conclude from this that geometry is, even partially, an experimental science. If it were experimental, it would be only approximate and provisional, and a rough approximation at that! Geometry would be nothing but the study of the movements of solids. However, in reality it does not concern itself with natural solids. Its objects are certain ideal, absolutely rigid bodies that are only a simplified and quite remote image of natural solids. The concept of these ideal bodies is entirely drawn from our mind, and experience is only an occasion inviting us to bring it out.

The object of geometry is the study of a particular "group." However, the general concept of group pre-exists, at least potentially, in our minds. Its use is required of us, not as a form of our sensibility, but as a form of our understanding. Nonetheless, out of all possible groups, we must choose one that will be the *standard* to which we bring natural phenomena. Experience guides us in this choice, which it does not impose upon us. It makes us recognize not which geometry is the truest, but which is the most *useful*. It will be noticed that I have been able to describe the aforementioned imaginary worlds *while continuing to*

[8] This is not in the first French edition. Poincaré uses the expression "Hilbert's space" (*l'espace de M. Hilbert*) to refer to non-Archimedean geometries following the previous chapter.

employ the language of ordinary geometry. In fact, we would not have to change languages if we were transported there. Natives would probably find it more useful to create a geometry different from ours, one that would be better adapted to their impressions. As for us, faced with the *same* impressions, it is certain that we would find it more useful not to change our habits.

5

Experience and Geometry

1

On several occasions above, I have tried to show that the principles of geometry are not experimental facts and more specifically that Euclid's postulate could not be experimentally demonstrated. No matter how definitive the aforementioned arguments may seem to me, I believe that I should lay further emphasis on the issue because therein lies a false idea deeply rooted in many minds.

2

What will be achieved by making a physical circle, measuring its radius and circumference, and trying to see if the ratio of these two lengths equals π? We would have performed an experiment on the properties not of space, but of the material used to craft this *ring* and of the material out of which the meter-stick used for the measurements is made.

3 Geometry and astronomy

The question has also been put in another way. If Lobachevskii's geometry is true, the parallax of a very distant star will be finite. If Riemann's geometry is true, it will be negative. These are results that seem experimentally accessible and it was hoped that astronomical observations would enable us to decide between the three geometries. However, what we call a "straight line" in astronomy is simply the path of a ray of light. Therefore, if by some remote chance we were to discover negative parallaxes or to demonstrate that all parallaxes are larger than a certain limit, we would have the choice between two conclusions: we could either abandon Euclidean geometry or modify the laws of optics and accept that light does not necessarily propagate in a straight line. Needless to say, everyone would regard the latter solution as more advantageous. Euclidean geometry has therefore nothing to fear from new experiments.

4

Can we maintain that certain of the phenomena that are possible in Euclidean space would be impossible in non-Euclidean space so that, by certifying the existence of these phenomena, experiments would directly contradict the non-Euclidean hypothesis? As far as I am concerned, such a question cannot arise. To my mind, it is exactly equivalent to the following question, whose absurdity is quite obvious to all: "Are there lengths that can be expressed in meters and centimeters, but cannot be measured in fathoms, feet, and inches, so that by certifying the existence of these lengths, experiments would directly contradict the hypothesis that there are fathoms divided into six feet?"

Let us examine the question more closely. I suppose that in Euclidean space the straight line has two unspecified properties (which I will call A and B), and that in non-Euclidean space the straight line still has property A, but it no longer has property B. I finally suppose that in both Euclidean and non-Euclidean space the straight line is the only line that has property A. If this were the case, experiments might be able to decide between Euclid's hypothesis and Lobachevskii's. Some concrete, experimentally accessible object—a beam of light for example—would be found to have property A, from which we would conclude that it is rectilinear and would then investigate whether it has property B or not. But *it is not the case*: no property exists that could, like this property A, act as an absolute criterion enabling us to recognize the straight line and to distinguish it from every other line. Will we say for instance: "This property will be as follows: the straight line is a line such that a figure containing this line cannot move unless the mutual distances between its points vary in such a way that all the points of this straight line remain fixed"? In both Euclidean and non-Euclidean space, this property belongs to the straight line and nothing else. But how would we experimentally establish whether it belongs to some concrete object or other? Distances would have to be measured, and how are we to know whether some concrete length I have measured with my physical instrument correctly represents the abstract distance? The difficulty has merely been postponed.

In fact, the property I have just stated is not a property of the straight line alone, it is a property of both the straight line and of distance. To serve as an absolute criterion, it would be necessary to establish not only that it does not also belong to any line other than the straight line and to distance, but also that it does not belong to any line other than the straight line and to any other magnitude than distance—but this is not the case. It is therefore impossible to imagine a concrete experiment that can be interpreted in the Euclidean system, but not in the Lobachevskiian system, so that I can conclude:[1] no experiment will ever contradict Euclid's postulate; on the other hand, no experiment will ever contradict Lobachevskii's postulate.

5

It is, however, not enough that Euclidean (or non-Euclidean) geometry can never be directly contradicted by experiment. Would it not be possible for it to agree with experiments only by violating the principle of sufficient reason and that of the relativity of space? Let me explain. Consider any physical system. We must take into account, on the one hand, the "state" of the various bodies of this system (for example their temperature, their electric potential, etc.) and, on the other hand, their position in space. Among the data enabling us to determine this position, we moreover distinguish between the mutual distances of these bodies, which define their relative positions, and the conditions defining the system's absolute position and its absolute orientation in space. The laws of phenomena arising in this system might depend on the state of these bodies and their mutual distances. However, because of the relativity and passivity of space, they will not depend on the absolute position and orientation of the system. In other words, the state of the bodies and their mutual distances at a given instant will depend only on the state of these same bodies and their mutual distances at the initial instant, but will in no way depend on the initial absolute position and initial absolute orientation of the system. For brevity's sake, this is what I might call *the law of relativity*.

Thus far, I have spoken as a Euclidean geometer. However, as I have said, while an experiment, whatever it may be, will admit of an interpretation on the Euclidean hypothesis, it will also admit of one on the non-Euclidean hypothesis. Well, we conducted a series of experiments. We interpreted them with the

[1] The first French edition reads: "Those who are not convinced by these considerations should offer a concrete experiment that can be interpreted in the Euclidean system, but not in the Lobachevskiian system. Knowing full well that this challenge will never be answered, I can conclude: [. . .]" (95).

Euclidean hypothesis and we recognized that thus interpreted these experiments do not violate this "law of relativity." We now interpret them with the non-Euclidean hypothesis, which is always possible. Only, in this new interpretation, the non-Euclidean distances of our various bodies will generally not be the same as the Euclidean distances of the original interpretation. Once interpreted in this manner, will our experiments still agree with our "law of relativity"? And if not, would we not also have the right to say that the experiment has falsified non-Euclidean geometry?

It is easy to see that this fear is unwarranted. In fact, to apply the law of relativity in all of its rigor, it must be applied to the entire universe, for if only a part of this universe were considered, and if the absolute position of this part happened to vary, its distances to the other bodies in the universe would also vary. Their influence on the part of the universe under examination could therefore increase or diminish, which could modify the laws of the phenomena occurring there. However, if our system is the entire universe, experiments are powerless to inform us of its absolute position and orientation in space. No matter how sophisticated our instruments may be, they can make known to us only the state of the different parts of the universe and their mutual distances, so that our law of relativity may be stated as: "The readings that we can make on our instruments at a given instant depend only on the readings that we could have made on these same instruments at the initial instant." Now such a statement is independent of any interpretation of the experiments. If the law is true in the Euclidean interpretation, it will also be true in the non-Euclidean interpretation.

Allow me a short digression on this point. Above I have spoken of the data defining the position of the different bodies of a system. I should also have spoken of those defining their speeds. I would then have had to differentiate between the speeds with which the mutual distances of the different bodies vary and, on the other hand, the translational and rotational speeds of the system, that is to say the speeds with which its absolute position and orientation vary. To fully satisfy the mind, the law of relativity should have been stateable as: "The state of bodies and their mutual distances at any given instant, as well as the speeds with which these distances vary at this very instant will depend only on the state of these bodies and their mutual distances at the initial instant, as well as on the speeds with which these distances were varying at this initial instant. However, they will depend neither on the initial absolute position of the system, nor on its absolute orientation, nor on the speeds with which this absolute position and orientation were varying at the initial moment."

Unfortunately, the law thus stated is not in agreement with experiments, at least not as usually interpreted. Let someone be transported to a planet where the skies are constantly covered by a thick blanket of clouds such that it is never possible to glimpse the other celestial bodies. Everyone would live on this planet as if it were isolated in space. Yet, this person could realize that it rotates either by measuring its oblateness (which is usually done with the help of astronomical observations, but could be done through purely geodesic means), or by repeating Foucault's pendulum experiment. The absolute rotation of this planet could thus be revealed. This fact shocks the philosopher, but the physicist just has to accept it.

We know that Newton used this fact to argue for the existence of absolute space. I myself can in no way accept this view and will explain why in Part Three. For now, I will not consider the problem. I have therefore had to resign myself to conflating speeds of every kind within the data defining the state of bodies in the statement of the law of relativity. Be that as it may, this difficulty is the same for both Euclid's geometry and Lobachevskii's. I need not worry about it then and have raised it only in passing. What matters is the conclusion. Experiments cannot decide between Euclid and Lobachevskii. In conclusion, however we look at it, it is impossible to find a rational meaning for geometrical empiricism.

6

Experiments only make known to us the relationships of bodies to one another. None of them concern, or can concern, the relations of bodies to space or the mutual relations of the different parts of space. "Yes," you reply, "a single experiment is insufficient, because it gives me only one equation with many unknowns, but once I have carried out enough experiments, I will have enough equations to calculate all my unknowns." Knowing the height of the mainmast is not sufficient to calculate the age of the captain. Once you will be done measuring all the pieces of wood making up the ship, you will have many equations, but you will not know this age any better. Since they have been made on pieces of wood, all your measurements can reveal nothing to you other than what concerns these pieces of wood. Similarly, as they bear only on the relationships of bodies to one another, your experiments, as numerous as they may be, bear only on the relationships between bodies and cannot reveal anything about the mutual relations of the different parts of space.

7

Will you say that while experiments concern bodies, they at the very least concern the geometric properties of bodies? In the first place, what do you understand by geometric properties of bodies? I assume that they have to do with the relationships of the bodies to space. These properties therefore cannot be determined through experiments concerning only the relationships between bodies. This alone would be enough to show that these relationships are not the ones in question. Let us first agree, however, on the meaning of the expression "geometric properties of bodies." When I say that a body is composed of several parts, I take it for granted that I am not thus stating a geometrical property, and that this statement would remain true even if I agreed to give the incorrect name of "points" to the smallest parts that I am considering. When I say that a certain part of a given body touches some part of another body, I am stating a proposition concerning the mutual relationship of these two bodies, not their relationship to space. I hope that you will agree with me that these are not geometric properties. I am certain at the least that you will agree that these properties are independent of any knowledge of metric geometry.

Given the above, I imagine a rigid body made of eight slender iron rods OA, OB, OC, OD, OE, OF, OG, OH joined at O, one of their extremities. We will also have a second rigid body, for example a piece of wood on which we will note three small ink spots which I will call α, β, γ. Suppose next that we realize that we can make αβγ touch AGO (I mean by this α with A at the same time as β with G and γ with O) because αβγ can be brought into contact with BGO, CGO, DGO, EGO, FGO, then with AHO, BHO, CHO, DHO, EHO, FHO, then αγ in contact with AB, BC, CD, DE, EF, FA.[2]

Here are findings that can be obtained in the absence of a preconceived notion of the structure or the metric properties of space. They are absolutely not about the "geometric properties of bodies." These findings would not be possible if the bodies on which we experimented moved in accordance with a group having the same structure as the Lobachevskiian group (by which I mean according to the same laws as the rigid bodies in Lobachevskii's geometry). They are therefore enough for demonstrating that these bodies move according to the Euclidean group or, at least, that they do not move according to the Lobachevskiian group. It is easy to see that these findings are compatible with the Euclidean

[2] Discussion of this rather complex example can be found in Roberto Torretti, *Philosophy of Geometry from Riemann to Poincaré* (Dordrecht: D. Reidel, [1978] 1984), 340.

group since they could be made if the body αβγ were a rigid body in our usual geometry in the shape of a right triangle and if the points A, B, C, D, E, F, G, H were the vertices of a polyhedron composed of two regular hexagonal pyramids in our usual geometry having as a common base ABCDEF and, as apices, G for the one and H for the other.

Now, suppose that instead of the preceding findings, we observe that as before we can place αβγ successively on AGO, BGO, CGO, DGO, EGO, FGO, AHO, BHO, CHO, DHO, EHO, FHO, and then that αβ (and not αγ anymore) can successively be placed on AB, BC, CD, DE, EF, and FA. We would make these findings if non-Euclidean geometry were true and if the bodies αβγ and OABCDEFGH were rigid bodies, the former being a right triangle and the latter a double regular hexagonal pyramid of appropriate dimensions. These new findings are therefore not possible if the bodies move in accordance with the Euclidean group, but they become so if we suppose that the bodies move in accordance with the Lobachevskiian group. They would therefore be sufficient (if carried out) to prove that the bodies in question do not move in accordance with the Euclidean group.

Thus, without making any hypothesis on the form or the nature of space or on the relationship between bodies and space and without attributing any geometric properties to bodies, I have obtained findings that have enabled me to show that in one case the bodies used in the experiment move in accordance with a group with a Euclidean structure, and that in the other case, they move in accordance with a group with a Lobachevskiian structure. It may not be said that the first set of findings might constitute an experiment proving that space is Euclidean and the second an experiment proving that space is not Euclidean. In fact, it would be possible to imagine (I say, imagine) bodies moving so as to make possible the second series of findings. As proof, the first machinist to come along could construct such bodies were this person willing to put in the effort and the price. Nevertheless, you will not conclude from this that space is non-Euclidean. In fact, since ordinary solid bodies would continue to exist even after the machinist constructed the strange bodies just mentioned, we would simply have to conclude that space is simultaneously Euclidean and non-Euclidean.

Suppose, for example, that we have a large sphere of radius R and that the temperature decreases from the center to the surface of this sphere according to the law I discussed when describing the non-Euclidean world. We could have bodies whose thermal expansion was negligible and that behaved like ordinary rigid bodies and, on the other hand, we could have bodies with a very large thermal expansion behaving like non-Euclidean rigid bodies. We could have two double pyramids OABCDEFGH and O′A′B′C′D′E′F′G′H′ and two triangles $\alpha\beta\gamma$ and $\alpha'\beta'\gamma'$. The first double pyramid would be rectilinear and the second one curvilinear. The triangle $\alpha\beta\gamma$ would be made from a material exhibiting no thermal expansion, the other from a material with a high thermal expansion coefficient. We could then make our first findings with the double pyramid OAH and the triangle $\alpha\beta\gamma$, and the second ones with the double pyramid O′H′A, and the triangle $\alpha'\beta'\gamma'$. The experiment would then seem to prove first that Euclidean geometry is true and then that it is false. *The experiments have therefore been about bodies, not space.*

SUPPLEMENT

8

For the sake of completeness, I ought to mention a very delicate issue that would require much explanation. It will suffice to summarize my presentation in the *Revue de métaphysique et de morale* and in *The Monist*.[3] When we say that space has three dimensions, what do we mean? We have seen the importance of these "internal changes" revealed to us by our muscular sensations. They can be used to characterize our body's various *stances*. Let us arbitrarily take as a starting point one of these stances, A. When we pass from this initial stance to another stance B, we experience a series of muscular sensations S, and this series S will define B. Notice however that we often consider two series S and S′ as defining the same stance B (since the initial and final stance A and B remain the same, the intermediary stances and corresponding sensations may differ). How then will we perceive the equivalence of these two series? It is because they can be used to compensate for the same external change or, more generally, because when it is a question of compensating for an external change, one series can be replaced by the other.

Among these series, we have singled out those that can compensate on their own for an external change and called them "displacements." Since we cannot differentiate between two displacements if they are too close to one another, the set of these displacements present the characteristics of a physical continuum. Experience teaches us that these characteristics are those of a six-dimensional

3 Henri Poincaré, "L'espace et ses trois dimensions," *Revue de métaphysique et de morale* 11 (1903): 281–301 and 407–29; Henri Poincaré, "On the Foundations of Geometry," *The Monist* 9 (1898): 1–43.

physical continuum, but we do not know yet how many dimensions space itself has. We must answer another question first.

What is a point in space? We all think that we know what it is, but that is an illusion. When we try to imagine a point in space, what we see is a black spot on white paper, a spot of chalk on a blackboard. It is always an object. The question should therefore be understood as follows: What do I mean when I say that object B is at the very same point previously occupied by object A? In other words, what criterion will allow me to recognize this? I mean that *although I have not moved* (something I know from my muscular sense), my thumb, that earlier touched object A, is now touching object B. I might have used other criteria, another finger or the sense of sight, for example, but the first criterion is sufficient. I know that if it answers in the affirmative, all the other criteria will yield the same answer. I know it *from experience*, I cannot know it *a priori*. For the same reason, I say that touch cannot operate at a distance, which is another way to state the same empirical fact and if, on the contrary, I say that sight operates at a distance, it means that the criterion provided by sight may answer affirmatively, while the others answer negatively. [In fact, although the object moved, it can still leave its image at the same point on the retina. Sight provides an affirmative response, the object has remained at the same point, while touch gives a negative response for my finger that was previously touching the object, does not touch it anymore. Had experience shown us that a finger can give a negative response while another finger responds affirmatively, we would say that touch can also operate at a distance.][4]

To summarize, my thumb determines a point for each stance of my body and it is this, and only this, that defines a point of space. For each stance there is thus a corresponding point, but it often happens that the same point corresponds to many different stances (in such a case, we say that our finger has not moved, but the rest of our body has). We therefore single out the changes of stances where the finger does not move. How do we reach this conclusion? It is because we often notice that during these changes the object touching the finger does not lose contact with it.

Let us therefore put in a single class all the stances that can be deduced from one another by one of the changes we have thus identified. A unique point of space will correspond to all the stances of a given class. Thus, to each class will correspond a point and to each point a class. But we could also say that what is experienced is not the point, but this class of changes or, better still, the class of

[4] This section of the text was not in the first edition.

corresponding muscular sensations. So when we say that space has three dimensions, we simply mean that the set of these classes appears to us to have the characteristics of a three-dimensional physical continuum.[5]

We might be tempted to conclude that experience has taught us how many dimensions space has. Yet, in reality, our experiences have here again not concerned space, but rather our body and its relationships to neighboring objects. What is more, our experiences are quite crude. The latent idea of a certain number of groups—those which Lie described in his theory—pre-existed in our mind. Which one will we choose as a kind of standard to which we will compare natural phenomena? Once this group is chosen, which one of its sub-groups will we take to characterize a point in space? Experience has guided us by showing us which choice best adapts itself to the properties of our body, but this has been the extent of its role.

Ancestral experience[6]

It has often been said that, although individual experience cannot have created geometry, the same is not true of ancestral experience. What does this mean? Do we mean that we cannot demonstrate Euclid's postulate experimentally, yet our ancestors were able to do so? Not in the least. We mean that, through natural selection, our minds have *adapted* to the conditions of the external world, that they have adopted the geometry *most advantageous* to the species or, in other words, *the most useful*. This is in complete agreement with our conclusions: geometry is not true, it is advantageous.

5 The 1902 edition included the following two paragraphs which were subsequently deleted:

> If instead of defining the points of space with the help of the thumb, I had used, for instance, another finger, would not the results have been the same? This is absolutely not obvious *a priori*. However, as we have seen, experience has shown us that all our criteria agree, thus enabling us to answer by the affirmative.
>
> If we come back to what we have called displacements, the set of which forms a group, as we have seen, we will be led to distinguish those where a finger does not move. According to our preceding discussion, these are the ones that can characterize a point in space and their set will form a sub-group of our group. To each such sub-group will therefore correspond a point of space.
>
> 1902: 108–9

6 This section was not in the first edition.

Part Three

Force

6

Classical Mechanics

The English teach mechanics as an experimental science, while on the Continent it is always presented as a more or less deductive and *a priori* science. It is clear that the English are right, but how has it been possible to pursue this erroneous thinking for so long? Why have the continental thinkers, who were trying to avoid the practices of their predecessors, failed most of the time to do so? What is more, if the principles of mechanics are based only on experimentation, are they then not merely approximate and provisional? Can new experiments lead us one day to modify or even abandon them?

These are the questions that naturally arise and are difficult to solve largely due to the fact that treatises on mechanics do not differentiate clearly between what is experiment, what is mathematical reasoning, what is convention, and what is hypothesis. Moreover:

1° There is no absolute space and we conceive only of relative motions. Nevertheless, mechanical facts are most often stated as if there were an absolute space with respect to which they could be described.

2° There is no absolute time. To say that two durations are equal is a claim that in and of itself has no meaning and can gain one only by convention.

3° Not only do we have no direct intuition of the equality of two durations, we do not even have one of the simultaneity of two events happening in different locations, as I have explained in an article entitled *La mesure du temps*.*

4° Finally, our Euclidean geometry is itself only a kind of linguistic convention. We could express physical facts by mapping them in non-Euclidean space which would be a less useful, but just as legitimate, point of reference as our ordinary space. The statement would thus become a lot more complicated, but it would remain possible.

* *Revue de métaphysique et de morale*, VI: 1–13 (January 1898); see also *The Value of Science, Chapter 2.*

In this way, absolute space, absolute time, and geometry itself are not necessary conditions of mechanics. None of these things precedes mechanics, no more than the French language logically precedes the truths expressed in French.

We could try to state the fundamental laws of mechanics in a language independent of all these conventions, which would certainly give us a better understanding of what these laws are in themselves. Andrade tried to do so, at least to some extent, in his *Leçon de mécanique physique.*[1] The statement of these laws would then of course become much more complicated, since all these conventions have been devised expressly to shorten and simplify their statement.

As for me, I will leave all these difficulties aside, except insofar as absolute space is concerned. Not because I do not recognize their importance, far from it, but because we have examined them sufficiently in the first two parts of the book. I will therefore accept absolute time and Euclidean geometry on a *provisional basis*.

The principle of inertia

A body on which no force is exerted can only move uniformly in a straight line. Is this a truth that our mind must recognize *a priori*? If this were the case, how could the Greeks have not known about it? How could they have believed that motion continues only as long as the initial force remains in play, or that in the absence of any opposing force, a body follows a circular path, the noblest of all motions? If we say that the speed of a body cannot change if there is no reason for it to change, could we not just as well maintain that the position of this body or the curvature of its trajectory cannot change unless an external cause happens to modify them?

Is the principle of inertia, which is not an *a priori* truth, then an experimental fact? Have we ever experimented on bodies freed from the influence of forces and, if so, how did we know that these bodies were not subject to any force? We usually give as an example a billiard ball rolling for a very long time on a marble tabletop, but why do we say that it is not subject to any force? Is it because it is too far from all other bodies to undergo any noticeable influence? The ball is still no further

1 Jules Andrade, *Leçon de mécanique physique* (Paris: Société d'éditions scientifiques, 1898) (http://gallica.bnf.fr/ark:/12148/bpt6k8832547/f11.item.r=andrade.langEN.zoom [accessed May 2, 2017]). Andrade also sums up his ideas on the "two schools of mechanics" in "Deux écoles en mécanique," *Revue de physique et de chimie et de leur applications industrielles* (1896–97, no. 10): 465–72, https://www.espci.fr/sites/www.espci.fr/IMG/pdf/RPC_1896-1897_10.pdf [accessed May 2, 2017].

from the earth than if we were to throw it freely in the air. Everyone knows that in this case it would be subject to the effect of gravity due to earth's attraction.

Professors of mechanics have the habit of glossing over the billiard ball example, but they add that the principle of inertia is verified indirectly by its consequences. They express themselves poorly. They obviously mean that we can verify various consequences of a more general principle, of which inertia is but a particular case.

I propose the following statement of this general principle: The acceleration of a body depends only on its position and velocity and those of neighboring bodies. Mathematicians would say that the motions of all the physical molecules in the universe depend on second-order differential equations.

Allow me to use my imagination to show that this is truly the natural generalization of the law of inertia. As mentioned above, the law of inertia is not necessary *a priori*—other laws would be just as compatible with the principle of sufficient reason. When a body is not subject to any force, rather than suppose that its speed does not change, we could suppose that its position or even its acceleration should not change. Let us imagine for a moment that one of these two hypothetical laws were a law of nature replacing our law of inertia. What would its natural generalization be? A moment's reflection will show us. In the first case, we would have to suppose that a body's speed depends only on its position and that of the neighboring bodies and, in the second case, that the change in a body's acceleration depends only on the positions, speeds, and accelerations of this body and the neighboring ones. Or, rather, employing the language of mathematics, the differential equations of motion would be of the first order in the first case and of the third order in the second case.

Let us modify our story slightly. Imagine a world analogous to our solar system, but where, improbably, the orbits of all the planets show neither eccentricity nor inclination. Moreover, I assume that the masses of these planets are too small for their mutual perturbations to be noticeable. Astronomers inhabiting one of these planets would have to conclude that the orbit of an astronomical body can only be circular and parallel to a certain plane. The position of an astronomical body at a given instant would then suffice to determine its speed and its entire trajectory. The law of inertia they would adopt would be the first of the two hypothetical laws I just discussed.

Let us imagine now that one day a very massive body coming from distant constellations happened to travel across this system at very high speed. All the orbits would be profoundly disturbed. Our astronomers would not be too surprised yet. They would of course guess that this new astronomical body alone was responsible for all the upheaval. However, they would say, once it has moved away, order will be re-established on its own; no doubt the planets' distances from the sun will not revert to what they were before the cataclysm, but once the perturbing astronomical body is gone, the orbits will become circular again. It would only be when the perturbing astronomical object was far away and the orbits became elliptical, rather than becoming circular again, that these astronomers would realize their mistake and become aware of the need to redo all of their mechanics.

I have stressed these hypotheses a bit because it seems to me that it is only possible to properly understand what our generalized law of inertia is by comparing it to a contrary hypothesis. So then, has this generalized law of inertia been experimentally confirmed and can it be so confirmed? When Newton wrote his *Principia*, he certainly regarded this truth as having been empirically derived and demonstrated, in his eyes, not only by the anthropomorphic idol,[2] which we will discuss later, but also by the work of Galileo. It had also been demonstrated by Kepler's laws themselves since, according to these laws, a planet's path is entirely determined by its initial position and initial speed, as our generalized principle of inertia requires. For this principle to be true only in appearance, for us to be able to fear needing to replace it someday with one of the analogous principles with which I compared it earlier, we would have had to have been led astray by some surprising coincidence, like the one that misled our imaginary astronomers in the hypothetical situation that I described above. Such a hypothesis is too unbelievable to deserve serious consideration. No one will believe that such coincidences could occur. No doubt, the probability of two eccentricities being exactly null, within the observational error, is not smaller than the probability of one being, for instance, precisely equal to 0.1 while the other is 0.2 within the observational errors. The probability of a simple event is not less than that of a complicated event, and yet, if the first one happened, we would not want to believe that nature deliberately deceived us. The hypothesis of such a mistake having been put aside, we can agree that our law has been empirically confirmed with respect to astronomy.

Astronomy is however not the whole of physics. Should we not be concerned that some new experiment would one day falsify the law at play in some area of physics? An experimental law is always subject to revision. We must always expect to see it replaced by a more precise law. Yet, no one really worries that the law

[2] "Idole anthropomorphique" sounds like this could be a reference to Bacon, in which case this would be "idols of the tribe" (a prejudice that comes from our common humanity, such as the range of what we can see or hear), but what Poincaré says is not the usual French translation, which is "idoles de la tribu." Furthermore, while he says that he will discuss this later, the only reference seems to be the section on anthropomorphic physics, with no reference to an idol there.

under discussion will someday have to be abandoned or amended. Why? Precisely because we will never be able to subject it to a decisive test. First of all, for this test to be exhaustive, all the bodies in the universe would have to return eventually to their initial positions with their initial speeds. We could then see whether from that time onward they resume the trajectories they once followed, but this test is impossible. It can only be partially carried out and, no matter how well it is conducted, there will always be some bodies that will not return to their initial position. Therefore, any deviation from the law will be readily explained away.

What is more, in astronomy, we *see* the bodies whose motions we are studying and we usually assume that they are not subject to the action of other invisible bodies. Under these conditions, our law will necessarily be either confirmed or not. In physics however, the situation is different in that when physical phenomena are caused by motions, it is the motions of molecules that we do not see. If then the acceleration of one of the bodies we see seems to depend on *something other* than the position or speed of the other visible bodies or invisible molecules whose existence we have already recognized, nothing will prevent us from supposing that this *other thing* is the position or speed of other molecules, the presence of which we had not yet suspected. The law will be upheld.

Allow me to use mathematical language for an instant to express the same thought in another form. Suppose that we observe n molecules and find that their $3n$ coordinates satisfy a system of $3n$ fourth-order differential equations (and not second-order equations, as the law of inertia would require). We know that with the introduction of $3n$ auxiliary variables, a system of $3n$ fourth-order equations can be reduced to a system of $6n$ second-order equations. If then we suppose that these $3n$ auxiliary variables represent the coordinates of n invisible molecules, the result is once again in conformity with the law of inertia. We conclude that this law, experimentally verified in some particular cases, can without hesitation be extended to more general cases, since we know that in these general cases experiments can neither confirm nor contradict it.

The law of acceleration

A body's acceleration is equal to the force acting on it divided by its mass. Can this law be experimentally tested? To do so, the three magnitudes appearing in the statement—acceleration, force, and mass—would have to be measured. I will accept that acceleration can be measured since I am setting aside the difficulty arising from the measurement of time. Still, how is force or mass to be measured?

We do not even know what they are. What is *mass*? It is, Newton answers, the product of volume and density. Thomson and Tait reply that it would be better to say that density is the ratio of the mass divided by the volume. What is *force*? Lagrange answers that it is a cause that brings about the motion of a body or tends to reproduce it. As Kirchhoff would say, it is the product of mass and *acceleration*. But then, why not say that mass is the quotient of force divided by acceleration? These difficulties are insoluble.

When we say that force is the cause of motion, we are talking metaphysics and this definition would be absolutely unproductive if we made do with it. For a definition to be useful, it must teach us how to *measure* force; which is all that we ask. It is absolutely not necessary for it to show us what force is *in itself*, nor whether it is the cause or the effect of motion.

We must therefore first define the equality of two forces. When might we say that two forces are equal? Some will answer that it is when they impart the same acceleration to the same mass or they balance each other out when set against each other. This definition is an illusion. We cannot uncouple a force from the body to which it is applied in order to attach it to another body as we would uncouple a locomotive to hitch it to another train. It is therefore impossible to know what acceleration a given force applied to a given body would impart to another body, *if* it were applied to it. It is impossible to know how two forces not working against each other would behave *if* they were directly in opposition.

It is this definition that we are trying to implement, so to speak, when we measure a force with a dynamometer or counterbalance it with a weight. Two forces F and F′, which I will take for simplicity's sake to be vertical and oriented from bottom to top, are applied respectively to two bodies C and C′. I suspend the same heavy body P first to body C, then to body C′. If equilibrium is reached in both cases, I will conclude that the two forces F and F′ are equal, since both are equal to the weight of body P. Yet am I certain that body P has maintained the same weight when I carried it from the first body to the second? Far from it: *I am certain of the contrary.* I know that gravitational force varies from one point to another and that it is stronger, for example, at the pole than at the equator. The difference, no doubt, is quite small and in practice I will not take it into account, but a well-articulated definition should display mathematical rigor, rigor which is lacking in this case. My comments on weight would obviously apply to the force exerted by the dynamometer's spring, which can vary with temperature and a large number of other conditions.

What is more, we cannot say that the weight of body P is applied to body C and directly balances force F. That which is applied to body C is the action A of body P on body C. Body P is subjected, on the one hand, to its weight and, on the other hand, to the reaction R of body C on P. Ultimately, force F is equal to force A, because it balances it out. Force A is equal to R by means of the principle of the equality of action and reaction. Finally, force R is equal to the weight of P because it balances it out. We derive the equality of F and the weight of P from these three equalities.

We are thus compelled to apply the principle of the equality of action and reaction itself in the definition of the equality of two forces. *In this case, this principle should no longer be considered an experimental law, but a definition.*

We now have two rules to establish the equality of two forces: the equality of two forces in equilibrium and the equality of action and reaction. However, as we have seen above, these two rules are insufficient. We must call on a third rule and admit that some forces, a body's weight for example, are constant in magnitude and direction. This third rule, however, is an experimental law, as I have said. It is only approximatively true. *It is a bad definition.*

We are therefore brought back to Kirchhoff's definition: *force is equal to mass multiplied by acceleration*. This "Newton's law" in turn is no longer considered an experimental law, but only a definition. But this definition is still inadequate since we do not know what mass is. The definition certainly allows us to calculate the ratio of two forces applied to the same body at different times, but it does not tell us anything about the ratio of two forces applied to two different bodies. To complete the definition, we must again call on Newton's third law (the equality of action and reaction), still considered a definition, not an experimental law. Two bodies A and B act on each other such that the acceleration of A multiplied by the mass of A is equal to the action of B on A. Likewise, the product of the acceleration of B and its mass is equal to the reaction of A on B. Since action equals reaction by definition, the masses of A and B are inversely proportional to the accelerations of these two bodies. The ratio of these two masses is thus defined and it is up to experiments to confirm that this ratio is constant.

All would be fine if the two bodies A and B were the only ones present and if they were not subject to any other forces. This is not the case: the acceleration of A is not only due to the action of B, but also to that of a number of other bodies C, D, . . . To apply the preceding rule, it is therefore necessary to break down the acceleration of A into many components and to distinguish which of these components is due to the action of B.

This decomposition would still be possible if we *assumed* that the action of C on A simply adds itself to that of B on A, without the presence of body C modifying the action of B on A, or the presence of B modifying the action of C on A; therefore, if we assumed that any two bodies attract one another and that their mutual action follows the straight line joining them together and depends only on their distance; if, in a word, we assumed the *hypothesis of central forces*.

We know that to determine the mass of astronomical bodies, we make use of a completely different principle. The law of gravity shows us that the attraction between two bodies is proportional to their masses. If r is the distance between them, m and m' their masses, and k is a constant, their attraction will be:

$$\frac{kmm'}{r^2}.$$

What we are measuring is therefore not the mass—the ratio of force and acceleration—but the active gravitational mass—not the body's inertia, but its force of attraction. This measurement is an indirect method, the application of which is not theoretically-speaking indispensable. Attraction might very well have been inversely proportional to the square of the distance, without being proportional to the product of the masses, that is, it might very well have been equal to:

$$\frac{f}{r^2}$$

but without $f = kmm'$ being the case.

If this were so, we could still measure the mass of the astronomical bodies through the observation of their *relative* motions.

However, have we the right to assume the hypothesis of central forces? Is this hypothesis strictly accurate? Is it certain that experiments will never contradict the hypothesis? Who would dare to make such a claim? If we had to abandon this hypothesis, the whole edifice, erected with so much effort, would collapse.

We can no longer speak of the component of the acceleration of A due to the action of B. We have no means to distinguish it from the action of C or of another body, and the rule for the measurement of masses becomes inapplicable. What is left then of the principle of the equality of action and reaction? If the hypothesis of the central forces is rejected, this principle must obviously be stated thus: the geometric resultant of all the forces applied to the various bodies of a system free from any external force will be zero. In other words, *the motion of this system's center of gravity will be in a straight line and uniform*.

We seem to have found then a way of defining mass. The position of the center of gravity obviously depends on the values attributed to the masses. It will be necessary to set these values such that the motion of this center of gravity will be in a straight line and uniform, which will always be possible if Newton's third law is true, and which will generally be possible only in one way.

No system, however, is independent of all external actions. All parts of the universe are more or less subject to the action of the other parts. *The law of motion of the center of gravity is strictly true only when applied to the entire universe.* To obtain the values of the masses from this law, it would then be necessary to observe the motion of the center of gravity of the universe. The absurdity of this consequence is clear. We know only relative motions. The motion of the center of gravity of the universe will always remain unknown to us. So nothing is left and our efforts have been in vain. We are pushed back to the following definition, which is no more than an admission of powerlessness: *masses are coefficients that are useful to introduce in calculations*.

We could reconstruct all of mechanics by attributing different values to all the masses. This new mechanics would not contradict experiments or the general principles of dynamics (principle of inertia, proportionality of forces to masses and accelerations, equality of action and reaction, rectilinear and uniform motion of the center of gravity, and principle of areas). The equations of this new mechanics would just be *less simple*. To be clear, only the first terms (those we came to know experimentally) would be less simple. The masses might perhaps be slightly altered without the *whole* equations gaining or losing in simplicity.

Hertz asked whether the principles of mechanics are strictly true. "In the view of many physicists," he says, "it will seem inconceivable that any further experiment will ever be able to change any aspect of the mechanics' unshakable principles and yet, whatever is produced by experiment can always be corrected by experiment."[3]

Such concerns seem beside the point considering what we have just said. The principles of dynamics appear to us *prima facie* as experimental truths, but we have been compelled to use them as definitions. It is *by definition* that force is equal to the product of mass by acceleration: from here on, it is a principle which is placed beyond the reach of any future experiment. Similarly, it is by definition that every action is equal to its reaction.

But then, some will say that these unverifiable principles are absolutely meaningless. Experiments cannot disprove them, but they cannot show us anything useful. Why then study dynamics? This overly swift condemnation would be unjust. In nature, no system is *perfectly* isolated, perfectly free from any external action, but

3 Translated from the French. For an alternative English translation, see Heinrich Hertz, *The Principles of Mechanics Presented in a New Form*, trans. D. E. Jones and J. T. Walley (London and New York: Macmillan and Co., 1899), 9, available on https://archive.org/stream/principlesofmech00hertuoft#page/n0/mode/2up [accessed May 2, 2017].

there are approximately isolated systems. By observing such a system, we may study not only the relative motion its various parts have to one another, but also the motion of its center of gravity with respect to the other parts of the universe. We then notice that the motion of this center of gravity is *approximately* in a straight line and uniform, in accordance with Newton's third law.[4]

There we have an experimental truth, but it cannot be contradicted by experiment. What would a more precise experiment teach us? It would teach us that the law was only approximately true, which we already knew. *We now understand how experiment could have served as a basis for the principles of mechanics although it will never be able to disprove those principles.*

Anthropomorphic mechanics

It may be said that Kirchhoff simply followed the mathematicians' general tendency towards nominalism. His skills as a physicist did not guard him against the tendency. He insisted on having a definition of force and accepted as such the first proposition that presented itself. However, we do not need a definition of force. The idea of force is a primitive, irreducible, and undefinable notion. We all know what it is, having a direct intuition of it. This direct intuition comes from the notion of effort, with which we are familiar since childhood.

Right off, even if this direct intuition showed us the true nature of force in itself, it would be an insufficient foundation for mechanics, not to mention completely useless. What matters is not knowing what force is, but knowing how to measure it. Anything that does not show us how to measure it is as useless to the physicist studying mechanics as are, for instance, the subjective concepts of heat and cold to the physicist studying heat. This subjective concept cannot be translated into numbers and therefore serves no purpose. A scientist whose skin was a poor heat conductor and who, consequently, would never have felt cold or heat, would read a thermometer as well as anyone else, and this would be enough to construct the whole theory of heat.

Yet, we cannot use this immediate notion of effort to measure force. For instance, it is clear that I would feel more tired lifting a 50-kilo weight than a man used to carrying heavy loads. Furthermore, this notion of effort does not show us the true nature of force. Ultimately, it is nothing more than a memory of

[4] It appears that Poincaré is referring to the first law, which would also fit with the claim in the next sentence that it is an experimental law.

muscular sensations and no one will maintain that the sun feels a muscular sensation when it attracts the earth.

All we can hope to find is a symbol, one which is less precise and less useful than the arrows geometers use, but just as remote from reality. Anthropomorphism has played a significant historical role in the origins of mechanics. Perhaps it will still at times provide a symbol that will seem useful to some minds, but it cannot ground anything of a truly scientific or philosophical character.

The "School of the Thread"

In his *Leçons de mécanique physique*, Andrade has revived anthropomorphic mechanics. To the school of physicists to which Kirchhoff belongs, he opposes what he calls rather oddly the "School of the Thread." This school tries to bring everything back to "the study of certain physical systems of negligible mass, pictured in a state of tension and capable of transmitting considerable strain on distant bodies, systems whose ideal type is the *thread*."[5] A thread transmitting any force is slightly elongated under the action of this force. The direction of the thread tells us the direction of the force, whose magnitude is measured by the elongation of the thread.

We may then imagine an experiment like the following: A body A is attached to a thread, at the other end of which we apply a force that we vary until the thread has grown by a length α, and we record the acceleration of body A. We detach A and attach body B to the same thread and we reapply the force or another force, varying it until the thread grows again by a length α, and we record the acceleration of body B. We repeat the experiment both with body A and body B, but in such a way that the thread grows by a length β. The four observed accelerations should be proportional. We thus have an experimental verification of the law of acceleration stated above. Or else we could subject a body to the simultaneous action of many identical threads, all equally stretched, and try to determine experimentally what the orientation of all these threads must be for the body to remain in equilibrium. We then have an experimental verification of the rule of composition of forces.

What did we really do, anyway? We defined the force to which the thread is subjected by the deformation it undergoes, which is reasonable enough. We

5 Jules Andrade "Deux écoles en mécanique," *Revue de physique et de chimie et de leur applications industrielles* (1896–97, no. 10): 468, available on https://www.espci.fr/sites/www.espci.fr/IMG/pdf/RPC_1896-1897_10.pdf [accessed May 2, 2017].

further assumed that if a body is attached to this thread, the strain the thread transmits to this body is equal to the action this body exerts on this thread. In the end, we used the principle of the equality of action and reaction, not as an experimental truth but as the very definition of force.

This definition is quite as conventional as Kirchhoff's, but far less general. Not all forces are transmitted by threads (besides, to be able to compare them, they would all have to be transmitted by identical threads). Even if we were to admit that the earth is attached to the sun by some invisible thread, we would agree at the very least that we have no means of measuring its elongation. Therefore, nine times out of ten, our definition would fail to deliver. We would find it meaningless and we would have to fall back on Kirchhoff's definition.

Why then take this detour? You accept a certain definition of force that has a meaning only in certain particular cases. In these cases, you experimentally verify that it leads to the law of acceleration. On the strength of this experiment, you then take the law of acceleration as the definition of force in all other cases. Would it not be simpler to consider the law of acceleration as a definition in all cases and to regard the experiments in question, not as verifications of this law, but as verifications of the principle of reaction, or as demonstrating that the deformations of an elastic body depend only on the forces to which this body is subjected? Quite apart from the fact that the conditions under which your definition could be accepted can only be imperfectly met, a thread is never without mass, and it is never shielded from all other forces except the reaction of the bodies attached to its ends.

Andrade's ideas are nonetheless very interesting. While they do not satisfy our need for logic, they help us better understand the historical origins of the fundamental concepts of mechanics. The reflections they suggest show how the human mind progressed from a naïve anthropomorphism to the current scientific concepts. We see at the beginning a very particular and ultimately rather rudimentary experience, and at the end, an entirely general, thoroughly precise law, whose certainty we regard as absolute. We are the ones who have, so to speak, freely bestowed certainty upon it by regarding it as a convention.

Are then the law of acceleration and the rule of composition of forces only arbitrary conventions? Conventions, yes; arbitrary, no. They would be arbitrary if we were to lose sight of the experiments that led the founders of mechanics to adopt the laws and which, as imperfect as these experiments may be, suffice to justify them. From time to time, it is fitting to bring our attention back to the experimental origins of these conventions.

7

Relative and Absolute Motion

The principle of relative motion

Attempts have been made to connect the law of acceleration to a more general principle. The motion of any system must obey the same laws, whether it is plotted on fixed frames of reference or on moving frames of reference carried along in a straight and uniform motion. Such is the principle of relative motion that we accept as necessary for two reasons: first, the most mundane experience confirms it, and second, the mind would be particularly loath to accept the contrary hypothesis.

Let us then assume the principle and consider a body subject to a force. The relative motion of this body with respect to an observer moving with a uniform speed equal to the body's initial speed should be identical to its absolute motion were it to start from a standstill. We conclude that its acceleration must not depend on its absolute speed, some having even attempted to derive from this a demonstration of the law of acceleration.[1] Traces of this demonstration have persisted a long time in French high school science curricula, despite the futility of the attempt. The obstacle that prevented us from demonstrating the law of acceleration was the absence of a definition of force, and that still stands, since the principle invoked did not furnish the missing definition.

The principle of relative motion is nonetheless highly interesting and deserves to be studied for its own sake. Let us first try to state it in a precise manner. We said above that the accelerations of different bodies belonging to an isolated system depend only on their relative speeds and positions, and not on their absolute speeds and positions, as long as the moving frames of reference to which the relative motion is plotted are carried along in a straight and uniform motion. Or, if we prefer, their accelerations depend only on the differences in

[1] In the first edition, this sentence read: "We conclude that its acceleration must not depend on its absolute speed and we attempt to obtain from there the whole law of acceleration" (1902: 135–6).

their speeds and their coordinates, and not on the absolute values of these speeds and coordinates. If this principle is true for relative accelerations or better for differences in accelerations, by combining it with the law of reaction, we can deduce that it remains true for absolute accelerations.

It then remains to be seen how we can demonstrate that differences in accelerations depend only on differences in speeds or coordinates or, in mathematical terms, that these differences in coordinates satisfy second-order differential equations. Can this demonstration be deduced from experiments or from *a priori* considerations? Readers will find the answer on their own by recalling what has been said above. Thus stated, the principle of relative motion is actually strikingly similar to what I called above the generalized principle of inertia, although it is not quite the same thing, since it is a question of the differences between coordinates rather than of the coordinates themselves. Therefore, the new principle shows us something more than the old, but the same discussion applies and would lead to the same conclusions. It is unnecessary to return to it.

Newton's argument

Here we come to a very important and even somewhat unsettling question. I said that for us the principle of relative motion is not simply the result of experiments, and that *a priori* the mind would be loath to accept any contrary hypothesis. Why then is the principle true only when the moving frames of reference are in straight and uniform motion? It should seem just as necessary to us whether or not this motion varied, or at least if it were limited to a uniform rotation. Yet, the principle is true in neither case.

I will not dwell on the case where the motion of the frames of reference is straight without being uniform: the paradox does not hold up under the briefest examination. If I am in a rail car and the train hits some obstacle and comes to a sudden stop, I will be thrown against the opposite seat even though I have not been directly subjected to any force. There is nothing mysterious in that. While I have not been subjected to the action of any external force, the train has experienced an external impact. There is nothing paradoxical in the relative motion of two bodies being perturbed when an external cause modifies the motion of one or the other.

I will spend more time on the case of relative motions plotted on uniformly rotating frames of reference. If the sky were always covered in clouds, if we had no means of observing astronomical bodies, we would be able to conclude nonetheless that the earth rotates. We would be alerted by the earth's flattening or then again by Foucault's pendulum experiment, and yet, would it make any sense in this case to say that the earth rotates? If there is no absolute space, can one rotate without rotating with respect to something? Besides, how could we accept Newton's conclusion and believe in absolute space?

But it is not enough to note that all possible solutions are equally problematic. To make a well-informed choice, we must analyse the reasons for our misapprehensions for each one of them. So please allow the following lengthy discussion.

Let us go back to our imaginary case: dense clouds hide the stars from humans who cannot observe them and do not even know of their existence. How will these people find out that earth rotates? Even more so than our ancestors, they will certainly regard the ground bearing them as fixed and immovable. They will wait a lot longer than we did for the advent of a Copernicus, but this Copernicus would come at last. How would he come?

The physicists in this world would not at first be confronted with an absolute contradiction. We encounter in the theory of relative motion, in addition to real forces, two fictive forces known as the centrifugal force and the Coriolis force.[2] Our imaginary scientists could therefore explain everything by considering these two forces as real, and they would see no contradiction with the generalized principle of inertia, since these forces would depend respectively on the relative positions of the various parts of the systems, as real attractions do, and on their relative speeds, as real frictions do.

However, many difficulties would soon attract their attention. If they succeeded in creating an isolated system, the center of gravity of this system would not have an approximately straight trajectory. To explain this fact, they could appeal to the centrifugal and Coriolis forces, that they would consider real and that they would undoubtedly attribute to the mutual actions of bodies. However, they would not see these forces cancel each other out at large distances, that is to say, to the degree that the system became more isolated. Far from it, centrifugal forces increase indefinitely with distance.

This difficulty would already seem quite substantial to them, and yet it would not stop them for long. They would soon imagine some very subtle medium, analogous to our ether, in which all bodies would be immersed and that would exert a repulsive action on them.

[2] In the original, Poincaré follows Coriolis in calling the two forces the "ordinary and compound centrifugal forces" (1917: 139).

Furthermore, space is symmetrical and yet the laws of motion would not present symmetry and would need to distinguish between right and left. Cyclones, for example, would always be seen rotating in the same direction, although these atmospheric phenomena should for reasons of symmetry rotate indifferently in either direction. If by dint of hard work our scientists managed to make their universe perfectly symmetrical, this symmetry would not endure, although there would be no apparent reason why it should be swayed in one direction rather than the other.

They would no doubt escape this predicament, inventing something that would be no more extraordinary than Ptolemy's glass spheres, and so they would go on like this, accumulating complications, until the long-awaited Copernicus would sweep all of them away in a single stroke by saying: It is much simpler to assume that the earth rotates.

Just as our own Copernicus told us: "It is more useful to suppose that the earth rotates because the laws of astronomy can thus be stated in a much simpler language," this other Copernicus would say: "It is more useful to suppose that the earth rotates, because the laws of mechanics can thus be stated in much simpler language."

Be that as it may, absolute space, that is to say the benchmark to which the earth would have to be plotted to know if it really rotates, has no objective existence. Consequently, the claim "the earth rotates" has no meaning, since it can never be verified by any experiment. Such an experiment could not only be neither realized nor dreamed up by the boldest Jules Verne, but also could not be conceptualized without introducing contradictions. Or rather, the two propositions: "the earth rotates," and "it is more useful to suppose that the earth rotates," have a single meaning. Neither has more to say than the other.

Some may be unsatisfied with this, finding it already objectionable that among all the hypotheses, or rather all the conventions we can come up with on the subject, one might be more useful than the others. But since we have readily granted this for the laws of astronomy, why object to it in the case of mechanics?

We have seen that the coordinates of bodies are determined by second-order differential equations, as are the differences in these coordinates. We have called these the generalized principle of inertia and the principle of relative motion. If the distances between these bodies were likewise determined by second-order equations, it seems that the mind would have to be completely satisfied. To what extent does the mind experience such satisfaction and why is it not pleased with what it encounters?

To understand this, it is better to take a simple example. I propose a system that is analogous to our solar system, but where we cannot see any of the fixed stars foreign to this system, so that astronomers can only observe the mutual distances of the planets and sun, but not the planets' absolute longitudes. If we derive them directly from Newton's law, the differential equations defining the changes in these distances will not be of the second order. I mean that if, in addition to Newton's law, we knew the initial values of these distances and their time derivatives, it would not be enough information to determine the values of these same distances at a future time. One piece of information would still be lacking, a piece of information that could be, for example, what astronomers call the constant of areas.

Two different perspectives may be adopted here. We may differentiate two sorts of constants. From the viewpoint of the physicist, the world is reducible to a succession of phenomena that depend solely, on the one hand, on the initial phenomena and, on the other hand, on the laws connecting consequents to antecedents. Therefore, when observation shows us that a certain quantity is a constant, then two perspectives are open to us. On the one hand, we could assume that there is a law that says that this quantity cannot vary and that it is by chance that, at the beginning of time, it happened to take on one value rather than another, a value that it has had to retain ever since. This quantity could then be called an *accidental* constant. On the other hand, we could assume that there is a law of nature imposing on this quantity one value rather than another. We would then have what could be called an *essential* constant. For example, according to Newton's laws, the duration of earth's revolution must be a constant. However, if it is equal to just over 366 sidereal days rather than 300 or 400, it is a consequence of who knows what initial coincidence. It is an accidental constant. If on the contrary, it is not by chance that the exponent of the distance figuring in the expression of the attractive force is equal to minus two and not minus three, it is because Newton's law requires it. It is an essential constant.

I do not know whether giving chance its role in such a way is in itself legitimate or whether this distinction that I am drawing has something artificial about it. It is certain, at least, that as long as nature has secrets, its implementation will be highly arbitrary and always precarious.

As for the constant of areas, we are in the habit of regarding it as accidental. Is it certain that our imaginary astronomers would do the same? Had they been able to compare two different solar systems, they would have the idea that this constant may take on many different values, but I proposed at the beginning that their system appeared to be isolated and that they could not see any foreign celestial bodies. Under these conditions, they could only see a unique constant with a unique and absolutely invariable value. They would no doubt be led to regard it as an essential constant.

Allow me a word in passing to forestall an objection. Since absolute longitudes would elude the inhabitants of this imaginary world, they could neither observe nor define the constant of areas as we do, but this would not prevent them from rapidly becoming aware of a certain constant naturally inserting itself in their equations, that which would be the same as what we call the constant of areas. Here is what will happen. If the constant of areas is regarded as essential, and as depending on a natural law, then to calculate the distances of the planets at a given time, it will be sufficient to know the initial values of these distances and those of their first derivatives. According to this new perspective, distances would be ruled by second-order differential equations.

Would this however completely satisfy the mind of these astronomers? I do not believe so. First, they would soon realize that by differentiating their equations to raise their order, these equations would become much simpler. Above all, they would be struck by the difficulty resulting from symmetry. Different laws would have to be adopted depending on whether all the planets appeared as the figure of a certain polyhedron or as its dual, a consequence that could be escaped only by considering the constant of areas as accidental.

I chose a very particular example since I supposed that astronomers would not concern themselves at all with terrestrial mechanics and that their viewpoint was limited to the solar system. Yet, our conclusions apply in all cases. Our universe is more extensive than theirs, since we have fixed stars, but it too is limited and we could be thinking about the whole of our universe, just as these astronomers would think about their solar system.

We see in this way that ultimately we would be led to the conclusion that equations defining distances are of an order greater than two. Why would we be shocked by this? Why do we find it so perfectly natural for the succession of phenomena to depend upon the initial values of the first derivatives of these distances, but hesitate to admit that they might depend on the initial values of the second derivatives? It can only be because of mental habits created in us by the constant study of the generalized principle of inertia and its consequences.

The values of the distances at any time depend on their initial values, on those of their first derivatives, as well as on something else. What is this *something else*? If we do not want it to be merely one of the second derivatives, our only choice is among hypotheses. Suppose, as we usually do, that this other thing is the absolute orientation of the universe in space, or the speed at which this orientation changes. This may be and certainly is the most useful solution for the geometer, but it is not the most satisfactory for the philosopher, since this orientation does not exist. We may suppose that this other thing is the position or speed of some invisible body, as some have done, even calling this body the body Alpha,[3] although we are destined never to know anything about it, except its name. We have here a device quite comparable to the one discussed at the end of the paragraph dedicated to my thoughts on the principle of inertia. Yet, the difficulty is ultimately artificial. All that is needed is that our instruments' future readings can only depend on the readings that they have or might have given us in the past, and thus, in this respect, we may rest assured.

[3] Carl Neumann introduced the "body Alpha" in 1870 as an imaginary, fixed universal point of reference. See Carl Neumann, *Über die Prinzipien der Galilei–Newtonschen Theorie* (Leipzig: Teubner, 1870).

8

Energy and Thermodynamics

Energetics

The difficulties raised by classical mechanics led certain thinkers to favor a new system, that they call *energetics*. While energetics arose from the discovery of the principle of the conservation of energy, Helmholtz gave it its definitive form. We will start by defining two quantities playing a fundamental role in this theory, *kinetic energy* or *vis viva* and *potential energy*.

All the changes that natural bodies may undergo are governed by two experimental laws:

1° The sum of kinetic and potential energy is a constant. This is the principle of the conservation of energy.
2° If a system of bodies is in position A at time t_0 and in position B at time t_1, it always moves from the first position to the second along a path such that the *mean* value of the difference between the two kinds of energy is always as small as possible during the time interval separating the two times t_0 and t_1. This is Hamilton's principle, one of the forms of the principle of least action.

Energetics presents the following advantages over the classical theory:

1° It is not as incomplete, that is, the principle of the conservation of energy and Hamilton's principle tells us more than the fundamental principles of classical theory and exclude some motions that do not occur in nature, but that would be compatible with classical theory.
2° It frees us from the atomic hypothesis that was almost unavoidable in classical theory.

Energetics however introduces new difficulties in that the definitions of the two kinds of energy are only slightly better than those of force and mass in the first system—although we would manage better with them, at least in the simplest cases.

Suppose an isolated system consisting of a certain number of physical points. Suppose that these points are subject to forces depending only on their relative position and mutual distances and independent of their speeds. According to the principle of the conservation of energy, a function of forces will exist.

In this simple case, the statement of the principle of the conservation of energy is extremely straightforward. A certain empirically accessible quantity must remain constant. This quantity is the sum of two terms, the first of which depends only on the positions of the physical points and is independent of their speeds. The second is proportional to the square of these speeds. There is only one way to carry out this decomposition.

The first of these terms, which I will call U, will be the potential energy. The second, which I will call T, will be the kinetic energy. It is true that if T + U is a constant, φ(T + U), a function of T + U, will also be a constant. This function φ(T + U) will, however, not be the sum of two terms, one independent of the speeds, the other proportional to the square of these speeds. Among the functions remaining constant, only one enjoys this property: T + U (or a linear function of T + U, which does not change anything since this linear function can always be reduced to T + U by a change of units and of origin). We will, then, call this energy. We will call the latter term potential energy, while the former term will be kinetic energy.[1] The definition of both kinds of energy can therefore be resolved without any ambiguity.

It is the same for the definition of masses. Kinetic energy or *vis viva* may be quite simply stated with the help of the masses and relative speeds of all the physical points, in reference to one of these points. These relative speeds are observable and, once we have the equation of kinetic energy as a function of these relative speeds, the coefficients of this expression will give us the masses.

The fundamental notions may thus be defined without any difficulty in this simple case. The difficulties, however, reappear in the more complicated cases and when forces also depend, for example, on speeds and not solely on distances. Weber, for example, supposes that the mutual action of two electric molecules depends not only on their distance, but on their speed and acceleration. If physical points attracted one another according to a similar law, U would depend on speed and might contain a term proportional to the square of speed.

Among the terms proportional to the squares of speeds, how can we distinguish the terms coming from T from those coming from U? How can the

[1] In the French editions, Poincaré here mistakenly calls T the potential energy and U the kinetic energy.

two components of energy be distinguished? Moreover, how is energy itself to be defined? We no longer have any reason to take T + U as its definition rather than any other function of T + U, once the property characterizing T + U, that of the sum of two terms of a specific form, disappears.

We must also take into consideration mechanical energy properly speaking, as well as other forms of energy: heat, chemical energy, electrical energy, etc. The principle of the conservation of energy should be written T + U + Q = const., where T would represent the tangible kinetic energy, U, the potential energy depending only on the position of the bodies, and Q, the internal molecular energy in its thermal, chemical, or electrical form.

All would go well if these three terms were absolutely distinct, if T were proportional to the square of speeds, U were independent of both these speeds and of the state of bodies, and Q were independent of the bodies' speeds and positions and dependent only on their internal state. The equation for energy could be decomposed into three terms of this form in only one way.

However this is not the case. Let us consider electrified bodies. The electrostatic energy due to their mutual action will obviously depend on their charge, that is to say on their state, but it will also depend on their position. If these bodies are moving, they will act electrodynamically upon one another and the electrodynamic energy will depend not only on their state and position, but also on their speeds. We therefore no longer have any means of distinguishing the terms that must be part of T, U, and Q or of separating the three components of energy.

If (T + U + Q) is constant, so is any function φ(T + U + Q). If T + U + Q had the specific form I considered above, the outcome would be unambiguous. Among the functions ϕ(T + U + Q) that remain constant, only one would have this specific form and it would be this one I would agree to call energy but, as I said, this is not exactly the case. Among the functions that remain constant, none can fit exactly this specific form. So then, how is it possible to choose the one that should be called energy from among these functions? We have nothing left to guide us in our choice, only a single statement of the principle of the conservation of energy: *there is something that remains constant*. In this form, it in turn happens to be out of the reach of experiment and is reduced to a kind of tautology. It is clear that if the world is governed by laws, some quantities will remain constant. Like Newton's principles and, for a similar reason, the principle of the conservation of energy, while grounded in experiment, could no longer be invalidated by it. Our discussion shows that progress was made in moving from the classical to the energetic system, but it shows at the same time that this progress was insufficient.

Another objection seems to me even more serious: the principle of least action is applicable to reversible phenomena, but is not at all adequate when it comes to irreversible phenomena. Helmholtz's attempt to extend it to this type of phenomena did not and could not succeed, leaving us with much work still to be done.

The very statement of the principle of least action is somewhat shocking to the mind. To go from one point to another, a physical molecule not subject to the action of any force, but compelled to move on a surface, will follow the geodesic line, that is to say the shortest path. This molecule seems to know the point where we want to take it, to foresee the time it will take to reach it by following one path or another, and then to choose the most suitable one. The statement presents the molecule to us as a living and free being, so to speak. Clearly, it would be better to substitute it with a less shocking statement where, as philosophers would say, final causes would not seem to replace efficient ones.

Thermodynamics*

The role of the two fundamental principles of thermodynamics is becoming over time increasingly important in all branches of natural philosophy. Abandoning the ambitious theories of forty years ago that were encumbered with molecular hypotheses, we are now attempting to uphold the whole edifice of mathematical physics relying on thermodynamics alone. Will the two principles, that of Mayer and that of Clausius, guarantee that thermodynamics' foundations are solid enough to last for some time? No one doubts it, but on what do we base our assurance?

Speaking about the law of errors, an eminent physicist told me one day: Everyone firmly believes it because mathematicians imagine that it is an observational fact and observers think that it is a mathematical theorem. It was the same for a long time for the principle of the conservation of energy; it is no longer the case today in that everyone recognizes that it is an experimental fact.

So what faculty then gives us the right to attribute to the principle itself greater generality and precision than we attribute to the experiments used to demonstrate it? This amounts to the same thing as asking whether it is legitimate to generalize empirical data, as we do every day, and I will not be so presumptuous as to discuss this question after so many philosophers have attempted in vain to

* The lines that follow partially reproduce the preface of my work *Thermodynamique*.

answer it. Only one thing is certain: if we were denied this faculty, science could not exist or, at least, if it were reduced to a kind of inventory, to the recording of isolated facts, it would have no value for us because it could not satisfy our need for order and harmony, nor our need to make predictions. Since the circumstances preceding any event will likely never all repeat themselves at a single point in time, an initial generalization is necessary in order to predict whether this fact will recur whenever there is even the slightest change in circumstances.

While all propositions can be generalized in an infinite number of ways, we have to make our choice out of all possible generalizations, and that choice must be in favor of the simplest one. We are compelled to act as if a simple law were more probable than a complicated one, all other things being equal. A half a century ago, this was openly recognized and one declared that nature loves simplicity. Since then, nature has provided us with too much evidence to the contrary. Nowadays, the idea that nature loves simplicity is no longer current, we retain only the bare minimum of this kind of thinking such that science remains possible. In formulating a simple and precise general law based on relatively few experiments, which moreover reveal certain discrepancies, we have simply done what the human mind is compelled to do.

However, there is something else going on which moves me to pursue the question further. No one doubts that Mayer's principle will outlive all the individual laws from which it was derived, just as Newton's law outlived those of Kepler from which it came, and which were only estimates, if perturbations are taken into account. Why does Mayer's principle thus occupy such a privileged place among all the physical laws? For many trivial reasons. First of all because we believe that we could not reject it or even question its flawless logic without admitting the possibility of perpetual motion. We are of course wary of this prospect and think that it is more prudent to accept rather than to reject Mayer's principle. Perhaps this is not quite right in that the impossibility of perpetual motion leads to the conservation of energy only for reversible phenomena.

The remarkable simplicity of Mayer's principle also serves to consolidate our belief in it. In a law deduced directly from experiment, like the law of Mariotte, this simplicity would instead be seen as a reason to question it. We, however, face here a different situation. We see straight away that apparently disparate elements are arranged in an unexpected order and form a harmonious whole. We refuse to believe that an unexpected harmony might simply be the result of chance. It seems that the more effort our conquest costs us, the more valuable it is to us or that we are more certain to have wrested from nature the true secret that she guarded from us so jealously.

These are only trivial reasons. To raise Mayer's law to an absolute principle, a more in-depth discussion is needed although, when we try to have such discussion, we see it is not even easy to state this absolute principle. In each specific case, we see exactly what energy is, and may give at least a provisional definition of it. It is, however, impossible to provide a general definition of energy. If we want to state the principle in all its generality and apply it to the universe, we see it disappear so to speak, and only the following remains: *There is something that remains constant*.

Does this even have a meaning? According to the deterministic hypothesis, the state of the universe is determined by an excessively large number of parameters n that I will call $x_1, x_2, \ldots x_n$. As soon as the values of these parameters are known for a given point in time, their time derivatives are known and the values of these same parameters at an earlier or later time may then be calculated. In other words, these n parameters satisfy n first-order differential equations. These equations allow $n - 1$ integrals and so, there are $n - 1$ functions of $x_1, x_2 \ldots x_n$ that remain constant. If we then say that *there is something that remains constant*, we are only stating a tautology. We would even be hard-pressed to say which of all our integrals should be the one bearing the name of energy.

Besides, we do not understand Mayer's principle in this sense when it is applied to a limited system. In this case we admit that p of our n parameters vary independently, so that we only have $n - p$ relations that are generally linear between our n parameters and their derivatives. To simplify the statement, suppose that the total of the external forces' work is zero as are the amounts of heat lost to the outside. Here then is the meaning of our principle: *There is a combination of these $n - p$ relations whose first term is an exact differential* and therefore, since this differential equation is null according to our $n - p$ relations, its integral is a constant and it is this integral that we call energy.

But how is it possible to have many parameters varying independently? Only under the influence of external forces can this happen (even though we have supposed for the sake of simplicity that the algebraic sum of these forces' work is zero). If the system were in fact completely free from any external action, the value of our n parameters at a given time would be sufficient for us to determine the state of the system at any later time as long as the deterministic hypothesis was retained. We would again face the same difficulty mentioned above.

If the future state of the system is not entirely determined by its current state, it is because it also depends on the state of the bodies external to the system. But could there be equations relating the parameters x defining the state of the system that are independent of this state of the external bodies? If in certain cases we think that we can find such equations, is it not only due to our ignorance and because the influence of these bodies is too weak to be detected by our experiment?

If the system is not regarded as completely isolated, it is likely that the absolutely exact expression of its internal energy must depend on the state of the external bodies. Again, I have posited above that the sum of the external work was zero and, in order to overcome this somewhat artificial restriction, the equation of the system's internal energy will become even more difficult. To formulate Mayer's principle and give it an absolute meaning, we must extend it to the whole universe and we would be faced again with the very same difficulty we were trying to avoid.

To conclude using ordinary language, the law of the conservation of energy can have only one meaning, namely that all the possibilities share a common property. Under the deterministic hypothesis there is, however, only one possibility and so the law no longer has any meaning. Conversely, under the indeterministic hypothesis, it would take on a meaning, even if we tried to understand it in an absolute sense. It would appear as a limit on freedom.

This word signals me that I am digressing and on the verge of leaving the domain of mathematics and physics. I will stop then, wanting to retain a single impression from this whole discussion, namely that Mayer's law has a rather adaptable form such that we can make of it almost anything we want. I do not mean by this that it does not correspond to any objective reality, nor that it is reducible to a mere tautology, since it has a perfectly clear meaning in each particular case provided that we do not try to reach the absolute. This adaptability is a reason for believing that it will be long-lasting. Furthermore, since it will disappear only to blend in a higher harmony, we can work with confidence when relying on it, certain beforehand that our labor will not be lost.

Almost everything I just said applies to Clausius' principle. What sets it apart is that it is stated as an inequality. Some may say that it is the same for all physical laws, since their precision is always limited by observational errors, but they at least pretend to be first approximations and one hopes to replace them little by little with increasingly more precise laws. On the contrary, if Clausius' principle is reduced to an inequality, it is not because of the imperfection of our means of observation, but because of the very nature of the question.

General Conclusions for Part Three

The principles of mechanics present themselves to us then in two different forms. On the one hand, they are empirically-grounded truths and to a large extent verified in the case of some quasi-isolated systems. On the other hand, they are postulates applicable to the entire universe and are regarded as strictly true.

If these postulates possess a generality and certainty that the experimental truths from which they are drawn were lacking, it is because they are in the end reducible to a mere convention that we can make because we are already certain that no experiment will contradict it. Even so, this convention is not completely arbitrary, nor is it the result of a whim. We adopt it because some experiments have shown us that it would be useful.

We can explain in this way how experiment has thus been able to build the principles of mechanics and yet cannot overturn them. By comparison, take geometry. The fundamental propositions of geometry, such as Euclid's postulate, are also nothing more than conventions and it is as absurd to try to find out whether they are true or false as it is to ask whether the metric system is true or false. Only, these conventions are useful, as certain experiments have shown us.

At first sight, the analogy is complete; the role of experiment seems the same in both cases. It is tempting then to say that either mechanics must be regarded as an experimental science, and then the same must be true of geometry, or rather on the contrary, that geometry is a deductive science and then the same can be said of mechanics.

Such a conclusion would be unjustified. The experiments that led us to adopt the fundamental conventions of geometry as more useful are concerned with objects that have nothing in common with those studied in geometry. They are concerned with the properties of rigid bodies and with the rectilinear propagation of light. They are experiments in mechanics, experiments in optics. In no way can we regard them as experiments in geometry. In fact, the main reason our geometry seems useful to us is that the different parts of our body, our eyes, our limbs, possess precisely the properties of rigid bodies. Looked at in this way, our fundamental experiments are first and foremost experiments in physiology not concerned with space, which is the object the geometer must study, but with the geometer's body, that is with the instrument that must be used for this study. Conversely, the fundamental conventions of mechanics and the experiments proving that they are useful are both actually concerned with either exactly the same objects or analogous ones. The conventional, general principles are the natural and direct generalization of the particular, experimental ones.

Let it not be said that in this way I am thus drawing artificial boundaries between the sciences, that since I am placing a barrier between geometry strictly speaking and the study of rigid bodies, I could just as well place one between experimental mechanics and the conventional mechanics of general principles. Who does not see in fact that by thus separating these two sciences, I would fundamentally alter both of them and that the very little that would be left of conventional mechanics after it was isolated would absolutely not be comparable to this superb body of knowledge called geometry?

We now see why the teaching of mechanics must remain experimental. Only in this way can it make us understand the origins of this science, which is essential for a complete understanding of mechanics itself. What is more, if we study mechanics, it is to apply it and it cannot be applied unless it remains objective. Now, as we have seen, what the principles gain in generality and certainty, they lose in objectivity. We must therefore familiarize ourselves right away especially with the objective side of the principles, which can be done only by going from the particular to the general instead of the opposite.

The principles are conventions and definitions in disguise. They are, however, drawn from experimental laws that have, so to speak, been raised into principles to which our mind attributes an absolute value. A few philosophers have overgeneralized, believing that the principles are all that there is to science and, consequently, that science is entirely conventional. This paradoxical doctrine, called nominalism, does not hold up to scrutiny.

How can a law become a principle? It expressed a relation between two real terms, A and B, but was not strictly true, rather only approximate. We arbitrarily introduce a more or less imaginary intermediary term C which has *by definition exactly* the relation with A that is expressed by the law. Our law has then split itself into an absolute and rigorous principle stating the relation of A to C, and an approximate and revisable experimental law stating the relation of C to B. Clearly some laws will always remain, however far we push this decomposition.

We will now enter the domain of genuine laws.

Part Four

Nature

9

Hypotheses in Physics

The role of experiment and generalization

Experiment is the sole source of truth, it alone can teach us something new, and it alone can give us certainty. These are two points no one can deny. Then if experiment is everything, what place will there be for mathematical physics? What could experimental physics make of such an assistant, seemingly useless and perhaps even dangerous?

Yet mathematical physics exists and it has rendered undeniable services. We have here a fact that needs to be explained. The explanation is that observation is not enough, that we must use our observations and, to do so, we must generalize as we have always done. Only, as the memory of past mistakes has made humans more and more circumspect, we have observed more and more and generalized less and less. Every century has ridiculed its predecessor, accusing it of having generalized too hastily and too naïvely. Descartes felt sorry for the Ionians while he in turn makes us smile. No doubt, our children will someday laugh at us.

Could we not get straight to the point? Is this not the means to escape this ridicule we expect? Cannot we settle for experiment alone? No, this would be impossible. It would be completely misunderstanding science's true character. The scientist must organize. Science is made with facts, like a house is made with stones, but an accumulation of facts is no more a science than a pile of stones is a house.

First and foremost, the scientist must make predictions. Carlyle has written somewhere something along these lines: "Facts alone matter. John Lackland was here, this is what is admirable. Here is a reality for which I would give all the theories of the world." Carlyle was Bacon's compatriot,[1] but Bacon would not

[1] In the 1902 French edition, Poincaré added "[. . .] like him [Bacon], he [Carlyle] was anxious to proclaim his worship *for the God of Things as they are* [. . .]" (1902: 168; the italics are in English in the text). The passage mentioned refers to Thomas Carlyle, *Past and Present: Thomas Carlyle's Collected Works*, *Vol. XIII* (2008).

have said that. It is the language of the historian. The physicist would rather say: "John Lackland was here; it is all the same to me since he will never come back."

We all know that there are both good experiments and poor ones, and the latter will accumulate in vain. Whether a hundred or a thousand are performed, a single piece of work by a true authority, a Pasteur for example, will be enough to sink them into oblivion. Bacon would have understood this very well, having invented the expression *crucial experiment*.[2] But Carlyle must not have understood. A fact is a fact. A schoolchild has read a certain number on his thermometer. He has not taken any precautions, never mind, he made the reading and, if only facts matter, this is as much a reality as the peregrinations of King John Lackland. Why is the fact that this schoolchild made this reading of no interest, whereas the fact that a skilled physicist might have made another reading would, on the contrary, be considered very important? It is because we cannot make any conclusions from the first reading.[3] What then is a good experiment? It is one that informs us of something besides an isolated fact, one that allows us to make predictions; that is, that allows us to generalize. For without generalization, we cannot make predictions. The circumstances under which we worked will never all be reproduced simultaneously. The observed fact will therefore never reoccur. The only thing that can be affirmed is that under analogous circumstances, an analogous fact would occur. To make predictions, it is at least necessary to call upon an analogy, which means already to generalize.

Even if tentatively, we need to fill in the gaps. Experiment only gives us a certain number of isolated points that must be joined together by a continuous line. This is a real generalization, yet we will go even further. The curve we will draw will pass between and close to the observed points, but not through them. We therefore do not limit ourselves to generalizing the experiment, we correct it. The physicist who would want to refrain from making corrections and to settle for the raw experiment would have to formulate quite extraordinary laws. Raw data are not sufficient, which is why we need ordered or, rather, organized science.

We often say that experimentation must be done without any preconceived ideas, but it is not possible. It would not only make all experiment fruitless, but even if we wanted to eliminate preconceptions, we could not do so. We all have our own conception of the world that we cannot easily dismiss. We certainly must use language, for example, and our language consists entirely of preconceived ideas. However, they are unconscious preconceived ideas, which

2 Poincaré gives the Latin "*experimentum crucis*."
3 These last two sentences are not in the first edition.

are so much more dangerous than other ideas. Will we say that if we bring in other preconceived ideas, of which we are fully conscious, we will only make matters worse? I do not think so. I believe that they will instead balance each other out or, I might say, counteract one another. They will generally not be very compatible and will come into conflict with each other, thus forcing us to consider things from different perspectives. It is enough to free us: we are no longer slaves if we can choose our master.

In this way, owing to generalization, each observed fact allows us to predict many more. However, we should not forget that only the first one is certain, while all the others are merely probable. However firmly anchored a prediction may seem to us, we are never *absolutely* certain that experiment will not disprove it if we attempt to verify it. However, probability is often high enough to be sufficient for all practical purposes. Better to predict without certainty, than not at all.

We should therefore never turn down a chance to make a verification. However, experiments are long and difficult, workers are few, and the number of facts we must predict is extremely high. Compared to this volume, the number of direct verifications we will be able to make will never be but a negligible quantity. We must make the most of the little we can have access to directly. Each experiment must allow us the largest possible number of predictions, with the highest degree of probability possible. The problem, so to speak, is to increase the performance of the scientific engine.

Let me compare science to a library that must grow forever. The librarian has insufficient funds for purchases and must try hard not to waste them. Experimental physics is in charge of purchasing and it alone can enlarge the library. As for mathematical physics, its mission will be to set up the catalogue. A well-constructed catalogue will not enlarge the library, but it will help the reader use its resources. Moreover, by indicating the gaps in the collections to the librarian, it will enable staff to use the library's funds more judiciously, which is all the more important given that these funds are completely inadequate.

Such is the role then of mathematical physics. It must direct generalization to increase what I called earlier the performance of science. It remains for us to investigate the means by which it manages to do so and how it can do so safely.

The unity of nature

First observe that any generalization presupposes to a certain extent a belief in the unity and the simplicity of nature. With respect to unity, there cannot be any difficulty. If the different parts of the universe were not like the organs of the same body, they would not act upon one another, they would be unaware of each other and we in particular would know only one of them. We do not have to ask if nature is one, but how it is one.

The second point is not so easy. It is not certain that nature is simple. Are there risks if we act as if it were? There was a time when the simplicity of Mariotte's law was used as an argument in favor of its accuracy, a time when Fresnel himself, after having said in a conversation with Laplace that nature does not trouble itself with analytical difficulties, felt obliged to qualify his position so as not to offend the prevailing opinion. Views are quite different nowadays and yet, those who do not believe that natural laws must be simple are still often forced to act as if they do. They would not be able to completely escape this imperative without making any generalizations, and consequently making all of science impossible.

It is clear that any fact can be generalized in an infinite number of ways and it is a question of choosing between them. The choice can only be guided by considerations of simplicity. Let us take the most mundane case, that of interpolation. We draw a continuous line between the points given by observation, and make it as regular as possible. Why do we avoid the most angular points, the bends that are too abrupt? Why do we not make our curve describe the most irregular twists and turns? It is because we know beforehand, or believe we know, that the law to be stated cannot be that complicated.

We can deduce Jupiter's mass from the motion of its satellites or from the perturbations of the gas giants or that of the asteroids. If we take the mean of the results obtained by these three methods, we get three very close but different numbers. We can interpret this result by supposing that the gravitational constant is not the same in the three cases, in which case the data would certainly be much better represented. Why do we reject this interpretation? Not because it is absurd, but because it is needlessly complicated. We will accept it only once it shows itself to be necessary, which it has not yet done.

In conclusion, a law is usually deemed simple until shown to be otherwise. This practice is forced upon physicists for the reasons just outlined, but how can it be justified given the discoveries that every day show us new and increasingly rich and complex details? How can it even be reconciled with the belief in the unity of nature? For if everything depends on everything else, the relations involving so many diverse objects can no longer be simple.

If we study the history of science, we see two opposite phenomena emerging: Sometimes it is simplicity that hides under complex appearances, sometimes it is conversely simplicity that is apparent and conceals extremely complicated realities.

What could be more complicated than the perturbed motion of planets, or simpler than Newton's law? Making light work of analytic difficulties, as Fresnel said, nature uses only simple means and generates, by combining them, an inextricable tangle. This is hidden simplicity, one that must be uncovered.

Examples to the contrary abound. In the kinetic theory of gases, we consider molecules moving at high speeds whose trajectories, distorted by constant collisions, have the most irregular shapes and crisscross space in every direction. The observable result is Mariotte's simple law. Each individual fact was complicated. The law of large numbers has re-established simplicity with the mean. Here simplicity is only apparent and only the crudeness of our senses prevents us from perceiving the complexity.

Many phenomena obey a law of proportionality, but why? It is because in these phenomena there is something very small. The observed simple law is therefore nothing but an instance of this general analytic rule by which an infinitely small increase in a function is proportional to the increase in the variable. Actually our increases are not infinitely small, but rather only very small, so the law of proportionality is just approximate, and simplicity is only apparent. What I just said applies to the rule for the superposition of small motions, whose use is so fruitful and which is the foundation of optics.

And Newton's law itself? Its long-hidden simplicity is perhaps only apparent. Who knows if it is not due to some complicated mechanism, to the collisions of some subtle matter moving with irregular motions, and if it has not become simple only through the interplay of averages and large numbers? In any case, it is difficult not to suppose that the true law contains complementary terms that would become perceptible at small distances. If in astronomy they are negligible compared to Newton's terms and the law thus regains its simplicity, it would be solely because of the immensity of astronomical distances.

Undoubtedly, if our tools of investigation became increasingly more sensitive, we would discover the simple under the complex, then the complex under the simple, then again the simple under the complex, and so on, without being able to predict which term would be the last. Of course, we have to stop somewhere and, for science to be possible, we must stop once we have found simplicity, which is the only ground on which we can build the edifice of our generalizations. Since simplicity is only apparent, will it be a solid enough ground? We would do well to look into this.

In order to do so, let us see what role belief in simplicity plays in our generalizations. We have verified a simple law in a rather large number of particular cases. We do not accept that this often repeated conjunction is merely due to chance and conclude that the law must be true in the general case. Kepler notices that the positions of a planet observed by Tycho are all on the same ellipse. He does not think for a moment that by an odd coincidence, Tycho has never observed the sky except when the true trajectory of the planet happened to follow this ellipse. What does it matter whether simplicity is real or conceals a complex truth? Whether it is due to the influence of large numbers that level out individual differences, or whether it is due to the large or small magnitude of certain quantities that allow us to neglect certain terms—in no case is it due to chance. Real or apparent, this simplicity always has a cause. We will then always be able to follow the same reasoning and if a simple law has been observed in many particular cases, we will be able to suppose legitimately that it will still be true in analogous cases. To fail to do so would be to attribute to chance an inadmissible role.

Nevertheless there is a difference. If simplicity were real and profound, it would resist the increasing precision of our means of measurement. If we believe nature to be profoundly simple, we should proceed from an approximate simplicity to a rigorous one. While we have done so in the past, we do not have the right to do so any more. The simplicity of Kepler's laws, for example, is only apparent. They are nevertheless still very nearly applicable to all systems analogous to the solar system, although this prevents them from being rigorously exact.

The role of hypotheses

Every generalization is a hypothesis. Hypotheses thus play a necessary role that no one has ever disputed. Only they should always be subjected to verification, as soon and as often as possible. It goes without saying that when they do not withstand this test, they must be abandoned without reservation. We usually do so, although sometimes somewhat testily.

Well, even this ill humor is not justified. The physicist who has just given up a hypothesis should, on the contrary, be overjoyed, having just found an unexpected opportunity for discovery. The hypothesis, I imagine, had not been adopted lightly. It took into account all known factors that seemed likely to come into play in the phenomenon. When verification does not happen, it is because there is something unexpected, extraordinary. It is because we are about to find something unknown and new.

Has a hypothesis thus overturned been fruitless? Far from it. We may say that it has been more helpful than a true hypothesis. Not only has it provided the opportunity for the decisive experiment, but had we done this experiment at random, without having the hypothesis, we would have gained nothing from it. We would not have seen anything extraordinary. We would have merely catalogued one more fact without drawing any conclusion from it.

Now on what condition can hypotheses be used without risk? The firm intention to follow experiment's lead is not sufficient. There are still dangerous hypotheses. They are first and foremost those hypotheses that are tacit and unconscious. Since we make them without being aware of it, we are unable to abandon them. So here again is a service that mathematical physics can provide us. Its characteristic precision compels us to formulate all the hypotheses we would use implicitly without it.

Notice also that it is important not to produce hypotheses endlessly one after another. If we build a theory based on multiple hypotheses and experiment condemns it, which of our premises do we need to change? It will be impossible to know. Conversely, if the experiment succeeds, will we believe that we have verified all these hypotheses at once? Will we believe that we determined many unknowns with a single equation?

We must also take care to distinguish between the different kinds of hypotheses. First there are those that are entirely natural and from which we cannot escape. It is difficult not to suppose that the influence of very distant bodies is altogether negligible, that small motions obey a linear law, that the effect is a continuous function of its cause. I would say the same of the conditions imposed by symmetry. All these hypotheses form, so to speak, the foundation common to all the theories of mathematical physics. They are the last ones that we should abandon.

There is a second category of hypotheses which I would describe as indifferent. In most situations, the analyst assumes at the start of the calculations that matter is either continuous or, conversely, made of atoms. The results would come out the same regardless of the initial assumption. It would only have been more difficult to obtain them. If experiment then confirms the conclusions, will the analyst believe that, for instance, the true existence of atoms has been demonstrated?

Of the two vectors introduced in optical theories, one is regarded as a speed, the other as a vortex. Once again, we have an indifferent hypothesis since we would have reached the same conclusions by doing precisely the opposite. The success of the experiment can therefore not prove that the first vector is definitely a speed. It only proves one thing, that it is a vector, which is the only hypothesis truly introduced in the premises. To give it this concrete appearance that the weakness of our mind requires, we had to consider it as either a speed or a vortex, just as we had to represent it by a letter, either *x* or *y*. Yet the result, whatever it may be, will not prove that it was right or wrong to regard it as a speed, any more than it would prove that it was right or wrong to call it *x* and not *y*.

These indifferent hypotheses are never dangerous as long as we understand their character. They may be useful, either as an arithmetic trick, or to assist our understanding with concrete images, to clarify our thoughts. There is therefore no reason to prohibit them.

The hypotheses of the third category are true generalizations. They are the ones that experiments must confirm or invalidate. Whether verified or condemned, they will always be fruitful. However, for the reasons I have given, they will be so only if we do not make too many of them.

Origin of mathematical physics

Let us forge ahead and investigate more closely the conditions that allowed the development of mathematical physics. We realize at once that the scientists' attempts have always tended towards dividing the complex phenomenon directly given by experiment into a very large number of elementary phenomena. One of three different approaches is always used. First, the phenomena may be decomposed with respect to time. Instead of embracing the progressive development of a phenomenon in its entirety, we simply seek to connect each instant to the one immediately preceding it, admitting that the present state of the world depends only on the most immediate past, without being directly influenced by the memory of a distant one. Thanks to this postulate, rather than directly studying the entire series of phenomena, it can simply write its "differential equation." We substitute Newton's law for Kepler's.

Next we try to decompose the phenomenon with respect to space. What experience gives us is a muddled set of facts taking place in a somewhat expansive setting. We must work to discern the elementary phenomenon which will be, on the contrary, localized in a very small region of space.

A few examples will perhaps help to make my thoughts clearer. If we wanted to study the distribution of the temperature in a cooling solid in all its complexity, we would never succeed. Everything becomes simple if we consider that a point in the solid cannot directly give off its heat to a distant point. It will directly yield some to the nearest points and, gradually, the heat flow will reach other parts of the solid. The elementary phenomenon is the heat exchange between two contiguous points. It is strictly localized and relatively simple if we assume, as is natural, that it is not influenced by the temperature of the molecules at an appreciable distance.

I bend a yardstick. It will take on a very complicated shape that we cannot study directly, but which I will be able to approach, if I observe that its bending is merely the combined result of the deformation of the very small components of the yardstick and that the deformation of each of these components depends only on the forces directly applied to it and not at all on those that might be acting on the other components.

In all these examples, which I could easily multiply, we assume that there is no action at a distance, or at least at a very large distance. This is a hypothesis that is not always true, as demonstrated by the law of gravity. It must therefore be subjected to verification. If it is confirmed, even approximatively, it is extremely valuable for it will enable us to do mathematical physics, at least by making successive approximations.

If it does not stand the test, we must search for something else that is analogous, for there are still other means to reach the elementary phenomenon.[4] If many bodies act simultaneously, their actions may happen to be independent and simply be added to one another, either as vectors, or as scalars. The elementary phenomenon is then the action of an isolated body. Or then again, we are faced with small motions or, more generally, small variations that obey the well-known law of superposition. The observed motion will then be decomposed into simple ones, the sound into its harmonics, for example, or the white light into its monochromatic components.

Once we have figured out from what direction the elementary phenomenon should be approached, by what means can it be reached?

First, to figure it out, or rather to figure out what is useful to us, it will often not be necessary to work out its mechanism. The law of large numbers will be sufficient. Let us go back to the heat propagation example. Each molecule radiates towards each neighboring molecule, according to a law that we do not need to know. If we were to venture a guess as to the law, it would be an indifferent and consequently a useless and unverifiable hypothesis. As a matter of fact, through the effects of the averages and thanks to the medium's symmetry, all the differences even out and, regardless of what hypothesis is made, the result is always the same.

[4] For discussion of elementary phenomena, see Olivier Darrigol, *Physics and Necessity* (Oxford: Oxford University Press, 2014), 202–3.

The same situation appears in the theory of elasticity or in that of capillarity. The neighboring molecules attract and repel each another, but we do not need to know which law they are following. It is enough for us that this attraction is discernible only at short distances, that there are numerous molecules, and that the medium is symmetrical, and we will only have to let the law of large numbers act.

Here again the simplicity of the elementary phenomenon was hiding under the intricacy of the resultant observable phenomenon. However, this simplicity was, in turn, merely apparent and hiding a very complex mechanism.

The best way to reach the elementary phenomenon would obviously be experiment. The complex bundle of evidence that nature offers for us to study should be broken apart by experimental devices and we should carefully study its elements in as purified a form as possible. For example, natural white light will be split into monochromatic light rays with the help of a prism and into polarized light rays with the help of the polarizer.

Unfortunately, this is neither always possible nor always sufficient, and the mind must sometimes get ahead of experiment. I will mention only one example that has always deeply struck me. If I split white light, I will be able to isolate a small portion of the spectrum but, however small it is, it will always retain a certain width. Similarly, what are called *monochromatic* natural lights give us a beam of light that is very narrow, although not infinitely so. We might suppose that by experimentally studying the properties of these natural lights, by operating with increasingly fine spectral lights and by finally "passing to the limit," so to speak, we would be able to know the properties of a strictly monochromatic light. This would not be exact. Suppose that two beams of light are coming from the same source, that we first polarize them along the two rectangular planes and then bring them back in the same polarization plane trying to cause interference. If the light was *strictly* monochromatic, they would interfere, but there will be no interference with our nearly monochromatic lights, however narrow the ray. For it to be otherwise, it would have to be many millions of times narrower than the narrowest known beams.

Such [experimental] "passage to the limit" would thus have fooled us. The mind had to get ahead of experience and, if it has done so successfully, it is because it has let itself be guided by the instinct of simplicity.

Our knowledge of the elementary fact enables us to write the problem as an equation. We only need then to deduce the observable and verifiable complex fact by combining. It is what we call *integration* and is the task of the mathematician.

We can wonder why generalization readily takes the mathematical form in the physical sciences. The reason is now easy to see. It is not only because we have to express numerical laws, but also because the observable phenomenon is due to the superposition of a great number of elementary phenomena that are *all alike*. Differential equations are thus quite naturally introduced.

It is not sufficient for each elementary phenomenon to obey simple laws, all those to be combined must obey the same law as well. Only then is mathematics' intervention useful. Mathematics do in fact teach us to combine like with like with the goal of guessing the result of a combination without needing to redo this combination bit by bit. If we have to repeat the same operation many times, mathematics allows us to avoid this repetition by making the result known to us beforehand, thanks to a kind of induction, as I have explained earlier, in the chapter on mathematical reasoning. However, to do this, all these operations must be alike. Otherwise, we would obviously have to resign ourselves to performing them one after another and mathematics would become useless. It is therefore thanks to the near homogeneity of the matter studied by physicists that mathematical physics could be born.

We no longer find in the natural sciences the following conditions: homogeneity, relative independence of distant parts, simplicity of the elementary fact, and this is why naturalists are compelled to resort to other modes of generalization.

10

Theories of Modern Physics

The meaning of physical theories

The educated public is surprised to see how short-lived scientific theories are. They see them abandoned after a few years of favor, the wreckage of one theory piled on the rubble of another, anticipating that today's fashionable theories will soon give way, and from this conclude then that these theories are completely futile. They call this *the bankruptcy of science*.

Their skepticism is superficial since they are not at all aware of what the goal and role of scientific theories are, otherwise they would understand that abandoned scientific theories are still useful. No theory seemed more solid than Fresnel's which attributed light to the motions of the ether. Nevertheless, we now prefer Maxwell's theory. Does this mean that Fresnel's accomplishments were in vain? No, for Fresnel's goal was not to know whether ether exists, nor whether or not it is made up of atoms, nor whether these atoms really move in this or that direction. The goal was to predict optical phenomena, which Fresnel's theory still enables us to do, now as well as before Maxwell. The differential equations remain true, they can still be integrated by the same methods, and the results of this integration still retain their full value.

This is not to say that we are thus reducing physical theories to the role of mere practical recipes. The equations express relations and if these equations remain true, it is because the relations preserve their reality. Now as before, they teach us that there is a particular relationship between something and something else. We formerly called this something *motion* and we now call it *electric current*, but these labels were only images standing in for the real objects that nature forever hides from us. The true relations between these real objects are the only reality we can reach, the only condition being that the relations between the objects are the same as those between the images standing in for the objects. It does not matter whether we find it useful to replace one image by another, as long as these relations are known to us.

Whether a given periodic phenomenon (an electric oscillation for example) is really due to the vibration of a specific atom that actually moves in this or that direction like a pendulum is neither certain nor interesting. However, we can maintain that between electric oscillations, the motion of pendulums, and all the other periodic phenomena, there is a close relationship corresponding to a deep reality and that this relationship, this similarity, or rather this parallelism still holds in the details and is a consequence of more general principles—those of energy and of least action. This is a truth that will remain forever the same under all the disguises that we may find useful to apply to it.

Many theories have been proposed for dispersion, the first of which were imperfect, containing little truth. Next came Helmholtz's theory, which was then modified in various ways, its author himself imagining another theory grounded in Maxwell's principles. Remarkably, all the scientists after Helmholtz arrived at the same equations from different and seemingly quite distant starting points. I dare to say that these theories are all simultaneously true, not merely because they allow us to predict the same phenomena, but because they bring to light a true relation, the one between absorption and anomalous dispersion. What is true in the premises of these theories is what is common to all the authors. It is the statement of this or that relationship between certain things called by one name by some and by another name by the others.

The kinetic theory of gases has provoked many objections that we could scarcely answer if we claimed to regard it as the absolute truth. All these objections, however, will not change the fact that the theory has been useful, and particularly so when it showed us the actual relationship between gas pressure and osmotic pressure, which otherwise had remained unknown to us. In this sense, we can say that the theory is true.

A physicist noticing a contradiction between two theories equally dear will sometimes say: "Let us not worry about this. Rather let us hold fast to the two ends of the chain, even though the middle links are hidden from us." An apologetic argument like this would be ridiculous if we applied the layman's understanding to physical theories. If there were a contradiction, at least one of the two theories would have to be considered false. If we only seek in theories what we should seek in them, this is no longer the case. Both theories may express true relations, the contradiction being only in the images draped over reality.

To those who find that we too strictly limit the domain accessible to the scientist, I answer: "These off-limits areas of inquiry that you cannot approach are not only insoluble, they are illusory and meaningless."

Some philosopher claims that physics can be completely explained by the collisions of atoms with each other. If the philosopher simply means that the same relations are found between physical phenomena, as between a large number of billiard balls bouncing off of each other, that is fine! It is verifiable, perhaps true. The philosopher means something more, however, and we think we understand because we think we know what a collision is in itself. Why? Simply because we have often watched billiard games. Are we to understand that God experiences the same sensations when contemplating his works as we do when we watch a game of billiards? If to the philosopher's claim we want to give neither this peculiar meaning nor the restricted one I explained earlier and is the right one, then this claim has no longer any meaning. Hypotheses of this kind therefore have only a metaphorical meaning. Scientists need not abstain from them any more than poets abstain from metaphors, but they should know their worth. Metaphors may be useful in bringing some satisfaction to the mind and will not be harmful provided they are only indifferent hypotheses.

The previous considerations explain why some theories, considered defunct and to have been disproven experimentally, suddenly rise from their ashes and are reborn. It is because they expressed true relations, continuing to do so when, for one reason or another, we believed it necessary to state the same relations in another language. They had in this way retained a kind of latent life.

Just fifteen years ago, was there anything more ridiculous, more naïvely old school than Coulomb's fluids? And yet, here they are reappearing under the name of *electrons*. In what way do these molecules with a permanent electric charge differ from Coulomb's electric molecules? It is true that in electrons, electricity is held by a little matter, but very little. In other words, they have a mass (and even then, nowadays we dispute this).[1] Furthermore, Coulomb did not deny that his fluids had mass or, if he did, it was only reluctantly. It would be ill-advised to claim that the belief in electrons will not fall out of favor again. It was nonetheless strange to notice this unexpected resurgence.

The most striking example is, however, that of Carnot's principle, which Carnot established from false hypotheses. When it was realized that heat is not indestructible, but can be transformed into work, his ideas were completely abandoned. Clausius later returned to them and assured their definitive acceptance. In its primitive form, Carnot's theory expressed, apart from true relations, other inexact relations, the remains of old ideas. But the presence of

[1] The material in parentheses was not in the first edition. The issue is raised again in Chapter 14, pp. 163–4.

the inexact relations did not change the reality of the true ones. Clausius only had to throw them aside as one prunes dead branches.

The result was the second fundamental law of thermodynamics. The relations were still the same, although no longer holding, at least in appearance, between the same objects. This was enough for the principle to retain its value. Even Carnot's arguments did not die out because the principle retained its value. They were applied to a body of knowledge riddled with errors, but their form (that is to say, what is essential) remained correct.

What I have just said sheds light on the role of general principles such as the principles of least action or of the conservation of energy. These principles are very valuable. We uncovered them by seeking the commonalities in the statements of numerous physical laws, so they represent the most essential features of countless observations.

Nevertheless, from their general nature itself follows a consequence to which I have drawn attention in Chapter 8, namely that these principles may no longer be verified. Since we cannot give a general definition of energy, the principle of the conservation of energy simply means that there is *something* that remains constant. Whatever new notions future experiments may give us of the world, we are certain beforehand that something will remain constant that we will be able to call *energy*.

Do we mean to say that the principle is meaningless and reduces to a tautology? Not at all, the principle signals that the different things we call *energy* are related by a true kinship. It states the existence of a real relationship between them. If this principle then has a meaning, it could be false. We might not have the right to extend its applications indefinitely and yet, it is already certain that the principle will be verified in the strict sense of the word. What will warn us, then, that we have applied it to as many cases as we legitimately can? It will simply cease to be of use to us; that is, it will no longer enable us to safely predict new phenomena. In such a case, we will be certain that the stated relationship is no longer real, for otherwise it would be fruitful. Without directly contradicting a new extension of the principle, experiment would nevertheless condemn it.

Physics and mechanism

Most theoreticians have a lasting predilection for explanation borrowed from mechanics or dynamics. Some would be satisfied if they could account for all phenomena by the motions of mutually attractive molecules that followed certain laws. Others are more demanding and would like to eliminate attractions at a distance. Their molecules would follow straight lines from which they would only deviate as a result of collisions. Others still, like Hertz, also eliminate forces, but assume that their molecules are subject to geometrical connections analogous, for example, to those of our mechanical linkages. They are trying in this way to reduce dynamics to a sort of kinematics.

In a nutshell, all the theoreticians are trying to bend nature into a certain shape to suit their intellectual taste. Will nature be flexible enough? We will consider the question in Chapter 12, which discusses Maxwell's theory. Every time that the principles of energy and of least action are satisfied, we will see that there is always an infinite number of possible mechanical explanations, not simply one. Thanks to a well-known theorem by König on mechanical linkages, it could be shown that there is an infinite number of ways to explain everything either with links, as Hertz does, or with central forces. We could probably demonstrate just as easily that everything may also be explained by simple collisions.

To do this, we cannot of course decide the question with ordinary matter, that which we perceive and whose motions we observe directly. Either we will suppose that this ordinary matter is composed of atoms whose internal motions elude us, leaving us to observe only the movement of the whole piece of matter, or we will imagine one of those subtle fluids which, under the name of *ether* or some other names, have always played such an important role in physical theories.

Some often go further and regard the ether as the only primitive matter, or even the only real matter. The more moderate of these thinkers consider ordinary matter as condensed ether, which is not at all shocking; but others give it even less importance, seeing it as nothing more than the geometric location of the ether's singularities. According to Lord Kelvin, for example, what we call *matter* is nothing but the location of the points around which the ether swirls. For Riemann, it was the location of the points where ether is continuously destroyed. According to other more recent authors, Wiechert or Larmor, it is the location of the points where the ether undergoes a kind of torsion of a very specific nature. If we want to adopt one of these points of view, I wonder what right we would have to extend to the ether, under the pretext that it is the true matter, the mechanical properties observed in ordinary matter, which is only some spurious matter.

Former fluids—caloric, electricity, etc.—were abandoned when we realized that heat is not indestructible, but they were also abandoned for another reason. By reifying them, their individuality was, so to speak, accentuated, creating between them a kind of gap that had to be filled once we gained a more acute sense of the unity of nature and perceived the close relations connecting all its parts. By multiplying the number of fluids, past physicists were not only creating entities needlessly, they were breaking apart true connections. A theory must not only avoid stating false relationships, it must also not conceal true ones.

Does our own ether really exist? We know where our belief in the ether originates. If light comes to us from a distant star, then for many years, it is no longer on the star, nor is it yet on earth. It must then be somewhere and be held aloft, so to speak, by some physical support.

We can express the same idea in a more mathematical and more abstract form. What we notice are the changes undergone by the physical molecules. We see, for example, that our photographic plate is subject to the consequences of the phenomena that took place on the incandescent mass of the star many years earlier. Now in ordinary mechanics the state of the system under consideration depends solely on its immediately prior state. Thus, the system satisfies some differential equations. To the contrary, if we did not believe in the ether, the state of the physical universe would depend not on the immediately prior state, but on much earlier states. The system would satisfy finite-difference equations. It is to avoid this departure from the general laws of mechanics that we have invented the ether. We would of course still be forced to fill the interplanetary void with ether, but not to make it penetrate within the physical realms themselves. Fizeau's experiment goes further still. Through the interference of light rays that passed through air or water in motion, the experiment seems to show us two different mediums entering each other and yet moving with respect to one another. We feel as if we were within a hair's breadth of the ether.

However, it is possible to conceive of experiments that would bring us even closer. Suppose that Newton's principle of the equality of action and reaction were no longer true when applied to matter *alone* and that we happened to notice it. The geometric sum of all the forces applied to all the physical molecules would no longer be zero. If we did not want to change all of mechanics, we would need to introduce the ether so that this action that matter apparently experiences would be counterbalanced by the reaction of matter on something else.

Or then again, suppose that we realized that optical and electrical phenomena are influenced by the earth's motion. We would be led to the conclusion that these phenomena could reveal to us not only the relative motion of material bodies, but what would seem to be their absolute motion. Again, an ether would be necessary so that these so-called absolute motions would not be the bodies' displacements with respect to an empty space, but their displacements with respect to something concrete.

Will we ever get there? I do not share this hope, and will say why later, yet it is not so absurd since others have entertained it. For example, if Lorentz's theory that I will discuss in detail later in Chapter 13 were true, Newton's principle would not apply to matter *only* and the difference would almost be measurable experimentally.

On the other hand, there have been many studies on the influence of the earth's motion and the results have always been negative. But these experiments were carried out because we were not certain of this result beforehand and in fact, according to the prevailing theories, the compensation would be only approximate and we should expect to see precise methods give positive results. I believe that such a hope is only an illusion. It was nonetheless interesting to show that a success of this kind would in some way open a new world to us.

Allow me a digression, for I must now explain why I do not believe that more precise observations will ever reveal anything other than the relative displacements of physical bodies, Lorentz notwithstanding. Experiments have been done that should have detected the first-order terms, but the results were negative. Could this be only a coincidence? No one has claimed such. A general explanation was sought and Lorentz found it. He showed that the first-order terms had to cancel out, but that it was not so for the second-order ones. More precise experiments were then performed. They were also negative, which could also not be a coincidence. An explanation was needed and it was found, as one always is. Hypotheses are the assets we lack the least.

But this is not enough. Who does not feel that this again leaves chance too large a role? Would it not be a strange combination of events if a specific circumstance happened just in time to cancel the first-order terms, while a second, but equally timely one, took care of cancelling the second-order terms? No, a single explanation must be found for both cases and so everything leads us to believe that this explanation will also hold for the higher-order terms and that the mutual cancelling of these terms will be rigorous and absolute.

The current state of physics

We see two opposing trends in the history of the development of physics. On the one hand, we continually discover new connections between objects that seemed destined to remain apart forever. Disconnected facts cease to be unrelated to one another. They tend to arrange themselves into an impressive synthesis. Science is marching toward unity and simplicity. On the other hand, observation reveals new phenomena every day that must wait a long time for their place and sometimes, to make room for them, a bit of the edifice must be torn down. Even within the known phenomena where our rudimentary senses showed us uniformity, we perceive details that become day by day more varied. What we believed simple becomes complex again and science seems to be on the road to diversity and complexity. Which of these two opposite trends, each of which alternatively appears to be triumphing, will win? If it is the first, science is possible, but nothing proves this *a priori* and we may fear that after futile efforts to bend nature to our ideal of unity despite itself, overwhelmed by the ever-rising flow of new riches, we will have to forego their classification, abandon our ideal, and reduce science to the recording of innumerable recipes.

We have no solution to this problem. All that we can do is observe today's science and compare it to that of yesterday. From this examination, we will undoubtedly be able to draw out a few assumptions. Expectations were running high half a century ago. The discovery of the conservation of energy and of its transformations had just revealed the unity of force and showed how molecular motions could explain molecular phenomena. The nature of these motions was not exactly known, but there was no doubt that it would soon be. As for light, the task seemed to have been fully accomplished. We were not as far along with respect to electricity. It had just annexed magnetism, an important step towards unity and a definitive one. However, how would electricity in turn fit into the general unity? How would it lead back to the universal mechanism? No one had the slightest idea. However, no one doubted the possibility of this reduction. We had faith. Finally, with respect to the molecular properties of physical bodies, the reduction seemed even easier, yet all the details remained in a fog. In a word, hopes ran high and they were bright, but they were vague.

What do we see today? For one thing, we see an immense progress. The relations between electricity and light are now known. The three previously separate fields of light, electricity, and magnetism are now one and this unification seems definitive. This victory, however, has required some sacrifices. Optical phenomena have been integrated as particular cases of electrical phenomena. As long as they were isolated in their own field, it was easy to explain them in terms of motions believed to be known in full detail. It was simple, but now for us to accept an explanation it must be applicable to the entire field of electricity, which presents some difficulties.

As we will see in Chapter 13,[2] the most satisfying theory, due to Lorentz, explains electric currents in terms of tiny electrified particles. It is unquestionably

2 The French edition reads "au dernier chapitre" ("in the last chapter"), that is, in the case of the first edition, Chapter 13. This reference, however, was not changed when a fourteenth chapter was added to the book.

the theory that best accounts for the known facts, the one that brings to light the greatest number of true relations, and the one in which we will find the most traces in the definitive construction. Nevertheless, his theory still has a serious flaw, as noted above. It conflicts with Newton's principle of the equality of action and reaction, or rather, in Lorentz's view, this principle would not be applicable to matter alone. For the principle to be true, we would have to take into account the actions the ether exerts on matter and the reaction of matter to ether. Until we have a completely new way of looking at the situation, it is likely that things do not work that way.

Be that as it may, thanks to Lorentz, Fizeau's results on the optics of moving bodies, the laws of normal and anomalous dispersion, and the laws of absorption are connected to one another as well as to ether's other properties by connections that undoubtedly will never come undone. Look at the ease with which the new Zeeman effect found a made-to-order place for itself and even helped to classify Faraday's magnetic rotation, which had kept defying Maxwell's efforts. Such ease really proves that Lorentz's theory is not an artificial concoction destined to fall apart. We will probably have to modify the theory, but not destroy it.

Lorentz had no other ambition than to bring all of optics and the electrodynamics of moving bodies together in a single field. He never claimed to give a mechanical explanation of them. Larmor goes further; preserving Lorentz's theory in its main lines, he grafts MacCullagh's ideas on the direction of the ether's motions onto it. According to him, the ether's speed would have the same direction and magnitude as the magnetic force. This speed is therefore known to us since the magnetic force is experimentally measurable. Ingenious though his attempt may be, the shortcoming of Lorentz's theory persists and becomes even more severe. Action is not equal to reaction. With Lorentz, we did not know what the motions of the ether were. Our not knowing allowed us to suppose that they re-established the equality of the action and reaction, by compensating for the motions of matter. With Larmor, we know the motions of the ether and we can observe that compensation does not take place.

Although Larmor has failed, in my opinion, does this mean that a mechanical explanation is impossible? Far from it. I said above that as soon as a phenomenon obeys the two principles of energy and of least action, it has an infinite number of mechanical explanations, and so it is for optical and electrical phenomena.

However, this will not do, since a good mechanical explanation must be simple. To choose one explanation out of all the possible ones, we must have reasons beyond the need to choose. We do not yet have a theory that satisfies this condition and that would therefore be useful. Should we regret this? That would be to forget that the desired goal is not the mechanism. The single true goal is unity.

We must therefore limit our ambition. Rather than trying to formulate a mechanical explanation, let us be satisfied with showing that we could always find one if we wished. In this, we have succeeded; the principle of the conservation of energy has received only confirmations. A second principle has joined it, that of least action stated in a form useful for physics. The least action principle has also always been verified, at least with respect to reversible phenomena that obey Lagrange's equations, that is, the most general laws of mechanics.

Irreversible phenomena are much more unruly. However, they also have an order and tend toward unity. Carnot's principle shed light on them. For a long time thermodynamics limited itself to the study of the expansion of bodies and their changes of state. It has recently grown bolder, considerably broadening its field of study. We owe the theories of both the battery and of thermoelectric phenomena to thermodynamics, which has explored all aspects of physics, and even tackled chemistry. The same laws prevail everywhere. Carnot's principle is found everywhere, under various guises. Likewise the so very abstract concept of entropy, as universal as that of energy and, like it, seeming to hide a reality, is found everywhere. We have recently seen radiant heat yield to the same laws, although it used to appear to escape them.

We find there new analogies, which often continue along the same lines: Electrical resistance is similar to the viscosity of liquids. Hysteresis is rather similar to the friction of solids. In all cases, friction seems to be that on which the most diverse irreversible phenomena model themselves, and this kinship is real and profound.

We also sought an actual mechanical explanation for these phenomena, but they did not lend themselves to one. To find such an explanation, it was necessary to suppose that irreversibility is only apparent, and that elementary phenomena are reversible and obey the known laws of dynamics. The elements are, however, extremely numerous and blend increasingly, so that, from our limited perspective, everything seems to tend towards uniformity; that is, everything seems to march in the same direction without any possibility of returning. The apparent irreversibility is then merely an effect of the law of large numbers. Only a being with infinitely acute senses, like Maxwell's imaginary demon, could disentangle this inextricably tangled coil and reverse the course of the world.

This conception, connected to the kinetic theory of gases, has required tremendous effort and has not been very fruitful, everything considered. It may become so, but this is not the place to examine whether or not it leads to contradictions or whether it is really in conformity with the true nature of things.

Let us nevertheless call attention to Gouy's original ideas on Brownian motion. According to this scientist, this unusual motion flies in the face of Carnot's principle. The particles it sets in motion would be smaller than the spaces between the strands of this very tight coil and would therefore be able to untangle them, thus making the world go backwards. It is as if Maxwell's demon were at work.

In conclusion, previously known phenomena are increasingly better classified, but new phenomena come to claim their own place in our theories. Most of them, like the Zeeman effect, found it right away. We still have cathode rays, x-rays, and the radiation from uranium and radium. There is a whole world no one suspected. So many unexpected guests must be housed! No one can yet predict the place they will occupy; however, I do not believe that they will destroy the general unity, but rather that they will complete it. On the one hand, the new forms of radiation seem related to phenomena of light. Not only do they excite fluorescence, but they sometimes arise under the same conditions. They are also related to the causes that create a spark by means of ultraviolet light. Finally, and most importantly, it is thought that in all these phenomena there are true ions animated, it is true, by speeds incomparably higher than the ones found in electrolytes. All of this is quite vague, but it will all become clearer. Phosphorescence, the action of light on the spark, were rather isolated areas of research and had been somewhat ignored by researchers. We can now hope to build a new line to facilitate their communications with universal science.

Not only do we discover new phenomena, but we discover unforeseen aspects of phenomena we thought we already understood. In free ether, the laws preserve their majestic simplicity, but matter itself seems increasingly complex. Everything we say about matter is always only approximate and our formulas constantly require new terms.

Nevertheless, the frameworks are not broken; the relations recognized between objects we believed to be simple still hold between these same objects once we know their complexity, and that is all that matters. True, our equations become increasingly more complicated to parallel nature's complexity ever more closely, but nothing changed in the relations that allowed the derivation of these equations from one another. Simply put, the form of these equations has held fast.

Let us consider, for example, the laws of reflection established by Fresnel, thanks to a simple and attractive theory apparently confirmed by experiment.

Since then, more precise research has proved that this confirmation was only approximate. They showed traces of elliptical polarization everywhere. However, it was readily found, with the support of a first-order approximation, that the cause of these anomalies was the presence of a transition layer, and Fresnel's theory has remained essentially correct.

Only, we cannot help but think that all these relations would have gone unnoticed had the complexity of the objects they connect been suspected in the first place. It was said long ago that had Tycho had ten times more precise instruments, neither Kepler, nor Newton, nor astronomy would have existed. It is unfortunate for a science to be born too late, when observational techniques have become too refined, as is happening now in physical chemistry. Its founders are hindered in their initial findings by the third and fourth decimal places. Fortunately, they have a firm conviction.

As we become more familiar with the properties of matter, we see that continuity dominates. Since the work of Andrews and van der Waals, we have come to realize how the transition between the liquid and gaseous states happens and that this transition is not sudden. Similarly, there is no sharp division between liquid and solid states. In the proceedings of a recent conference, a paper on the flow of solids appears next to work on the rigidity of liquids.

Simplicity undoubtedly loses to this tendency. A phenomenon used to be represented by many straight lines, while now it is necessary to connect these straight lines by more or less complicated curves —this is to unity's advantage. These sharply drawn categories soothed the mind but failed to satisfy it.

At last, the methods of physics have spread to a new field, that of chemistry. Physical chemistry was born. It is still quite young, but we can already see that it will allow us to connect phenomena such as electrolysis, osmosis, and the motions of ions to one another.

What should we conclude from this rapid survey? All things considered, we are now closer to unity. Not having always taken the expected path, we have not been as rapid as had been hoped fifty years ago, but when all is said and done, we have gained a lot of ground.

11

Probability Calculus

It will certainly come as a surprise to find a discussion of probability calculus at this point. What does it have to do with the method of physical sciences? Still, the questions I will raise without solving naturally present themselves to the philosopher who wants to think about physics, and so much so that in the two preceding chapters I have often been led to utter the words "probability" and "chance." Predictions are merely probable, I said above, "however firmly anchored a prediction may seem to us, we are never absolutely certain that experiment will not disprove it [. . .]. However, probability is often high enough to be sufficient for all practical purposes." And a little later, I added: "Let us see what role belief in simplicity plays in our generalizations. We have verified a simple law in a [. . .] large number of particular cases. We do not accept that this often repeated conjunction is merely due to chance [. . .]."[1]

Thus, in various circumstances, physicists find themselves in the same position as the gambler who calculates the odds. Every time they use induction, they more or less consciously make use of probability calculus,[2] which is why I am obliged to pause and interrupt our study of the method of physical sciences in order to examine a little more closely what this calculus is worth and how much trust it deserves.

The very name of probability calculus is a paradox: probability, in contrast to certainty, is what we do not know. How is it possible to calculate what we do not know? Yet, many distinguished thinkers preoccupied themselves with this calculus, which has certainly benefited science. How should this apparent contradiction be explained? Has probability been defined? Can it even be defined? And if not, how can we reason about it? One will say that the definition is quite simple: the probability of an event is the ratio of the number of positive

1 These quotations are from Chapter 9, pp. 105 and 107–8 respectively. Poincaré includes the first part of the first sentence in the quotation, but it does not appear above.

2 Because of a typo, the first French edition read: "probability calculus is more or less consciously needed" (1902: 214).

outcomes for this event to the total number of possible outcomes. A simple example will make clear how incomplete this definition is. I roll two dice. What is the probability that at least one of these two dice will come up a six? Each die can fall on one of six different faces. The number of possible outcomes is $6 \times 6 = 36$, while the number of positive outcomes is 11, so the probability is 11/36. This is the correct solution, but could I not just as well say that the numbers on the faces shown by the two dice form $\frac{6 \times 7}{2} = 21$ different combinations, among these combinations, 6 are positive, so the probability is 6/21? Why is the first way of enumerating the possible outcomes more legitimate than the second? In any case, our definition does not give us the answer.

We are therefore compelled to completing this definition by saying: ". . . to the total number of possible outcomes, provided that all these outcomes are equality probable." So here we are compelled to define the probable by the probable. How will we come to know that two possible outcomes are equally probable? Will it be by convention? If we place an explicit convention at the beginning of each problem, everything will work out. We will only have to apply the rules of arithmetic and algebra to complete our calculation without our result giving way to doubt. But as soon as we want to make any use of it,[3] we will have to prove that our convention was legitimate and we will again be confronted with the difficulty we believed to have eluded.

Can we say that common sense by itself can tell us what convention to adopt? Unfortunately not! Bertrand undertook solving a simple problem: "What is the probability that, within a circle, a chord will be longer than the side of the inscribed equilateral triangle?"[4] The renowned geometer tried two conventions, each of which seemed equally dictated by common sense, and found 1/2 with the one and 1/3 with the other.

The conclusion that seems to follow from all this is that probability calculus is a futile science, that we should not trust this vague instinct that we called common sense and to which we appealed to legitimize our conventions. Nonetheless, we cannot subscribe to this conclusion. We cannot do without this vague instinct without which science would be impossible, and we could neither discover a law, nor apply it. Do we have the right, for example, to proclaim Newton's law? Many observations certainly are in agreement with it, but is this not simply coincidental? Besides, how do we know if this law, which has remained

[3] Early editions read: "But if we want to make even the least use of it" (1902: 215–16).

[4] Poincaré is here referring to the "Bertrand paradox," introduced by Joseph Bertrand in his 1888 *Calcul des probabilités*. Paris: Gauthier-Villars, 4 (see http://gallica.bnf.fr/ark:/12148/bpt6k99602b.r=.langEN [accessed May 2, 2017].

true for so many centuries, will still be true next year? You will find no response to this objection except: "That is not very probable." However let us accept the law, thanks to which I believe that I can calculate Jupiter's position one year from now. May I do so? Who can say whether a gigantic mass with an enormous velocity will not pass near the solar system before then and produce unforeseen perturbations? Here again, there is no answer, except: "That is not very probable." Following this line of reasoning, all the sciences would be nothing but unconscious applications of probability calculus. To rule out this calculus would be to block science in its entirety.

I will place less stress on scientific problems in which the role of probability calculus is more evident. At the forefront of these is the problem of interpolation in which, knowing a certain number of a function's values, we attempt to guess the intermediate values. I shall also mention the famous theory of observational error, to which I will come back later, as well as the kinetic theory of gases, a well-known hypothesis in which each gas molecule is thought to describe a very complicated trajectory, but in which, through the effect of large numbers, the average phenomena—the only ones that are observable—obey simple laws, those of Mariotte and of Gay-Lussac. All these theories rest on the law of large numbers, and probability calculus would obviously bring them down with it if it were to fail. It is true that they are only of limited interest and that, except as far as interpolation is concerned, we could accept these sacrifices. Although, as I mentioned above, it would be all of science whose legitimacy would be called into question, it would not only be these partial sacrifices that would be at play here.

I see clearly that one could say: "We do not know and yet we must act. In order to act, we do not have time to carry out an investigation capable of dispelling our ignorance. Besides, such an investigation would require an infinite amount of time. We must therefore make up our minds without knowing, acting at random, following rules without quite believing in them. What I know is not that something is true, but that the best thing for me is to act as if it were true. Probability calculus, and thus science itself, would only have a pragmatic value.

Unfortunately, the difficulty does not thereby disappear. A gambler wants to try his luck, asking me for advice. If I give it to him, I will draw on probability calculus, but I will not guarantee him success. I will call this *subjective probability*. In this case, we could be satisfied with the explanation just outlined. But suppose that an observer witnesses the game, that he writes down all of the rounds, and that the game goes on for a long time. When he reviews his notes, he will find that the events are distributed according to the laws of probability calculus.

This is what I will call *objective probability* and it is this phenomenon that must be explained. There are many insurance companies who apply the rules of probability calculus and they distribute to their shareholders dividends whose objective reality cannot be questioned. To justify them, it is not sufficient to invoke our ignorance and the need to act. Absolute skepticism is therefore unacceptable. We must be on guard, but we cannot condemn everything outright. Discussion is needed.

I: Classification of problems of probability

To classify the problems arising in connection with probabilities, we may look at things from many different perspectives, and first of all from the *perspective of generality.* I said above that the probability of an event is the ratio of the number of positive outcomes to the number of possible outcomes. What, for lack of a better term, I call generality will increase with the number of possible outcomes. The number may be finite as, for instance, when we contemplate a dice roll where the number of possible outcomes is 36. This is the first degree of generality.

However, if we ask, for example, what is the probability that a point within a circle is also within the inscribed square, there are as many possible outcomes as there are points in the circle, that is to say, an infinity. This is the second degree of generality. Generality can be pushed even further: We can enquire about the probability that a function will satisfy a given condition. There are, then, as many possible outcomes as we can imagine different functions. It is the third degree of generality, which we reach, for instance, when we seek to guess the most probable law given a finite number of observations.

We can take a quite different perspective. If we were not ignorant, there would be no probability, there would only be room for certainty. But our ignorance cannot be absolute, otherwise there would also be no probability, since a little light is still needed to attain even this degree of knowledge. The problems of probability may thus be classified according to the greater or lesser depth of this ignorance.

We can already set out probability problems in mathematics. What is the probability that the fifth decimal of a logarithm taken at random from a table will be a 9? We will not hesitate to answer that this probability is 1/10. Here, we have access to all the data for the problem. We know how to calculate our logarithm without needing the table, but we do not want to go to the trouble. This is the first degree of ignorance.

In physical sciences, our ignorance is already greater. The state of a system at a given time depends on two things: its initial state and the law according to which this state varies. If we knew both this law and this initial state, we would have only a mathematical problem to solve and we would be back to the first degree of ignorance. Often, however, the law is known, but not the initial state. One might ask, for instance, what the present distribution of the asteroids is. We know that they always obeyed Kepler's laws, but we do not know what their initial distribution was. In the kinetic theory of gases, we assume that gas molecules follow rectilinear trajectories and obey the laws of elastic collisions. However, as we know nothing of their initial speeds, we know nothing of their present speeds. On its own, probability calculus allows us to predict the average phenomena resulting from the combination of these speeds. That is the second degree of ignorance.

Finally, it is possible that not only the initial condition but the laws themselves are unknown. At that point, we reach the third degree of ignorance and, in general, we can no longer assert anything at all as to the probability of a phenomenon.

It often happens that, instead of trying to predict an event by using our more or less imperfect knowledge of the law, we know the events and are trying to discover the law; instead of deducing the effects from the causes, we wish to deduce the causes from the effects. These are the so-called problems of *probabilistic causation*, the most interesting ones from the perspective of their scientific applications. I am playing *écarté*[5] with a gentleman I know to be perfectly honest. He is about to deal. What is the probability that he will turn up a king? It is 1/8. This is a problem in probability of effects. I play with a gentleman I do not know. He has dealt ten times and has turned up a king six times. What is the probability that he is a cheater? This is a problem in probabilistic causation. It may be said that this is the main problem of the experimental method. I observed n values of x and the corresponding values of y. I observed that the ratio of the latter to the former is more or less constant. If the event is defined this way, what is its cause? Is it probable that a general law exists according to which y is proportional to x, and that the small divergences are due to observational errors? This is a kind of question we are constantly led to ask and are unconsciously solving whenever we do scientific research.

[5] *Écarté* was a popular two-person card game of nineteenth-century French salons, played with a 32-card deck obtained by removing cards 2 to 6. After passing five cards to both players, the dealer would turn the top card for trumps, receiving a point if it was a king (*Encyclopaedia Britannica* (1911)).

I will now review these different categories of problems by considering in turn what I have called above subjective and objective probability.

II: Probability in mathematics

The impossibility of squaring the circle was proven in 1883.[6] However, well before this recent date, all geometers considered this impossibility so "probable" that the Academy of Sciences was rejecting out of hand the all too numerous papers on the topic sent in every year by a few wretched souls. Was the Academy in the wrong? Of course not, and it knew full well that by responding in this way it did not run the risk of stifling any important discovery. They could not have proved that they were right, but they knew quite well that their instinct was not deceiving them. Had you asked the Academicians, they would have answered: "We compared the probability of an unknown scholar finding what has been sought in vain for so long with the probability of having one more madman on earth. The latter seemed greater to us." These are very good reasons, but there is nothing mathematical about them, they are purely psychological.

Had you pressed them further, they would have added: "Why do you expect a particular value of a transcendental function to be an algebraic number? Were π the root of an algebraic equation, why would you expect this root, but not the other roots of the same equation, to be a period of the function sin $2x$?" All considered, they would have invoked the principle of sufficient reason in its vaguest form. What could they take from it anyway? At most, a rule of conduct for how to use their time, more usefully spent on their usual tasks than on the reading of some wild imaginings that inspired in them a justified mistrust. However, what I called above objective probability has nothing to do with this first problem.

It is not the same for the second problem. Consider the first 10,000 logarithms found in a table. Among these 10,000 logarithms, I take one at random. What is the probability that its third decimal is an even number? You will not hesitate to answer ½, and in fact if you copy the third decimals of these 10,000 numbers

[6] In the 1902 edition, Poincaré dates the proof to 1885. In the 1906 and 1917 editions, he gives 1883. Carl Louis Ferdinand Lindemann published the proof in June 1882, the date given in Halsted's English translation and Lindemann's German translation of *Science and Hypothesis* (see H. Schubert. 1891. "The Squaring of the Circle. An Historical Sketch of the Problem from the Earliest Times to the Present Day," *The Monist* 1 (2): 197–228).

in a table, you will find almost as many even digits as odd ones. Or, if you prefer, let us write down 10,000 numbers corresponding to our 10,000 logarithms, each of these numbers being equal to +1 if the third decimal of the corresponding logarithm is even, and −1 otherwise. Then take the mean of these 10,000 numbers. I will not hesitate to say that the mean of these 10,000 numbers is probably zero and, were I actually to calculate it, I would confirm that it is extremely small.

However, this confirmation is useless. I could have rigorously proved that this mean is smaller than 0.003. To establish this result would require a rather long calculation that would be out of place here and instead I simply refer the reader to an article I published in the *Revue générale des sciences*, on April 15, 1899.[7] The only point to which I must draw attention is the following: In this calculation, I would have needed to rely on only two facts, namely that the first and second derivatives of the logarithm remain, in the interval under consideration, within certain limits, hence this first result that the property is true not only of the logarithm but of any continuous function, since the derivatives of any continuous functions are limited.

If I was certain beforehand of the result, it is first, because I had often observed analogous facts for other continuous functions, and next, because, I was more or less unconsciously and imperfectly going through the reasoning in my mind that led me to the preceding inequalities, like a skilled calculator realizing, before completing a multiplication, that "the answer will be approximately so much." Anyway, as what I called my intuition was only a partial glimpse of a true reasoning, we understand why observation confirmed my predictions, why objective probability agreed with subjective probability.

As a third example, I will choose the following problem. A number u is taken at random and n is a given very large whole number. What is the expected value of $\sin nu$? On its own, this problem makes no sense. To give it one, a convention is needed. We *will agree* that the probability of a number u to lie between a and $a + da$ is equal to $\varphi(a)da$ and that it is consequently proportional to the length of the infinitely small interval da and equal to this length multiplied by a function $\varphi(a)$ depending only on a. As for this function, I will choose it arbitrarily, but I must suppose it to be continuous, with the value of $\sin nu$ remaining the same when u increases by 2π, I may, without loss of generality, suppose that u lies between 0 and 2π, and I will thus be led to suppose that $\varphi(a)$ is a periodic

7 Poincaré is referring the reader to the original version of this chapter (see sources of the chapters, p. xxvi).

function whose period is 2π. The expected value sought is readily expressed by a simple integral, and it is easy to show that this integral is less than:

$$2\pi M_k / n^k,$$

M_k being the maximum value of the k^{th} derivative of $\varphi(u)$.[8] We see then that if the k^{th} derivative is finite, our expected value will tend towards zero as n goes to infinity, and will do so more rapidly than $1/n^{k-1}$.

The expected value of sin *nu* for a very large *n* is therefore zero. To determine this value, I required a convention, but the result remains the same *whatever this convention may be*. By supposing that the function $\phi(a)$ is continuous and periodical, I imposed upon myself only weak restrictions, and these hypotheses are so natural that one wonders how they could be avoided.

Examination of the three preceding examples, so different in all respects, has already given us a glimpse on the one hand, of the role of what philosophers call the principle of sufficient reason and, on the other, of the importance of the fact that certain properties are common to all continuous functions. The study of probability in physical sciences will lead us to the same result.

III: Probability in the physical sciences

Let us now come to the problems related to what I have called above the second degree of ignorance, those in which the law is known, but the initial state of the system is not. I could multiply examples, but I will pick only one. What is the probable present spatial distribution of the asteroids in the zodiac? We know that they obey Kepler's laws. Without changing the nature of the problem, we can suppose that their orbits are all circular and located in the same plane and that we would know this. On the other hand, we have no idea what their initial distribution was, and yet, we do not hesitate to assert that today this distribution is nearly uniform. Why?

Let b be the longitude of an asteroid at time zero, and let a be its mean displacement. Its longitude at the present time, that is at time t, will be $at + b$. To say that the present distribution is uniform is to say that the average value of the sines and cosines of the multiples of $at + b$ is zero. Why do we make this claim?

8 There is a mistake in some French editions. We read M_k rather than M^k in this sentence in order to match what is in the equation.

Let us represent each asteroid by a point on a plane, namely the point whose coordinates are precisely *a* and *b*. All these representative points will be contained in a certain region on the plane, but since there are quite a lot of them this region will appear riddled with points. We know nothing about the distribution of these points. What do we do when we wish to apply probability calculus to such a question? What is the probability that one or many of the representative points are in a given region of the plane? In our ignorance, we are driven to make an arbitrary hypothesis. To explain the nature of this hypothesis, allow me to use a rough but concrete image instead of a mathematical formula. Imagine that we spread an imaginary substance whose density varies in a continuous manner over the surface of our plane. We will then agree to say that the probable number of representative points found in a region of the plane is proportional to the quantity of imaginary substance found there. If we then have two regions of the plane with the same area, the probabilities that a point representing one of our asteroids will be found in one or the other of these regions will be the same as the mean densities of the imaginary substance in one or the other region. Here then are two distributions, one real, where the representative points are very numerous, very close together, but discrete like the molecules of matter on the atomic hypothesis—the other far from reality, where our representative points are replaced by a continuous imaginary substance. We know that the latter cannot be real, but our lack of knowledge forces us to adopt it.

If we had at least some idea of the real distribution of the representative points, we could arrange things so that in a region of a certain area, the density of this continuous imaginary substance would be nearly proportional to the number of representative points or, if you prefer, of atoms contained in this region. This in itself is impossible and our lack of knowledge is so complete that we are forced to choose arbitrarily the function defining the density of our fictitious matter. We will be driven to adopt only one hypothesis, one which we can hardly avoid—the supposition that this function is continuous. As we will see, this is enough to enable us to reach a conclusion.

What is the probable distribution of asteroids at time *t*, or rather, what is the probable value of the sine of the longitude at time *t*, that is to say of sin $(at + b)$? At the outset, we proposed an arbitrary convention; however, if we adopt it, this expected value is entirely defined. Let us decompose the plane into surface elements. Consider the value of sin $(at + b)$ at the center of each of these elements. Multiply this value by the element's area and the corresponding density of the imaginary substance. Let us then take the sum of all the elements of the plane. This sum will be by definition the probable mean value sought, which will thus be expressed by a double integral. We might think at first that this mean value will depend on the choice of the function ϕ that defines the density of the imaginary substance and as this function ϕ is arbitrary, depending on the arbitrary choice we would make, we might obtain any given mean value. Nothing of the sort is true.

A simple calculation shows that our double integral decreases very rapidly as t increases. So, I was not quite sure what hypothesis to make as to the probability of such or such initial distribution. However, whatever hypothesis is made, the result will be the same, and this is what gets me out of trouble. Whatever the function φ may be, the mean value tends towards zero as t increases, and since the asteroids have certainly completed a very high number of revolutions, I may assert that this mean value is very small. I may choose φ as I wish, except for one restriction: this function must be continuous. In fact, from the perspective of subjective probability, the choice of a discontinuous function would have been absurd. What reason could I have for supposing, for example, that the initial longitude might be exactly equal to 0°, but could not lie between 0° and 1°?

The difficulty reappears if we look at it from the perspective of objective probability, that is, if we change from our imaginary distribution, where the imaginary substance was supposed to be continuous, to the real distribution where our representative points are similar to discrete atoms. The mean value of $\sin(at + b)$ will be simply represented by:

$$\frac{1}{n}\Sigma \sin(at+b),$$

n being the number of asteroids. Instead of a double integral of a continuous function, we have a sum of discrete terms. And yet, no one will seriously doubt that this mean value is actually very small. Given that our representative points are very close together, our discrete sum will generally differ very little from an integral. An integral is the limit towards which a sum tends when the number of its terms increases indefinitely. If the terms are very numerous, the sum will differ very little from its limit; that is to say from the integral, and what I said about the latter will still be true of the sum itself.

There are nevertheless exceptions. If, for instance, for all asteroids, we had:

$$b = \pi/2 - at,$$

at time t, the longitude of all the planets would be $\pi/2$ and the mean value would obviously be equal to 1. For this to happen, it would be necessary for all the asteroids to be, at time 0, lying on a kind of spiral of a particular form, made of arms extremely close together. Everyone will conclude that such an initial distribution is extremely improbable (and, even if the distribution was such initially and still not uniform at the present time—on January 1, 1900, for instance—it would become so again within a few years).

All considered, why do we believe this initial distribution to be improbable? An explanation is necessary for, if we had no reason to reject as unbelievable this ridiculous hypothesis, everything would collapse and we would no longer be able to make any affirmation as to the probability of this or that present distribution. We are appealing to the principle of sufficient reason to which we must always return. We could suppose that initially the planets were placed almost in a straight line or we could admit that they were irregularly distributed. Still there seems to be no sufficient reason for the unknown cause that produced them to have acted along such a regular, and yet so complicated a curve, which would seem moreover to have been expressly chosen so that the present distribution would not be uniform.

IV: Red and black

The questions raised by games of chance like roulette are at heart completely analogous to those we just discussed.[9] For instance, a dial is divided into a great number of equal sections, alternatively red and black. A pointer is spun vigorously and, after spinning around a large number of times, it stops on one of these sections. The probability that this section is red is obviously ½. The pointer will turn by an angle θ, including many full rotations. I do not know what the probability is that the pointer will be spun with a force such that this angle will lie between θ and $\theta + d\theta$. However, I can adopt a convention. I can suppose that this probability is $\varphi(\theta)d\theta$. As for the function $\varphi(\theta)$, I may select it in an entirely arbitrary manner, since there is nothing that can guide me in my choice. However, I am naturally led to suppose that this function is continuous.

Let ε be the length (measured on the circumference of unit radius) of each red or black section. The integral of $\varphi(\theta)d\theta$ must be calculated by extending it, on the one hand, to all the red sections, and on the other hand, to all the black sections, and the results must be compared. Consider an interval 2ε, comprising of both a red section and the black section following it. Let M and m be the maximum and minimum values of the function $\varphi(\theta)$ in this interval. Extended

[9] What Poincaré describes next is not roulette, but rather an equivalent game with a spinner.

to the red sections, the integral will be less than $\Sigma M\varepsilon$. Extended to the black sections, the integral will be more than $\Sigma m\varepsilon$. The difference will therefore be less than $\Sigma(M - m)\varepsilon$. However, if the function φ is considered to be continuous and if, on the other hand, the interval ε is very small compared to the total angle described by the pointer, the difference $M - m$ will be very small. The difference between these two integrals will therefore be very small, and the probability will be very nearly ½.

As I have no knowledge of the function φ, it is understood that the probability is ½. From an objective perspective, it explains as well why observation will give me more or less the same number of blacks as reds when I observe a certain number of trials. All gamblers know this objective law, but it leads them to make a remarkable error, which has often been noted, but into which they always fall. When red comes up six times in a row, for example, they bet everything on black, believing they will certainly win since, they say, it is very infrequent that red comes up seven times in a row. In reality, their probability of winning remains ½. Observation shows, it is true, that series of seven consecutive reds are very rare; however, series of six reds followed by a black are just as rare. They noticed the rarity of the series of seven reds. If they have failed to notice the rarity of the series of six reds and a black, it is only because such series attract less attention.

V: Probabilistic causation

I now come to the problems of probabilistic causation, the most important from the perspective of scientific applications. Two stars, for example, are very close together on the celestial sphere. Is this apparent proximity purely the result of chance?[10] Despite being approximately on the same line of sight, are these stars located at very different distances from the earth and consequently are they very distant from one another? Or does this apparent proximity correspond to a real one? That is a problem of probabilistic causation.

Let me first remind you that we always had to put in place a more or less justified convention at the beginning of each of the problems of the probability of events with which we have so far been concerned. If the result was often to a

[10] Poincaré is here referring to Michell's problem. John Michell argued that the numbers of "double stars", that is, stars appearing very close to one another in the night sky, was much larger than expected if stars had simply been uniformly distributed through space. (see J. Michell. 1767. "An inquiry into the probable parallax and magnitude of the fixed stars from the quantity of light which they afford us, and the particular circumstances of their situation," *Philosophical Transactions of the Royal Society of London*, 57: 234–64).

certain extent independent of this convention, it was so only on the condition of certain hypotheses which enabled us to reject *a priori* discontinuous functions, for instance, or absurd conventions.

We find again something comparable when dealing with probabilistic causation. An effect may be produced by cause A or cause B. The effect has just been observed and we ask the probability that it was caused by A. This is the probability of the cause *a posteriori*; however, I would not be able to calculate it if a more or less justified convention did not tell me *in advance* what the *a priori* probability is that cause A will enter into play, by which I mean the probability of this event for someone who would not have yet observed the effect.

To make myself clearer, I will return to the example of the game of *écarté* mentioned above. My opponent deals for the first time and turns up a king. What is the probability that he is a cheater? The formulas usually taught give 8/9, an obviously rather surprising result. Upon closer examination, we see that the calculation is made as if I had considered, *before sitting down at the gaming table*, that there was one chance in two that my adversary was not honest. An absurd hypothesis since in that case I would certainly not have played with him, and this explains the absurdity of the conclusion. The convention about the *a priori* probability was unjustified, which is why the calculation of the *a posteriori* probability led me to an unacceptable result. The importance of this preliminary convention is obvious. I would even add that, if no convention were adopted, the question of *a posteriori* probability would make no sense. We must always follow a convention, either explicitly or tacitly.

Let us turn to an example of a more scientific nature. I wish to establish an experimental law. Once I know it, it will be possible to represent this law by a curve. I make a certain number of isolated observations, each one of which will be represented by a point. After determining these different points, I will draw a curve between them, doing my best to remain as close to them as possible while insuring that my curve retains a regular form, without angles, overly accentuated bends, or sudden variations of the radius of curvature. This curve represents the probable law to me and, admittedly, not only does it provide me with the values of the function that lie between the observed ones, but it also reveals the observed values themselves more exactly than direct observation (which is why I make it pass near my points rather than through these points themselves).

We have here a problem of probabilistic causation. The effects are the measurements I recorded that depend on the combination of two causes: the true law of the phenomenon and the observational errors. Knowing the effects, the aim is to find the probability that the phenomenon obeys such a law so that the observations have been affected by such an error. The most probable law corresponds, then, to the curve drawn and the most probable error affecting an observation is represented by the distance between the corresponding point and the curve.

The problem would be meaningless, however, if, before any observation, I had not formed an *a priori* idea of the probability of some law or other and of my chances of error. If my instruments are good (and I knew this before making observations), I would not let my curve pass far from the points representing the raw data. If they are faulty, I will be able to vary a little more from them so that I will get a less sinuous curve and I will sacrifice more to regularity.

Why then am I seeking to trace a non-sinuous curve? It is because, *a priori*, I consider a law represented by a continuous function (or by a function whose higher-order derivatives are small) as more likely than a law that does not satisfy those conditions. Without this belief, the problem we are discussing would have no meaning. No law could be deduced from a finite number of observations and science would not exist.

Fifty years ago, physicists believed that a simple law was more probable than a complicated one, other things being equal. They even invoked this principle in favor of Mariotte's law against Regnault's experiments. They have now abandoned this belief, yet, how many times are they compelled to act as if they still held it! However that may be, what remains of this inclination is the belief in continuity and, as we just saw, if this belief were in turn to disappear, experimental science would become impossible.

VI: Theory of errors

We are thus led to discuss the theory of errors, which is directly connected to the problem of probabilistic causation. Here again we notice *effects*, namely, a certain number of conflicting observations, and we seek to guess the *causes*, which are on the one hand, the true value of the quantity to be measured and, on the other hand, the errors made during each isolated observation. It would be necessary to calculate the *a posteriori* probable magnitude of each error and so the expected value of the quantity to be measured. As I have just explained, such a calculation could not be undertaken, however, without admitting *a priori*, that is to say before any observation, a probabilistic law of errors. Is there a law of errors?

The law of errors accepted by all statisticians is Gauss' law of errors, which is represented by a certain transcendental curve known as the "bell curve." It is appropriate, however, to recall the classical distinction between systematic and random errors.[11] When measuring a length with a meter stick that is too long, we will always find too small a number and it will be useless to repeat the measurement many times. This is a systematic error. When measuring the length with an accurate meter stick, we may however make mistakes, but we will be sometimes above the correct value, sometimes below and, when averaged over a large number of measurements, the error will tend to decrease. These are random errors.

It is clear first of all that systematic errors cannot comply with Gauss' law, but do random errors? Numerous proofs have been attempted, and almost all of them are crude paralogisms. Gauss' law can nevertheless be demonstrated starting from the following hypotheses: the random error made results from numerous partial and independent errors, each of the partial errors is very small and follows some probabilistic law where the probability of a positive error is the same as that of an equal, negative one. It is clear that these conditions will often, but not always, be fulfilled, and we may reserve the name "random" for errors satisfying them.

We see that the method of least squares is not legitimate in every case. Physicists usually question it more than astronomers, no doubt because, besides the systematic errors that they encounter along with physicists, astronomers must contend with an extremely important source of error which is wholly random—I am referring to atmospheric turbulence. It is very odd to listen to a physicist and an astronomer discussing observational method. Convinced that a good measurement is better than many bad ones, the physicist is first and foremost concerned with eliminating all systematic errors by taking every precaution, while the astronomer retorts: "But in this way you will only be able to observe a small number of stars and the random errors will not disappear."

What should we conclude? Should the method of least squares continue to be used? We must make some important distinctions. We have eliminated all suspected systematic errors. We are well aware that there are still more, but we cannot find them. However, we must make up our minds and choose a definitive value, which will be regarded as the probable one and, to do so, it is evident that the best thing to do is to apply Gauss' method. We have merely applied a practical rule referring to subjective probability. There is nothing more to be said.

Yet we want to go further and say that not only the probable value is some figure, but the probable error made on the result is another figure, *which is*

[11] Poincaré uses the term "accidental errors" ("erreurs accidentelles"), which is sometimes, although rarely, used today.

absolutely unjustified. It would be true only if we were certain that all systematic errors are eliminated, which we do not know at all. We have two series of observations. By applying the rule of least squares, we find that the probable error for the first series is half of what it is for the second. Yet the second series may be better than the first, as the first series may be distorted by a large systematic error. All we can say is that the first series is *probably* better than the second, because its random error is smaller and because we have no reason to claim that the systematic error is larger for one of the series than for the other, our lack of knowledge as to this point being absolute.

VII: Conclusions

In the preceding lines, I raised many problems without solving any. However, I do not regret having written them as they will perhaps encourage the reader to reflect upon these subtle questions.

Be that as it may, a number of points seem well established. To undertake any calculation of probability, and for this calculation even to have any meaning, it is necessary to take, as a starting point, a hypothesis or convention, which always entails a certain degree of arbitrariness. In the selection of this convention, we can be guided only by the principle of sufficient reason. Unfortunately, this principle is vague and quite elastic and, in the cursory examination we have just made, we have seen it assume many different forms. The form in which we have most often found it is the belief in continuity, a belief that would be difficult to justify by apodictic reasoning, but without which all of science would be impossible. Finally, the problems where probability calculus may be profitably applied are those where the result is independent of the hypothesis made at the outset, provided only that this hypothesis satisfies the continuity condition.

12

Optics and Electricity

Fresnel's theory

The best example* we could choose [to illustrate the role of hypotheses in science][1] is the theory of light and its relations to the theory of electricity. Thanks to Fresnel, optics is the most advanced branch of physics. While what is termed wave theory appeals to the intellect, we must not ask of it what it cannot give us. The purpose of mathematical theories is not to reveal the true nature of things. Such a claim would be unreasonable. Their only goal is to coordinate the physical laws that experiment reveals to us, but that we could not even state without the help of mathematics.

It matters little to us whether the ether really exists, a question we will leave to metaphysicians. Of primary importance to us is the fact that everything happens as if it existed and that this hypothesis is useful for explaining phenomena. After all, have we any other reason for believing in the existence of physical objects? This too is only a useful hypothesis, except that it will always be useful, while the day will undoubtedly come when the ether will be rejected as useless. Nevertheless, on that very day, the laws of optics and the equations which translate them analytically will remain true, at least to a first approximation. It will therefore always be useful to study any theoretical approach that connects all these equations.

Wave theory rests on a molecular hypothesis which is an advantage for those who believe that they are uncovering the cause behind the law. For the rest, it provokes mistrust, but their mistrust seems to me as unfounded as the belief of the first group. These hypotheses play only a secondary role and they could be sacrificed, which is not usually done because the account would lose in clarity,

* This chapter is a partial reproduction of the prefaces to two of my works: *Théorie mathématique de la lumière* (Paris: Naud, 1889) and *Électricité et optique* (Paris: Naud, 1901).

[1] Poincaré omits the material in brackets in the French original, leaving the point of the example unclear. In this amendment, we follow his comments at the end of the introduction.

but this is the only reason. In fact, were we to look at the situation more closely, we would see that only two things are borrowed from the molecular hypotheses: the principle of the conservation of energy and the linear form of the equations that is the general law of small motions, as it is of all small variations. This explains why most of Fresnel's conclusions remain unchanged when the electromagnetic theory of light is adopted.

Maxwell's theory

As we know, it was Maxwell who drew a close connection between optics and electricity, two branches of physics which until then had been completely separate from one another. By melding itself with a larger whole, with a greater unity, Fresnel's optics remained vital. Its various parts live on and their interactions are still the same. Only, the language we use to express them has changed. Furthermore, Maxwell has revealed to us other relationships between the different parts of optics and the domain of electricity that were unknown before him.

When a French reader first opens Maxwell's book, his admiration is initially mixed with a feeling of unease and often even of mistrust. It is only after prolonged acquaintance and at the cost of great effort that this impression disappears. Some eminent thinkers never shake the feeling. Why is so much effort required for the ideas of the English scientist to take root among us? It is undoubtedly because the instruction received by most educated French people predisposes them to appreciate precision and logic above any other quality. In this respect, earlier theories of mathematical physics were entirely satisfactory to us. All our great scientists, from Laplace to Cauchy, proceeded in the same manner. Starting from clearly stated hypotheses, they deduced all their results with mathematical rigor before comparing them to experiment. They seemed to want to give the same precision as that found in celestial mechanics to each branch of physics.

A mind accustomed to admiring such models is not easily satisfied by theory. Not only will it not tolerate the slightest appearance of contradiction, but it will demand that its various parts be logically connected to one another and that the number of distinct hypotheses be reduced to a minimum. Not only that, it will have still other demands, which seem to me less reasonable. The intellect will want to have access to the matter that lies beyond that to which our senses have access and that experiments reveal to us, that is the only real matter from its point of view, that which only has purely geometrical qualities and of which the atoms will be simply mathematical points subject only to the laws of dynamics.

And yet, by some unconscious contradiction, the mind will seek to picture these invisible and colorless atoms and, consequently, to assimilate them as much as possible to common matter. Only then will it be fully satisfied and will it seem to have disclosed the secret of the universe. This satisfaction is as erroneous as it is difficult to give up.

In approaching Maxwell, French readers expect to find a theoretical system as logical and precise as physical optics based on the ether hypothesis, thus setting themselves up for a disappointment that I would like to spare the readers by telling them right off what they ought to look for in Maxwell and what they will not be able to find there. Maxwell does not give a mechanical explanation of electricity and magnetism. He limits himself to demonstrating that this explanation is possible. He also shows that optical phenomena are only a special case of electromagnetic phenomena. It is possible then to derive a theory of light directly from any theory of electricity. Unfortunately, the converse is not true, since it is not always easy to draw a complete explanation of electrical phenomena from a complete explanation of light. It is especially difficult if we wish to start from Fresnel's theory. While it would not be impossible, we still have to ask if we will not be forced to give up some remarkable scientific findings that we thought were firmly established. It would seem to be a step backwards that many of the finest minds will not want to accept.

Once readers have agreed to lower their expectations, they will yet again encounter other difficulties. The English scientist does not seek to construct a single, definitive and well-ordered edifice; rather, he seems to be raising a large number of provisional and independent constructions, between which communications are difficult and sometimes impossible. Let us take as an example the chapter where electrostatic attractions are explained in terms of pressures and tensions at work in the dielectric medium. This chapter could be omitted without making the rest of the volume less clear or less complete. In addition, it contains a comprehensive theory and could be understood without reading even a single one of the lines preceding or following it. It is not only independent from the rest of the work, it is also at odds with the book's fundamental ideas. Maxwell does not even attempt this conciliation, simply saying: "I have not been able to make the next step, namely, to account by mechanical considerations for these stresses in the dielectric."[2] While this example will suffice to make my thoughts clear, I could mention many more. To cite one, who would suspect that there is identity between

2 In English in the original. See article 111 of James Clerk Maxwell, *A Treatise on Electricity and Magnetism* (Oxford: Clarendon, 1873), 132.

optical and magnetic phenomena while reading the pages devoted to magnetic rotary polarization?

One should therefore not hope to avoid all contradictions, but rather come to terms with them. Two contradictory theories may most certainly be both useful research instruments provided we do not combine them and do not look for the reality of things in them. Perhaps the reading of Maxwell would be less evocative had he not introduced so many new branching lines of inquiry to us. However, the fundamental idea still remains somewhat disguised, so much so that in most works intended for the general public, it is the only point that is completely neglected. In order to bring this neglected point to light, I need to explain the fundamental idea in a short but necessary digression.

The mechanical explanation of physical phenomena

In every physical phenomenon, there are a certain number of parameters to which experiment has direct access and that can be measured experimentally, which I will call *q* parameters. Observation then reveals the laws of the variations of these parameters, laws that can generally be put in the form of differential equations that relate the *q* parameters to time. What do we need to do to offer a mechanical interpretation of such a phenomenon? We will try to explain it either in terms of the motions of ordinary matter, or with respect to one or many hypothetical fluids. These fluids will be considered to be composed of a great number of isolated molecules *m*. When do we say then that we have a complete mechanical explanation of the phenomenon? On the one hand, we say that an explanation is complete when we know the differential equations satisfied by the coordinates of these hypothetical molecules *m*, equations which, furthermore, must conform to the principles of dynamics. On the other hand, it is complete when we know the relations defining the coordinates of the molecules *m* as functions of the *q* parameters that can be measured experimentally.

As I said, these equations must conform to the principles of dynamics and in particular to the principle of the conservation of energy and the principle of least action. The first of these two principles shows us that the total energy is constant and that this energy may be divided into two components:

1. Kinetic energy or *vis viva*, which depends on the masses of the hypothetical molecules *m* and their speeds that I call T.
2. Potential energy, which depends solely on the coordinates of these molecules, and that I call U. It is the sum of the two energies T and U that is constant.

What does the principle of least action show us now? It shows us that to go from the initial position it occupies at time $t_{0,}$ to the final position it occupies at time t_1, the system must follow a path such that during the time interval that elapses between the two instants t_0 and t_1, the mean value of the "action" (that is to say of the *difference* between the two energies T and U) will be as small as possible. The first of the two principles is, moreover, a consequence of the second. The latter is sufficient to determine the equations of motion if the two functions T and U are known. Among all the paths that can be followed to go from one position to another, there is obviously one for which the mean value of the action is smaller than for all of the others. There is, furthermore, only one, which means that the principle of least action is sufficient to determine the path followed and, consequently, the equations of motion.

We thus obtain what we call Lagrange's equations, in which the independent variables are the coordinates of the hypothetical *m* molecules, although I now suppose that the experimentally measurable *q* parameters are taken as variables. The two components of energy will then have to be expressed as functions of the *q* parameters and their derivatives. Obviously, they will appear in this form to the experimenter, who will naturally try to define potential and kinetic energy by means of the quantities directly observed.*

Under these conditions, the system will always move from one position to another along a path such that the mean action will be minimal. It does not really matter that T and U are now expressed with the help of the *q* parameters and their derivatives. It does not really matter that it is also by way of these parameters that we define the initial and final positions. The principle of least action always remains true. So here again, out of all the paths that lead from one location to another, there is one and only one for which the mean action is minimal. The principle of least action is therefore sufficient to determine the differential equations that define the variations of the *q* parameters.

The equations thus obtained are another form of Lagrange's equations. To form these equations, we do not need to know the relationships connecting the *q* parameters to the coordinates of the hypothetical molecules, nor the masses of these molecules, nor the expression of U as a function of the coordinates of these molecules. All we need to know is the expression of U as a function of the *q*s and of T as a function of the *q*s and their derivatives, that is to say, the expressions of the kinetic and potential energy as functions of the experimental data.

* Let us add that U will depend only on the *q* parameters, that T will depend on the *q* parameters and their time derivatives and will be a homogenous second-degree polynomial with respect to these derivatives.

We are faced with two possibilities: Either, given a suitable choice of the functions T and U, the Lagrangian equations constructed as we just said will be identical to the differential equations deduced from the experiments, or else there will be no T and U functions for which this agreement occurs. In the latter case, it is clear that no mechanical explanation is possible.

For a mechanical explanation to be possible, the *necessary* condition is therefore the ability to choose the functions T and U so as to satisfy the principle of least action, which entails the principle of the conservation of energy. This condition is moreover *sufficient*. Suppose that a function U—representing one of the components of energy—has been found in terms of the *q* parameters, that the other component of energy, represented by T, is a function of the q parameters and their derivatives as well as a homogenous second-degree polynomial with respect to these derivatives, and finally that the Lagrangian equations formed by means of these two functions T and U agree with the experimental data. What is required to deduce a mechanical explanation from this? It must be possible to view U as a system's potential energy and T as the *vis viva* of the same system. There is no difficulty as far as U is concerned, but will it be possible to view T as the *vis viva* of a material system? It is easy to show that this is always possible, and even in an infinite number of ways. I will just refer the reader to the preface of my work *Électricité et optique* for more details.

So if the principle of least action cannot be satisfied, no mechanical explanation is possible. If it can be satisfied, there is not only one, but an infinite number of explanations, which means that as soon as there is one explanation, there are infinitely many more.

One more remark: Among the quantities that experiments enable us to reach directly, we regard some of them as functions of the coordinates of our hypothetical molecules. These are our *q* parameters. We consider the others as depending not only on the coordinates, but also on the speeds or, which amounts to the same thing, on the derivatives of the *q* parameters, or as combinations of these parameters and their derivatives.

So the question arises: Among all these experimentally-measurable quantities, which should we choose to represent the *q* parameters? Which should we prefer to regard as these parameters' derivatives? This choice remains to a very large extent arbitrary, but for a mechanical explanation to be possible, it is enough that we can choose in such a way that it stays in compliance with the principle of least action.

At this point, Maxwell asked himself whether he could make both this choice and the one concerning the two energies T and U in such a way that electrical phenomena would comply with this principle. Experiment shows us that the energy of an electromagnetic field can be broken down into two parts: electrostatic energy and electrodynamic energy. Maxwell recognized that if we regard the first one as representing the potential energy U and the second as representing the kinetic energy T and if, in addition, the conductors' electrostatic charges are considered as q parameters and the intensities of the different currents as the derivatives of other q parameters, then under such conditions, electric phenomena do satisfy the least action principle. From that point on, he was certain that a mechanical explanation was possible. Had he stated this idea at the beginning of his book instead of relegating it to an obscure part of the second volume, it would not have escaped most readers.

So if a phenomenon has a complete mechanical explanation, it will have an infinite number of others that will each account equally well for all the specific details revealed by experiment. The history of every branch of physics confirms this. In optics, for example, Fresnel believes vibration to be perpendicular to the polarization plane. Neumann, however, regards it as parallel to this plane. A "crucial experiment" that would allow us to decide between these two theories has long been sought, but could not be found. Similarly, without leaving the domain of electricity, we can see that both the two-fluid and the single-fluid theories account for all the observed laws of electrostatics in an equally satisfactory manner. All these facts are readily explained by the properties of the Lagrangian equations, to which I just referred.

It is now easy to understand Maxwell's fundamental idea. To demonstrate the possibility of a mechanical explanation of electricity, we need not concern ourselves with finding the explanation itself. It is enough to know the expression of the two functions T and U, which are the two components of energy, to use these two functions to form the Lagrangian equations, and then to compare these equations to the experimental laws.

How can a choice be made between all the possible explanations when at this point there is no experiment to help us decide between them? The day may come when physicists will lose interest in these questions that are inaccessible to positive methods,[3] and will abandon them to metaphysicians, yet this day has not arrived. It is not so easy to resign ourselves to not ever knowing the true nature of things.

Our choice can only be guided by considerations in which personal judgment plays a large part. There are, however, solutions that everyone will reject because

[3] "Positive methods" means experimental methods, that is, things that are observable.

of their awkwardness, and others which everyone will prefer because of their simplicity. In the case of electricity and magnetism, Maxwell refrains from choosing. It is not that he is systematically averse to anything unattainable by positive methods as is amply attested by the time he devoted to the kinetic theory of gases. While in his great work he does not develop any complete explanation, I should add that he had previously attempted to give one in an article in *Philosophical Magazine*. The strangeness and complexity of the hypotheses he had been compelled to make led him to abandon it afterwards.

The same spirit is found throughout the whole work. What is essential, that is what should remain common to all theories, is brought to light. Anything that would only fit a specific theory is almost always omitted. Readers then encounter a form almost devoid of substance that gives them the impression of a fleeting and elusive shadow. However, the effort that readers are compelled to make leads to reflection and the ultimate understanding of what was often somewhat artificial in the theoretical constructs they once admired.

13

Electrodynamics

The history of electrodynamics is especially instructive from our perspective. Ampère called his timeless work *Théorie des phénomènes électrodynamiques, uniquement fondée sur l'expérience*.[1] He was under the impression that he had not made *any* hypothesis, however, as we will see in a moment, he had indeed made hypotheses, although he did so without realizing it. However, those who came after him did spot them, since their attention was drawn to the flaws in his formulation. They posited new hypotheses with full awareness of doing so. However, the hypotheses had to be changed many times before arriving at the classical system now in place, which is still perhaps not definitive, as we will see.

I Ampère's theory

When Ampère studied the interaction of currents experimentally, he only worked on closed currents and could only do so, although he did not deny the possibility of open currents. If two conductors have an opposite electrical charge and if we connect them with a wire, a current is established between the two until the two potentials become equivalent. In the thinking of Ampère's time, this was an open current. They saw clearly that the current flowed from the first conductor to the second; however, they did not see the current going back from the second to the first. Ampère considered currents of this type, like the discharge currents of capacitors, open currents; however, he was not able to study them experimentally because their duration was too short.

We can also imagine another kind of open current. Two conductors A and B are connected with a wire AMB. Small conducting bodies in motion are in

[1] Emphasis is Poincaré's. André-Marie Ampère, *Théorie mathématique des phénomènes électrodynamiques uniquement déduite de l'expérience* (Paris: Chez Firmin Didot, 1827; reprint Paris: Jacques Gabay, 1990). Some editions leave out *mathématique*, as does Poincaré, but all we have found say "derived from experiment" rather than "founded on experiment."

contact first with conductor B taking from it an electrical charge, then breaking contact with B, they travel along BNA carrying along the charge, and come into contact with A where they discharge and return then to B by way of wire AMB.

In a sense, this is really a closed circuit, since electricity flows through the closed circuit BNAMB; however, the two parts of this current are very different. In wire AMB, electricity moves *through* a fixed conductor as in a voltaic current, overcoming electrical resistance and producing heat. We say that electricity moves by *conduction*. In section BNA, electricity is *transported* by a moving conductor, in which case we say that it moves by *convection*.

If the convection current is then considered to be exactly the same as the conduction current, the circuit BNAMB is closed. If, on the contrary, the convection current is not "a real current" and, for example, does not have an effect on magnets, all that is left is conduction current AMB, which is *open*. If we connect the two poles of a Holtz machine[2] with a wire, the charged rotating disk brings electricity from one pole to the other by convection, then the electricity travels back along the wire to the first pole by conduction. Currents of this type with appreciable strength are nonetheless very difficult to produce. Considering the means at Ampère's disposal, we might say that it would have been impossible.

In brief, Ampère was able to conceptualize two kinds of open current, but he could not work with either one because they were too strong or because they lasted too little time. Experimentation could only show him then the action of a closed current on another closed current or, strictly speaking, the action of a closed current on a portion of current, since a current can be made to flow through a closed circuit composed of a moving part and a fixed one. We can then study the displacements of the moving part subject to another closed current. On the other hand, Ampère had no way of studying the action of an open current either on a closed current or on another open current.

1 The case of closed currents

In the case of the interaction of two closed currents, experimentation revealed some remarkably simple laws to Ampère. I will briefly mention the laws that will be pertinent to us later.

1° *If the strength of the currents is kept constant*, and if the two circuits, having undergone any displacements and deformations, go back to their initial

[2] A kind of electrostatic generator, introduced in 1864. W. Holtz, "On a New Electrical Machine," *Philosophical Magazine 4* (4th series), no. 30 (Dec. 1865): 425–33.

positions, the total work of the electrodynamic activity will be nil. In other words, there is an *electrodynamic potential* of the two circuits proportional to the product of the strengths and depending on the form and relative position of the circuits. The work produced by electrodynamic activities is equal to change in this potential.

2° The action of a closed solenoid is nil.

3° The action of a circuit C on another voltaic circuit C′ depends only on the "magnetic field" created by circuit C. At every point in space we can in fact determine the magnitude and the direction of a particular force called *magnetic force*[3] that has the following properties:

(a) The force exerted by C on a magnetic pole is applied to that pole. It is equal to the magnetic force multiplied by the magnetic mass of the pole.

(b) A very short magnetized needle tends to align itself with the direction of the magnetic force and the opposing force that tends to bring it back is proportional to the product of the magnetic force of the needle's magnetic momentum, and of the sine of the angle of displacement.

(c) If circuit C′ moves, the work of the electrodynamic force exerted by C on C′ will be equal to the increase of the "flux of magnetic force"[4] through the circuit.

2 Action of a closed current on a portion of current

Since Ampère was not able to set up an open current *per se*, he had only one way to study the action of a closed current on a portion of current, which was to work on a circuit C′ composed of two parts, of which one was fixed and the other movable. The movable part was, say, a movable wire $\alpha\beta$ whose ends α and β could slide along a fixed wire. In one of the movable wire's positions, end α was resting on point A on the fixed wire and end β was resting on point B of the wire. The current flowed from α to β, that is from A to B along the movable wire, and it returned then from B to A along the fixed wire. *This current was therefore closed.*

[3] "Force magnétique" is defined as the force that would act on a unit magnetic pole if it were placed at the location. In the modern terminology, it corresponds to the magnetic field.

[4] "Flux de force magnétique," which corresponds to the flux of the magnetic field in modern terminology.

In the second position, since the movable wire had slipped, end α rested on another point A' on the fixed wire and end β rested on another point B' of the fixed wire. The current circled then from α to β, that is from A' to B' along the movable wire and returned next from B' to B, then from B to A, and finally from A to A', always following the fixed wire. The current was therefore still closed. If a similar circuit is subject to the action of a closed current C, the movable part will move as if the work of a force were applied to it. Ampère *assumes* that the apparent force to which the movable part AB seems to be subjected, representing the action of C on segment $\alpha\beta$ of the current, is the same as if $\alpha\beta$ were traversed by an open current that stopped at α and at β, rather than being traversed by a closed current that returned to α across the fixed part of the circuit after having arrived at β.

This hypothesis may seem rather natural and Ampère supposed it unwittingly; nevertheless, *it is not necessary*, since Helmholtz rejected it, as we will see later. Although Ampère never managed to create an open current, the hypothesis nonetheless allowed him to state laws of the action of a closed current on an open current, or even a current element.

The laws remain simple:

1° The force that acts on a current element is applied to that element. It is at a right angle to that element and to the magnetic force and it is proportional to the component of the magnetic force that is at a right angle to the element.
2° The action of a closed solenoid on a current element is nil.

However, there is no more electrodynamic potential, which means that when a closed current and an open current whose strengths have been kept constant return to their initial positions, the sum of the work is not nil.

3 Continuous rotations

The strangest of the electrodynamic experiments are those where continuous rotations were produced; they are sometimes called *unipolar induction* experiments. A magnet can turn around its axis. A current flows first through a fixed wire, enters the magnet, for example through its pole N, flows halfway through the magnet, and leaves through a sliding contact, returning to the fixed wire. The magnet then starts to rotate continuously, without ever being able to reach equilibrium. This is Faraday's experiment.

How is this possible? If we were dealing with two circuits of invariable form, with a fixed one C and the other one, C', able to move around an axis, the latter would never be able to even enter into continuous rotation, because an electrodynamic potential exists. There will therefore necessarily be a position where equilibrium is reached, which will be the one where the electrodynamic potential will be the greatest.

Continuous rotations are then only possible if the circuit C′ is composed of two parts, one being fixed and the other movable around an axis, as shown in Faraday's experiment. It is still useful to make a distinction: The transition from the fixed part to the moving part or the inverse can take place, either through a simple contact (the same point of the moving part staying in constant contact with the same point on the fixed part), or through a sliding contact (the same point on the moving part coming successively into contact with different points of the fixed part).

It is only in the second case that there can be continuous rotation. What happens then is that the system tends to find a point of equilibrium, but when equilibrium is almost reached, the sliding contact places the moving part in communication with a new point on the fixed part. The new point changes the connections, changing therefore the conditions for equilibrium in such a way that the point of equilibrium is running away, so to speak, from the system that is trying to catch it. Rotation can thus perpetuate itself indefinitely.

Ampère assumes that the activity in a circuit on the movable part of C′ is the same as if the fixed part C′ did not exist and as if, as a result, the current that flows in the moving part was open. He concludes then that the action of a closed current on an open current, or inversely the action of an open current on a closed one, can give rise to continuous rotation. This conclusion, however, depends on the hypothesis that I just stated and that is not considered acceptable by Helmholtz, as I said above.

4 Interaction of two open currents

With regards to the interaction of two open currents and, in particular, that of two current elements, experiments are entirely lacking. Ampère appeals to hypotheses, supposing:

1° That the interaction of two elements is reduced to a force oriented along the straight line connecting them;

2° That the action of the closed currents is the result of the interactions of their various elements, which are, furthermore, the same as if the elements were isolated.

Here again, Ampère presupposes these two hypotheses without being aware of it, remarkably enough. Nonetheless, these two hypotheses in conjunction with the experiments on closed currents allow us to arrive at the law of the interaction of two elements. Yet the majority of the simple laws that we have encountered in the case of closed currents are no longer true. First of all, there is no electrodynamic potential, nor was there ever any, as we saw in the case of the closed current acting on an open current. Next, there is no longer any magnetic force *per se*. In effect, we attributed above three different definitions to this force:

1° According to the action to which a magnetic pole is subjected;
2° According to the directing torque that orients the magnetized needle;
3° According to the action undergone by a current element.

In the situation now under consideration, these three definitions not only are in conflict with each other, but each one of them is meaningless. In fact:

1° A magnetic pole is no longer simply subjected to a single force applied at that pole. We saw that the force due to the action of a current element on a pole is not applied to the pole but to the element. What is more, it can be replaced by a force applied to the pole by torque.
2° The torque that acts on a magnetized needle is no longer a simple directing torque, since its momentum with respect to the axis of the needle is not nil. It breaks down into a directing torque, strictly speaking, and an additional torque that tends to produce the continuous rotation mentioned above.
3° Finally, the force experienced by a current element is not at a right angle to that element.

In other words, *the unity of the magnetic force has disappeared*. This unity consists of the following: Two systems that have the same effect on a magnetic pole will have the same effect on an infinitesimally small magnetized needle or on a current element placed at the same point in space where the pole was, which would be true if these two systems only contained closed currents, but would no longer be true, according to Ampère, if these systems contained open currents.

Let us say simply that if a magnetic pole is placed on A and an element on B, and the direction of the element aligned on the extension of the straight line AB, that element will have no effect on this pole, although it would have an effect either on the magnetized needle at point A, or on a current element at point A.

5 Induction

We know that the discovery of electrodynamic induction followed close behind Ampère's legendary work. As long as it is a question only of closed currents, there is no problem. Helmholtz even remarked that the principle of the conservation of energy could be all it takes to deduce the laws of induction from Ampère's electrodynamic laws, under one condition, however. Bertrand showed clearly that we would have to accept as well a number of hypotheses. The same principle also makes this deduction possible in the case of open currents, even though, of course, the results cannot be verified experimentally, since we cannot set up such currents.

If we want to apply this kind of analysis to Ampère's theory on open currents, we will get results likely to surprise us. First, induction cannot be derived from the variation in the magnetic field according to the formula well known to theoretical and experimental scientists. In fact, as mentioned, there is no longer a magnetic field *per se*. Moreover, if a circuit C is subjected to induction from a variable voltaic system[5] S, if this system S moves and changes shape in any way, if the strength of the currents in the system vary according to any law, and if the system goes back to its initial state after the changes, it seems reasonable to suppose that the *mean* electromotive force created in the circuit C is nil. This is true if the circuit C is closed and the system S only includes closed currents. As soon as there would be open current, this would no longer be true if we accepted Ampère's theory. As a result, not only will induction no longer be the variation in the flow of the magnetic force in the usual sense of the word, but it will not be able to be expressed as the variation in anything.

II Helmholtz's theory

I have stressed the consequences of Ampère's theory and of his way of understanding the behavior of open currents. It is easy to recognize the paradoxical and artificial character of the propositions that his theory implies and we are led to think that "it should not be like that." We understand then that Helmholtz came to look for something else. Helmholtz rejects Ampère's fundamental hypothesis that the interaction of two current elements amounts to a force directed along the straight line that unites them. He supposes that a current element is not subject to

[5] "Système voltaique variable," which means, in modern terminology, a circuit in which a variable current occurs.

a single force, but to a force and to torque, which is precisely what gave rise to the famous polemic between Bertrand and Helmholtz.

Helmholtz replaces Ampère's hypothesis with the following one: Two current elements always allow for an electrodynamic potential, which depends only on their position and their orientation. The work that the two forces exert on each other is equal to the variation in this potential. In this way Helmholtz is as dependent on hypotheses as is Ampère, but at least he makes his dependence explicit.

In the case of closed currents, the only case that can be considered experimentally, the two theories are in agreement, whereas in all other cases they differ. Contrary to what Ampère held, the force to which the movable part of the closed current is subjected is not the same as that which that movable part would undergo if it were isolated and consisted of an open current.

Let us return to circuit C′ mentioned above that was made of a movable wire αβ sliding on a fixed wire. In the only feasible experiment, the movable part αβ is not isolated, but is part of a closed circuit. When the movable part goes from AB to A′B′, the total electrodynamic potential varies for two reasons:

1° It undergoes an initial increase since the potential of A′B′ with respect to circuit C is not the same as that of AB.
2° It undergoes a second increase since the potentials of elements AA′ and B′B with respect to C must be increased.

It is this *double* increase that represents the work of the force to which section AB seems subjected. If, on the other hand, αβ was isolated, the potential would only undergo the first increase and this increase alone would determine the work of the force acting on AB.

Second, there cannot be continuous rotation without sliding contact. In fact, as we saw in connection with closed currents, this is an immediate consequence of the existence of electrodynamic potential. In Faraday's experiment, if the magnet is fixed and the part of the current outside the magnet flows through a movable wire, the moving part can show continuous rotation. Nevertheless, this does not mean that if contacts of the wire with the magnet were eliminated and we made an *open* current flow down the wire, the wire would take up a movement of continuous rotation. I just said, in fact, that an *isolated* element does not undergo the same activity as that of a movable element that is a component of a closed circuit. Another difference is that the action of a closed solenoid on a closed current is nil according to experiment and according to both theories. Its action on an open current would also be nil according to Ampère, but not according to Helmholtz.

An important consequence follows from this. Above we gave three definitions of magnetic force, the third of which is meaningless here since a current element is no longer subject to a single force. The first does not make sense either. After all, what is a magnetic pole? It is the end of an indefinite linear magnet. This magnet can be substituted with an indefinite solenoid. In order to make the definition of magnetic force meaningful, it would be necessary that the action exerted on an open current on an indefinite solenoid be only dependent on the position of the end of the solenoid, which is to say that the action on the closed solenoid be nil. We just said, however, that this is not true. On the other hand, nothing stops us from adopting the second definition, the one based on the measurement of the directing torque that tends to guide a magnetized needle, yet if we adopt it, neither the effects of induction nor the effects of electrodynamics will depend only on the distribution of the magnetic lines of force.

III Difficulties raised by these theories

Helmholtz's theory represents progress over Ampère's, although we are far from ironing out all of the difficulties. In both theories, the magnetic field is meaningless, or if a meaning is attributed to it by a more or less artificial convention, ordinary laws so familiar to all electricians no longer apply. For example, the electromotive force induced in a wire is no longer measured by the number of lines of force the wire encounters.

Our reluctance comes not only from the difficulty in giving up set ways of speaking and thinking, but from something else. If we do not believe in actions at a distance, we need to explain electrodynamic phenomena as a change in a medium. It is precisely this change that we call the magnetic field, so then the electrodynamic effects should depend only on this field. All of these problems come from the open-current hypothesis.

IV Maxwell's theory

These were the problems raised by accepted theories, when Maxwell appeared and, with the stroke of a pen, made them all disappear. In his way of thinking, there are only closed currents. Maxwell's view is that if an electric field in a dielectric changes, this dielectric becomes the site of a particular phenomenon acting on a galvanometer like a current, which he calls *displacement current*. If two conductors carrying opposite charges are then connected with a wire, an open current takes over in the wire during the discharge, while at the same time in the surrounding dielectric, displacement currents are produced that close this conduction current.

We know that Maxwell's theory leads to the explanation of optical phenomena, thought to be due to very rapid electrical oscillations. At the time, such a notion was simply a bold hypothesis that could not be justified by any experiment. Twenty years later, Maxwell's ideas received experimental confirmation. Hertz managed to produce systems of electrical oscillations that reproduce all of the properties of light and only differ from them in terms of wavelength, as violet differs from red. In a sense, he synthesized light. As everyone knows, here lies the origins of the wireless telegraph.

One might say that Hertz did not demonstrate directly Maxwell's fundamental idea, that of the action of the displacement current on the galvanometer. It is true in a sense. After all, he only showed directly that electromagnetic induction is not instantaneous, as was thought, but rather occurs at the speed of light. However, supposing that there is no displacement current and that induction is propagated at the speed of light *is the same as* supposing that displacement currents produce induction effects and that induction occurs instantaneously. I cannot even hope to summarize here that which we do not see at first glance, but which we prove analytically.

V Rowland's experiments

As mentioned above, there are two types of open currents, the first being the currents of the discharge of a capacitor or of any conductor. There are also cases where electrical charges describe a closed curve, moving by conduction in one part of the circuit and by convection in the other.

The question could be seen as solved for open currents of the first type. They were closed by displacement currents. As for open currents of the second type, the solution seemed even simpler. If the current was closed, it seemed that it could only be closed by the convection current itself. For this to be the case, one just needed to accept that a "convection current," which means a charged conductor in motion, could act on a galvanometer. Experimental confirmation was still needed, however. It seemed difficult to produce a strong enough current, even when turning up the charge and the speed of the conductors as much as possible.

It was Rowland, a very skilled experimental researcher, who first triumphed over these difficulties. A disk revolving at very high speed received a strong electrostatic charge, so that an astatic magnetic system placed next to the disk was subjected to deviations. Rowland carried out the experiment twice, once in Berlin and once in Baltimore, and Himstedt carried it out next. These two physicists even believed that they could claim to have been able to make quantitative measurements. All physicists accepted Rowland's law without questioning it. Besides, everything seemed to confirm the law. The spark definitely produces a magnetic effect. Does it not seem likely that the release of the spark is due to particles pulled from one of the electrodes and carried to the other electrode with their charge? Even the analysis of the spectrum of the spark reveals the rays of the electrode's metal. Is this not proof? The spark would then be a real convection current.

On the other hand, we also accept that, in an electrolyte, electricity is conducted by ions in motion. The current in an electrolyte would then also be a convection current, yet it acts on a magnetized needle. The same is true for cathode rays. Crookes attributes these rays to the effect of a very subtle substance charged with negative electricity and moving at very high speed. In other words, he considers them to be like convection currents and his once-contested viewpoint is now adopted everywhere. These cathode rays are deflected by the magnet. By virtue of the principle of action and reaction, they should in turn deflect the magnetized needle.

It is true that Hertz believed he had proved that cathode rays do not carry negative electricity and that they do not act on a magnetized needle, but he was wrong. First Perrin was able to collect electricity carried by these rays whose existence Hertz denied. The German scientist seems to have been misled by effects due to the action of X-rays, which had not yet been discovered. Later and quite recently, the activity of cathode rays on a magnetized needle was demonstrated and the source of Hertz's error recognized. So all of the phenomena regarded as convection currents—sparks, electrolytic currents, and cathode rays—act on a galvanometer in the same way and in accordance with Rowland's law.

VI Lorentz's theory

Science soon made further progress. According to Lorentz's law, conduction currents themselves would be actual convection currents. Electricity would remain inextricably attached to certain physical particles called *electrons*. The circulation of electrons through bodies would produce voltaic currents. Conductors could be distinguished from insulators by the fact that electrons pass through conductors whereas they are blocked by insulators. Lorentz's theory is very seductive in that it provides a very simple explanation for certain phenomena for which former theories, even Maxwell's in its original form, could not account in a satisfactory way, such as, for example, the aberration of light, the partial dragging of light waves, magnetic polarization, or Zeeman's experiment.

Some objections still remained. The phenomena belonging to a system seem to have to depend on the absolute speed of translation of the center of gravity of the system, which runs contrary to the idea that we have of the relativity of space. During Crémieu's doctoral defense, Lippmann offered this objection in a striking way. Take two charged conductors, moving at the same translational speed. Although at relative rest, both of them are equivalent to a convection current, so they should attract each other, and their absolute speed could be measured if we measured this attraction.

Lorentz's followers objected, responding that what would be measured in that case is not their absolute speed, but their relative speed *with respect to the ether*, such that the principle of relativity is left unscathed. Besides, Lorentz has found since a more nuanced response that is much more satisfactory.[6]

These last objections notwithstanding, the construction of the electrodynamics edifice seems, by and large, definitively completed. Everything appears most satisfactory. Ampère's and Helmoltz's theories seem to have only a purely historical interest now, given that they were created for open currents that no longer exists.[7] The history of these changes is still instructive since it points out the traps that scientists face and shows how one can hope to avoid them.[8]

6 This last sentence was not in the 1902 edition.

7 In the 1902 edition, Poincaré used the imperfect, expressing a certain uncertainty as to whether electrodynamics had yet truly found its definitive form, adding: "and the inextricable complications to which these theories led were more or less forgotten. This peace of mind was recently disturbed by Crémieu's experiments which, for a moment, seemed to contradict the results previously obtained by Rowland" (1902: 281). In 1906, this passage was slightly edited and was followed by the sentence (deleted in the 1917 edition): "New researches have not confirmed them and Lorentz's theory has withstood the test victoriously" (1906: 281).

8 The last sentence replaces the following passage of the first editions: "Numerous researchers have tried to solve the problem and new experiments are underway. What will be their result? I will certainly not venture a prediction that could be invalidated between the day when I hand in my final corrected proofs and the one when this book will be on sale" (1902: 281).

14

The End of Matter*

One of the most surprising discoveries that physicists have announced in the last few years is that matter does not exist. Let us say right now that these findings are not yet definitive. The primary attribute of matter is its mass, its inertia. Mass is what remains constant everywhere and throughout time. It is that which continues to exist when a chemical transformation has altered all of the perceptible qualities of the substance and seems to have changed it into another substance. If then we came to demonstrate that mass, the inertia of matter, did not belong to it in reality, that it is a borrowed extra that it carries, that this constant *par excellence* is itself subject to change, we could very well say that matter does not exist, which is exactly what we are stating.

The speeds that we were able to observe up to this point were relatively low, since astronomical bodies, which leave far behind them all of our automobiles, nevertheless go only sixty to one hundred "kilometers" per second. It is true that light goes three thousand times faster, although it is not a substance that is moving, but rather a disturbance that courses through a relatively still substance, like a wave moving over the surface of the ocean. All observations made at such low speeds showed the invariability of mass and no one questioned if mass would remain constant at higher speeds.

The tiniest of particles beat the speed record of Mercury, the fastest planet. I am speaking of particles whose motions produce cathode rays and the rays emitted by radium. We know that these sources of radiation are due to veritable molecular bombardment. The projectiles shot out in this bombardment have a negative electrical charge and we can be sure of it if we collect this electricity in a Faraday cylinder. They are deflected both by a magnetic field and by an electric field because of their charge and the comparison of these deviations can let us know their speed and the relationship of their charge to their mass.

* See *L'Évolution de la matière* by Gustave Le Bon [available at: http://gallica.bnf.fr/ark:/12148/bpt6k5453827p [accessed May 2, 2017]].

Yet these measurements have revealed, on the one hand, that their speed is tremendous, that it is a tenth or a third of the speed of light, a thousand times that of the planets and, on the other hand, that their charge is extremely large in respect to their mass. Each particle in movement represents then a significant electric current. We know that electrical currents present a sort of special inertia called *self-induction*. Once established, a current tends to continue to flow, which is why we see a spark fly at the breaking point when we try to break the current by cutting the conductor through which the current flows. In this way, the current tends to conserve its strength just as a body in motion tends to conserve its speed.

Our cathode particle will then resist the forces that would alter its speed for two reasons: by its own inertia first and then by self-induction, since all change in speed would be as well a change in the corresponding current. The particle—*the electron*, as we call it—will therefore have two inertias: mechanical inertia and electromagnetic inertia.

Abraham and Kaufman, the first a theoretician and the second an experimental scientist, combined their efforts to determine the role of the two kinds of inertia. They were compelled to accept a hypothesis, supposing that all negative electrons are identical, that they carry the same essentially constant charge, and that the differences noted between individual electrons comes solely from the different speeds they exhibit. When the speed varies, the real mass, the mechanical mass, stays the same, which is, so to speak, in itself its definition. Electromagnetic inertia contributes to form the apparent mass and increases with the speed according to a certain law. There should therefore be a connection between the speed and the ratio of the mass to the charge, quantities that we can calculate, as mentioned, by observing the bending of rays under the influence of a magnet or an electrical field. The study of this interaction allows us to determine the role of the two inertias. The findings that *real mass is nonexistent* is a complete surprise. It is true that one has to accept the hypothesis posited at the outset, but the concordance of the theoretical curve and the experimental curve is great enough to make this hypothesis quite plausible.

Thus, these negative electrons have no mass, strictly speaking. If they seem to be endowed with inertia, it is because they cannot change speed without disrupting the ether. Their apparent inertia is no more than a loan from the ether, rather than something proper to electrons themselves. These negative electrons, however, do not constitute all matter. We could then suppose that besides electrons there is a kind of real matter endowed with its own inertia. There are certain kinds of radiation, like Goldstein's canal rays and the α rays of radium, that are caused by a rain of positively charged projectiles. Are these positive electrons also void of mass? It is impossible to tell, because they are a lot heavier and a lot slower than negative electrons. So then, two hypotheses are still admissible: Either these electrons are heavier, since they have mechanical inertia beyond their borrowed electromagnetic inertia, which then makes them the real matter; or, actually, these electrons have no mass, just like the other ones, and if they seem heavier it is because they are smaller. I do mean smaller, although that might seem paradoxical, for in this model the particle would be nothing more than a bit of emptiness in the ether, only real, only endowed with inertia.

Up to this point, matter is not at too much risk, so we can still adopt the first hypothesis or even believe that beyond positive and negative electrons there are neutral atoms. Lorentz's recent research will disavow us of this last option. We move along with the movement of the earth, which is very fast. Will not optical and electrical phenomena be changed by this movement? We thought so for a long time and we supposed that observations would detect some differences, according to the orientation of the instruments in relation to the movement of the earth. It did not work out that way and the most careful measurements did not show anything of the sort. In this respect, the experiments justified a general aversion on the part of all physicists. If something had indeed been found, we would have discovered not only the relative motion of the earth with respect to the sun, but its absolute motion with respect to the ether as well. Many people have difficulty believing that no experiment is capable of showing something other than relative motion and would more willingly accept that matter has no mass.

The negative results obtained were therefore not so surprising, even though they ran contrary to accepted theories, they favored a deep instinct that preceded all these theories. It was still necessary to modify these theories accordingly to bring them in agreement with the experimental facts. This is what Fitzgerald did in proposing a surprising hypothesis. He supposes that all bodies undergo a contraction of about one hundred millionth in the direction of the movement of the earth. A perfect sphere becomes a flattened ellipsoid and if you make it spin, it changes shape such that the short axis of the ellipsoid always remains parallel to the speed of the earth. Since the measuring instruments undergo the same deformations as the objects being measured, we do not notice anything, unless we go to the trouble to determine the time that it takes for light to run the length of the object.

This hypothesis takes into consideration observed facts, but it is not enough. One day we will make observations that are even more precise and this time the results will be positive. Will they allow us to determine the absolute movement of the earth? Lorentz did not think so, believing that this determination would always be impossible. The instinct held by all physicians and the lack of success encountered until now are proof enough to him. Let us consider then this impossibility as a general law of nature that we will accept as a postulate. What will be the consequences? This is what Lorentz wanted to discover and he found that the inertia of all atoms and all electrons, positive or negative, should vary with speed according to exactly the same laws. In this way, each bit of matter would be formed of small, heavy, positive electrons and big, light, negative electrons. If the perceived substance does not seem electrically charged, it is because the two types of electrons are approximately equal in number. Both of them are devoid of mass and have only borrowed inertia. In this system, there is no true matter, only holes in the ether.

For Langevin, matter would be liquefied ether and, having lost its properties when matter would move, it would not be that liquefied mass that would travel through the ether, but the liquefaction would gradually spread to new portions of the ether, while at the back, the first parts liquefied would return to their original state. In moving, matter would not maintain its identity.

So here is where the question stood not long ago, and we now hear that Kaufmann has presented the results of new experiments. The negative electron whose speed is tremendous should undergo Fitzgerald's contraction, which should modify the relationship between speed and mass. Yet these recent experiments do not confirm this prediction. It would all fall flat then and matter would regain its right to exist. However, these experiments are complicated and a definitive conclusion would be premature at this point in time.

Index

absolute and relative motion 83–9
absolute space 63, 71, 72, 86
absolute time 71, 72
acceleration, law of 75–80, 82, 83–4
  see also relative motion
accidental constant 87–8
addition 10–11
algebra 12–13
Ampère's electromagnetic theory 151–7, 162
Analysis situs/topology 29
ancestral experience 68
Andrade, J. 72, 81, 82
anthropomorphic mechanics 80–1
Archimedes' axiom 41
association of ideas 48
astronomy
  classical mechanics 73–4, 75, 78–9
  distribution of asteroids and stars 131, 134–7, 138
  and geometry 59–60
  principle of relative motion 84–9
  simplicity 107–8
axioms 33–4
  implicit 38–9, 40–1
  nature of 41–3

Bacon, F. 103–4
bankruptcy of science 115
bell curve 140–1
Beltrami, E. 36–7
Bertrand, J. 128, 157
bodily senses 46–50, 52, 55–6, 66–8, 80–1
Bolyai, J. 33–4
Brownian motion 125

Carlyle, T. 103–4
Carnot's principle 117–18, 124, 125
causation, probabilistic 131, 138–40
centrifugal force 85
"chance" and "probability" distinction 127
classical mechanics 71–2
  anthropomorphism 80–1
  and energetics 91–4
  law of acceleration 75–80, 82, 83–4
  principle of inertia 72–5
  "School of the Thread" 81–2
Clausius' principle 94, 97, 117–18
commensurable numbers 19, 21, 23
commutativity 11, 12
compensation, conditions of 50
conservation of energy, principle of 92, 94–7, 118, 122, 124, 146, 148, 157
  see also Clausius' principle, Mayer's principle/law
constant curvature, surfaces of 36–7
constants
  accidental and essential 87–8
"construction" procedure in mathematics 16–17
continuity 126
  physical laws
  visual space 46
  see also mathematical continuum
contradiction, principle of 7, 14, 22, 25, 26–7
conventions 43, 71–2, 82, 86, 99–100, 128, 133–9, 142, 159
Copernicus, N. 85, 86
Coulomb, C.-A. 117
Crémieu, V. 162
crucial experiment 104, 149

Dedekind, R. 21
Descartes, R. 103
displacement current 159–60
displacements 51–2, 66–7
  non-Euclidean 54, 55
distribution of asteroids and stars 131, 134–7, 138
du Bois-Reymond, E. 26

écarté (card game) 131, 139
electricity
development of modern physics 115–16, 117, 122–3
and optics 143–50
electrodynamics 151–62, 163–6
continuous rotations 154–5, 158
dispersion theories 116
elementary phenomena 110–13, 124
elementary mathematical theorems 9–12
energetics 91–4, 96, 118, 146–7
see also conservation of energy, principle of; thermodynamics; least action, principles of
energy
chemical 93
electrical 93
kinetic (T) 91, 92–3, 146–9
potential (U) 91, 92–3, 146–9
thermal 93
equality of action and reaction 77, 79, 82, 120, 123
errors, theory of 140–2
essential constant 87–8
ether 119–21, 123
Euclidean geometry 33–4, 35–6, 37, 39, 43, 59–62, 64–5, 68
Euclid's postulate 33–4, 35, 39, 59, 61, 68, 99
experience
anthropomorphism 80–2
and geometry 59–68
and mathematical magnitude 19–29
relative and absolute motion 86–9
experimental laws
energetics 91
and probability 139–40
experiments
Fechner's law 22, 27
and generalization 99–100, 103–5, 108–10
geometry 61–3, 64–6
law of acceleration 79–80
law of inertia 74–5
mechanical explanations of physical phenomena 120–1, 146–50
role of hypotheses 108–9
Rowland's (thermodynamics) 160–1
School of the Thread 81–2
Faraday, M. 154–5, 158
Fechner's law 22, 27
Fizeau, H. 120, 123
fluids 117, 119–20, 146, 149
force(s)
Coriolis 85
friction 85
gravitational 76
magnetic 153, 154, 156, 158
measurement and definition of 75–8, 80–2
four-dimensional world 55–6
fourth geometry 40
Fresnel, A.-J. 115, 125–6, 143–4, 149

games of chance 131, 137–8, 139
Gauss' law of errors 140–2
general principles, role of 118
generalization
and experiments 99–100, 103–5, 108–10
mathematics 7, 15–17
physics 107–8
thermodynamics 94–5
unity of nature 105–6
generalized law of inertia 73–4, 84, 85, 86
geometric postulates
See Archimedes' axiom; Euclid's postulate
"geometric properties of bodies" 64–6
geometry 26–7
(or all of Part 2) (pp. 36–68)
construction 16–17
Euclidean 33–4, 35–6, 37, 39, 43, 59–62, 64–5, 68
and space 45–57
topology 28–9
see also non-Euclidian geometries
Gouy, L.G. 125
gravitational mass 78
gravity, centre of 78–9, 80, 85

heat propagation 110–11
Helmholtz, H. von 91, 116, 119, 154, 155, 157–9, 162
Hertz, H. 79, 161
Hilbert, D. 41
Hilbert's geometry 41
Hilbert's space 56
homogeneity, law of 52–3

hypotheses 3–4, 34–5, 53–5, 60, 71, 74, 78, 83–4, 86, 91, 94, 96–7, 108–11, 121, 134–5, 138, 142–5, 151, 154–8, 164–5

incommensurables in mathematical continuum 20–1, 23–4
induction
  electrodynamic 157, 164
  mathematical 10, 12–17, 113
inertia
  generalized law of 73–4, 84, 85, 86
  hypotheses 164–5
  principle of 72–5
infinitesimals 26
integration 112
intuition 14, 15, 24, 26–7, 80, 133
intuitive propositions 25
irreversible phenomena 124–5

Kant, I. 42
Kelvin, Lord 119
Kepler, J. 108, 110, 126, 131, 134
kinetic theory of gases 107, 116, 124–5, 129, 131, 150
Kirchoff, G. 76, 77, 80, 81, 82
Kronecker, L. 20, 24

Lagrange, J.-L. 124, 147–8, 149
Laplace, P.-S. 106
large numbers, law of 107, 111–12, 124, 129
Le Roy, E. 2
least action, principle of 94, 124, 146–7, 148
Leibniz, G.W. 8–9
Lie's theorem 40–1
light
  classification of new phenomena 125–6
  see also optics
Lippmann, G. 162
Lobachevskii, N. 33–4, 35–6, 37–8, 41–2, 59, 60, 61, 63
logarithms and probability 132–3
Lorentz, H. 121, 122–3, 161–2, 165–6

Mariotte's law 106, 107, 129, 140
mass, measurement and definition of 75–9
mathematical continuum 19–20
  important questions 26–7
  incommensurables, definition and necessity of 20–1, 23–4
  measurable magnitudes 25
  multidimensional 28–9
  physical continuum 22–4, 25
    multidimensional 27–8
  stages of creation 22–5
mathematical induction 10, 12–17, 113
mathematical physics, origin of 110–13
mathematical reasoning
  elementary theorems 9–12
  generalization 7, 15–17
  principle of contradiction 7, 14, 22, 25, 26–7
  proof and verification 8–9
Maxwell, K.C. 115, 124, 125, 144–6, 150, 159–60
Mayer's principle/law 94, 95–6, 97
measurable magnitudes 25
measurement: law of acceleration 75–80
mechanical explanation of physical phenomena 118–21, 146–50
  see also classical mechanics
metaphors 117
Michell's problem 138
Mill, J. S. 38–9
modern physics, theories of
  current state of 121–6
  meaning of 115–18
  mechanical explanations 118–21
motion *see* acceleration, law of; classical mechanics; electrodynamics; energetics; relative motion; thermodynamics;
motor space 47–8
  see also muscular sensations
multidimensional mathematical continuum 28–9
multidimensional physical continuum 27–8
multiplication
  definition of 11–12
muscular sensations 46–8, 49–50, 52, 55–6, 66–8, 80–1

Newton, I. 63, 74, 76, 77, 78–9, 80, 84–9, 107, 110, 126
nominalism 2, 80, 100
non-Archimedean geometry 41, 56
non-Euclidian geometries 33–45, 53–5
  Euclidean and non-Euclidian space 59–62, 64–5

objective and subjective probability 129–30, 133
optics 109–10, 123
  and electricity 143–50
  laws of reflection 125–6

physical chemistry 126
physical continuum, *see* mathematical continuum
physics
  origin of mathematical physics 110–13
  and probability 134–7
  role of experiment and generalization 103–5, 108–10
  role of hypothesis 108–10
  unity of nature 105–8, 121–2, 124
  see also classical mechanics; modern physics, theories of; thermodynamics
position/stance and geometry 49–50, 66–8
preconceived ideas 104–5
probability 127–42
  classification of problems of 130–2
  games of chance 131, 137–8, 139
  generality perspective and probability 130
  in mathematics 132–4
  in physical sciences 134–7
  theory of errors 140–2
  see also causation probabilistics subjective and objective probability
proof by recurrence *see* mathematical induction
proof and verification 8–9
proportionality, law of 107

q parameters 146–9

random and systematic errors 140–2
reflection, laws of 125–6
relative motion
  and absolute motion 83–9
  principle of 83–4
relativity, law of 61, 62–3
representative space and geometrical space 45–6
reversible and irreversible phenomena 124–5
Riemann, B. 34, 35–6, 41, 59, 119
rotation
  continuous (electrodynamics) 154–5, 158
  of earth 84–6
Rowland, H. 160–1

"School of the Thread" 81–2
self-induction 164
simplicity 105–8, 121–2, 125–6
  elementary phenomena 110–13
  see also unity of nature
solid bodies and geometry 51–2
space and geometry 45–57
spherical geometry 35, 36–7
subjective and objective probability 129–30, 133
surfaces of constant curvature 36–7
syllogisms 7, 8, 12, 13, 14
symbols 25, 26
synthetic *a priori* judgments 7, 14–15, 42, 43
systematic and random errors 140–2

tactile space/touch 47–8, 50, 67
  see also bodily senses; visual space/sight
Tannery, J. 19
theory of errors 140–2
thermodynamics 94–7, 124
  heat propagation 110–11
  second fundamental law of 117–18
  see also Carnot's principle; Clausius' principle; conservation of energy, principle of; energetics;

energy; irreversible phenomena; Mayer's principle
Tycho, S. 108, 126

unity of nature 105–8, 121–2, 124
see also simplicity

verification 8–9, 13–14, 105, 108

Veronese, G. 41
visual space/sight 46–7, 48–50, 55–6, 67
see also bodily senses; tactile space/ touch

wave theory of light 143–4

Zeeman effect 123, 125, 162