Science and Hypothesis

HENRI POINCARÉ
1854–1912

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La Science et l'Hypothèse

Translated from the French with AI (Claude) and edited



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INTRODUCTION

Henri Poincaré was the leading mathematician of his age, a founder of topology and of the theory of chaos, and a co-discoverer of much of the mathematics of relativity. In 1902 he gathered a decade of essays into a short book for a general audience, *La Science et l'Hypothèse*, and asked a plain question: how much of what science tells us is fact, and how much is our own choice? To doubt everything or to believe everything, he says at the outset, are two equally convenient ways of not having to think.

His answer moves through the sciences in order. Arithmetic is not a mere tautology, because reasoning by recurrence lets the mind leap past any finite check. The axioms of geometry are neither truths known in advance nor facts of experience but conventions: asking whether Euclidean geometry is true is like asking whether the metric system is true; it is only more convenient. The principles of mechanics, too, began as experimental laws and hardened into definitions that no experiment can now refute.

Physics is different, and the last part defends it against the sceptics who concluded that science is only a set of useful recipes. Theories come and go, Poincaré admits, but the relations they capture survive; science can know not things in themselves but the true relations between them. Along the way he discusses probability, the ether, and the electrodynamics of Maxwell and Lorentz on the eve of Einstein, who read the book with his friends in Bern. Its thesis, conventionalism, shaped the philosophy of science for the rest of the century.

Introduction

Introduction
Neither doubt everything nor believe everything: the different kinds of hypothesis in science, and which of them are legitimate.

Intro.1 To a superficial observer, scientific truth is beyond the reach of doubt; the logic of science is infallible, and if scientists are sometimes mistaken, it is because they have disregarded its rules.

Intro.2 Mathematical truths are derived from a small number of self-evident propositions by a chain of flawless reasoning; they are binding not only on us but on nature itself. They fetter the Creator, so to speak, and allow him only to choose among a relatively small number of solutions. A few experiments will then be enough to tell us which choice he has made. From each experiment a host of consequences can follow through a series of mathematical deductions, and in this way each experiment will show us a corner of the Universe.

Intro.3 This, for many educated laypeople and for schoolchildren getting their first ideas of physics, is the origin of scientific certainty. This is how they understand the role of experiment and of mathematics. And this is also how it was understood a hundred years ago by many scientists, who dreamed of constructing the world while borrowing as little material as possible from experience.

Intro.4 When people thought about it a little more, they saw the place held by hypothesis; they saw that the mathematician cannot do without it, and that the experimenter does not do without

it either. And then people asked whether all these constructions were really sound, and believed that a breath would knock them down. To be skeptical in this way is still to be superficial. To doubt everything and to believe everything are two equally convenient solutions; each spares us from having to think.

Intro.5 Instead of passing a summary condemnation, then, we must examine the role of hypothesis carefully; we shall then recognize not only that it is necessary but that, more often than not, it is legitimate. We shall also see that there are several kinds of hypotheses: that some are verifiable, and once confirmed by experiment become fruitful truths; that others, though they cannot lead us into error, can be useful to us by giving our thought something fixed to hold on to; and that others, finally, are hypotheses only in appearance and reduce to definitions or to disguised conventions.

Intro.6 These last are found above all in mathematics and in the sciences related to it. It is precisely from them that these sciences derive their rigor; these conventions are the work of the free activity of our mind, which in this domain recognizes no obstacle. Here our mind may affirm because it decrees; but let us be clear: these decrees are binding on *our* science, which without them would be impossible; they are not binding on nature. Are these decrees arbitrary, then? No; otherwise they would be sterile. Experience leaves us free to choose, but it guides our choice by helping us to discern the most convenient path. Our decrees are therefore like those of an absolute but wise prince who would consult his Council of State.

Intro.7 Some people have been struck by this character of free convention that we recognize in certain fundamental principles of the sciences. They have tried to generalize beyond measure, and at the same time they have forgotten that freedom is not arbitrariness. They have thus arrived at what is called *nominalism*, and have asked themselves whether the scientist is not the dupe of his own definitions, and whether the world he believes

he is discovering is not simply created by his own whim[1]. In that case, science would be certain, but devoid of significance.

Intro.8 If that were so, science would be powerless. Yet every day we see it at work before our eyes. That could not be unless it taught us something about reality; but what it can reach is not things themselves, as naive dogmatists think, but only the relations between things; apart from these relations there is no knowable reality.

THE PLAN OF THE BOOK

Poincaré previews his route from arithmetic and geometry through mechanics to experimental physics.

Intro.9 Such is the conclusion we shall reach; but to get there we shall have to go through the whole series of sciences, from arithmetic and geometry to mechanics and experimental physics.

Intro.10 What is the nature of mathematical reasoning? Is it really deductive, as is commonly believed? A thorough analysis shows us that it is nothing of the kind: that to a certain extent it shares in the nature of inductive reasoning, and that this is what makes it fruitful. It nonetheless retains its character of absolute rigor; that is what we had to show first.

Intro.11 Now that we know better one of the instruments that mathematics puts into the researcher's hands, we had to analyze another fundamental notion, that of mathematical magnitude. Do we find it in nature, or is it we who introduce it there? And in the latter case, do we not risk distorting everything? Comparing the raw data of our senses with that extremely complex and subtle concept that mathematicians call magnitude, we are forced to acknowledge a discrepancy; this framework into which we want to fit everything is therefore one we made ourselves; but we did not make it at random; we made it, so to

1 POINCARÉ'S NOTE. See Le Roy, Science et Philosophie (*Revue de Métaphysique et de Morale*, 1901.)

TRANSLATOR'S NOTE. Édouard Le Roy (1870–1954), French philosopher and mathematician, a follower of Bergson, who argued that scientific facts and laws are largely made by the scientist.

speak, to measure, and that is why we can fit the facts into it without distorting what is essential in them.

Intro.12 Another framework that we impose on the world is space. Where do the first principles of geometry come from? Are they imposed on us by logic? Lobachevsky showed that they are not, by creating the non-Euclidean geometries. Is space revealed to us by our senses? Again no, for the space our senses could show us differs absolutely from the space of the geometer. Does geometry derive from experience? A thorough discussion will show us that it does not. We shall therefore conclude that its principles are only conventions; but these conventions are not arbitrary, and if we were transported into another world (which I call the non-Euclidean world and which I try to imagine), we would have been led to adopt different ones.[2]

Intro.13 In mechanics we would be led to similar conclusions, and we would see that the principles of this science, though more directly based on experience, still share in the conventional character of the postulates of geometry. So far nominalism triumphs; but now we come to the physical sciences proper. Here the scene changes; we meet another kind of hypothesis, and we see how fruitful it is. No doubt, at first sight, theories seem fragile to us, and the history of science proves that they are short-lived; yet they do not die entirely, and something of each of them remains. It is this something that we must try to disentangle, because it is there, and there alone, that true reality lies.

Intro.14 The method of the physical sciences rests on induction, which leads us to expect a phenomenon to recur when the circumstances in which it first arose are repeated. If *all* these circumstances could be reproduced at once, this principle could be applied without fear; but that will never happen; some of the circumstances will always be missing. Are we absolutely sure that they do not matter? Obviously not. It may be likely; it cannot be strictly certain. Hence the considerable role played in the

2 Nikolai Lobachevsky (1792–1856), Russian mathematician who published one of the first non-Euclidean geometries in 1829.

physical sciences by the notion of probability. The calculus of probabilities is therefore not merely a pastime or a guide for baccarat players, and we must try to get to the bottom of its principles. In this respect I have been able to give only very incomplete results, so stubbornly does that vague instinct which lets us discern likelihood resist analysis.

Intro.15 Having studied the conditions under which the physicist works, I thought I ought to show him at work. For this I took a few examples from the history of optics and from that of electricity. We shall see where the ideas of Fresnel and of Maxwell came from, and what unconscious hypotheses were made by Ampère and the other founders of electrodynamics.[3]

3 Augustin Fresnel (1788–1827), French physicist of the wave theory of light; James Clerk Maxwell (1831–1879), Scottish physicist of electromagnetic theory; André-Marie Ampère (1775–1836), French founder of electrodynamics.

Part One: Number and Magnitude

Le nombre et la grandeur
Why mathematical induction makes arithmetic more than a tautology, and how the mathematical continuum is built out of the crude continuum of the senses (Chapters I–II).

CHAPITRE PREMIER. SUR LA NATURE DU RAISONNEMENT MATHÉMATIQUE.

1.1.1 The very possibility of mathematical science seems an insoluble contradiction. If this science is deductive only in appearance, where does it get that perfect rigor which no one dreams of questioning? If, on the other hand, all the propositions it states can be derived from one another by the rules of formal logic, why is mathematics not reduced to an immense tautology? The syllogism can teach us nothing essentially new, and if everything were to spring from the principle of identity, everything should also be reducible to it. Are we then to admit that the statements of all those theorems that fill so many volumes are nothing but roundabout ways of saying that A is A?

1.1.2 No doubt we can go back to the axioms that are the source of all reasoning. If we judge that they cannot be reduced to the principle of contradiction, and if we also refuse to see them as experimental facts, which could not share in mathematical necessity, we still have the resource of classing them among synthetic *a priori* judgments. That is not to solve the difficulty, only to christen it; and even if the nature of synthetic judgments held no more mystery for us, the contradiction would not have vanished; it would only have been pushed back. Syllogistic reasoning remains incapable of adding anything to the data it is

given; these data reduce to a few axioms, and we ought to find nothing else in the conclusions.

1.1.3 No theorem ought to be new unless a new axiom came into its proof; reasoning could give us back only the immediately evident truths borrowed from direct intuition; it would be nothing but a parasitic intermediary, and should we not then have reason to ask whether the whole syllogistic apparatus serves only to disguise our borrowing?

1.1.4 The contradiction will strike us even more if we open any book of mathematics at all; on every page the author will announce his intention of generalizing a proposition already known. Does mathematical method, then, proceed from the particular to the general, and if so, how can it be called deductive?

1.1.5 Finally, if the science of number were purely analytic, or could be derived analytically from a small number of synthetic judgments, it seems that a sufficiently powerful mind could perceive all its truths at a single glance; what am I saying?—one might even hope that one day a language would be invented to express them that was simple enough for them to appear immediately, in this way, to an ordinary intelligence.

1.1.6 If we refuse to accept these consequences, we must concede that mathematical reasoning has in itself a sort of creative power, and therefore that it differs from the syllogism.

1.1.7 The difference must even be a profound one. We shall not, for example, find the key to the mystery in the frequent use of the rule according to which one and the same uniform operation applied to two equal numbers will give identical results.

1.1.8 All these modes of reasoning, whether or not they can be reduced to the syllogism proper, keep their analytic character and are, for that very reason, powerless.

LEIBNIZ PROVES THAT 2 AND 2 MAKE 4

Such proofs of particular equalities are mere verifications: analytic, sterile and no science.

1.1.9 The debate is an old one; Leibniz already tried to prove that 2 and 2 make 4; let us take a brief look at his proof.[1]

1.1.10 Suppose we have defined the number 1 and the operation $x + 1$, which consists in adding one to a given number x.

1.1.11 These definitions, whatever they may be, will play no part in the rest of the reasoning.

1.1.12 I then define the numbers 2, 3 and 4 by the equalities: (1) 1 + 1 = 2; (2) 2 + 1 = 3; (3) 3 + 1 = 4.

1.1.13 In the same way I define the operation $x + 2$ by the relation: (4) $x + 2 = (x + 1) + 1$.

1.1.14 This being granted, we have: 2 + 2 = (2 + 1) + 1 (Definition 4); (2 + 1) + 1 = 3 + 1 (Definition 2); 3 + 1 = 4 (Definition 3), whence 2 + 2 = 4 Q.E.D.

1.1.15 It cannot be denied that this reasoning is purely analytic. But ask any mathematician: "That is not a proof proper," he will tell you, "it is a verification." We have merely set two purely conventional definitions side by side and observed that they are identical; we have learned nothing new. *Verification* differs from true proof precisely because it is purely analytic and because it is sterile. It is sterile because the conclusion is only a translation of the premises into another language. True proof, on the contrary, is fruitful because in it the conclusion is, in a sense, more general than the premises.

1.1.16 The equality 2 + 2 = 4 could be verified in this way only because it is particular. Every particular statement in mathematics can always be verified in this way. But if mathematics were reduced to a series of such verifications, it would not be a science. In the same way a chess player, for example, does not cre-

1 Gottfried Wilhelm Leibniz (1646–1716), German philosopher and mathematician; the proof appears in his *New Essays on Human Understanding*.

ate a science by winning a game. There is no science except of the general.

1.1.17 We may even say that the very purpose of the exact sciences is to spare us these direct verifications.

ARITHMETIC REBUILT FROM SCRATCH

Addition and multiplication are defined by recurrence and their laws proved step by step.

1.1.18 Let us watch the mathematician at work, then, and try to catch his methods in the act.

1.1.19 The task is not without difficulty; it is not enough to open a book at random and analyze any proof whatever in it.

1.1.20 We must first exclude geometry, where the question is complicated by thorny problems concerning the role of postulates and the nature and origin of the notion of space. For similar reasons we cannot turn to infinitesimal analysis. We must look for mathematical thought where it has remained pure, that is, in arithmetic.

1.1.21 Even then we must choose; in the higher parts of the theory of numbers, the primitive mathematical notions have already been worked over so thoroughly that it becomes difficult to analyze them.

1.1.22 It is therefore at the beginning of arithmetic that we must expect to find the explanation we are looking for; but it so happens that it is precisely in the proofs of the most elementary theorems that the authors of the standard textbooks have shown the least precision and rigor. We must not hold it against them; they were obeying a necessity. Beginners are not prepared for true mathematical rigor; they would see in it only empty and tedious subtleties; it would be a waste of time to try to make them more demanding too soon; they must travel again, quickly but without skipping any stages, the road that the founders of the science traveled slowly.

1.1.23 Why is so long a preparation needed to get used to this perfect rigor, which, it seems, ought to come naturally to every

sound mind? That is a logical and psychological problem well worth pondering.

1.1.24 But we shall not dwell on it; it lies outside our subject. All I want to retain is that, if we are not to miss our goal, we must redo the proofs of the most elementary theorems and give them not the crude form in which they are left so as not to weary beginners, but a form that can satisfy a practiced mathematician.

1.1.25 Definition of addition.—Suppose we have already defined the operation $x + 1$, which consists in adding the number 1 to a given number x.

1.1.26 This definition, whatever it may be, will play no further part in the rest of the reasoning.

1.1.27 The task now is to define the operation $x + a$, which consists in adding the number a to a given number x.

1.1.28 Suppose we have defined the operation $x + (a - 1)$; then the operation $x + a$ will be defined by the equality: (1) $x + a = [x + (a - 1)] + 1$.

1.1.29 We shall thus know what $x + a$ is when we know what $x + (a - 1)$ is; and since I assumed at the outset that we knew what $x + 1$ is, we can define successively and "by recurrence" the operations $x + 2$, $x + 3$, etc.[2]

1.1.30 This definition deserves a moment's attention; it is of a peculiar nature that already distinguishes it from a purely logical definition: equality (1) in fact contains an infinity of distinct definitions, each of which has meaning only when we know the one before it.

1.1.31 Properties of addition.—*Associativity.*—I say that $a + (b + c) = (a + b) + c$.

1.1.32 Indeed, the theorem is true for $c = 1$; it then reads $a + (b + 1) = (a + b) + 1$, which, apart from the difference in notation, is nothing other than equality (1), by which I have just defined addition.

1.1.33 Suppose the theorem is true for $c = \gamma$; I say that it will be true for $c = \gamma + 1$. Indeed, let $(a + b) + \gamma = a + (b + \gamma)$; we shall

deduce from this successively: $[(a + b) + \gamma] + 1 = [a + (b + \gamma)] + 1$, or, by virtue of definition (1), $(a + b) + (\gamma + 1) = a + (b + \gamma + 1) = a + [b + (\gamma + 1)]$,

1.1.34 Since it is true for $c = 1$, we would thus see successively that it is true for $c = 2$, for $c = 3$, etc.

1.1.35 *Commutativity.*—1. I say that $a + 1 = 1 + a$.

1.1.36 The theorem is obviously true for $a = 1$; we could *verify* by purely analytic reasoning that if it is true for $a = \gamma$, it will be true for $a = \gamma + 1$; now it is true for $a = 1$, so it will be true for $a = 2$, for $a = 3$, etc.; this is what we express by saying that the proposition stated is proved by recurrence.

1.1.37 2. I say that $a + b = b + a$.

1.1.38 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 $b = \beta + 1$.

1.1.39 The proposition is therefore established by recurrence.

1.1.40 Definition of multiplication.—We shall define multiplication by the equalities $a \times 1 = a$. (2) $a \times b = [a \times (b - 1)] + a$.

1.1.41 Equality (2), like equality (1), contains an infinity of definitions; once $a \times 1$ has been defined, it allows us to define successively: $a \times 2$, $a \times 3$, etc.

1.1.42 Properties of multiplication.—*Distributivity.*—I say that $(a + b) \times c = (a \times c) + (b \times c)$.

1.1.43 We verify analytically that the equality is true for $c = 1$, and then that if the theorem is true for $c = \gamma$, it will be true for $c = \gamma + 1$.

1.1.44 The proposition is again proved by recurrence.

1.1.45 *Commutativity.*—1. I say that $a \times 1 = 1 \times a$. The theorem is obvious for $a = 1$.

1.1.46 We verify analytically that if it is true for $a = \alpha$, it will be true for $a = \alpha + 1$.

1.1.47 2. I say that $a \times b = b \times a$. The theorem has just been proved for $b = 1$. We could verify analytically that if it is true for $b = \beta$, it will be true for $b = \beta + 1$.

2 Poincaré's "recurrence" is what mathematicians now call mathematical induction; a definition "by recurrence" is what is now called a recursive definition.

PROOF BY RECURRENCE

Each proof used one procedure: true for 1, and true for n if true for n − 1.

1.1.48 I shall stop this monotonous series of arguments here. But its very monotony has brought out more clearly the procedure, which is uniform and which we meet again at every step.

1.1.49 This procedure is proof by recurrence. We first establish a theorem for $n = 1$; we then show that if it is true of $n - 1$, it is true of n, and we conclude from this that it is true for all whole numbers.

1.1.50 We have just seen how it can be used to prove the rules of addition and multiplication, that is, the rules of algebraic calculation; this calculus is an instrument of transformation that lends itself to many more varied combinations than the simple syllogism; but it is still a purely analytic instrument, incapable of teaching us anything new. If mathematics had no other, its development would therefore be halted at once; but it has recourse once again to the same procedure, that is, to reasoning by recurrence, and it can continue its forward march.

1.1.51 At every step, if we look closely, we meet this mode of reasoning again, either in the simple form we have just given it or in a more or less modified form.

AN INFINITY OF SYLLOGISMS IN ONE FORMULA

Recurrence condenses endless syllogisms into one rule, letting the mind pass from the finite to the infinite.

1.1.52 The essential feature of reasoning by recurrence is that it contains, condensed so to speak into a single formula, an infinity of syllogisms.

1.1.53 So that this may be better grasped, I shall state these syllogisms one after another; they are, if I may be allowed the expression, arranged in a cascade.

1.1.54 They are, of course, hypothetical syllogisms.

1.1.55 The theorem is true of the number 1. Now if it is true of 1, it is true of 2. Therefore it is true of 2. Now if it is true of 2, it is true of 3. Therefore it is true of 3, and so on.

1.1.56 We see that the conclusion of each syllogism serves as the minor premise of the next.

1.1.57 Moreover, the major premises of all our syllogisms can be reduced to a single formula.

1.1.58 If the theorem is true of $n - 1$, it is true of n.

1.1.59 We see, then, that in reasoning by recurrence we confine ourselves to stating the minor premise of the first syllogism, and the general formula that contains all the major premises as particular cases.

1.1.60 This never-ending series of syllogisms is thus reduced to a sentence of a few lines.

1.1.61 It is now easy to understand why every particular consequence of a theorem can, as I explained above, be verified by purely analytic procedures.

1.1.62 If, instead of showing that our theorem is true of all numbers, we want only to show that it is true of the number 6, for example, it will be enough for us to establish the first 5 syllogisms of our cascade; we would need 9 if we wanted to prove the theorem for the number 10; we would need still more for a larger number; but however large that number might be, we would always reach it in the end, and analytic verification would be possible.

1.1.63 And yet, however far we went in this way, we would never rise to the general theorem, applicable to all numbers, which alone can be an object of science. To reach it would take an infinity of syllogisms; it would mean crossing an abyss that the patience of the analyst, confined to the resources of formal logic alone, will never succeed in filling.

1.1.64 I asked at the beginning why we could not conceive of a mind powerful enough to perceive the whole body of mathematical truths at a single glance.

1.1.65 The answer is easy now. A chess player can work out four or five moves ahead; but however extraordinary we suppose

him to be, he will never plan more than a finite number of them. If he applies his faculties to arithmetic, he will not be able to perceive its general truths by a single direct intuition; to reach the smallest theorem, he cannot dispense with the help of reasoning by recurrence, because it is an instrument that lets us pass from the finite to the infinite.

1.1.66 This instrument is always useful, since, by letting us leap over as many stages as we wish at a single bound, it spares us long, tedious and monotonous verifications that would quickly become impracticable. But it becomes indispensable as soon as we aim at the general theorem, to which analytic verification would bring us ever closer without ever allowing us to reach it.

1.1.67 In this field of arithmetic we might think ourselves very far from infinitesimal analysis; and yet, as we have just seen, the idea of the mathematical infinite already plays a dominant role, and without it there would be no science, because there would be nothing general.

A SYNTHETIC A PRIORI JUDGMENT

The rule of recurrence comes neither from logic nor from experience; it expresses a power of the mind.

1.1.68 The judgment on which reasoning by recurrence rests can be put in other forms; we can say, for example, that in an infinite collection of different whole numbers there is always one that is smaller than all the others.

1.1.69 One can easily pass from one statement to the other and so give oneself the illusion of having proved the legitimacy of reasoning by recurrence. But one will always be brought up short; one will always arrive at an unprovable axiom that is, at bottom, only the proposition to be proved translated into another language.

1.1.70 We cannot, then, escape the conclusion that the rule of reasoning by recurrence is irreducible to the principle of contradiction.

1.1.71 Nor can this rule come to us from experience; what experience could teach us is that the rule is true for the first ten or the first hundred numbers, for example; it cannot reach the indefinite series of numbers, but only a portion of that series, longer or shorter but always limited.

1.1.72 Now, if that were all that was at stake, the principle of contradiction would suffice; it would always allow us to develop as many syllogisms as we wished. It is only when an infinity of them must be enclosed in a single formula, it is only before the infinite, that this principle fails; and it is there too that experience becomes powerless. This rule, inaccessible both to analytic proof and to experience, is the true type of the synthetic *a priori* judgment. On the other hand, we cannot think of seeing it as a convention, as with some of the postulates of geometry.

1.1.73 Why, then, does this judgment force itself on us with irresistible self-evidence? Because it is only the affirmation of the power of the mind, which knows itself capable of conceiving the indefinite repetition of one and the same act once that act is possible at all. The mind has a direct intuition of this power, and experience can only be for it an occasion to use it, and thereby to become aware of it.

1.1.74 But, it will be said, if raw experience cannot justify reasoning by recurrence, is the same true of experience aided by induction? We see successively that a theorem is true of the number 1, of the number 2, of the number 3, and so on; *the law is evident*, we say, and it is evident in the same way as any physical law based on observations that are very numerous but limited in number.

1.1.75 It cannot be denied that there is a striking analogy here with the usual procedures of induction. But an essential difference remains. Induction, as applied to the physical sciences, is always uncertain, because it rests on the belief in a general order of the Universe, an order that is outside us. Mathematical induction, that is, proof by recurrence, on the contrary forces itself on us necessarily, because it is only the affirmation of a property of the mind itself.

FROM THE PARTICULAR TO THE GENERAL

Mathematical construction is fruitful only because recurrence lets mathematics rise to general theorems.

1.1.76 Mathematicians, as I said above, always strive to *generalize* the propositions they have obtained; and, to look no further for an example, we proved just now the equality: $a + 1 = 1 + a$, and then used it to establish the equality $a + b = b + a$, which is manifestly more general.

1.1.77 Mathematics can therefore, like the other sciences, proceed from the particular to the general.

1.1.78 Here is a fact that would have seemed incomprehensible to us at the start of this study, but that no longer holds any mystery for us, now that we have observed the analogies between proof by recurrence and ordinary induction.

1.1.79 No doubt recurrent mathematical reasoning and inductive physical reasoning rest on different foundations; but they move in parallel and in the same direction, that is, from the particular to the general.

1.1.80 Let us look at the matter a little more closely.

1.1.81 To prove the equality: (1) $a + 2 = 2 + a$, it is enough for us to apply the rule $a + 1 = 1 + a$ twice and write: (2) $a + 2 = a + 1 + 1 = 1 + a + 1 = 1 + 1 + a = 2 + a$.

1.1.82 Equality (2), deduced in this purely analytic way from equality (1), is not, however, simply a particular case of it: it is something else.

1.1.83 We cannot, then, even say that in the truly analytic and deductive part of mathematical reasoning we proceed from the general to the particular, in the ordinary sense of the word.

1.1.84 The two sides of equality (2) are simply more complicated combinations than the two sides of equality (1), and analysis serves only to separate the elements that enter into these combinations and to study their relations.

1.1.85 Mathematicians therefore proceed "by construction"; they "construct" more and more complicated combinations.

Then, returning by analysis from these combinations, these wholes so to speak, to their primitive elements, they perceive the relations between these elements and deduce from them the relations between the wholes themselves.

1.1.86 This is a purely analytic procedure, but it is not, for all that, a procedure from the general to the particular, for the wholes obviously cannot be regarded as more particular than their elements.

1.1.87 Great importance has been attached, and rightly so, to this procedure of "construction," and some have wished to see in it the necessary and sufficient condition for the progress of the exact sciences.

1.1.88 Necessary, no doubt; but sufficient, no.

1.1.89 For a construction to be useful, for it not to be a pointless strain on the mind, for it to serve as a stepping stone for anyone who wants to climb higher, it must first possess a kind of unity that lets us see in it something other than the juxtaposition of its elements.

1.1.90 Or, more precisely, there must be some advantage in considering the construction rather than its elements themselves.

1.1.91 What can this advantage be?

1.1.92 Why reason, for example, about a polygon, which can always be broken down into triangles, and not about the elementary triangles?

1.1.93 Because there are properties that can be proved for polygons with any number of sides and then applied immediately to any particular polygon.

1.1.94 Most often, on the other hand, they could be recovered only at the cost of the longest efforts by studying the relations of the elementary triangles directly. Knowing the general theorem spares us these efforts.

1.1.95 A construction, then, becomes interesting only when it can be placed alongside other similar constructions, forming species of a single genus.

1.1.96 If the quadrilateral is something other than the juxtaposition of two triangles, it is because it belongs to the genus polygon.

1.1.97 And even then, we must be able to prove the properties of the genus without being forced to establish them one by one for each of the species.

1.1.98 To achieve this, we must necessarily climb back up from the particular to the general, ascending one or more rungs.

1.1.99 The analytic procedure "by construction" does not oblige us to come down, but it leaves us at the same level.

1.1.100 We can rise only by mathematical induction, which alone can teach us something new. Without the help of this induction, which differs in certain respects from physical induction but is as fruitful as it is, construction would be powerless to create science.

1.1.101 Let us note, in closing, that this induction is possible only if one and the same operation can be repeated indefinitely. That is why the theory of chess can never become a science, since the different moves of a single game do not resemble one another.

CHAPTER II: MATHEMATICAL MAGNITUDE AND EXPERIENCE

What analysts mean by the continuum, and how Dedekind defines the irrational numbers.

1.2.1 If we want to know what mathematicians mean by a continuum, it is not geometry we should ask. The geometer always tries, more or less, to picture the figures he is studying, but his pictures are only instruments to him; he does geometry with extension as he does it with chalk; so we must take care not to attach too much importance to accidents that often matter no more than the whiteness of the chalk.

1.2.2 The pure analyst need not fear this pitfall. He has freed mathematical science from all foreign elements, and he can answer our question: What exactly is this continuum about which mathematicians reason? Many of them, who know how to

reflect on their art, have already done so: Tannery, for example, in his *Introduction à la théorie des Fonctions d'une variable*.[3]

1.2.3 Let us start from the scale of whole numbers; between two consecutive steps let us insert one or more intermediate steps, then between these new steps still others, and so on indefinitely. We shall thus have an unlimited number of terms; these will be the numbers called fractional, rational or commensurable. But even that is not enough; between these terms, which are already infinite in number, we must insert still others, which are called irrational or incommensurable.

1.2.4 Before going further, let us make a first remark. The continuum so conceived is nothing but a collection of individuals arranged in a certain order, infinite in number, it is true, but *external* to one another. This is not the ordinary conception, in which we suppose between the elements of the continuum a sort of intimate bond that makes them a whole, in which the point does not exist before the line, but the line before the point. Of the famous formula "the continuum is unity in multiplicity," only the multiplicity remains; the unity has disappeared. The analysts are nonetheless right to define their continuum as they do, since it is always this one they reason about, ever since they have prided themselves on rigor. But it is enough to warn us that the true mathematical continuum is something quite different from that of the physicists and that of the metaphysicians.

1.2.5 It may also be said that mathematicians who are content with this definition are the dupes of words; that they ought to say precisely what each of these intermediate steps is, explain how they are to be inserted, and prove that it is possible to do so. But that would be wrong; the only property of these steps that enters into their reasoning[4] is that of coming before or after certain other steps; so it alone must also enter into the definition.

3 Jules Tannery (1848–1910), French mathematician; his *Introduction to the Theory of Functions of One Variable* appeared in 1886.

4 POINCARÉ'S NOTE. Together with those contained in the special conventions that serve to define addition, which we shall discuss later.

1.2.6 Thus there is no need to worry about how the intermediate terms are to be inserted; and on the other hand, no one will doubt that this operation is possible, unless he forgets that this last word, in the language of geometers, simply means free from contradiction.

1.2.7 Our definition, however, is not yet complete, and I return to it after this overlong digression.

1.2.8 Definition of incommensurable numbers.—The mathematicians of the Berlin school, Kronecker in particular, have set out to construct this continuous scale of fractional and irrational numbers using no other material than the whole number. On this view, the mathematical continuum would be a pure creation of the mind, in which experience would have no part.[5]

1.2.9 Since the notion of rational number did not seem to them to present any difficulty, they have chiefly tried to define the incommensurable number. But before reproducing their definition here, I must make an observation, to forestall the astonishment it would not fail to arouse in readers little acquainted with the habits of mathematicians.

1.2.10 Mathematicians do not study objects but relations between objects; it is therefore a matter of indifference to them to replace these objects by others, provided the relations do not change. The matter does not concern them; only the form interests them.

1.2.11 Unless we remembered this, we could not understand how Dedekind can give the name *incommensurable number* to a mere symbol, that is, to something very different from the idea we think we have of a quantity, which ought to be measurable and almost tangible.[6]

1.2.12 Here, now, is Dedekind's definition:

1.2.13 The commensurable numbers can be divided into two classes in infinitely many ways, subject to the condition that any

5 Leopold Kronecker (1823–1891), German mathematician who held that all of mathematics should be built up from the whole numbers.

6 Richard Dedekind (1831–1916), German mathematician who in 1872 defined the irrational numbers by means of "cuts" in the rational numbers.

number of the first class is greater than any number of the second class.

1.2.14 It may happen that among the numbers of the first class there is one smaller than all the others. If, for example, we put into the first class all the numbers greater than 2, together with 2 itself, and into the second class all the numbers less than 2, it is clear that 2 will be the smallest of all the numbers of the first class. The number 2 may then be chosen as the symbol of this division.

1.2.15 It may happen, on the contrary, that among the numbers of the second class there is one greater than all the others. This is the case, for example, if the first class includes all the numbers greater than 2, and the second all the numbers less than 2, together with 2 itself. Here again the number 2 may be chosen as the symbol of this division.

1.2.16 But it may equally happen that we can find neither a number in the first class smaller than all the others, nor a number in the second class greater than all the others. Suppose, for example, that we put into the first class all the commensurable numbers whose square is greater than 2, and into the second all those whose square is less than 2. We know that there is none whose square is exactly equal to 2. There will obviously be no number in the first class smaller than all the others, for however near to 2 the square of a number may be, we can always find a commensurable number whose square is nearer still to 2.

1.2.17 On Dedekind's view, the incommensurable number $\sqrt{2}$ is nothing but the symbol of this particular way of dividing the commensurable numbers. And so to each way of dividing them there corresponds a number, commensurable or not, which serves as its symbol.

1.2.18 But to be content with this would be to forget too readily where these symbols came from. It remains to explain how we came to attribute to them a kind of concrete existence. Besides, does not the difficulty begin with the fractional numbers themselves? Would we have the notion of these numbers if we did not

already know a matter that we conceive as infinitely divisible—that is, as a continuum?

THE PHYSICAL CONTINUUM

Our sensations give A = B, B = C, A < C, a contradiction that the mathematical continuum removes.

1.2.19 The physical continuum.—We are then led to ask whether the notion of the mathematical continuum is not simply drawn from experience. If it were, the raw data of experience, which are our sensations, would be capable of measurement. We might be tempted to believe that this is indeed so, since in recent times there have been attempts to measure them. A law has even been formulated, known as Fechner's law, according to which sensation is proportional to the logarithm of the stimulus.[7]

1.2.20 But if we examine closely the experiments by which people have sought to establish this law, we shall be led to the opposite conclusion. It has been observed, for example, that a weight A of 10 grams and a weight B of 11 grams produced identical sensations. The weight B could not be distinguished from a weight C of 12 grams either, yet the weight A was easily distinguished from the weight C. The raw results of experience can therefore be expressed by the following relations: A = B, B = C, A < C, which may be regarded as the formula of the physical continuum.

1.2.21 Here there is an intolerable disagreement with the principle of contradiction, and it is the need to put an end to it that has forced us to invent the mathematical continuum.

1.2.22 We are therefore forced to conclude that this notion was created wholesale by the mind, but that it was experience that provided the occasion.

1.2.23 We cannot believe that two quantities equal to the same third quantity are not equal to each other. And so we are led to

7 Gustav Theodor Fechner (1801–1887), German physicist and philosopher, a founder of psychophysics.

suppose that A is different from B, and B from C, but that the imperfection of our senses had not allowed us to tell them apart.

1.2.24 Creation of the mathematical continuum.—*First stage.*—So far, to account for the facts, it might be enough to insert between A and B a small number of terms that would remain discrete. What happens now if we have recourse to some instrument to make up for the weakness of our senses—if, for example, we use a microscope? Terms that we could not distinguish from each other, as was the case a moment ago with A and B, now appear to us as distinct. But between A and B, now distinct, a new term D will be inserted, which we shall be able to distinguish neither from A nor from B. However refined the methods we employ, the raw results of our experience will always present the characteristics of the physical continuum, with the contradiction inherent in it.

1.2.25 We shall escape it only by constantly inserting new terms between the terms already distinguished, and this operation will have to be continued indefinitely. We could conceive of its having to stop only if we pictured some instrument powerful enough to break the physical continuum down into discrete elements, as the telescope resolves the Milky Way into stars. But we cannot imagine this. For it is always with our senses that we use our instruments: it is with the eye that we observe the image enlarged by the microscope. This image must therefore always retain the characteristics of visual sensation, and therefore those of the physical continuum.

1.2.26 Nothing distinguishes a length observed directly from half that length doubled by the microscope. The whole is homogeneous with the part. This is a new contradiction—or rather it would be one if the number of terms were supposed finite. For it is clear that the part, containing fewer terms than the whole, could not be similar to the whole.

1.2.27 The contradiction ceases as soon as the number of terms is regarded as infinite. Nothing prevents us, for example, from regarding the set of whole numbers as similar to the set of even

numbers, which is nevertheless only a part of it. Indeed, to each whole number there corresponds an even number, its double.

1.2.28 But it is not only to escape this contradiction contained in the empirical data that the mind is led to create the concept of a continuum formed of an indefinite number of terms.

1.2.29 It is just as with the sequence of whole numbers. We have the faculty of conceiving that a unit can be added to a collection of units. It is thanks to experience that we have occasion to exercise this faculty and become aware of it. But from that moment we feel that our power has no limit and that we could count indefinitely, although we have never had to count more than a finite number of objects.

1.2.30 In the same way, as soon as we have been led to insert means between two consecutive terms of a series, we feel that this operation can be continued beyond any limit, and that there is, so to speak, no intrinsic reason to stop.

1.2.31 To shorten the language, let me call a mathematical continuum of the first order any set of terms formed according to the same law as the scale of commensurable numbers. If we then insert new steps into it according to the law by which incommensurable numbers are formed, we shall obtain what we shall call a continuum of the second order.

1.2.32 *Second stage.*—So far we have taken only the first step: we have explained the origin of continua of the first order. But we must now see why they still could not suffice, and why it was necessary to invent incommensurable numbers.

1.2.33 If we try to imagine a line, we can do so only with the characteristics of the physical continuum; that is, we can picture it only with a certain width. Two lines will then appear to us as two narrow strips. If we are content with this crude image, it is evident that if the two lines cross, they will have a part in common.

1.2.34 But the pure geometer makes a further effort. Without entirely giving up the help of his senses, he wants to arrive at the concept of a line without width, of a point without extension. He can do so only by regarding the line as the limit toward

which a strip tends as it grows thinner and thinner, and the point as the limit toward which an area tends as it grows smaller and smaller. And then our two strips, however narrow they may be, will always have a common area, which will be smaller the narrower they are, and whose limit will be what the pure geometer calls a point.

1.2.35 That is why we say that two lines that cross have a point in common, and this truth seems intuitive.

1.2.36 But it would imply a contradiction if lines were conceived as continua of the first order—that is, if the lines drawn by the geometer were to contain only points whose coordinates are rational numbers. The contradiction would be manifest as soon as one asserted, for example, the existence of straight lines and circles.

1.2.37 It is clear, in fact, that if only the points with commensurable coordinates were regarded as real, the circle inscribed in a square and the diagonal of that square would not intersect, since the coordinates of the point of intersection are incommensurable.

1.2.38 Even that would not be enough, for in this way we would obtain only certain incommensurable numbers, and not all of them.

1.2.39 But let us picture a straight line divided into two half-lines. Each of these half-lines will appear to our imagination as a strip of a certain width. These strips will, moreover, overlap, since there must be no gap between them. The common part will appear to us as a point which will always remain, however thin we try to imagine our strips becoming. So we shall accept as an intuitive truth that if a straight line is divided into two half-lines, the common boundary of these two lines is a point. Here we recognize Kronecker's conception, in which an incommensurable number was regarded as the common boundary of two classes of rational numbers.

1.2.40 Such is the origin of the continuum of the second order, which is the mathematical continuum properly so called.

1.2.41 *Summary.*—In short, the mind has the faculty of creating symbols, and it is in this way that it has constructed the mathematical continuum, which is only a particular system of symbols. Its power is limited only by the need to avoid all contradiction. But the mind uses this power only if experience gives it a reason to.

1.2.42 In the case before us, this reason was the notion of the physical continuum, drawn from the raw data of the senses. But this notion leads to a series of contradictions from which we must free ourselves one after another. In this way we are compelled to imagine a more and more complicated system of symbols. The one at which we finally stop is not only free of internal contradiction—that was already true at every stage we have passed through. It is also not in contradiction with various so-called intuitive propositions, which are drawn from more or less elaborated empirical notions.

MEASURABLE MAGNITUDE

To measure a continuum we need a new convention fixing when two intervals are equal.

1.2.43 Measurable magnitude.—The magnitudes we have studied so far are not *measurable*. We can indeed say whether one of these magnitudes is greater than another, but not whether it is twice or three times as great.

1.2.44 So far, in fact, I have been concerned only with the order in which our terms are arranged. But that is not enough for most applications. We must learn to compare the interval separating any two terms. Only on this condition does the continuum become a measurable magnitude, to which the operations of arithmetic can be applied.

1.2.45 This can be done only with the help of a new and special *convention*. We shall *agree* that in such and such a case the interval between the terms A and B is equal to the interval separating C and D. For example, at the beginning of our work we started from the scale of whole numbers and supposed that *n* interme-

diate steps were inserted between two consecutive steps. Well, these new steps will be regarded, by convention, as equidistant.

1.2.46 This is one way of defining the addition of two magnitudes; for if the interval AB is by definition equal to the interval CD, the interval AD will by definition be the sum of the intervals AB and AC.

1.2.47 This definition is arbitrary to a very large extent. Yet it is not entirely so. It is subject to certain conditions—for example, to the commutative and associative laws of addition. But provided the definition chosen satisfies these rules, the choice does not matter, and there is no need to specify it.

1.2.48 Various remarks.—We may ask ourselves several important questions:

1.2.49 1. Is the creative power of the mind exhausted by the creation of the mathematical continuum?

1.2.50 No: the work of Du Bois-Reymond demonstrates this in a striking way.[8]

1.2.51 We know that mathematicians distinguish infinitesimals of different orders, and that those of the second order are infinitely small not only absolutely but also relative to those of the first order. It is not difficult to imagine infinitesimals of fractional or even irrational order. And so we come back to that scale of the mathematical continuum which was the subject of the preceding pages.

1.2.52 But there is more. There exist infinitesimals that are infinitely small relative to those of the first order and, on the contrary, infinitely large relative to those of order $1 + \varepsilon$, however small ε may be. Here, then, are new terms inserted into our series. If I may return to the language I was using a moment ago, which is quite convenient although not sanctioned by usage, I shall say that in this way a kind of continuum of the third order has been created.

1.2.53 It would be easy to go further, but that would be an idle game of the mind. We would be imagining only symbols with no

8 Paul du Bois-Reymond (1831–1889), German mathematician who studied the comparative rates of growth of functions.

possible application, and no one will think of doing so. The continuum of the third order to which the consideration of the various orders of infinitesimals leads is itself of too little use to have earned full acceptance, and mathematicians regard it only as a mere curiosity. The mind uses its creative faculty only when experience imposes the necessity upon it.

1.2.54 2. Once in possession of the concept of the mathematical continuum, are we safe from contradictions like those that gave rise to it?

1.2.55 No, and I shall give an example.

1.2.56 It takes a great deal of learning not to regard it as obvious that every curve has a tangent. And indeed, if we picture this curve and a straight line as two narrow strips, we can always arrange them so that they have a part in common without crossing. If we then imagine the width of these two strips decreasing indefinitely, this common part can always remain. In the limit, so to speak, the two lines will have a point in common without crossing—that is, they will touch.

1.2.57 The geometer who reasoned in this way, consciously or not, would be doing nothing other than what we did above to show that two lines that cross have a point in common, and his intuition might seem just as legitimate.

1.2.58 Yet it would deceive him. One can prove that there are curves that have no tangent, if the curve is defined as an analytic continuum of the second order.

1.2.59 No doubt some device like those we studied above would have made it possible to remove the contradiction. But as it arises only in very exceptional cases, no one has bothered about it. Instead of trying to reconcile intuition with analysis, people have been content to sacrifice one of the two; and since analysis must remain flawless, it is intuition that has been declared in the wrong.

CONTINUA OF SEVERAL DIMENSIONS

Dimension is defined by the kinds of cuts needed to divide a physical continuum.

1.2.60 The physical continuum of several dimensions.—Above, I studied the physical continuum as it emerges from the immediate data of our senses, 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, B = C, A < C.

1.2.61 Let us now see how this notion was generalized, and how the concept of continua of several dimensions could arise from it.

1.2.62 Consider any two sets of sensations. Either we shall be able to tell them apart or we shall not, just as in Fechner's experiments a weight of 10 grams could be distinguished from a weight of 12 grams, but not from one of 11 grams. I need nothing more to construct the continuum of several dimensions.

1.2.63 Let us call one of these sets of sensations an *element*. It will be something analogous to the mathematicians' *point*, yet it will not be quite the same thing. We cannot say that our element is without extension, since we cannot distinguish it from neighboring elements, and it is thus surrounded by a sort of fog. If I may use an astronomical comparison, our "elements" would be like nebulae, while mathematical points would be like stars.

1.2.64 This being granted, a system of elements will form a *continuum* if we can pass from any one of them to any other by a series of consecutive elements, linked in such a way that each cannot be distinguished from the one before. This *chain* is to the mathematician's *line* what an isolated *element* was to the point.

1.2.65 Before going further, I must explain what a *cut* is. Consider a continuum C and remove from it some of its elements, which for the moment we shall regard as no longer belonging to that continuum. The set of elements so removed will be called a cut. It may happen that, by means of this cut, C is *subdivided* into

several distinct continua, the set of remaining elements ceasing to form a single continuum.

1.2.66 There will then be two elements of C, A and B, which must be regarded as belonging to two distinct continua. This will be recognized by the fact that it will be impossible to find a *chain* of consecutive elements of C starting from A and going to B, each element being indistinguishable from the one before, *unless one of the elements of this chain is indistinguishable from one of the elements of the cut, and must therefore be excluded*.

1.2.67 It may happen, on the contrary, that the cut made is not enough to subdivide the continuum C. To classify physical continua, we shall examine precisely which cuts must be made in them in order to subdivide them.

1.2.68 If a physical continuum C can be subdivided by a cut consisting of a finite number of elements, all distinguishable from one another (and therefore forming neither one continuum nor several), we shall say that C is a continuum *of one dimension*.

1.2.69 If, on the other hand, C can be subdivided only by cuts that are themselves continua, we shall say that C has several dimensions. If cuts that are continua of one dimension suffice, we shall say that C has two dimensions; if cuts of two dimensions suffice, we shall say that C has three dimensions; and so on.

1.2.70 Thus the notion of the physical continuum of several dimensions is defined, thanks to the very simple fact that two sets of sensations may be distinguishable or indistinguishable.

1.2.71 The mathematical continuum of several dimensions.— The notion of the mathematical continuum of n dimensions arose from it quite naturally, by a process just like the one we studied at the beginning of this chapter. A point of such a continuum appears to us, as we know, as defined by a system of n distinct magnitudes called its coordinates.

1.2.72 It is not always necessary for these magnitudes to be measurable. There is, for example, a branch of geometry in which the measurement of these magnitudes is disregarded. In it one is concerned only with knowing, say, whether on a curve

ABC the point B lies between the points A and C—and not whether the arc AB is equal to the arc BC or twice as great. This is what is called *Analysis Situs*.[9]

1.2.73 It is a whole body of doctrine that has attracted the attention of the greatest mathematicians, and in which we see a series of remarkable theorems arise one from another. What distinguishes these theorems from those of ordinary geometry is that they are purely qualitative. They would remain true if the figures were copied by a clumsy draftsman who grossly distorted their proportions and replaced the straight lines by strokes that were more or less curved.

1.2.74 It was when people tried to introduce measurement into the continuum we have just defined that this continuum became space and geometry was born. But I reserve this study for the second part.

9 Latin for "analysis of position"; the branch of mathematics now called topology.

Part Two: Space

L'espace
Non-Euclidean geometries, the space of geometry against the space of perception, and why the axioms of geometry are neither synthetic a priori truths nor experimental facts but conventions (Chapters III–V).

CHAPITRE III. LES GÉOMÉTRIES NON EUCLIDIENNES.

2.3.1 Every conclusion presupposes premises. These premises themselves are either self-evident and need no proof, or can be established only by relying on other propositions. Since we cannot go back in this way indefinitely, every deductive science, and geometry in particular, must rest on a certain number of unprovable axioms. All treatises on geometry therefore begin by stating these axioms. But a distinction must be made among them. Some, such as this one—"two quantities equal to the same third quantity are equal to each other"—are not propositions of geometry but propositions of analysis. I regard them as analytic *a priori* judgments, and I shall not deal with them.

2.3.2 But I must dwell on other axioms that are peculiar to geometry. Most treatises state three of them explicitly:

2.3.3 1. Through two points only one straight line can pass;

2.3.4 2. The straight line is the shortest path from one point to another.

2.3.5 3. Through a point only one parallel to a given straight line can be drawn.

2.3.6 Although it is generally thought unnecessary to prove the second of these axioms, it could be deduced from the other two

and from those—far more numerous—that are implicitly accepted without being stated, as I shall explain later.

2.3.7 For a long time people tried in vain to prove the third axiom as well, known as *Euclid's postulate*. The amount of effort spent in this chimerical hope is truly unimaginable. At last, early in the nineteenth century and at about the same time, two scientists, a Russian and a Hungarian, Lobachevsky and Bolyai, established irrefutably that this proof is impossible. They have more or less rid us of the inventors of geometries without the postulate; since then the Academy of Sciences receives hardly more than one or two new proofs a year.[1]

2.3.8 The question was not exhausted. It soon took a great step forward with the publication of Riemann's famous memoir entitled *Ueber die Hypothesen welche der Geometrie zum Grunde liegen*. This short work inspired most of the recent studies I shall discuss later, among which those of Beltrami and Helmholtz deserve mention.[2]

LOBACHEVSKY'S GEOMETRY

Denying Euclid's postulate yields a consistent geometry with strange but coherent theorems.

2.3.9 Lobachevsky's geometry.—If it were possible to deduce Euclid's postulate from the other axioms, then clearly, by denying the postulate while accepting the other axioms, we would be led to contradictory consequences. It would therefore be impossible to base a coherent geometry on such premises.

2.3.10 Now that is precisely what Lobachevsky did. He assumes at the outset that:

1 Euclid's postulate is the parallel postulate. János Bolyai (1802–1860), Hungarian mathematician, published his non-Euclidean geometry in 1832, independently of Lobachevsky.

2 Bernhard Riemann (1826–1866), German mathematician; the memoir, "On the Hypotheses Which Lie at the Foundations of Geometry," was a lecture of 1854. Eugenio Beltrami (1835–1900) was an Italian mathematician; Hermann von Helmholtz (1821–1894) a German physicist and physiologist.

2.3.11 *Through a point several parallels to a given straight line can be drawn;*

2.3.12 And otherwise he keeps all of Euclid's other axioms. From these hypotheses he deduces a series of theorems among which it is impossible to find any contradiction, and he constructs a geometry whose flawless logic is in no way inferior to that of Euclidean geometry.

2.3.13 The theorems are, of course, very different from those we are used to, and at first they are somewhat disconcerting.

2.3.14 Thus the sum of the angles of a triangle is always less than two right angles, and the difference between this sum and two right angles is proportional to the area of the triangle.

2.3.15 It is impossible to construct a figure similar to a given figure but of different dimensions.

2.3.16 If a circle is divided into n equal parts and tangents are drawn at the points of division, these n tangents will form a polygon if the radius of the circle is small enough; but if the radius is large enough, they will not meet.

2.3.17 There is no need to multiply these examples. Lobachevsky's propositions no longer bear any relation to Euclid's, but they are no less logically connected with one another.

RIEMANN'S GEOMETRY

Imaginary flat beings on a sphere illustrate a space that is finite yet unbounded.

2.3.18 Riemann's geometry.—Let us imagine a world inhabited solely by beings without thickness, and suppose that these "infinitely flat" animals all lie in one and the same plane and cannot leave it. Let us further assume that this world is far enough from the others to be free from their influence. While we are busy making hypotheses, it costs us nothing more to endow these beings with reason and to believe them capable of doing geometry. In that case they will certainly attribute to space only two dimensions.

2.3.19 But now suppose that these imaginary animals, while still without thickness, have the shape of a spherical figure rather than a plane one, and all lie on one and the same sphere, unable to move away from it. What geometry will they be able to construct? It is clear, first, that they will attribute to space only two dimensions. What will play for them the part of the straight line will be the shortest path from one point to another on the sphere—that is, an arc of a great circle. In a word, their geometry will be spherical geometry.

2.3.20 What they will call space will be this sphere, which they cannot leave and on which all the phenomena they can know about take place. Their space will therefore be *unbounded*, since on a sphere one can always go straight ahead without ever being stopped; and yet it will be *finite*. One will never find its end, but one will be able to go all the way around it.

2.3.21 Well then, Riemann's geometry is spherical geometry extended to three dimensions. To construct it, the German mathematician had to throw overboard not only Euclid's postulate but also the first axiom: *Through two points only one straight line can pass*.

2.3.22 On a sphere, through two given points only one great circle can *in general* pass (which, as we have just seen, would play the part of the straight line for our imaginary beings). But there is an exception: if the two given points are diametrically opposite, infinitely many great circles can be drawn through them.

2.3.23 Similarly, in Riemann's geometry (at least in one of its forms), through two points there will in general pass only one straight line; but there are exceptional cases in which infinitely many straight lines may pass through two points.

2.3.24 There is a kind of opposition between Riemann's geometry and Lobachevsky's.

2.3.25 Thus the sum of the angles of a triangle is:

2.3.26 Equal to two right angles in Euclid's geometry.

2.3.27 Less than two right angles in Lobachevsky's.

2.3.28 Greater than two right angles in Riemann's.

2.3.29 The number of parallels that can be drawn to a given straight line through a given point is equal to:

2.3.30 One in Euclid's geometry,

2.3.31 Zero in Riemann's,

2.3.32 Infinity in Lobachevsky's.

2.3.33 Let us add that Riemann's space is finite, though unbounded, in the sense given above to these two words.

2.3.34 Surfaces of constant curvature.—One objection, however, still remained possible. The theorems of Lobachevsky and Riemann present no contradiction. But however numerous the consequences these two geometers drew from their hypotheses, they must have stopped before exhausting them all, for their number would be infinite. Who can tell us, then, that if they had pushed their deductions further, they would not eventually have arrived at some contradiction?

2.3.35 This difficulty does not exist for Riemann's geometry, provided we confine ourselves to two dimensions. For two-dimensional Riemannian geometry, as we have seen, does not differ from spherical geometry, which is only a branch of ordinary geometry and is therefore beyond all dispute.

2.3.36 Beltrami, by likewise reducing Lobachevsky's two-dimensional geometry to a mere branch of ordinary geometry, has also refuted the objection as far as that geometry is concerned.

2.3.37 Here is how he did it. Consider any figure on a surface. Imagine that this figure is drawn on a flexible and inextensible cloth fitted to this surface, in such a way that when the cloth moves and is deformed, the various lines of this figure can change their shape without changing their length. In general, this flexible and inextensible figure cannot move without leaving the surface. But there are certain particular surfaces for which such a motion would be possible: these are the surfaces of constant curvature.

2.3.38 If we return to the comparison we made above and imagine beings without thickness living on one of these surfaces, they will regard as possible the motion of a figure all of whose lines keep a constant length. Such a motion would, on the con-

trary, seem absurd to animals without thickness living on a surface of variable curvature.

2.3.39 These surfaces of constant curvature are of two kinds:

2.3.40 Some are *of positive curvature*, and can be deformed so as to be fitted onto a sphere. The geometry of these surfaces therefore reduces to spherical geometry, which is Riemann's.

2.3.41 The others are *of negative curvature*. Beltrami showed that the geometry of these surfaces is none other than Lobachevsky's. The two-dimensional geometries of Riemann and Lobachevsky are thus connected to Euclidean geometry.

THE DICTIONARY

A translation table turns Lobachevsky's theorems into theorems of ordinary geometry, proving them consistent.

2.3.42 Interpretation of the non-Euclidean geometries.—Thus the objection vanishes as far as two-dimensional geometries are concerned.

2.3.43 It would be easy to extend Beltrami's reasoning to three-dimensional geometries. Minds not put off by four-dimensional space will see no difficulty in it, but they are few. I therefore prefer to proceed differently.

2.3.44 Let us consider a certain plane, which I shall call fundamental, and construct a kind of dictionary. We set in one-to-one correspondence a double series of terms written in two columns, just as in ordinary dictionaries the words of two languages that have the same meaning correspond: *Space*—Portion of space above the fundamental plane. *Plane*—Sphere cutting the fundamental plane orthogonally. *Straight line*—Circle cutting the fundamental plane orthogonally. *Sphere*—Sphere. *Circle*—Circle. *Angle*—Angle. *Distance between two points*—Logarithm of the cross-ratio of these two points and the intersections of the fun-

damental plane with a circle passing through these two points and cutting it orthogonally. Etc.[3]

2.3.45 Next let us take Lobachevsky'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. *We shall thus obtain theorems of ordinary geometry*.

2.3.46 For example, this theorem of Lobachevsky's—"the sum of the angles of a triangle is less than two right angles"—translates as follows: "If a curvilinear triangle has as its sides arcs of circles which, if extended, would cut the fundamental plane orthogonally, the sum of the angles of this curvilinear triangle will be less than two right angles." Thus, however far we push the consequences of Lobachevsky's hypotheses, we shall never be led to a contradiction. For if two of Lobachevsky's theorems were contradictory, the same would be true of the translations of these two theorems made with our dictionary. But these translations are theorems of ordinary geometry, and no one doubts that ordinary geometry is free from contradiction. Where does this certainty come from, and is it justified? That is a question I cannot deal with here, for it would require some development. So nothing remains of the objection I raised above.

2.3.47 That is not all. Lobachevsky's geometry, being open to a concrete interpretation, ceases to be an idle exercise in logic and can be put to use. I have no time here to speak of these applications, nor of the use Klein and I have made of them for the integration of linear equations.[4]

2.3.48 Moreover, this interpretation is not unique. Several dictionaries like the one above could be drawn up, all of which would make it possible, by a simple "translation," to turn Lobachevsky's theorems into theorems of ordinary geometry.

3 The cross-ratio (older "anharmonic ratio") is a quantity formed from four points on a line or circle. This dictionary describes what is now called Poincaré's half-space model.

4 Felix Klein (1849–1925), German mathematician; in the early 1880s he and Poincaré both worked on functions connected with linear differential equations.

THE IMPLICIT AXIOMS

Treatises silently assume that figures can be moved without deformation; Lie's theorem limits the possible geometries.

2.3.49 The implicit axioms.—Are the axioms explicitly stated in the treatises the only foundations of geometry? We can be sure they are not when we see that, after they have been abandoned one after another, a few propositions common to the theories of Euclid, Lobachevsky and Riemann are still left standing. These propositions must rest on certain premises that geometers accept without stating them. It is interesting to try to extract them from the classical proofs.

2.3.50 Stuart Mill claimed that every definition contains an axiom, since in defining we implicitly assert the existence of the object defined. This is going much too far. In mathematics a definition is rarely given without being followed by a proof of the existence of the object defined, and when this is omitted, it is generally because the reader can easily supply it. We must not forget that the word existence does not have the same meaning when it is a question of a mathematical entity as when a material object is concerned. A mathematical entity exists provided its definition implies no contradiction, either in itself or with the propositions previously accepted.[5]

2.3.51 But if Stuart Mill's observation cannot apply to all definitions, it is nonetheless correct for some of them. The plane is sometimes defined as follows:

2.3.52 The plane is a surface such that the straight line joining any two of its points lies wholly on that surface.

2.3.53 This definition obviously conceals a new axiom. It could, it is true, be changed, and that would be better; but then the axiom would have to be stated explicitly.

2.3.54 Other definitions may give rise to reflections that are no less important.

5 John Stuart Mill (1806–1873), English philosopher, author of *A System of Logic* (1843).

2.3.55 Such, for example, is the definition of the equality of two figures: two figures are equal when they can be superposed. To superpose them, one of them must be moved until it coincides with the other. But how is it to be moved? If we asked, we would no doubt be told that it must be done without deforming it, in the manner of an invariable solid. The vicious circle would then be obvious.

2.3.56 In fact this definition defines nothing; it would have no meaning for a being who lived in a world containing only fluids. If it seems clear to us, that is because we are accustomed to the properties of natural solids, which do not differ much from those of ideal solids, all of whose dimensions are invariable.

2.3.57 Yet, imperfect as it is, this definition implies an axiom.

2.3.58 That an invariable figure can move is not a self-evident truth. Or at least it is self-evident only in the way Euclid's postulate is, and not as an analytic *a priori* judgment would be.

2.3.59 Besides, when we study the definitions and proofs of geometry, we see that we are obliged to accept, without proof, not only that such motion is possible but also some of its properties.

2.3.60 This is clear, to begin with, from the definition of the straight line. Many faulty definitions have been given, but the true one is the one implied in every proof in which the straight line plays a part:

2.3.61 "It may happen that an invariable figure moves in such a way that all the points of a line belonging to that figure remain fixed, while all the points lying outside that line move. Such a line will be called a straight line." In this statement we have deliberately separated the definition from the axiom it implies.

2.3.62 Many proofs, such as those of the cases of equality of triangles, or of the possibility of dropping a perpendicular from a point to a straight line, presuppose propositions that are never stated. For they compel us to accept that a figure can be moved through space in a certain way.

2.3.63 The fourth geometry.—Among these implicit axioms there is one that seems to me to deserve some attention, because by

abandoning it we can construct a fourth geometry, as coherent as those of Euclid, Lobachevsky and Riemann.

2.3.64 To prove that a perpendicular to a straight line AB can always be erected at a point A, we consider a straight line AC that can turn about the point A and that initially coincides with the fixed line AB. We then turn it about the point A until it lies in the extension of AB.

2.3.65 We thus assume two propositions: first, that such a rotation is possible; and second, that it can be continued until the two lines lie in the extension of each other.

2.3.66 If we accept the first point and reject the second, we are led to a series of theorems even stranger than those of Lobachevsky and Riemann, but equally free from contradiction.

2.3.67 I shall quote only one of these theorems, and I shall not choose the oddest: *a real straight line can be perpendicular to itself*.

2.3.68 Lie's theorem.—The number of axioms implicitly introduced in the classical proofs is greater than necessary, and efforts are being made to reduce it to a minimum. Hilbert seems to have given the definitive solution to this problem. One might first have asked, *a priori*, whether such a reduction is possible at all: whether the number of necessary axioms, and the number of conceivable geometries, might not be infinite.[6]

2.3.69 A theorem of Sophus Lie governs this whole discussion. It may be stated as follows:[7]

2.3.70 Suppose we accept the following premises:

2.3.71 1. Space has n dimensions;

2.3.72 2. The motion of an invariable figure is possible.

2.3.73 3. p conditions are needed to determine the position of this figure in space.

2.3.74 *The number of geometries compatible with these premises will be limited.*

6 David Hilbert (1862–1943), German mathematician whose *Grundlagen der Geometrie* (1899) set out a complete system of axioms for Euclidean geometry.

7 Sophus Lie (1842–1899), Norwegian mathematician, founder of the theory of continuous groups of transformations.

2.3.75 I may even add that if n is given, an upper limit can be assigned to p.

2.3.76 If, then, we accept that motion is possible, we can invent only a finite (and indeed fairly small) number of three-dimensional geometries.

2.3.77 Riemann's geometries.—Yet this result seems to be contradicted by Riemann, for he constructs an infinity of different geometries, and the one usually called by his name is only a special case among them.

2.3.78 Everything depends, he says, on how the length of a curve is defined. Now there are infinitely many ways of defining this length, and each of them can become the starting point of a new geometry.

2.3.79 That is perfectly correct; but most of these definitions are incompatible with the motion of an invariable figure, which Lie's theorem assumes to be possible. These geometries of Riemann, interesting as they are in various respects, could therefore never be anything but purely analytical, and would not lend themselves to proofs like those of Euclid.

2.3.80 Hilbert's geometries.—Finally, Veronese and Hilbert have devised new geometries stranger still, which they call *non-Archimedean*. They construct them by rejecting the *axiom of Archimedes*, according to which any given length, multiplied by a sufficiently large whole number, will end up exceeding any other given length, however great. On a non-Archimedean straight line all the points of our ordinary geometry exist, but there are infinitely many others slotted in between them. So between two segments that geometers of the old school would have regarded as adjacent, an infinity of new points can be fitted.[8] In a word, non-Archimedean space is no longer a continuum of the second order, to use the language of the previous chapter, but a continuum of the third order.

8 Giuseppe Veronese (1854–1917), Italian mathematician who developed non-Archimedean geometry in the 1890s.

WHAT ARE THE AXIOMS OF GEOMETRY?

They are neither synthetic a priori judgments nor experimental facts, but conventions.

2.3.81 On the nature of the axioms.—Most mathematicians regard Lobachevsky's geometry as a mere logical curiosity; some of them, however, have gone further. Since several geometries are possible, is it certain that ours is the true one? Experience no doubt teaches us that the sum of the angles of a triangle equals two right angles. But that is because we work only with triangles that are too small. According to Lobachevsky, the difference is proportional to the area of the triangle: might it not become perceptible when we work with larger triangles, or when our measurements become more precise? Euclidean geometry would then be only a provisional geometry.

2.3.82 To discuss this view, we must first ask what the nature of the geometric axioms is.

2.3.83 Are they synthetic *a priori* judgments, as Kant said?

2.3.84 In that case they would impose themselves on us with such force that we could neither conceive the contrary proposition nor build a theoretical structure upon it. There would be no non-Euclidean geometry.

2.3.85 To be convinced of this, take a genuine synthetic *a priori* judgment—for example the following one, whose leading role we saw in the first chapter:

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

2.3.87 Then try to escape from it, and, by denying this proposition, to found a false arithmetic analogous to non-Euclidean geometry—you will not succeed. At first sight one would even be tempted to regard such judgments as analytic.

2.3.88 Besides, let us return to our fiction of animals without thickness. Can we really suppose that these beings, if their

minds were made like ours, would adopt Euclidean geometry, which all their experience would contradict?

2.3.89 Must we then conclude that the axioms of geometry are experimental truths? But we do not experiment on ideal straight lines or circles; we can experiment only on material objects. What, then, would the experiments that served as the foundation of geometry be about? The answer is easy.

2.3.90 We saw above that we constantly reason as if geometric figures behaved like solids. What geometry would borrow from experience, then, would be the properties of these bodies.

2.3.91 The properties of light and its propagation in straight lines have also been the occasion from which some of the propositions of geometry arose, in particular those of projective geometry. From this point of view one would be tempted to say that metric geometry is the study of solids, and projective geometry the study of light.

2.3.92 But a difficulty remains, and it is insurmountable. If geometry were an experimental science, it would not be an exact science; it would be subject to constant revision. What am I saying? It would already stand convicted of error, since we know that no rigorously invariable solid exists.

2.3.93 *The axioms of geometry, then, are neither synthetic* a priori *judgments nor experimental facts*.

2.3.94 They are *conventions*; our choice among all possible conventions is *guided* by experimental facts, but it remains *free*, and is limited only by the need to avoid all contradiction. That is how the postulates can remain *rigorously* true even though the experimental laws that determined their adoption are only approximate.

2.3.95 In other words, *the axioms of geometry* (I am not speaking of those of arithmetic) *are only disguised definitions.*

2.3.96 What, then, are we to think of the question: Is Euclidean geometry true?

2.3.97 It has no meaning.

2.3.98 One might as well ask whether the metric system is true and the old measures false; whether Cartesian coordinates are

true and polar coordinates false. One geometry cannot be more true than another; it can only be *more convenient*.

2.3.99 Now Euclidean geometry is, and will remain, the most convenient:

2.3.100 1. Because it is the simplest. And it is not so merely because of our habits of mind, or because of some direct intuition we might have of Euclidean space. It is the simplest in itself, just as a polynomial of the first degree is simpler than a polynomial of the second degree. The formulas of spherical trigonometry are more complicated than those of plane trigonometry, and they would still seem so to an analyst who knew nothing of their geometric meaning.

2.3.101 2. Because it agrees fairly well with the properties of natural solids—those bodies that our limbs and our eye resemble, and out of which we make our measuring instruments.

CHAPTER IV: THE SPACE OF GEOMETRY

Beings raised in a suitable world could build a non-Euclidean or four-dimensional geometry.

2.4.1 Let us begin with a little paradox.

2.4.2 Beings whose minds were made like ours, and who had the same senses as we do, but who had received no prior education, might receive from a suitably chosen external world impressions such that they would be led to construct a geometry other than Euclid's, and to locate the phenomena of that external world in a non-Euclidean space, or even in a space of four dimensions.

2.4.3 As for us, whose education has been shaped by our present world, if we were suddenly transported into that new world we would have no difficulty in referring its phenomena to our Euclidean space. Conversely, if those beings were transported into our world, they would be led to refer our phenomena to non-Euclidean space.

2.4.4 What is more, with a little effort we could do the same. Someone who devoted their life to it might perhaps manage to picture the fourth dimension.

GEOMETRIC SPACE AND REPRESENTATIVE SPACE

The space of our sensations, visual, tactile and motor, differs in every way from the geometer's space.

2.4.5 Geometric space and representative space.—It is often said that the images of external objects are localized in space, and even that they can only be formed on that condition. It is also said that this space, which thus serves as a ready-made *framework* for our sensations and our representations, is identical with the space of the geometers, and possesses all its properties.

2.4.6 To all the sensible minds who think this way, the preceding statement must have seemed very extraordinary. But we should see whether they are not under some illusion that a thorough analysis might dispel.

2.4.7 First, what are the properties of space properly so called? I mean the space that is the object of geometry, which I shall call *geometric space*. Here are some of the most essential:

2.4.8 1. It is continuous;

2.4.9 2. It is infinite;

2.4.10 3. It has three dimensions;

2.4.11 4. It is homogeneous, that is, all its points are identical with one another;

2.4.12 5. It is isotropic, that is, all the straight lines passing through any one point are identical with one another.

2.4.13 Let us now compare it with the framework of our representations and sensations, which I might call *representative space*.

2.4.14 Visual space.—Let us first consider a purely visual impression, due to an image formed at the back of the retina.

2.4.15 A brief analysis shows us this image as continuous, but as having only two dimensions. This already distinguishes what may be called *pure visual space* from geometric space.

2.4.16 Moreover, this image is enclosed within a limited frame.

2.4.17 Finally, there is another difference, no less important: *this pure visual space is not homogeneous*. Not all points of the retina play the same role, quite apart from the images that may form there. The yellow spot cannot on any account be regarded as identical with a point at the edge of the retina. Not only does the same object produce much more vivid impressions there; in any *limited* frame, the point at the center of the frame will not appear identical with a point near one of its edges.[9]

2.4.18 A more thorough analysis would no doubt show us that this continuity of visual space and its two dimensions are themselves only an illusion. That would set it still further apart from geometric space; but let us pass over this remark, whose consequences were examined sufficiently in Chapter II.

2.4.19 Sight does, however, allow us to judge distances, and therefore to perceive a third dimension. But everyone knows that this perception of the third dimension comes down to the feeling of the effort of accommodation we must make, and to the feeling of the convergence we must give to the two eyes, in order to see an object distinctly.

2.4.20 These are muscular sensations, entirely different from the visual sensations that gave us the notion of the first two dimensions. So the third dimension will not appear to us to play the same role as the other two. What may be called *complete visual space* is therefore not an isotropic space.

2.4.21 It has, it is true, exactly three dimensions. This means that the elements of our visual sensations (at least those that contribute to forming the notion of extension) will be completely determined when three of them are known; to use mathematical language, they will be functions of three independent variables.

2.4.22 But let us look at the matter a little more closely. The third dimension is revealed to us in two different ways: by the effort of accommodation and by the convergence of the eyes.

2.4.23 No doubt these two indications always agree; there is a constant relation between them. In mathematical terms, the two

variables that measure these two muscular sensations do not appear to us as independent. Or again, to avoid appealing to mathematical notions that are already rather refined, we may return to the language of Chapter II and state the same fact as follows: if two sensations of convergence A and B are indistinguishable, the two sensations of accommodation A′ and B′ that respectively accompany them will also be indistinguishable.

2.4.24 But this is, so to speak, an experimental fact. Nothing prevents us from supposing the contrary *a priori*; and if the contrary occurs—if these two muscular sensations vary independently of each other—we shall have to take one more independent variable into account, and "complete visual space" will appear to us as a physical continuum of four dimensions.

2.4.25 This is even, I would add, a fact of *external* experience. Nothing prevents us from supposing that a being whose mind is made like ours, and who has the same sense organs as we do, is placed in a world where light reaches it only after passing through refracting media of complicated shape. The two indications we use to judge distances would then cease to be linked by a constant relation. A being who trained its senses in such a world would no doubt ascribe four dimensions to complete visual space.

2.4.26 Tactile space and motor space.—"Tactile space" is even more complicated than visual space, and further removed from geometric space. There is no need to repeat for touch the discussion I gave for sight.

2.4.27 But apart from the data of sight and touch, there are other sensations that contribute as much as they do, and more, to the genesis of the notion of space. These are the ones everyone knows, which accompany all our movements and are usually called muscular.

2.4.28 The corresponding framework constitutes what may be called *motor space*.

2.4.29 Each muscle gives rise to a particular sensation that can increase or decrease, so that the totality of our muscular sensa-

tions will depend on as many variables as we have muscles. From this point of view, *motor space would have as many dimensions as we have muscles*.

2.4.30 I know it will be said that if muscular sensations help to form the notion of space, it is because we have a feeling of the *direction* of each movement, and that this feeling is an integral part of the sensation. If that were so—if a muscular sensation could arise only accompanied by this geometric feeling of direction—then geometric space would indeed be a form imposed on our sensibility.

2.4.31 But this is something I do not perceive at all when I analyze my sensations.

2.4.32 What I see is that the sensations corresponding to movements in the same direction are linked in my mind by a simple *association of ideas*. What we call "the feeling of direction" comes down to this association. So this feeling cannot be found in a single sensation.

2.4.33 This association is extremely complex, since the contraction of one and the same muscle may correspond, depending on the position of the limbs, to movements in very different directions.

2.4.34 Moreover, it is obviously acquired. Like all associations of ideas, it is the result of a *habit*, and this habit itself results from a great many *experiences*. Beyond any doubt, if our senses had been trained in a different environment, where we had received different impressions, contrary habits would have arisen, and our muscular sensations would have become associated according to other laws.

2.4.35 Characteristics of representative space.—Thus representative space, in its threefold form—visual, tactile and motor—is essentially different from geometric space.

2.4.36 It is neither homogeneous nor isotropic; we cannot even say that it has three dimensions.

9 The yellow spot (macula lutea) is the small central region of the retina responsible for the sharpest vision.

2.4.37 It is often said that we "project" the objects of our external perception into geometric space; that we "localize" them.

2.4.38 Does this have a meaning, and if so, what meaning?

2.4.39 Does it mean that we *represent* external objects to ourselves in geometric space?

2.4.40 Our representations are only reproductions of our sensations; so they can be arranged only in the same framework as those sensations, that is, in representative space.

2.4.41 It is as impossible for us to represent external bodies to ourselves in geometric space as it is for a painter to paint objects with their three dimensions on a flat canvas.

2.4.42 Representative space is only an image of geometric space, an image distorted by a kind of perspective; and we can represent objects to ourselves only by bending them to the laws of that perspective.

2.4.43 So we do not *represent* external bodies to ourselves in geometric space; but we *reason* about these bodies as if they were located in geometric space.

2.4.44 On the other hand, when we say that we "localize" a given object at a given point in space, what does that mean?

2.4.45 *It simply means that we represent to ourselves the movements that must be made to reach that object.* And let no one say that to represent these movements we must ourselves project them into space, and that the notion of space must therefore exist beforehand.

2.4.46 When I say that we represent these movements to ourselves, I mean only that we represent to ourselves the muscular sensations that accompany them. These have no geometric character, and so in no way imply that the notion of space exists beforehand.

CHANGES OF STATE AND CHANGES OF POSITION

We learn of displacements by finding that some changes can be corrected by moving our own bodies.

2.4.47 Changes of state and changes of position.—But, it will be asked, if the idea of geometric space is not imposed on our mind, and if on the other hand none of our sensations can supply it, how could it have arisen?

2.4.48 That is what we must now examine, and it will take us some time; but I can sum up in a few words the attempted explanation I am about to develop.

2.4.49 *None of our sensations, taken in isolation, could have led us to the idea of space; we are led to it only by studying the laws according to which these sensations follow one another*.

2.4.50 We see first that our impressions are subject to change; but among the changes we observe, we are soon led to draw a distinction.

2.4.51 Sometimes we say that the objects causing these impressions have changed their state; sometimes that they have changed their position, that they have merely moved.

2.4.52 Whether an object changes its state or only its position, this always shows up for us in the same way: *as a modification in a set of impressions.*

2.4.53 How, then, could we have come to distinguish them? It is easy to see. If there has been only a change of position, we can restore the original set of impressions by making movements that put us back in the same *relative* situation with respect to the moving object. We thus *correct* the modification that has occurred, and re-establish the initial state by an inverse modification.

2.4.54 If, for example, sight is involved, and an object moves in front of our eye, we can "follow it with the eye" and keep its image at the same point of the retina by appropriate movements of the eyeball.

2.4.55 We are conscious of these movements because they are voluntary and because they are accompanied by muscular sensations; but that does not mean that we represent them to ourselves in geometric space.

2.4.56 So what characterizes a change of position, what distinguishes it from a change of state, is that it can be *corrected* by this means.

2.4.57 It may therefore happen that we pass from the set of impressions A to the set B in two different ways: 1. involuntarily and without experiencing muscular sensations—this is what happens when it is the object that moves; 2. voluntarily and with muscular sensations—this is what happens when the object is stationary but we ourselves move, so that the object has a relative motion with respect to us.

2.4.58 If this is so, the passage from set A to set B is only a change of position.

2.4.59 It follows that sight and touch could not have given us the notion of space without the help of the "muscular sense."

2.4.60 Not only could this notion arise not from a single sensation but only *from a series of sensations*; what is more, an *immobile* being could never have acquired it. For, being unable to *correct* by its own movements the effects of changes in the position of external objects, it would have had no reason to distinguish them from changes of state. Nor could it have acquired the notion if its movements were not voluntary, or if they were not accompanied by sensations of some kind.

2.4.61 Condition of compensation.—How is such compensation possible, so that two changes, otherwise independent of each other, correct each other?

2.4.62 A mind *that already knew geometry* would reason as follows:

2.4.63 For compensation to occur, the various parts of the external object on the one hand, and the various organs of our senses on the other, must obviously end up, after the double change, in the same *relative* position. And for that, the various parts of the external object must also have kept the same relat-

ive position with respect to one another, and the same must be true of the various parts of our body with respect to one another.

2.4.64 In other words, in the first change the external object must move like an invariable solid, and so must our body as a whole in the second change, which corrects the first.

2.4.65 Under these conditions compensation can occur.

2.4.66 But we, *who do not yet know geometry*, since for us the notion of space is not yet formed, cannot reason in this way; we cannot foresee *a priori* whether compensation is possible. But experience teaches us that it sometimes occurs, and it is from this experimental fact that we start in order to distinguish changes of state from changes of position.

2.4.67 Solid bodies and geometry.—Among the objects around us, there are some that frequently undergo displacements that can be corrected in this way by a *correlative* movement of our own body: these are *solid bodies*.

2.4.68 Other objects, whose shape is variable, undergo such displacements (change of position without change of shape) only exceptionally. When a body has moved while *changing shape*, we can no longer, by appropriate movements, bring our sense organs back into the same *relative* situation with respect to that body; and so we can no longer restore the original set of impressions.

2.4.69 Only later, and as a result of new experiences, do we learn to break up bodies of variable shape into smaller elements, each of which moves according to roughly the same laws as solid bodies. We thus distinguish "deformations" from other changes of state. In these deformations each element undergoes a simple change of position, which can be corrected; but the modification undergone by the whole is more profound, and can no longer be corrected by a correlative movement.

2.4.70 Such a notion is already very complex and can only have appeared relatively late. Besides, it could not have arisen if the observation of solid bodies had not already taught us to distinguish changes of position.

2.4.71 *If, then, there were no solid bodies in nature, there would be no geometry*.

2.4.72 Another remark also deserves a moment's attention. Suppose a solid body first occupies the position α and then passes to the position β. In its first position it will produce in us the set of impressions A, and in its second position the set of impressions B. Now take a second solid body, with qualities entirely different from the first—of a different color, for example. Suppose again that it passes from the position α, where it produces in us the set of impressions A′, to the position β, where it produces in us the set of impressions B′.

2.4.73 In general, set A will have nothing in common with set A′, nor set B with set B′. The passage from set A to set B and the passage from set A′ to set B′ are therefore two changes that, *in themselves*, generally have nothing in common.

2.4.74 And yet we regard both of these changes as displacements; better still, we consider them to be the *same* displacement. How does this come about?

2.4.75 Simply because both can be corrected by the *same* correlative movement of our body.

2.4.76 It is therefore the "correlative movement" that constitutes the *only link* between two phenomena that we would otherwise never have thought of connecting.

2.4.77 On the other hand, our body, thanks to the number of its joints and muscles, can make a host of different movements; but not all of them can "correct" a modification of external objects. Only those can do so in which our whole body, or at least all those of our sense organs that come into play, move as a block—that is, without their relative positions changing—like a solid body.

2.4.78 To sum up:

2.4.79 1. We are first led to distinguish two categories of phenomena:

2.4.80 Some, which are involuntary and not accompanied by muscular sensations, we ascribe to external objects; these are external changes;

2.4.81 The others, whose characteristics are the opposite and which we ascribe to the movements of our own body, are internal changes;

2.4.82 2. We notice that certain changes in each of these categories can be corrected by a correlative change in the other category;

2.4.83 3. Among external changes, we distinguish those that have a correlative in the other category in this way; these are what we call displacements. Likewise, among internal changes, we distinguish those that have a correlative in the first category.

2.4.84 Thanks to this reciprocity, a particular class of phenomena is thus defined, which we call displacements. *It is the laws of these phenomena that are the object of geometry*.

2.4.85 Law of homogeneity.—The first of these laws is the law of homogeneity.

2.4.86 Suppose that by 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.

2.4.87 Now suppose that another external change α′ again makes us pass from set A to set B.

2.4.88 Experience then teaches us that this change α′, like α, can be corrected by a voluntary correlative movement β′, and that this movement β′ corresponds to the same muscular sensations as the movement β that corrected α.

2.4.89 This is the fact that is usually expressed by saying that *space is homogeneous and isotropic*.

2.4.90 We may also say that a movement that has occurred once can be repeated a second time, a third time, and so on, without its properties changing.

2.4.91 In the first chapter, where we studied the nature of mathematical reasoning, we saw how much importance should be attached to the possibility of repeating the same operation indefinitely.

2.4.92 It is from this repetition that mathematical reasoning draws its power; so it is thanks to the law of homogeneity that it has a hold on geometric facts.

2.4.93 To be complete, we ought to add to the law of homogeneity a host of other similar laws, whose details I do not wish to go into, but which mathematicians sum up in a word by saying that displacements form "a group."[10]

THE NON-EUCLIDEAN WORLD

In a sphere where heat shrinks every body, the inhabitants would naturally adopt non-Euclidean geometry.

2.4.94 The non-Euclidean world.—If geometric space were a framework imposed on *each* of our representations, taken individually, it would be impossible to picture an image stripped of that framework, and we could change nothing in our geometry.

2.4.95 But this is not so; geometry is only the summary of the laws according to which these images *follow one another*. Nothing then prevents us from imagining a series of representations, in every respect like our ordinary ones, but following one another according to laws different from those we are used to.

2.4.96 We can then conceive that beings educated in an environment where these laws were thus overturned might have a geometry very different from ours.

2.4.97 Suppose, for example, a world enclosed in a large sphere and subject to the following laws:

2.4.98 The temperature in it is not uniform. It is greatest at the center and decreases as one moves away from it, falling to absolute zero when one reaches the sphere in which this world is enclosed.

2.4.99 Let me state more precisely the law according to which this temperature varies. Let R be the radius of the bounding sphere, and let r be the distance of the point in question from

10 In mathematics, a group is a set of operations such that combining any two gives another member of the set, and each operation can be undone by one in the set.

the center of this sphere. The absolute temperature will be proportional to $R^2 - r^2$.

2.4.100 I shall further suppose that in this world all bodies have the same coefficient of expansion, so that the length of any rule is proportional to its absolute temperature.

2.4.101 Finally, I shall suppose that an object carried from one point to another at a different temperature immediately comes into thermal equilibrium with its new surroundings.

2.4.102 Nothing in these hypotheses is contradictory or unimaginable.

2.4.103 A moving object will then become smaller and smaller as it approaches the bounding sphere.

2.4.104 Let us note first that, although this world is finite from the point of view of our ordinary geometry, it will seem infinite to its inhabitants.

2.4.105 For when they try to approach the bounding sphere, they grow colder and become smaller and smaller. The steps they take are therefore also smaller and smaller, so that they can never reach the bounding sphere.

2.4.106 If for us geometry is only the study of the laws according to which invariable solids move, for these imaginary beings it will be the study of the laws according to which solids move when *deformed by the differences of temperature* I have just described.

2.4.107 No doubt, in our world too, natural solids undergo changes of shape and volume due to heating or cooling. But we neglect these changes when laying the foundations of geometry; for besides being very small, they are irregular and therefore seem to us accidental.

2.4.108 In this hypothetical world that would no longer be the case; these changes would follow regular and very simple laws.

2.4.109 Moreover, the various solid pieces making up the bodies of its inhabitants would undergo the same changes of shape and volume.

2.4.110 I shall make one more hypothesis: I shall suppose that light passes through media of varying refractive power, such

that the refractive index is inversely proportional to $R^2 - r^2$. It is easy to see that under these conditions rays of light would not be straight but circular.

2.4.111 To justify what precedes, I still have to show that certain changes in the position of external objects can be *corrected* by correlative movements of the sentient beings who inhabit this imaginary world, and in such a way as to restore the original set of impressions experienced by those beings.

2.4.112 Suppose, in fact, that an object moves and changes shape, not like an invariable solid, but like a solid undergoing unequal expansions that conform exactly to the law of temperature I assumed above. To shorten the language, allow me to call such a movement a *non-Euclidean displacement*.

2.4.113 If a sentient being happens to be nearby, its impressions will be altered by the object's displacement, but it will be able to restore them by moving itself in a suitable way. All that is needed is that, in the end, the object and the sentient being together, considered as forming a single body, have undergone one of those particular displacements I have just called non-Euclidean. This is possible if we suppose that the limbs of these beings expand according to the same law as the other bodies of the world they inhabit.

2.4.114 Although, from the point of view of our ordinary geometry, the bodies have been deformed in this displacement, and their various parts no longer stand in the same relative situation, we shall see that the impressions of the sentient being have nevertheless become the same again.

2.4.115 For although the mutual distances of the various parts may have changed, the parts originally in contact have come back into contact. So the tactile impressions have not changed.

2.4.116 On the other hand, given the hypothesis made above about the refraction and curvature of rays of light, the visual impressions too will have remained the same.

2.4.117 These imaginary beings will therefore, like us, be led to classify the phenomena they witness, and to distinguish among

them the "changes of position" that can be corrected by a voluntary correlative movement.

2.4.118 If they found a geometry, it will not be, like ours, the study of the movements of our invariable solids. It will be the study of the changes of position they have distinguished in this way, which are none other than the "non-Euclidean displacements": *it will be non-Euclidean geometry*.

2.4.119 Thus beings like ourselves, educated in such a world, would not have the same geometry as we do.

THE FOUR-DIMENSIONAL WORLD

Successive perspectives linked by muscular sensations could let us picture a fourth dimension.

2.4.120 The four-dimensional world.—Just as we can picture a non-Euclidean world, so we can picture a world of four dimensions.

2.4.121 The sense of sight, even with a single eye, together with the muscular sensations relating to the movements of the eyeball, could suffice to acquaint us with three-dimensional space.

2.4.122 The images of external objects are painted on the retina, which is a two-dimensional canvas; they are *perspectives*.

2.4.123 But since these objects can move, and so can our eye, we see in succession various perspectives of the same body, taken from several different points of view.

2.4.124 At the same time we observe that the passage from one perspective to another is often accompanied by muscular sensations.

2.4.125 If the passage from perspective A to perspective B, and that from perspective A′ to perspective B′, are accompanied by the same muscular sensations, we group them together as operations of the same kind.

2.4.126 Next, studying the laws by which these operations combine, we recognize that they form a group, which has the same structure as the group of motions of invariable solids.

2.4.127 Now, we have seen that it is from the properties of this group that we derived the notion of geometric space and that of three dimensions.

2.4.128 We thus understand how the idea of a three-dimensional space could arise from the spectacle of these perspectives, even though each of them has only two dimensions, because *they follow one another according to certain laws*.

2.4.129 Well then, just as we can draw on a plane the perspective of a three-dimensional figure, we can draw the perspective of a four-dimensional figure on a three-dimensional (or two-dimensional) picture. For the geometer this is mere play.

2.4.130 We can even take several perspectives of the same figure from several different points of view.

2.4.131 We can easily picture these perspectives to ourselves, since they have only three dimensions.

2.4.132 Let us imagine that the various perspectives of the same object follow one another, and that the passage from one to the next is accompanied by muscular sensations.

2.4.133 We shall of course regard two of these passages as two operations of the same kind when they are associated with the same muscular sensations.

2.4.134 Nothing then prevents us from imagining that these operations combine according to any law we please—for example, so as to form a group with the same structure as that of the motions of an invariable four-dimensional solid.

2.4.135 There is nothing here that cannot be pictured, and yet these sensations are precisely those that would be experienced by a being equipped with a two-dimensional retina who could move about in four-dimensional space.

2.4.136 It is in this sense that we may say one could picture the fourth dimension.

2.4.137 It would not be possible to picture in this way Hilbert's space, of which we spoke in the previous chapter, because that space is no longer a continuum of the second order. It therefore differs far too profoundly from our ordinary space.

2.4.138 Conclusions.—We see that experience plays an indispensable part in the genesis of geometry; but it would be a mistake to conclude from this that geometry is an experimental science, even in part.

2.4.139 If it were experimental, it would be only approximate and provisional. And what a rough approximation!

2.4.140 Geometry would be nothing but the study of the motions of solids; but in reality it is not concerned with natural solids. Its object is certain ideal solids, absolutely invariable, which are only a simplified and very distant image of them.

2.4.141 The notion of these ideal bodies is drawn entirely from our own mind, and experience is merely an occasion that prompts us to bring it out.

2.4.142 What geometry studies is a particular "group"; but the general concept of group pre-exists in our mind, at least potentially. It imposes itself on us not as a form of our sensibility, but as a form of our understanding.

2.4.143 Only, among all possible groups, we must choose the one that will be, so to speak, the *standard* to which we shall refer natural phenomena.

2.4.144 Experience guides us in this choice but does not impose it on us; it shows us not which geometry is the most true, but which is the most *convenient*.

2.4.145 It will be noticed that I was able to describe the fanciful worlds I imagined above *without ceasing to use the language of ordinary geometry*.

2.4.146 And indeed we would not need to change that language if we were transported there.

2.4.147 Beings who were educated in those worlds would no doubt find it more convenient to create a geometry different from ours, one better suited to their impressions. As for us, faced with the *same* impressions, we would certainly find it more convenient not to change our habits.

CHAPTER V: EXPERIENCE AND GEOMETRY

No measurement on circles, stars or light rays can decide between Euclid and Lobachevsky.

2.5.1 1. In the foregoing pages I have already tried more than once to show that the principles of geometry are not experimental facts, and in particular that Euclid's postulate cannot be proved by experiment.

2.5.2 However conclusive the reasons already given seem to me, I feel I must press the point, because there is a false idea here that is deeply rooted in many minds.

2.5.3 2. Suppose we make a material circle, measure its radius and its circumference, and try to see whether the ratio of these two lengths is equal to π. What will we have done? We will have made an experiment, not on the properties of space, but on those of the material from which we made this *round* object, and of the material of the meter rule used for the measurements.

2.5.4 3. Geometry and astronomy.—The question has also been put another way. If Lobachevsky's geometry is true, the parallax of a very distant star will be finite; if Riemann's is true, it will be negative. These are results that seem accessible to experiment, and it was hoped that astronomical observations might make it possible to decide among the three geometries.

2.5.5 But what is called a straight line in astronomy is simply the path of a ray of light. If, then, to suppose the impossible, we were to discover negative parallaxes, or to prove that all parallaxes are greater than a certain limit, we would have a choice between two conclusions: we could give up Euclidean geometry, or else modify the laws of optics and admit that light does not travel exactly in a straight line.

2.5.6 Needless to say, everyone would regard the latter solution as the more advantageous.

2.5.7 Euclidean geometry, then, has nothing to fear from new experiments.

2.5.8 4. Can it be maintained that certain phenomena, possible in Euclidean space, would be impossible in non-Euclidean space, so that experiment, by establishing these phenomena, would directly contradict the non-Euclidean hypothesis? For my part, I hold that such a question cannot even be asked. In my view it is exactly equivalent to the following one, whose absurdity is obvious to everyone: are there lengths that can be expressed in meters and centimeters but cannot be measured in toises, feet and inches, so that experiment, by establishing the existence of these lengths, would directly contradict the hypothesis that there are toises divided into six feet?[11]

2.5.9 Let us look at the question more closely. Suppose that in Euclidean space the straight line has any two properties, which I shall call A and B; that in non-Euclidean space it still has property A but no longer has property B; and finally that, in Euclidean and non-Euclidean space alike, the straight line is the only line that has property A.

2.5.10 If that were so, experiment might be able to decide between Euclid's hypothesis and Lobachevsky's. We would establish that some concrete object accessible to experiment—for instance a pencil of light rays—has property A; we would conclude that it is rectilinear, and we would then look to see whether or not it has property B.

2.5.11 But *this is not so*: there is no property that can, like this property A, be an absolute criterion for recognizing the straight line and distinguishing it from every other line.

2.5.12 Will it be said, for example: "This property will be the following: the straight line is a line such that a figure of which this line forms part can move without the mutual distances of its points changing, and in such a way that all the points of this line remain fixed"?

2.5.13 Here indeed is a property which, in Euclidean or non-Euclidean space, belongs to the straight line and to it alone. But how shall we recognize by experiment whether it belongs to this

11 The toise was an old French unit of length (about 1.95 m), divided into six pieds (feet); it was replaced by the metric system.

or that concrete object? We shall have to measure distances—and how shall we know that a given concrete magnitude that I have measured with my material instrument really represents the abstract distance?

2.5.14 We have merely pushed the difficulty back.

2.5.15 In reality, the property I have just stated is not a property of the straight line alone; it is a property of the straight line and of distance. For it to serve as an absolute criterion, we would have to establish not only that it does not also belong to some line other than the straight line taken together with distance, but also that it does not belong to some line other than the straight line taken together with some magnitude other than distance. Now that is not true.

2.5.16 It is therefore impossible to imagine a concrete experiment that could be interpreted in the Euclidean system and could not be interpreted in the Lobachevskian system, so that I may conclude:

2.5.17 No experiment will ever be in contradiction with Euclid's postulate; but, on the other hand, no experiment will ever be in contradiction with Lobachevsky's postulate.

2.5.18 5. But it is not enough that Euclidean (or non-Euclidean) geometry can never be directly contradicted by experiment. Might it not happen that it can be made to agree with experiment only by violating the principle of sufficient reason and that of the relativity of space?

2.5.19 Let me explain. Consider any material system. We shall have to take into account, on the one hand, the "state" of the various bodies of this system (for example their temperature, their electric potential, and so on), and on the other hand their position in space; and among the data that allow us to define this position we shall further distinguish the mutual distances of these bodies, which define their relative positions, from the conditions that define the absolute position of the system and its absolute orientation in space.

2.5.20 The laws of the phenomena that occur in this system may depend on the state of these bodies and on their mutual dis-

tances; but, because of the relativity and the passivity of space, they will not depend on the absolute position and orientation of the system.

2.5.21 In other words, the state of the bodies and their mutual distances at any instant will depend only on the state of those same bodies and their mutual distances at the initial instant, and will not depend at all on the initial absolute position of the system or on its initial absolute orientation. This is what, for brevity, I may call *the law of relativity*.

2.5.22 So far I have spoken as a Euclidean geometer. But, as I have said, any experiment whatever admits of an interpretation on the Euclidean hypothesis; it equally admits of one on the non-Euclidean hypothesis. Well then, we have made a series of experiments, we have interpreted them on the Euclidean hypothesis, and we have found that these experiments, so interpreted, do not violate this "law of relativity."

2.5.23 Now let us interpret them on the non-Euclidean hypothesis. This is always possible; only, the non-Euclidean distances between our various bodies in this new interpretation will generally not be the same as the Euclidean distances in the original interpretation.

2.5.24 Will our experiments, interpreted in this new way, still agree with our "law of relativity"? And if they did not, would we not still be entitled to say that experiment had proved non-Euclidean geometry false?

2.5.25 It is easy to see that this fear is groundless. For the law of relativity to be applied with full rigor, it must be applied to the entire universe. For if we considered only part of the universe, and if the absolute position of that part were to change, its distances from the other bodies of the universe would change as well; their influence on the part of the universe under consideration might therefore increase or diminish, which could alter the laws of the phenomena taking place in it.

2.5.26 But if our system is the entire universe, experiment is powerless to tell us anything about its absolute position and orientation in space. All that our instruments, however perfect

they may be, can tell us will be the state of the various parts of the universe and their mutual distances.

2.5.27 So our law of relativity can be stated as follows:

2.5.28 The readings we can take on our instruments at any instant will depend only on the readings we could have taken on those same instruments at the initial instant.

2.5.29 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.

2.5.30 Allow me a short digression on this subject. Above, I spoke of the data that define the position of the various bodies of the system; I ought also to have spoken of those that define their velocities. I would then have had to distinguish, on the one hand, the velocity with which the mutual distances of the various bodies change and, on the other, the velocities of translation and rotation of the system, that is, the velocities with which its absolute position and orientation change.

2.5.31 For the mind to be fully satisfied, it would have been necessary for the law of relativity to be statable as follows:

2.5.32 The state of the bodies and their mutual distances at any instant, as well as the velocities with which these distances are changing at that same instant, will depend only on the state of these bodies and their mutual distances at the initial instant, and on the velocities with which these distances were changing at that initial instant; but they will depend neither on the initial absolute position of the system, nor on its absolute orientation, nor on the velocities with which that absolute position and orientation were changing at the initial instant.

2.5.33 Unfortunately the law so stated does not agree with experiment, at least as experiments are usually interpreted.

2.5.34 Suppose a man were transported to a planet whose sky was permanently covered by a thick curtain of cloud, so that the other heavenly bodies could never be seen; on that planet one would live as if it were isolated in space. Yet this man could discover that it rotates, either by measuring its flattening (which is

usually done with the help of astronomical observations, but could be done by purely geodetic means) or by repeating Foucault's pendulum experiment. The absolute rotation of this planet could therefore be demonstrated.[12]

2.5.35 Here is a fact that shocks the philosopher, but which the physicist is obliged to accept.

2.5.36 We know that from this fact Newton concluded that absolute space exists. I cannot in any way adopt this view; I shall explain why in the third part. For the moment I have not wished to tackle this difficulty.

2.5.37 So, in stating the law of relativity, I have had to resign myself to lumping velocities of every kind in with the data that define the state of the bodies.

2.5.38 Be that as it may, this difficulty is the same for Euclid's geometry and for Lobachevsky's; so I need not worry about it, and I have mentioned it only in passing.

2.5.39 What matters is the conclusion: experiment cannot decide between Euclid and Lobachevsky.

2.5.40 In short, whichever way we turn, it is impossible to find any reasonable meaning in geometric empiricism.

EXPERIMENTS CONCERN BODIES, NOT SPACE

Like measuring a ship's timbers to find the captain's age, experiments reveal only relations among bodies.

2.5.41 6. Experiments tell us only about the relations of bodies to one another; none of them bears, or can bear, on the relations of bodies to space, or on the mutual relations of the various parts of space.

2.5.42 "Yes," you reply, "a single experiment is not enough, because it gives me only one equation with several unknowns; but when I have made enough experiments, I shall have enough equations to calculate all my unknowns."

2.5.43 Knowing the height of the mainmast is not enough to calculate the captain's age. When you have measured every piece of

wood in the ship, you will have plenty of equations, but you will know that age no better. All your measurements, having been made on your pieces of wood, can reveal nothing to you except what concerns those pieces of wood. In the same way your experiments, however numerous they may be, having dealt only with the relations of bodies to one another, will reveal nothing to us about the mutual relations of the various parts of space.

2.5.44 7. Will you say that, if experiments deal with bodies, they at least deal with the geometric properties of bodies?

2.5.45 First of all, what do you mean by the geometric properties of bodies? I suppose you mean the relations of bodies to space; these properties are therefore inaccessible to experiments that deal only with the relations of bodies to one another. That alone would be enough to show that these cannot be what is in question.

2.5.46 Let us nevertheless begin by agreeing on the meaning of this phrase: geometric properties of bodies. When I say that a body is made up of several parts, I take it that I am not stating a geometric property; and this would remain true even if I agreed to give the improper name of points to the smallest parts I consider.

2.5.47 When I say that a certain part of one body is in contact with a certain part of another body, I am stating a proposition that concerns the mutual relations of these two bodies, and not their relations to space.

2.5.48 I suppose you will grant me that these are not geometric properties; at least I am sure you will grant me that these properties are independent of any knowledge of metric geometry.

2.5.49 That being established, let us imagine a solid body made of eight thin iron rods OA, OB, OC, OD, OE, OF, OG, OH, joined at one of their ends, O. We shall also have a second solid body, say a piece of wood, on which we notice three small ink spots, which I shall call α, β, γ. Next, suppose we find that we can bring α β γ into contact with AGO (by which I mean α with A, while at

12 In 1851 Léon Foucault showed the earth's rotation with a long pendulum whose plane of swing slowly turns.

the same time β is in contact with G and γ with O), and then that we can successively bring α β γ into contact with BGO, CGO, DGO, EGO, FGO, then with AHO, BHO, CHO, DHO, EHO, FHO, then α γ successively with AB, BC, CD, DE, EF, FA.

2.5.50 These are observations that can be made without any prior notion of the form or the metric properties of space. They in no way concern the "geometric properties of bodies." And these observations will not be possible if the bodies experimented on move according to a group with the same structure as the Lobachevskian group (I mean, according to the same laws as solid bodies in Lobachevsky's geometry). They are therefore enough to prove that these bodies move according to the Euclidean group, or at least that they do not move according to the Lobachevskian group.

2.5.51 That they are compatible with the Euclidean group is easy to see.

2.5.52 For they could be made if the body α β γ were an invariable solid of our ordinary geometry in the shape of a right-angled triangle, and if the points A B C D E F G H were the vertices of a polyhedron formed of two regular hexagonal pyramids of our ordinary geometry, having A B C D E F as their common base and having G and H respectively as their apexes.

2.5.53 Now suppose that, instead of the preceding observations, we find that we can, as before, apply α β γ successively to AGO, BGO, CGO, DGO, EGO, FGO, AHO, BHO, CHO, DHO, EHO, FHO, and then that we can apply α β (and no longer α γ) successively to AB, BC, CD, DE, EF and FA.

2.5.54 These are the observations that could be made if non-Euclidean geometry were true, if the bodies α β γ, O A B C D E F G H were invariable solids, and if the first were a right-angled triangle and the second a regular hexagonal double pyramid of suitable dimensions.

2.5.55 These new observations are therefore not possible if bodies move according to the Euclidean group; but they become possible if we suppose that bodies move according to the Lobachevskian group. They would therefore be enough (if they were

made) to prove that the bodies in question do not move according to the Euclidean group.

2.5.56 Thus, without making any hypothesis about the form or the nature of space, or about the relations of bodies to space, and without attributing any geometric property to bodies, I have made observations that have enabled me to show in one case that the bodies experimented on move according to a group whose structure is Euclidean, and in the other case that they move according to a group whose structure is Lobachevskian.

2.5.57 And let no one say that the first set of observations would constitute an experiment proving that space is Euclidean, and the second an experiment proving that space is non-Euclidean.

2.5.58 Indeed, one could imagine (I say imagine) bodies moving in such a way as to make the second series of observations possible. And the proof is that any mechanic could build such bodies, if he cared to take the trouble and pay the cost. Yet you will not conclude from this that space is non-Euclidean.

2.5.59 Indeed, since ordinary solid bodies would go on existing once the mechanic had built the strange bodies I have just described, we would have to conclude that space is at once Euclidean and non-Euclidean.

2.5.60 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 mentioned when describing the non-Euclidean world.

2.5.61 We could have bodies whose expansion is negligible and which would behave like ordinary invariable solids; and, on the other hand, highly expansible bodies which would behave like non-Euclidean solids. We could have two double pyramids O A B C D E F G H and O′ A′ B′ C′ D′ E′ F′ G′ H′ and two triangles α β γ and α′ β′ γ′. The first double pyramid would be rectilinear and the second curvilinear; the triangle α β γ would be made of a non-expanding material and the other of a highly expansible material.

2.5.62 We could then make the first observations with the double pyramid OAH and the triangle α β γ, and the second with

the double pyramid O′H′A′ and the triangle α′ β′ γ′. And then experiment would seem to prove first that Euclidean geometry is true and then that it is false.

2.5.63 *Experiments, therefore, have dealt not with space but with bodies.*

SUPPLEMENT: WHY THREE DIMENSIONS?

A point in space is defined through classes of muscular sensations; ancestral experience made geometry convenient.

2.5.64 8. To be complete, I ought to discuss a very delicate question that would call for lengthy treatment; I shall confine myself here to summarizing what I have set out in the *Revue de Métaphysique et de Morale* and in *The Monist*. When we say that space has three dimensions, what do we mean?[13]

2.5.65 We have seen the importance of those "internal changes" that are revealed to us by our muscular sensations. They can serve to characterize the various *attitudes* of our body. Let us arbitrarily take one of these attitudes, A, as our origin. When we pass from this initial attitude to any other attitude B, we experience a series S of muscular sensations, and this series S will define B. Let us note, however, that we shall often regard two series S and S′ as defining the same attitude B (since, while the initial and final attitudes A and B remain the same, the intermediate attitudes and the corresponding sensations may differ). How, then, shall we recognize the equivalence of these two series? Because they can serve to compensate for the same external change, or, more generally, because, when it is a matter of compensating for an external change, one of the series can be replaced by the other.

2.5.66 Among these series we have singled out those that can by themselves compensate for an external change, and which we have called "displacements." Since we cannot distinguish two displacements that are too close together, the set of these dis-

13 The *Revue de Métaphysique et de Morale* is a French philosophical journal founded in 1893; *The Monist* is an American philosophical journal founded in 1890.

placements has the characteristics of a physical continuum; experience teaches us that they are those of a six-dimensional physical continuum. But we do not yet know how many dimensions space itself has; we must solve another question.

2.5.67 What is a point in space? Everyone thinks he knows, but that is an illusion. What we see, when we try to picture a point in space, is a black spot on white paper, a chalk mark on a blackboard—it is always an object. The question must therefore be understood as follows:

2.5.68 What do I mean when I say that object B is at the same point that object A occupied a moment ago? Or again, what criterion will enable me to recognize this?

2.5.69 I mean that, *although I have not moved* (as my muscular sense tells me), my first finger, which a moment ago was touching object A, is now touching object B. I could have used other criteria—another finger, for example, or the sense of sight. But the first criterion is enough; I know that if it answers yes, all the other criteria will give the same answer. I know this *by experience*; I cannot know it *a priori*. It is for this reason, too, that I say that touch cannot act at a distance; that is another way of stating the same experimental fact. And if, on the other hand, I say that sight acts at a distance, this means that the criterion provided by sight may answer yes while the others answer no.

2.5.70 And indeed the object, although it has moved away, may form its image at the same point of the retina. Sight answers yes, the object has stayed at the same point; and touch answers no, for my finger, which a moment ago was touching the object, no longer touches it. If experience had shown us that one finger can answer no when the other says yes, we would likewise say that touch acts at a distance.

2.5.71 In short, for each attitude of my body, my first finger determines a point, and it is this, and this alone, that defines a point in space.

2.5.72 To each attitude there thus corresponds a point; but it often happens that the same point corresponds to several different attitudes (in that case we say that our finger has not moved,

but the rest of the body has). We therefore single out, among changes of attitude, those in which the finger does not move. How are we led to do so? Because we often notice that in these changes the object in contact with the finger does not lose that contact.

2.5.73 Let us therefore put in one class all the attitudes that can be derived from one another by one of the changes we have thus singled out. To all the attitudes of the same class there will correspond the same point in space. So to each class there will correspond a point, and to each point a class. But we may say that what experience reaches is not the point; it is this class of changes, or better, the corresponding class of muscular sensations.

2.5.74 And so, when we say that space has three dimensions, we simply mean that the totality of these classes appears to us with the characteristics of a three-dimensional physical continuum.

2.5.75 One might be tempted to conclude that it is experience that has taught us how many dimensions space has. But in reality, here again, our experiments have dealt not with space but with our body and its relations with neighboring objects. Moreover, they are exceedingly crude.

2.5.76 In our mind there pre-existed the latent idea of a certain number of groups—those whose theory Lie worked out. Which shall we choose to make into a kind of standard against which we shall compare natural phenomena? And, once this group is chosen, which of its subgroups shall we take to characterize a point in space? Experience has guided us by showing us which choice best fits the properties of our body. But its role ended there. *Ancestral experience*.

2.5.77 It has often been said that, if individual experience could not create geometry, the same is not true of ancestral experience. But what is meant by this? Is it meant that we cannot prove Euclid's postulate experimentally, but that our ancestors were able to do so? Not in the least. What is meant is that through natural selection our mind has *adapted* itself to the conditions of the external world, that it has adopted the geometry

most advantageous to the species—or, in other words, *the most convenient*. This is entirely in keeping with our conclusions: geometry is not true, it is advantageous.

Part Three: Force

La force
The principles of classical mechanics, relative and absolute motion, energy and thermodynamics: how experimental laws harden into conventions (Chapters VI–VIII).

CHAPITRE VI. LA MÉCANIQUE CLASSIQUE.

3.6.1 The English teach mechanics as an experimental science; on the Continent it is always presented more or less as a deductive, *a priori* science. The English are right, needless to say; but how could the other course have been persisted in for so long? Why have Continental scientists, who tried to escape the habits of their predecessors, for the most part been unable to free themselves from them completely?

3.6.2 On the other hand, if the principles of mechanics have no source other than experience, are they then only approximate and provisional? Might new experiments not one day lead us to modify them or even abandon them?

3.6.3 Such are the questions that naturally arise, and the difficulty of answering them comes mainly from the fact that treatises on mechanics do not clearly distinguish what is experiment, what is mathematical reasoning, what is convention, and what is hypothesis.

3.6.4 That is not all:

3.6.5 1. There is no absolute space, and we can conceive only of relative motions; yet mechanical facts are usually stated as though there were an absolute space to which they could be referred;

3.6.6 2. There is no absolute time; to say that two durations are equal is an assertion that has no meaning in itself and can acquire one only by convention;

3.6.7 3. Not only do we have no direct intuition of the equality of two durations, but we do not even have a direct intuition of the simultaneity of two events occurring in different places; this is what I explained in an article entitled *La Mesure du temps*[1];[2]

3.6.8 4. Finally, our Euclidean geometry is itself only a sort of convention of language; we could state mechanical facts by referring them to a non-Euclidean space, which would be a less convenient frame of reference but just as legitimate as our ordinary space; the statement would then become much more complicated, but it would remain possible.

3.6.9 Thus absolute space, absolute time, even geometry itself, are not conditions imposed on mechanics; none of these things pre-exists mechanics, any more than the French language logically pre-exists the truths that are expressed in French.

3.6.10 We might try to state the fundamental laws of mechanics in a language independent of all these conventions; no doubt we would thereby get a better idea of what these laws are in themselves. That is what Andrade has attempted to do, at least in part, in his *Leçons de Mécanique physique*.[3]

3.6.11 The statement of these laws would of course become much more complicated, since all these conventions were devised precisely to shorten and simplify that statement.

3.6.12 For my part, except as regards absolute space, I shall set all these difficulties aside—not that I fail to recognize them, far from it; but we have examined enough of them in the first two parts.

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1 POINCARÉ'S NOTE. *Revue de Métaphysique et de Morale*, vol. VI, pp. 1–13 (January 1898); see also *La Valeur de la Science*, chapter II

TRANSLATOR'S NOTE. *La Valeur de la Science* (*The Value of Science*, 1905) is Poincaré's sequel to this book.

2 The title means "The Measure of Time."

3 Jules Andrade (1857–1933), French mathematician and physicist; the title means "Lessons in Physical Mechanics" (1898).

3.6.13 I shall therefore *provisionally* accept absolute time and Euclidean geometry.

3.6.14 The principle of inertia.—A body acted on by no force can have only a rectilinear and uniform motion.

3.6.15 Is this a truth that imposes itself on the mind *a priori*? If so, how could the Greeks have failed to recognize it? How could they have believed that motion stops as soon as the cause that gave rise to it ceases? Or again, that every body, if nothing interferes with it, will take on a circular motion, the noblest of all motions?

3.6.16 If we say that the velocity of a body cannot change unless there is a reason for it to change, could we not just as well maintain that the position of this body cannot change, or that the curvature of its path cannot change, unless some external cause comes to modify them?

3.6.17 Is the principle of inertia, then, which is not an *a priori* truth, an experimental fact? But has anyone ever experimented on bodies removed from the action of every force? And if so, how did they know that these bodies were subject to no force? The usual example cited is that of a ball rolling for a very long time on a marble table; but why do we say that it is subject to no force? Is it because it is too far from all other bodies to experience any appreciable action from them? Yet it is no farther from the earth than if it were thrown freely into the air; and everyone knows that in that case it would be subject to the influence of gravity, which is due to the attraction of the earth.

3.6.18 Teachers of mechanics usually pass quickly over the example of the ball; but they add that the principle of inertia is verified indirectly by its consequences. They express themselves badly; what they obviously mean is that we can verify various consequences of a more general principle, of which the principle of inertia is only a special case.

3.6.19 For this general principle I would propose the following statement:

3.6.20 The acceleration of a body depends only on the position of this body and of the neighboring bodies, and on their velocities.

3.6.21 Mathematicians would say that the motions of all the material molecules of the universe depend on differential equations of the second order.

3.6.22 To show that this really is the natural generalization of the law of inertia, I shall ask leave to use a fiction. The law of inertia, as I said above, does not impose itself on us *a priori*; other laws would be just as compatible as it is with the principle of sufficient reason. If a body is subject to no force, instead of supposing that its velocity does not change, we might suppose that it is its position, or else its acceleration, that must not change.[4]

3.6.23 Well then, let us imagine for a moment that one of these two hypothetical laws is the law of nature and replaces our law of inertia. What would its natural generalization be? A minute's reflection will show us.

3.6.24 In the first case, we would have to suppose that the velocity of a body depends only on its position and on that of the neighboring bodies; in the second case, that the variation of the acceleration of a body depends only on the position of this body and of the neighboring bodies, on their velocities, and on their accelerations.

3.6.25 Or, to speak 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.

3.6.26 Let us modify our fiction a little. Suppose a world similar to our solar system, but in which, by a strange chance, the orbits of all the planets have neither eccentricity nor inclination. Suppose further that the masses of these planets are too small for their mutual perturbations to be appreciable. Astronomers living on one of these planets would not fail to conclude that the orbit of a heavenly body can only be circular and parallel to a

4 The principle of sufficient reason, classically formulated by Leibniz, holds that nothing is or happens without a reason why it is so and not otherwise.

certain plane; the position of a body at a given instant would then be enough to determine its velocity and its whole path. The law of inertia they would adopt would be the first of the two hypothetical laws I have just mentioned.

3.6.27 Now let us imagine that one day this system is crossed at great speed by a body of great mass, coming from distant constellations. All the orbits will be profoundly disturbed. Our astronomers would not yet be too astonished; they would readily guess that this new heavenly body alone was to blame for all the damage. But, they would say, once it has moved away, order will restore itself. No doubt the distances of the planets from the sun will not return to what they were before the cataclysm, but once the disturbing body is gone, the orbits will become circular again.

3.6.28 Only when the disturbing body was far away, and the orbits, instead of becoming circular again, became elliptical—only then would these astronomers notice their error and see that they had to rebuild their whole mechanics.

3.6.29 I have dwelt a little on these hypotheses, because it seems to me that one cannot properly understand what our generalized law of inertia is except by contrasting it with a contrary hypothesis.

3.6.30 Well, now: has this generalized law of inertia been verified by experiment, and can it be? When Newton wrote the *Principia,* he certainly regarded this truth as established and experimentally demonstrated. In his eyes it was so, not only through the anthropomorphic idol of which we shall speak again, but through the work of Galileo. It was so also through Kepler's laws themselves. According to these laws, the path of a planet is entirely determined by its initial position and its initial velocity, and that is exactly what our generalized principle of inertia requires.[5]

3.6.31 Suppose this principle were true only in appearance, so that we might fear having one day to replace it with one of the

analogous principles I set against it a moment ago. For that, we would have to have been deceived by some astonishing stroke of chance, like the one that, in the fiction I developed above, misled our imaginary astronomers.

3.6.32 Such a hypothesis is too improbable to dwell on. No one will believe that such chances can occur. No doubt the probability that two eccentricities should both be exactly zero, within the errors of observation, is no smaller than the probability that one should be exactly 0.1, say, and the other 0.2, within the errors of observation. The probability of a simple event is no smaller than that of a complicated one. And yet, if the simple event occurs, we shall refuse to believe that nature has deliberately set out to deceive us. Once the hypothesis of an error of this kind is set aside, we may therefore admit that, as far as astronomy is concerned, our law has been verified by experiment.

3.6.33 But astronomy is not the whole of physics.

3.6.34 Might we not fear that some new experiment will one day show the law to fail in some corner of physics? An experimental law is always subject to revision; we must always expect to see it replaced by another, more precise law.

3.6.35 Yet no one seriously fears that the law we are discussing will ever have to be abandoned or amended. Why? Precisely because it can never be put to a decisive test.

3.6.36 To begin with, for this test to be complete, all the bodies in the universe would have to return after a certain time to their initial positions with their initial velocities. We would then see whether, from that moment on, they resumed the paths they had already followed once.

3.6.37 But this test is impossible. It can be carried out only in part, and however well it is carried out, there will always be some bodies that do not return to their initial position. So any departure from the law will easily find its explanation.

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5 Poincaré borrows the term "idols," for habitual errors of the mind, from Francis Bacon (1561–1626), the English philosopher of experimental method. The anthropomorphic view of force is discussed later in this chapter.

3.6.38 That is not all. In astronomy we *see* the bodies whose motions we study, and we generally assume that they are not acted on by other, invisible bodies. Under these conditions our law must either be verified or not.

3.6.39 But in physics it is otherwise. If physical phenomena are due to motions, they are the motions of molecules we do not see. Suppose, then, that the acceleration of one of the bodies we see appears to depend on *something else* besides the positions or velocities of the other visible bodies, or of the invisible molecules whose existence we were previously led to admit. Nothing will prevent us from supposing that this *something else* is the position or velocity of other molecules whose presence we had not suspected until then. The law will be safeguarded.

3.6.40 Let me use mathematical language for a moment to express the same thought in another form. Suppose we observe n molecules and find that their $3n$ coordinates satisfy a system of $3n$ differential equations of the fourth order (and not of the second order, as the law of inertia would require). We know that, by introducing $3n$ auxiliary variables, a system of $3n$ equations of the fourth order can be reduced to a system of $6n$ equations of the second order. If we then suppose that these $3n$ auxiliary variables represent the coordinates of n invisible molecules, the result once again agrees with the law of inertia.

3.6.41 To sum up: this law has been verified experimentally in a few particular cases. It can safely be extended to the most general cases, because we know that in these general cases experiment can no longer either confirm it or contradict it.

THE LAW OF ACCELERATION

Force and mass cannot be measured independently, so the law of acceleration turns into a definition.

3.6.42 The law of acceleration.—The acceleration of a body is equal to the force acting on it divided by its mass.

3.6.43 Can this law be verified by experiment? To do so, we would have to measure the three magnitudes that appear in its statement: acceleration, force and mass.

3.6.44 I grant that acceleration can be measured, because I am passing over the difficulty that arises from the measurement of time. But how are we to measure force, or mass? We do not even know what they are.

3.6.45 What is *mass?* It is, Newton replies, the product of volume and density.—It would be better to say, reply Thomson and Tait, that density is the quotient of mass by volume.[6]

3.6.46 —What is *force*? It is, Lagrange replies, a cause that produces the motion of a body or tends to produce it.—It is, Kirchhoff will say, the product of mass and *acceleration*. But then why not say that mass is the quotient of force by acceleration?[7]

3.6.47 These difficulties are inextricable.

3.6.48 When we say that force is the cause of a motion, we are doing metaphysics, and this definition, if we had to be content with it, would be utterly sterile. For a definition to be of any use, it must teach us to *measure* force. That is enough, moreover: it is not at all necessary for it to teach us what force is *in itself*, or whether it is the cause or the effect of motion.

3.6.49 So we must first define the equality of two forces. When shall we say that two forces are equal? When, comes the answer, applied to the same mass, they give it the same acceleration, or when, directly opposed to each other, they balance. This definition is nothing but a trompe-l'œil. We cannot unhook a force applied to one body and hook it onto another, as we unhook a locomotive to couple it to another train. It is therefore impossible to know what acceleration a given force, applied to a given body, would impart to some other body *if* it were applied to that body. It is impossible to know how two forces that are not directly opposed would behave *if* they were directly opposed.

6 William Thomson (Lord Kelvin, 1824–1907) and Peter Guthrie Tait (1831–1901), British physicists, authors of the *Treatise on Natural Philosophy* (1867).

7 Joseph-Louis Lagrange (1736–1813), mathematician, author of the *Mécanique analytique*. Gustav Kirchhoff (1824–1887), German physicist, whose lectures on mechanics treat force simply as mass times acceleration.

3.6.50 It is this definition that we try to give material form, so to speak, when we measure a force with a dynamometer, or by balancing it with a weight. Two forces F and F′, which for simplicity I shall suppose vertical and directed upward, are applied respectively to two bodies C and C′. I hang the same heavy body P first from body C, then from body C′. If there is equilibrium in both cases, I shall conclude that the two forces F and F′ are equal to each other, since both are equal to the weight of body P.

3.6.51 But am I sure that body P kept the same weight when I carried it from the first body to the second? Far from it: *I am sure of the contrary;* I know that the intensity of gravity varies from one place to another, and that it is stronger, for example, at the pole than at the equator. No doubt the difference is very small, and in practice I shall not take it into account. But a well-made definition ought to have mathematical rigor, and that rigor does not exist. What I say of weight would obviously apply to the force of the spring of a dynamometer, which temperature and a host of other circumstances can cause to vary.

3.6.52 That is not all: we cannot say that the weight of body P is applied to body C and directly balances force F. What is applied to body C is the action A of body P on body C. Body P, for its part, is subject on the one hand to its own weight, and on the other to the reaction R of body C on P. In the end, force F is equal to force A because it balances it; force A is equal to R by virtue of the principle of the equality of action and reaction; finally, force R is equal to the weight of P because it balances it. It is from these three equalities that we deduce, as a consequence, the equality of F and the weight of P.

3.6.53 So we are obliged to bring the very principle of the equality of action and reaction into the definition of the equality of two forces; *on this reckoning, that principle should no longer be regarded as an experimental law, but as a definition*.

3.6.54 Here we are, then, with two rules for recognizing the equality of two forces: the equality of two forces that balance each other, and the equality of action and reaction. But, as we saw above, these two rules are insufficient. We are obliged to

resort to a third rule and to assume that certain forces—the weight of a body, for example—are constant in magnitude and direction. But this third rule, as I have said, is an experimental law; it is only approximately true; *it is a bad definition*.

3.6.55 We are thus brought back to Kirchhoff's definition: *force is equal to mass multiplied by acceleration*. This "law of Newton" in its turn ceases to be regarded as an experimental law; it is now no more than a definition. But this definition is still insufficient, since we do not know what mass is. No doubt it allows us to calculate the ratio of two forces applied to the same body at different instants; it tells us nothing about the ratio of two forces applied to two different bodies.

3.6.56 To complete it, we must again resort to Newton's third law (the equality of action and reaction), regarded once more not as an experimental law but as a definition. Two bodies A and B act on each other. 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 by its mass is equal to the reaction of A on B. Since, by definition, action is equal to reaction, the masses of A and B are in inverse ratio to the accelerations of the two bodies. There we have the ratio of the two masses defined, and it is for experiment to verify that this ratio is constant.

3.6.57 That would be all very well if the two bodies A and B were alone together and removed from the action of the rest of the world. They are nothing of the sort. The acceleration of A is due not only to the action of B but to that of a host of other bodies C, D... To apply the preceding rule, we must therefore resolve the acceleration of A into several components and discern which of these components is due to the action of B.

3.6.58 This resolution would still be possible if we *assumed* that the action of C on A simply adds 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. We would have to assume, therefore, that any two bodies attract each other, that their mutual action is directed along the straight line joining

them and depends only on their distance—in a word, we would have to assume the *hypothesis of central forces*.

3.6.59 As is well known, a quite different principle is used to determine the masses of the heavenly bodies. The law of gravitation tells us that the attraction of two bodies is proportional to their masses; if r is their distance, m and m' their masses, and k a constant, their attraction will be kmm'/r^2.

3.6.60 What is measured in that case is not mass, the ratio of force to acceleration, but attracting mass; not the inertia of the body, but its power of attraction.

3.6.61 This is an indirect procedure, and its use is not theoretically indispensable. It might very well have been that attraction was inversely proportional to the square of the distance without being proportional to the product of the masses—that it was equal to f/r^2, but without our having $f = kmm'$.

3.6.62 If that were so, we could nevertheless measure the masses of the heavenly bodies by observing their *relative* motions.

3.6.63 But have we the right to assume the hypothesis of central forces? Is this hypothesis rigorously exact? Is it certain that it will never be contradicted by experiment? Who would dare to say so? And if we must abandon this hypothesis, the whole edifice so laboriously raised will collapse.

3.6.64 We no longer have the right to speak of the component of A's acceleration that is due to the action of B. We have no means of distinguishing it from the component due to the action of C or of some other body. The rule for measuring masses becomes inapplicable.

3.6.65 What, then, remains of the principle of the equality of action and reaction? If the hypothesis of central forces is rejected, this principle must obviously be stated as follows: the geometric resultant of all the forces applied to the various bodies of a system removed from all external action will be zero. Or, in other words, *the motion of the center of gravity of this system will be rectilinear and uniform*.

3.6.66 Here, it seems, is a way of defining mass. The position of the center of gravity obviously depends on the values assigned to the masses. We shall have to choose these values so that the motion of this center of gravity is rectilinear and uniform. This will always be possible if Newton's third law is true, and in general it will be possible in only one way.

3.6.67 But there is no system removed from all external action. Every part of the universe is acted on, more or less strongly, by all the other parts. *The law of the motion of the center of gravity is rigorously true only if it is applied to the universe as a whole*.

3.6.68 But then, to derive the values of the masses from it, we would have to observe the motion of the center of gravity of the universe. The absurdity of this consequence is plain. We know only relative motions; the motion of the center of gravity of the universe will remain forever unknown to us.

3.6.69 So nothing remains, and our efforts have been fruitless. We are driven to the following definition, which is merely an admission of helplessness: *masses are coefficients that it is convenient to introduce into calculations*.

3.6.70 We could rebuild the whole of mechanics by giving all the masses different values. This new mechanics would contradict neither experiment nor the general principles of dynamics (the principle of inertia, the proportionality of forces to masses and accelerations, the equality of action and reaction, the rectilinear and uniform motion of the center of gravity, the principle of areas).[8]

3.6.71 Only, the equations of this new mechanics would be *less simple*. Let us be clear: only the first terms would be less simple, that is, the terms that experiment has already made known to us. Perhaps the masses could be altered by small amounts without the *complete* equations gaining or losing in simplicity.

3.6.72 Hertz has asked whether the principles of mechanics are rigorously true. "In the opinion of many physicists," he says, "it will seem inconceivable that the remotest experience could ever

8 The principle of areas generalizes Kepler's second law; in modern terms it expresses the conservation of angular momentum.

change anything in the unshakable principles of mechanics; and yet what comes from experience can always be corrected by experience."[9]

3.6.73 After what we have just said, these fears will seem needless. The principles of dynamics at first appeared to us as experimental truths, but we have been obliged to use them as definitions. It is *by definition* that force is equal to the product of mass and acceleration; here is a principle that is henceforth placed beyond the reach of any future experiment. In the same way, it is by definition that action is equal to reaction.

3.6.74 But then, it will be said, these unverifiable principles are utterly devoid of meaning. Experiment cannot contradict them, but they can teach us nothing useful. So what is the point of studying dynamics?

3.6.75 This overhasty condemnation would be unjust. There is in nature no *perfectly* isolated system, perfectly removed from all external action; but there are systems that are *approximately* isolated.

3.6.76 If we observe such a system, we can study not only the relative motion of its various parts with respect to one another, but also the motion of its center of gravity with respect to the other parts of the universe. We then find that the motion of this center of gravity is *approximately* rectilinear and uniform, in accordance with Newton's third law.

3.6.77 That is an experimental truth, but it cannot be overturned by experiment. For what would a more precise experiment teach us? It would teach us that the law was only approximately true; but that we knew already.

3.6.78 *We can now see how experiment could serve as the basis for the principles of mechanics, and yet can never contradict them.*

9 Heinrich Hertz (1857–1894), German physicist who first produced and detected electromagnetic waves. The quotation comes from the introduction to his *Principles of Mechanics* (1894).

ANTHROPOMORPHIC MECHANICS

The feeling of effort and Andrade's school of the thread fail to give force a better definition.

3.6.79 Anthropomorphic mechanics.—Kirchhoff, it will be said, merely obeyed the general tendency of mathematicians toward nominalism; his skill as a physicist did not protect him from it. He insisted on having a definition of force, and for that purpose he took the first proposition that came to hand. But we have no need of a definition of force: the idea of force is a primitive notion, irreducible and indefinable. We all know what it is; we have a direct intuition of it. This direct intuition comes from the notion of effort, which has been familiar to us since childhood.

3.6.80 But, first of all, even if this direct intuition revealed to us the true nature of force in itself, it would not be enough to serve as a foundation for mechanics; and it would, moreover, be entirely useless. What matters is not knowing what force is, but knowing how to measure it.

3.6.81 Whatever does not teach us to measure it is as useless to the specialist in mechanics as, for example, the subjective notion of hot and cold is to the physicist who studies heat. This subjective notion cannot be translated into numbers, so it serves no purpose. A scientist whose skin was an absolutely poor conductor of heat, and who had therefore never felt either sensations of cold or sensations of heat, could read a thermometer just as well as anyone else, and that would be enough for him to construct the whole theory of heat.

3.6.82 Now this immediate notion of effort cannot help us to measure force. It is clear, for example, that I shall feel more fatigue in lifting a weight of fifty kilos than a man accustomed to carrying loads.

3.6.83 But there is more: this notion of effort does not reveal to us the true nature of force. In the end it comes down to a memory of muscular sensations, and no one will maintain that

the sun experiences a muscular sensation when it attracts the earth.

3.6.84 All we can look for in it is a symbol, less precise and less convenient than the arrows that geometers use, but just as remote from reality.

3.6.85 Anthropomorphism has played a considerable historical role in the genesis of mechanics. Perhaps it will still sometimes supply a symbol that some minds will find convenient; but it can found nothing that has a truly scientific or a truly philosophical character.

3.6.86 "The school of the thread."—Andrade, in his *Leçons de Mécanique physique*, has given new life to anthropomorphic mechanics. Against the school of theorists of mechanics to which Kirchhoff belongs, he sets what he rather oddly calls the school of the thread.

3.6.87 This school seeks to reduce everything to "the consideration of certain material systems of negligible mass, regarded in a state of tension and capable of transmitting considerable efforts to distant bodies, systems whose ideal type is the *thread*."

3.6.88 A thread that transmits any force stretches slightly under the action of that force. The direction of the thread tells us the direction of the force, whose magnitude is measured by the elongation of the thread.

3.6.89 We can then conceive an experiment such as the following. A body A is attached to a thread. At the other end of the thread we apply some force, which we vary until the thread reaches an elongation *a*, and we note the acceleration of body A. We detach A and attach body B to the same thread; we apply the force again, or another force, and vary it until the thread again reaches the elongation *a*; we note the acceleration of body B. We repeat the experiment with both body A and body B, but so that the thread reaches the elongation β. The four accelerations observed must be proportional. We thus have an experimental verification of the law of acceleration stated above.

3.6.90 Or again, we subject a body to the simultaneous action of several identical threads under equal tension, and we find by

experiment what the orientations of all these threads must be for the body to remain in equilibrium. We then have an experimental verification of the rule for the composition of forces.

3.6.91 But, all things considered, what have we done? We have defined the force to which the thread is subject by the deformation the thread undergoes, which is reasonable enough. We have then assumed that, if a body is attached to this thread, the effort transmitted to it by the thread is equal to the action that this body exerts on the thread. In the end, we have used the principle of the equality of action and reaction, treating it not as a truth of experience but as the very definition of force.

3.6.92 This definition is just as conventional as Kirchhoff's, but it is much less general.

3.6.93 Not all forces are transmitted by threads (and, for them to be compared, they would all have to be transmitted by identical threads). Even if we granted that the earth is tied to the sun by some invisible thread, it would at least be admitted that we have no means of measuring its elongation.

3.6.94 Nine times out of ten, therefore, our definition would fail. We could give it no meaning whatever, and we would have to go back to Kirchhoff's.

3.6.95 Why, then, take this detour? You accept a certain definition of force that has meaning only in certain particular cases. In those cases you verify by experiment 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.

3.6.96 Would it not be simpler to regard the law of acceleration as a definition in all cases? We could then regard the experiments in question not as verifications of that 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 that body is subject.

3.6.97 Not to mention that the conditions under which your definition could be accepted are only ever imperfectly fulfilled:

a thread is never without mass, and it is never free from every force other than the reaction of the bodies attached to its ends.

3.6.98 Andrade's ideas are none the less very interesting. If they do not satisfy our need for logic, they help us to understand better the historical genesis of the fundamental notions of mechanics. The reflections they suggest show us how the human mind has risen from a naive anthropomorphism to the current conceptions of science.

3.6.99 At the starting point we see a very particular and, all in all, rather crude experiment; at the end point, a law that is entirely general, entirely precise, and whose certainty we regard as absolute. This certainty is something we ourselves have conferred on it, freely so to speak, by regarding it as a convention.

3.6.100 Are the law of acceleration and the rule for the composition of forces, then, nothing but arbitrary conventions? Conventions, yes; arbitrary, no. They would be arbitrary if we lost sight of the experiments that led the founders of science to adopt them, and which, imperfect as they may be, are enough to justify them. It is good that our attention should be drawn back from time to time to the experimental origin of these conventions.

CHAPTER VII: RELATIVE AND ABSOLUTE MOTION

Why the principle of relative motion holds for uniform motion but seems to fail for rotation.

3.7.1 The principle of relative motion.—Attempts have sometimes been made to link the law of acceleration to a more general principle. The motion of any system must obey the same laws whether we refer it to fixed axes or to moving axes carried along in a rectilinear and uniform motion. This is the principle of relative motion, which forces itself on us for two reasons: first, the most commonplace experience confirms it; and second, the contrary hypothesis would be singularly repugnant to the mind.

3.7.2 Let us accept it, then, and consider a body subject to a force. The relative motion of this body, with respect to an

observer moving with a uniform velocity equal to the body's initial velocity, must be identical to what its absolute motion would be if it started from rest. From this we conclude that its acceleration cannot depend on its absolute velocity; attempts have even been made to derive from this a proof of the law of acceleration.

3.7.3 For a long time traces of this proof survived in the syllabus for the *baccalauréat ès sciences*. Clearly the attempt is futile. The obstacle that prevented us from proving the law of acceleration was that we had no definition of force. That obstacle remains entirely intact, since the principle invoked has not supplied the definition we lacked.[10]

3.7.4 The principle of relative motion is none the less very interesting and deserves to be studied for its own sake. Let us first try to state it precisely.

3.7.5 We said above that the accelerations of the various bodies forming part of an isolated system depend only on their relative velocities and positions, and not on their absolute velocities and positions, provided that the moving axes to which the relative motion is referred are carried along in a rectilinear and uniform motion. Or, if you prefer, their accelerations depend only on the differences of their velocities and the differences of their coordinates, and not on the absolute values of these velocities and coordinates.

3.7.6 If this principle is true for relative accelerations, or better, for differences of acceleration, then by combining it with the law of reaction we can deduce that it is also true for absolute accelerations.

3.7.7 It remains, then, to see how one can prove that the differences of the accelerations depend only on the differences of the velocities and coordinates—or, in mathematical language, that these differences of coordinates satisfy differential equations of the second order.

3.7.8 Can this proof be derived from experiments, or from *a priori* considerations?

10 The French school-leaving examination in its science track.

3.7.9 Remembering what we said above, the reader will supply the answer unaided.

3.7.10 Stated in this way, in fact, the principle of relative motion bears a singular resemblance to what I called above the generalized principle of inertia. It is not quite the same thing, since it concerns differences of coordinates and not the coordinates themselves. The new principle therefore tells us something more than the old one, but the same discussion applies to it and would lead to the same conclusions; there is no need to go back over it.

NEWTON'S ARGUMENT

A cloud-covered planet shows that "the earth rotates" means only that this assumption is more convenient.

3.7.11 Newton's argument.—Here we come upon a very important and even somewhat disturbing question. I said that the principle of relative motion was for us not merely a result of experiment, and that *a priori* any contrary hypothesis would be repugnant to the mind.

3.7.12 But then why is the principle true only if the motion of the moving axes is rectilinear and uniform? It seems that it ought to force itself on us with the same strength if this motion is variable, or at least if it reduces to a uniform rotation. Yet in these two cases the principle is not true.

3.7.13 I shall not dwell long on the case where the motion of the axes is rectilinear but not uniform; the paradox does not survive a moment's examination. If I am in a railway carriage and the train strikes some obstacle and stops abruptly, I shall be thrown onto the opposite seat, even though I have not been directly subjected to any force. There is nothing mysterious in this. If I have not been acted on by any external force, the train has received an external shock. That the relative motion of two bodies should be disturbed as soon as the motion of one or the other is altered by an external cause—there can be nothing paradoxical in that.

3.7.14 I shall dwell longer on the case of relative motions referred to axes rotating uniformly. If the sky were permanently covered with clouds, if we had no means of observing the stars, we could nevertheless conclude that the earth rotates. We would learn it from its flattening, or else from Foucault's pendulum experiment.

3.7.15 And yet, in that case, would saying that the earth rotates have any meaning? If there is no absolute space, can one rotate without rotating relative to something? And on the other hand, how could we accept Newton's conclusion and believe in absolute space?[11]

3.7.16 But it is not enough to note that all the possible solutions shock us equally. We must analyze, for each of them, the reasons for our repugnance, so that we can make our choice with full knowledge of the case. The long discussion that follows will therefore be excused.

3.7.17 Let us return to our fiction. Thick clouds hide the stars from human beings, who cannot observe them and are even unaware that they exist. How will these people know that the earth rotates? Even more than our ancestors, no doubt, they will regard the ground beneath them as fixed and unshakable; they will wait much longer for the coming of a Copernicus. But in the end this Copernicus would come at last. How would he come?

3.7.18 The experts in mechanics of that world would not at first run into an absolute contradiction. In the theory of relative motion, one considers, besides the real forces, two fictitious forces called ordinary centrifugal force and compound centrifugal force. Our imaginary scientists could therefore explain everything by regarding these two forces as real. They would see no contradiction in this with the generalized principle of inertia, for these forces would depend, the one on the relative positions of the various parts of the system, like real attractions, the other on their relative velocities, like real frictions.[12]

11 In the *Principia* Newton argued, notably with his rotating bucket of water, that rotation reveals the existence of absolute space.

12 Compound centrifugal force is what is now called the Coriolis force.

3.7.19 Many difficulties, however, would soon catch their attention. If they succeeded in producing an isolated system, the center of gravity of that system would not follow an approximately straight path. To explain this fact they could invoke the centrifugal forces, which they would regard as real and would no doubt attribute to the mutual actions of bodies. Only, they would not see these forces vanish at great distances—that is, as the isolation became more complete. Far from it: centrifugal force increases without limit with distance.

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

3.7.21 But that is not all. Space is symmetrical, and yet the laws of motion would show no symmetry; they would have to distinguish between right and left. It would be seen, for instance, that cyclones always turn in the same direction, whereas by reason of symmetry these atmospheric phenomena ought to turn in either direction indifferently. Suppose our scientists had succeeded by dint of hard work in making their universe perfectly symmetrical: that symmetry would not last, even though there is no apparent reason why it should be disturbed in one direction rather than the other.

3.7.22 No doubt they would find a way out. They would invent something no more extraordinary than Ptolemy's glass spheres, and so things would go on, complications piling up, until the long-awaited Copernicus swept them all away at one stroke, saying: It is much simpler to assume that the earth rotates.

3.7.23 And just as our own Copernicus told us: it is more convenient to suppose that the earth rotates, because the laws of astronomy are thereby expressed in a much simpler language, so theirs would say: It is more convenient to suppose that the earth rotates, because the laws of mechanics are thereby expressed in a much simpler language.

3.7.24 That does not alter the fact that absolute space—that is, the frame of reference to which the earth would have to be referred in order to know whether it really rotates—has no objective existence. Hence the assertion "the earth rotates" has no meaning, since no experiment will make it possible to verify it. Such an experiment not only could not be carried out, or even dreamed of by the boldest Jules Verne, but cannot be conceived without contradiction. Or rather, these two propositions, "the earth rotates" and "it is more convenient to suppose that the earth rotates," have one and the same meaning; there is nothing more in the one than in the other.

3.7.25 Perhaps people will still not be content with this, and will already find it shocking that, among all the hypotheses, or rather all the conventions, that we can make on this subject, there should be one that is more convenient than the others.

3.7.26 But if this was accepted without difficulty where the laws of astronomy were concerned, why should it shock us where mechanics is concerned?

3.7.27 We have seen that the coordinates of bodies are determined by differential equations of the second order, and that the same is true of the differences of these coordinates. This is what we have called the generalized principle of inertia and the principle of relative motion. If the distances between these bodies were likewise determined by equations of the second order, it seems that the mind ought to be entirely satisfied. To what extent does the mind receive this satisfaction, and why is it not content with it?

3.7.28 To see this, it is best to take a simple example. Suppose a system like our solar system, but from which no fixed stars outside the system can be seen, so that astronomers can observe only the mutual distances of the planets and the sun, and not the absolute longitudes of the planets. If we deduce directly from Newton's law the differential equations that define how these distances vary, these equations will not be of the second order. I mean that if, besides Newton's law, we knew the initial values of these distances and of their derivatives with respect to

time, that would not be enough to determine the values of the same distances at a later instant. One more datum would still be missing, and this datum might be, for example, what astronomers call the constant of areas.[13]

3.7.29 But here we can take two different points of view; we can distinguish two kinds of constants. In the eyes of the physicist, the world reduces to a series of phenomena that depend solely, on the one hand, on the initial phenomena and, on the other, on the laws that link consequents to antecedents. If, then, observation teaches us that a certain quantity is a constant, we shall have a choice between two ways of looking at it.

3.7.30 Either we shall admit that there is a law requiring that this quantity cannot vary, but that it was by chance that it happened, at the beginning of the ages, to have this value rather than that, a value it has had to keep ever since. This quantity might then be called an *accidental* constant.

3.7.31 Or else we shall admit, on the contrary, that there is a law of nature that imposes on this quantity this value and no other. We shall then have what may be called an *essential* constant.

3.7.32 For example, by virtue of Newton's laws, the length of the earth's revolution must be constant. But if it is equal to 366 sidereal days and something over, and not to 300 or 400, that is the result of some initial chance or other. It is an accidental constant. If, on the other hand, the exponent of the distance that appears in the expression of the attractive force is equal to −2 and not −3, that is not by chance; it is because Newton's law requires it. It is an essential constant.

3.7.33 I do not know whether this way of giving chance its share is legitimate in itself, or whether this distinction is not somewhat artificial; what is certain, at least, is that as long as nature has secrets, the distinction will be highly arbitrary in its application and always precarious.

3.7.34 As for the constant of areas, we are accustomed to regard it as accidental. Is it certain that our imaginary astronomers

13 The constant in Kepler's law of areas, proportional to the total angular momentum of the system.

would do the same? If they had been able to compare two different solar systems, they would have the idea that this constant can have several different values; but I supposed at the outset precisely that their system appeared isolated, and that they observed no heavenly body outside it. Under these conditions they could see only a single constant with a single, absolutely invariable value; they would doubtless be inclined to regard it as an essential constant.

3.7.35 A word in passing, to forestall an objection: the inhabitants of this imaginary world could neither observe nor define the constant of areas as we do, since absolute longitudes are beyond their reach; that would not prevent them from being quickly led to notice a certain constant that would enter naturally into their equations and that would be nothing other than what we call the constant of areas.

3.7.36 But then here is what will happen. If the constant of areas is regarded as essential, as depending on a law of nature, then to calculate the distances of the planets at any instant it will be enough to know the initial values of these distances and those of their first derivatives. From this new point of view, the distances will be governed by differential equations of the second order.

3.7.37 Would these astronomers' minds, however, be completely satisfied? I do not think so. First, they would soon notice that if they differentiated their equations, so as to raise their order, the equations would become much simpler. And above all they would be struck by the difficulty that arises from symmetry. They would have to admit different laws according as the planets as a whole formed the figure of a certain polyhedron or of the symmetric polyhedron, and they could escape this consequence only by regarding the constant of areas as accidental.

3.7.38 I have taken a very special example, since I have supposed astronomers who would not concern themselves at all with terrestrial mechanics and whose view would be limited to the solar system. But our conclusions apply to every case. Our universe is more extensive than theirs, since we have the fixed stars; yet it too is limited, and so we could reason about our uni-

verse as a whole as these astronomers do about their solar system.

3.7.39 We see, then, that in the end we should be led to conclude that the equations defining the distances are of an order higher than the second. Why should that shock us? Why do we find it quite natural that the sequence of phenomena should depend on the initial values of the first derivatives of these distances, while we hesitate to admit that it might depend on the initial values of the second derivatives? It can only be because of the habits of mind created in us by our constant study of the generalized principle of inertia and its consequences.

3.7.40 The values of the distances at any instant depend on their initial values, on those of their first derivatives, and on something else besides. What is this *something else?*

3.7.41 If we do not want it simply to be one of the second derivatives, we have only a choice of hypotheses. To suppose, as is usually done, that this something else is the absolute orientation of the universe in space, or the rate at which this orientation varies, may be—certainly is—the most convenient solution for the mathematician; it is not the most satisfying one for the philosopher, since this orientation does not exist.

3.7.42 We may suppose that this something else is the position or velocity of some invisible body; that is what certain people have done, and they have even called it body alpha, though we are destined never to know anything about this body but its name. This is an artifice entirely analogous to the one I spoke of at the end of the section devoted to my reflections on the principle of inertia.[14]

3.7.43 But in sum the difficulty is artificial. Provided the future readings of our instruments can depend only on the readings they have given us, or might have given us, in the past, that is all we need. And on that score we may rest easy.

14 The "body alpha" was proposed by the German mathematician Carl Neumann in 1870 as a reference body for absolute motion.

CHAPTER VIII: ENERGY AND THERMODYNAMICS

The energetic system avoids some difficulties of classical mechanics but cannot define energy in general.

3.8.1 The energetic system.—The difficulties raised by classical mechanics have led certain minds to prefer to it a new system, which they call *energetics*.

3.8.2 The energetic system arose following the discovery of the principle of the conservation of energy. It was Helmholtz who gave it its definitive form.

3.8.3 We begin by defining two quantities that play the fundamental role in this theory. These two quantities are, on the one hand, *kinetic energy* or vis viva; on the other, *potential energy*.[15]

3.8.4 All the changes that bodies in nature can undergo are governed by two experimental laws:

3.8.5 1. The sum of the kinetic energy and the potential energy is a constant. This is the principle of the conservation of energy.

3.8.6 2. If a system of bodies is in situation A at time t_0 and in situation B at time t_1, it always goes from the first situation to the second by a path such that the *mean* value of the difference between the two kinds of energy, over the interval of time separating the two times t_0 and t_1, is as small as possible.

3.8.7 This is Hamilton's principle, which is one of the forms of the principle of least action.[16]

3.8.8 The energetic theory has the following advantages over the classical theory:

3.8.9 1. It is less incomplete; that is, the principles of the conservation of energy and of Hamilton tell us more than the fundamental principles of the classical theory, and they exclude certain motions that nature does not produce but that would be compatible with the classical theory;

15 Vis viva ("living force") is the older name for what is now called kinetic energy.
16 William Rowan Hamilton (1805–1865), Irish mathematician who reformulated mechanics in terms of a variational principle.

3.8.10 2. It spares us the hypothesis of atoms, which was almost impossible to avoid with the classical theory.

3.8.11 But it raises new difficulties in its turn:

3.8.12 The definitions of the two kinds of energy are hardly easier than those of force and mass in the first system. Still, one would manage more easily, at least in the simplest cases.

3.8.13 Suppose an isolated system made up of a certain number of material points; suppose these points are subject to forces that depend only on their relative position and their mutual distances, and are independent of their velocities. By virtue of the principle of the conservation of energy, there must be a force function.

3.8.14 In this simple case, the statement of the principle of the conservation of energy is extremely simple. A certain quantity, accessible to experiment, must remain constant. This quantity is the sum of two terms; the first depends only on the position of the material points and is independent of their velocities; the second is proportional to the square of these velocities. This decomposition can be made in only one way.

3.8.15 The first of these terms, which I shall call U, will be the potential energy; the second, which I shall call T, will be the kinetic energy.

3.8.16 It is true that if T + U is a constant, so is any function whatever of T + U, $\phi(T + U)$.

3.8.17 But this function $\phi(T + U)$ will not be the sum of two terms, one independent of the velocities, the other proportional to the square of these velocities. Among the functions that remain constant, only one has this property, namely T + U (or a linear function of T + U, which makes no difference, since such a linear function can always be brought back to T + U by a change of unit and of origin). This, then, is what we shall call the energy; the first term is what we shall call the potential energy, and the second will be the kinetic energy. The definition of the two kinds of energy can therefore be carried through to the end without any ambiguity.

3.8.18 The same is true of the definition of masses. Kinetic energy, or vis viva, is expressed very simply in terms of the masses and of the velocities of all the material points relative to one of them. These relative velocities are accessible to observation, and when we have the expression of the kinetic energy as a function of these relative velocities, the coefficients of this expression will give us the masses.

3.8.19 Thus, in this simple case, the fundamental notions can be defined without difficulty. But the difficulties reappear in more complicated cases—for example, if the forces, instead of depending only on the distances, depend on the velocities as well. Weber, for example, supposes that the mutual action of two electrical molecules depends not only on their distance but on their velocity and their acceleration. If material points attracted one another according to a similar law, U would depend on the velocity, and it might contain a term proportional to the square of the velocity.[17]

3.8.20 Among the terms proportional to the squares of the velocities, how are we to tell which come from T and which from U? How, consequently, are we to distinguish the two parts of the energy?

3.8.21 But there is more. How are we to define energy itself? We no longer have any reason to take T + U as the definition rather than any other function of T + U, once the property that characterized T + U—that of being the sum of two terms of a particular form—has disappeared.

3.8.22 But that is not all: we must take account not only of mechanical energy properly so called but of the other forms of energy—heat, chemical energy, electrical energy, and so on. The principle of the conservation of energy must be written: T + U + Q = const., where T would represent the sensible kinetic energy, U the potential energy of position, depending only on the position of the bodies, and Q the internal molecular energy, in thermal, chemical or electrical form.

17 Wilhelm Weber (1804–1891), German physicist whose law of electrical force (1846) depends on the relative velocity and acceleration of the charges.

3.8.23 All would be well if these three terms were absolutely distinct: if T were proportional to the square of the velocities, U independent of these velocities and of the state of the bodies, and Q independent of the velocities and positions of the bodies and dependent only on their internal state.

3.8.24 The expression of the energy could then be decomposed in only one way into three terms of this form.

3.8.25 But this is not so. Consider electrified bodies: the electrostatic energy due to their mutual action will obviously depend on their charge, that is, on their state; but it will also depend on their position. If these bodies are in motion, they will act on each other electrodynamically, and the electrodynamic energy will depend not only on their state and position but on their velocities.

3.8.26 We therefore no longer have any means of sorting out the terms that must belong to T, to U and to Q, and of separating the three parts of the energy.

3.8.27 If (T + U + Q) is constant, so is any function whatever of it, $\phi(T + U + Q)$.

3.8.28 If T + U + Q were of the particular form I considered above, no ambiguity would result; among the functions $\phi(T + U + Q)$ that remain constant there would be only one of this particular form, and that is the one I should agree to call energy.

3.8.29 But, as I have said, this is not strictly so; among the functions that remain constant, there are none that can strictly be put into this particular form. How, then, are we to choose among them the one that should be called the energy? We no longer have anything that can guide us in our choice.

3.8.30 There remains for us only one statement of the principle of the conservation of energy: *there is something that remains constant*. In this form it is in its turn beyond the reach of experiment and reduces to a sort of tautology. It is clear that if the world is governed by laws, there will be quantities that remain constant. Like Newton's principles, and for a similar reason, the principle of the conservation of energy, founded on experiment, could no longer be invalidated by it.

3.8.31 This discussion shows that in passing from the classical system to the energetic system an advance has been made; but it shows at the same time that this advance is insufficient.

3.8.32 Another objection seems to me still more serious: the principle of least action applies to reversible phenomena, but it is by no means satisfactory as regards irreversible phenomena. Helmholtz's attempt to extend it to phenomena of this kind did not succeed and could not succeed: in this respect everything remains to be done.

3.8.33 The very statement of the principle of least action has something shocking to the mind. To get from one point to another, a material molecule, free from the action of any force but constrained to move on a surface, will take the geodesic line, that is, the shortest path.

3.8.34 This molecule seems to know the point to which we want to bring it, to foresee the time it will take to reach it by following this path or that, and then to choose the most suitable path. The statement presents it to us, so to speak, as a living and free being. It is clear that it would be better to replace it with a less shocking statement, one in which, as the philosophers would say, final causes would not seem to take the place of efficient causes.

THERMODYNAMICS

The conservation of energy reduces at last to the statement that something remains constant.

3.8.35 Thermodynamics[18].—The role of the two fundamental principles of thermodynamics in every branch of natural philosophy is becoming more important day by day. Abandoning the ambitious theories of forty years ago, cluttered with molecular hypotheses, we are now trying to raise the whole edifice of mathematical physics on thermodynamics alone. Will the two principles of Mayer and Clausius provide it with foundations solid enough

18 POINCARÉ'S NOTE. The lines that follow partly reproduce the preface of my book *Thermodynamique*.

for it to last for some time? Nobody doubts it; but where does this confidence come from?[19]

3.8.36 An eminent physicist once said to me, apropos of the law of errors: "Everybody believes in it firmly, because the mathematicians imagine that it is a fact of observation, and the observers that it is a theorem of mathematics." For a long time it was like that with the principle of the conservation of energy. It is no longer so today; everyone knows that it is an experimental fact.

3.8.37 But then what gives us the right to attribute to the principle itself more generality and more precision than to the experiments that served to demonstrate it? That is to ask whether it is legitimate, as is done every day, to generalize empirical data, and I shall not be so presumptuous as to discuss this question after so many philosophers have vainly striven to settle it. One thing alone is certain: if this faculty were denied us, science could not exist; or at least, reduced to a sort of inventory, to the recording of isolated facts, it would have no value for us, since it could not satisfy our need for order and harmony, and it would at the same time be incapable of prediction. Since the circumstances that preceded any given fact will in all likelihood never all recur at once, a first generalization is already needed to predict whether this fact will happen again as soon as the slightest of those circumstances is changed.

3.8.38 But every proposition can be generalized in an infinity of ways. Among all the possible generalizations we must choose, and we can choose only the simplest. We are thus led to act as if a simple law were, other things being equal, more probable than a complicated one.

3.8.39 Half a century ago this was frankly confessed, and it was proclaimed that nature loves simplicity; since then nature has given us the lie too often. Today this tendency is no longer admitted, and only so much of it is kept as is indispensable to keep science from becoming impossible.

19 Julius Robert Mayer (1814–1878) stated the conservation of energy in 1842 (Poincaré spells the name "Meyer"); Rudolf Clausius (1822–1888) formulated the second law of thermodynamics, here called Clausius's principle.

3.8.40 In formulating a general, simple and precise law after experiments that are relatively few in number and show certain discrepancies, we have therefore merely obeyed a necessity from which the human mind cannot escape.

3.8.41 But there is something more, and that is why I dwell on the point.

3.8.42 Nobody doubts that Mayer's principle is destined to outlive all the particular laws from which it was derived, just as Newton's law outlived Kepler's laws, from which it had emerged and which are now only approximate if perturbations are taken into account.

3.8.43 Why does this principle thus hold a sort of privileged place among all physical laws? There are many small reasons for it.

3.8.44 First of all, it is believed that we could not reject it, or even doubt its absolute rigor, without admitting the possibility of perpetual motion; we are of course wary of such a prospect, and we think ourselves less rash in affirming than in denying.

3.8.45 This is perhaps not quite accurate; the impossibility of perpetual motion entails the conservation of energy only for reversible phenomena.

3.8.46 The imposing simplicity of Mayer's principle also helps to strengthen our faith. In a law deduced directly from experiment, such as Mariotte's, this simplicity would rather seem to us a reason for distrust; but here it is otherwise. We see elements that at first glance seem disparate fall into an unexpected order and form a harmonious whole, and we refuse to believe that an unforeseen harmony is a mere effect of chance. It seems that our conquest is the dearer to us the more effort it has cost us, or that we are the surer of having wrested its true secret from nature the more jealously nature seemed to hide it from us.[20]

3.8.47 But these are only small reasons; to raise Mayer's law to the rank of an absolute principle, a more thorough discussion

20 Mariotte's law is known in English as Boyle's law: at constant temperature, the pressure of a gas varies inversely with its volume.

would be needed. But if we try to carry it out, we see that this absolute principle is not even easy to state.

3.8.48 In each particular case we see clearly what energy is, and we can give at least a provisional definition of it; but it is impossible to find a general definition of it.

3.8.49 If we wish to state the principle in all its generality and apply it to the universe, we see it, so to speak, vanish, and nothing is left but this: *There is something that remains constant*.

3.8.50 But does even this have a meaning? On the determinist hypothesis, the state of the universe is determined by an extremely large number n of parameters, which I shall call x_1, x_2, ..., x_n. As soon as we know the values of these n parameters at any instant, we also know their derivatives with respect to time, and we can therefore calculate the values of the same parameters at any earlier or later instant. In other words, these n parameters satisfy n differential equations of the first order.

3.8.51 These equations admit $n - 1$ integrals, and there are therefore $n - 1$ functions of x_1, x_2, ..., x_n that remain constant. If, then, we say that *there is something that remains constant*, we are merely stating a tautology. We should even be at a loss to say which among all our integrals ought to keep the name of energy.

3.8.52 Besides, it is not in this sense that Mayer's principle is understood when it is applied to a limited system.

3.8.53 It is then assumed that p of our n parameters vary independently, so that we have only $n - p$ relations, generally linear, among our n parameters and their derivatives.

3.8.54 To simplify the statement, let us suppose that the sum of the work done by the external forces is zero, as is the sum of the quantities of heat given off to the outside. Then this is what our principle will mean:

3.8.55 *There is a combination of these* n − p *relations whose left-hand side is an exact differential;* and then, since this differential is zero by virtue of our $n - p$ relations, its integral is a constant, and it is this integral that is called the energy.

3.8.56 But how can it happen that there are several parameters whose variations are independent? That can occur only under

the influence of external forces (even though we have supposed, for simplicity, that the algebraic sum of the work done by these forces is zero). For if the system were completely removed from all external action, the values of our n parameters at a given instant would suffice to determine the state of the system at any later instant, provided, at least, that we keep to the determinist hypothesis; we should therefore fall back into the same difficulty as before.

3.8.57 If the future state of the system is not entirely determined by its present state, it is because it depends in addition on the state of bodies outside the system. But then is it likely that there exist, among the parameters x that define the state of the system, equations independent of this state of the external bodies? And if in certain cases we think we can find some, is it not solely because of our ignorance, and because the influence of these bodies is too weak for our experiments to detect it?

3.8.58 If the system is not regarded as completely isolated, it is likely that the strictly exact expression of its internal energy will have to depend on the state of the external bodies. Moreover, I supposed above that the sum of the external work was zero, and if we wish to free ourselves from this somewhat artificial restriction, the statement becomes more difficult still.

3.8.59 To formulate Mayer's principle so as to give it an absolute meaning, we must therefore extend it to the whole universe, and then we find ourselves facing the very difficulty we were trying to avoid.

3.8.60 In short, and to use ordinary language, the law of the conservation of energy can have only one meaning: that there is a property common to all possibilities; but on the determinist hypothesis there is only one possibility, and then the law no longer has any meaning.

3.8.61 On the indeterminist hypothesis, by contrast, it would take on a meaning, even if we wished to understand it in an absolute sense; it would appear as a limit imposed on freedom.

3.8.62 But this word warns me that I am straying and am about to leave the domain of mathematics and physics. I therefore

stop, and from this whole discussion I wish to retain only one impression: that Mayer's law is a form flexible enough for almost anything we like to be fitted into it. I do not mean by this that it corresponds to no objective reality, or that it reduces to a mere tautology, since in each particular case, provided we do not try to push it to the absolute, it has a perfectly clear meaning.

3.8.63 This flexibility is a reason to believe it will last a long time; and since, on the other hand, it will disappear only to be merged in a higher harmony, we can work with confidence, relying on it, certain in advance that our work will not be lost.

3.8.64 Almost everything I have just said applies to Clausius's principle. What distinguishes it is that it is expressed by an inequality. It may be said that the same is true of all physical laws, since their precision is always limited by errors of observation. But they at least lay claim to being first approximations, and we hope to replace them little by little with more and more precise laws. If, on the contrary, Clausius's principle reduces to an inequality, the cause is not the imperfection of our means of observation but the very nature of the question.

GENERAL CONCLUSIONS OF PART THREE

The principles of mechanics are conventions suggested by experiment, unlike geometry's conventions.

3.8.65 The principles of mechanics, then, present themselves to us under two different aspects. On the one hand, they are truths founded on experiment and verified very closely as regards nearly isolated systems. On the other hand, they are postulates applicable to the whole of the universe and regarded as rigorously true.

3.8.66 If these postulates possess a generality and a certainty that were lacking in the experimental truths from which they are drawn, it is because in the last analysis they reduce to a mere convention that we have the right to make, because we are certain in advance that no experiment will ever contradict it.

3.8.67 This convention, however, is not absolutely arbitrary; it does not spring from our whim; we adopt it because certain experiments have shown us that it would be convenient.

3.8.68 We can thus understand how experiment was able to build up the principles of mechanics, and yet why it will never be able to overthrow them.

3.8.69 Let us compare this with geometry. The fundamental propositions of geometry, such as Euclid's postulate, are likewise nothing but conventions, and it is just as unreasonable to ask whether they are true or false as to ask whether the metric system is true or false.

3.8.70 Only, these conventions are convenient, and it is certain experiments that teach us so.

3.8.71 At first sight the analogy is complete; the role of experiment seems the same. We shall therefore be tempted to say: either mechanics must be regarded as an experimental science, and then the same must hold for geometry; or else, on the contrary, geometry is a deductive science, and then the same can be said of mechanics.

3.8.72 Such a conclusion would be illegitimate. The experiments that led us to adopt the fundamental conventions of geometry as more convenient bear on objects that have nothing in common with those that geometry studies; they bear on the properties of solid bodies, on the propagation of light in straight lines. They are experiments in mechanics, experiments in optics; they cannot in any sense be regarded as experiments in geometry. And indeed the main reason why our geometry seems convenient to us is that the various parts of our body, our eye, our limbs, possess precisely the properties of solid bodies. On this reckoning, our fundamental experiments are above all experiments in physiology, which bear not on space, the object the geometer has to study, but on his body, that is, on the instrument he must use for this study.

3.8.73 The fundamental conventions of mechanics, on the contrary, and the experiments that show us they are convenient, do bear on the same objects or on similar ones. The conventional

and general principles are the natural and direct generalization of the experimental and particular principles.

3.8.74 Let no one say that I am thus drawing artificial boundaries between the sciences; that if I put up a barrier between geometry proper and the study of solid bodies, I could just as well raise one between experimental mechanics and the conventional mechanics of the general principles. For who does not see that by separating these two sciences I should mutilate them both, and that what would remain of conventional mechanics once isolated would amount to very little, and could in no way be compared with that superb body of doctrine called geometry?

3.8.75 We can now understand why the teaching of mechanics should remain experimental.

3.8.76 Only in this way can it make us understand how science came into being, and that is indispensable for a full understanding of science itself.

3.8.77 Besides, if we study mechanics, it is in order to apply it; and we can apply it only if it remains objective. Now, as we have seen, what the principles gain in generality and certainty they lose in objectivity. It is therefore above all with the objective side of the principles that we should become familiar early on, and this can be done only by going from the particular to the general, instead of following the reverse course.

3.8.78 The principles are conventions and disguised definitions. Yet they are drawn from experimental laws; these laws have been, so to speak, raised to the rank of principles, to which our mind attributes an absolute value.

3.8.79 Some philosophers have generalized too far; they have believed that the principles were the whole of science, and consequently that the whole of science was conventional.

3.8.80 This paradoxical doctrine, which has been called nominalism, does not stand up to examination.

3.8.81 How can a law become a principle? It expressed a relation between two real terms A and B. But it was not rigorously true; it was only approximate. We arbitrarily introduce an intermediate

term C, more or less fictitious, and C is *by definition* that which has *exactly* the relation to A expressed by the law.

3.8.82 Then our law has been broken up into an absolute and rigorous principle, which expresses the relation of A to C, and an approximate and revisable experimental law, which expresses the relation of C to B. It is clear that however far this decomposition is pushed, there will always remain laws.

3.8.83 We shall now enter the domain of laws properly so called.

Part Four: Nature

La nature
Hypotheses in physics, the meaning of physical theories, the calculus of probabilities, optics and electrodynamics from Fresnel and Maxwell to Lorentz, and the end of matter (Chapters IX–XIV).

CHAPITRE IX. LES HYPOTHÈSES EN PHYSIQUE.

4.9.1 The role of experiment and generalization.—Experiment is the sole source of truth: it alone can teach us anything new; it alone can give us certainty. These are two points that no one can dispute.

4.9.2 But then, if experiment is everything, what place will be left for mathematical physics? What has experimental physics to do with such an auxiliary, which seems useless and perhaps even dangerous?

4.9.3 And yet mathematical physics exists; it has rendered undeniable services; this is a fact that needs to be explained.

4.9.4 The explanation is that it is not enough to observe; we must make use of our observations, and for that we must generalize. This has been done in every age; only, as the memory of past errors has made man more and more cautious, people have observed more and more and generalized less and less.

4.9.5 Each century mocked the one before, accusing it of having generalized too quickly and too naively. Descartes pitied the

Ionians; Descartes in his turn makes us smile; no doubt our children will laugh at us some day.[1]

4.9.6 But then can we not go straight to the end? Is that not the way to escape the ridicule we foresee? Can we not be content with bare experiment?

4.9.7 No, that is impossible; it would be to misunderstand completely the true character of science. The scientist must set things in order. Science is built of facts, as a house is built of stones; but a pile of facts is no more a science than a heap of stones is a house.

4.9.8 And above all the scientist must predict. Carlyle wrote somewhere something like this: "Fact alone matters; John Lackland passed by here—that is what is admirable, that is a reality for which I would give all the theories in the world." Carlyle was a fellow countryman of Bacon; but Bacon would not have said that. That is the language of the historian. The physicist would rather say: "John Lackland passed by here; that is all the same to me, since he will never pass this way again."[2]

4.9.9 We all know that there are good experiments and bad ones. The bad ones will pile up in vain; whether a hundred have been made or a thousand, a single piece of work by a true master, by a Pasteur for example, will be enough to consign them to oblivion. Bacon would have understood this well; it was he who invented the term *experimentum crucis*. But Carlyle could hardly be expected to understand it. A fact is a fact: a schoolboy read such and such a number on his thermometer; he had taken no precautions; no matter, he read it, and if only the fact counts, it is a reality just as much as the wanderings of King John Lackland.[3] Why is the fact that this schoolboy took this reading of no interest, while the fact that a skilled physicist had taken another reading would, on the contrary, be very important? Because

1 The Ionians: the earliest Greek natural philosophers (6th century BC), such as Thales and Anaximenes, who sought a single principle behind all things.

2 Thomas Carlyle (1795–1881), Scottish historian and essayist; John Lackland is King John of England (reigned 1199–1216).

3 Latin, "crucial experiment"; Bacon himself spoke of an *instantia crucis*, a "crucial instance."

from the first reading we can conclude nothing. What, then, is a good experiment? It is one that teaches us something other than an isolated fact; it is one that enables us to predict, that is, one that enables us to generalize.

4.9.10 For without generalization prediction is impossible. The circumstances in which one has worked will never all recur at once. The observed fact will therefore never happen again; the only thing that can be asserted is that in similar circumstances a similar fact will occur. To predict, then, we must at least invoke analogy—that is, we must already generalize.

4.9.11 However timid one may be, one has to interpolate. Experiment gives us only a certain number of isolated points; we must join them by a continuous line, and that is a genuine generalization. But we do more: the curve we draw will pass between the observed points and close to them; it will not pass through the points themselves. Thus we do not merely generalize experiment, we correct it; and the physicist who tried to abstain from these corrections and truly content himself with bare experiment would be forced to state very extraordinary laws.

4.9.12 Bare facts, then, cannot suffice for us; that is why we need science that is ordered, or rather organized.

4.9.13 It is often said that one should experiment without preconceived ideas. That is not possible. Not only would it make every experiment sterile, but even if we wanted to, we could not. Everyone carries within himself his own conception of the world, and he cannot shake it off so easily. We must, for example, use language, and our language is kneaded through and through with preconceived ideas and can be made of nothing else. Only, these are unconscious preconceived ideas, a thousand times more dangerous than the others.

4.9.14 Shall we say that if we bring in others, of which we are fully conscious, we shall only make matters worse? I do not think so. I rather believe that they will serve as counterweights to one another—I was going to say as antidotes. They will generally agree poorly with one another; they will come into conflict, and in doing so they will force us to look at things from different

sides. That is enough to set us free: one is no longer a slave when one can choose one's master.

4.9.15 Thus, thanks to generalization, each observed fact lets us foresee a great many others. Only, we must not forget that the first alone is certain and that all the others are merely probable. However firmly grounded a prediction may seem to us, we are never *absolutely* sure that experiment will not refute it if we set about verifying it. But the probability is often great enough for us to be content with it in practice. Better to foresee without certainty than not to foresee at all.

4.9.16 So we must never disdain to make a verification when the opportunity arises. But every experiment is long and difficult, and workers are few, while the number of facts we need to foresee is immense. Beside this mass, the number of direct verifications we can make will never be more than a negligible quantity.

4.9.17 Of the little we can reach directly, we must make the best use. Each experiment must allow us the greatest possible number of predictions, with the highest degree of probability that can be had. The problem is, so to speak, to increase the yield of the scientific machine.

4.9.18 Let me compare science to a library that must keep on growing. The librarian has only insufficient funds for his purchases, and he must try not to waste them.

4.9.19 It is experimental physics that is in charge of purchases; it alone, then, can enrich the library.

4.9.20 As for mathematical physics, its task will be to draw up the catalogue. If this catalogue is well made, the library will be none the richer for it. But it may help the reader to make use of its riches.

4.9.21 And further, by showing the librarian the gaps in his collections, it will allow him to spend his funds wisely. This is all the more important because those funds are quite inadequate.

4.9.22 Such, then, is the role of mathematical physics: it must guide generalization so as to increase what I called a moment ago the yield of science. By what means it achieves this, and

how it can do so without danger, is what remains for us to examine.

4.9.23 The unity of nature.—Let us first observe that every generalization presupposes, to some extent, a belief in the unity and simplicity of nature. As for unity, there can be no difficulty. If the various parts of the universe were not like the organs of a single body, they would not act on one another; they would know nothing of each other; and we in particular would know only one of them. So we need not ask whether nature is one, but how it is one.

4.9.24 On the second point, things are not so easy. It is not certain that nature is simple. Can we, without danger, act as if it were?

4.9.25 There was a time when the simplicity of Mariotte's law was put forward as an argument for its accuracy. In those days Fresnel himself, having said in a conversation with Laplace that nature is not concerned with analytical difficulties, felt obliged to offer explanations so as not to clash too sharply with prevailing opinion.[4]

4.9.26 Today ideas have changed greatly. And yet those who do not believe that natural laws must be simple are still often obliged to act as if they believed it. They could not escape this necessity entirely without making all generalization impossible, and consequently all science.

4.9.27 It is clear that any fact can be generalized in an infinity of ways, and we must choose; the choice can be guided only by considerations of simplicity. Take the most commonplace case, that of interpolation. We draw a continuous line, as regular as possible, between the points given by observation. Why do we avoid sharp corners and overly abrupt inflections? Why do we not make our curve trace the most capricious zigzags? Because we know in advance, or think we know, that the law to be expressed cannot be as complicated as that.

4.9.28 The mass of Jupiter can be deduced from the motions of its satellites, from the perturbations of the major planets, or

4 Pierre-Simon Laplace (1749–1827), French mathematician and astronomer.

from those of the minor planets. If we take the mean of the determinations obtained by each of these three methods, we find three numbers very close together but different. One could interpret this result by supposing that the coefficient of gravitation is not the same in the three cases; the observations would certainly be represented much better. Why do we reject this interpretation? Not because it is absurd, but because it is needlessly complicated. We shall accept it only on the day it forces itself upon us, and it does not do so yet.

4.9.29 In short, most often every law is held to be simple until the contrary is proved.

4.9.30 This habit is imposed on physicists for the reasons I have just explained. But how can it be justified in the face of discoveries that show us every day new details, richer and more complex? How can it even be reconciled with the sense of the unity of nature? For if everything depends on everything, relations involving so many different objects can no longer be simple.

4.9.31 If we study the history of science, we see two phenomena occur that are, so to speak, the reverse of each other. Sometimes simplicity lies hidden beneath complex appearances. Sometimes, on the contrary, it is the simplicity that is apparent, and it conceals extremely complicated realities.

4.9.32 What could be more complicated than the perturbed motions of the planets, and what simpler than Newton's law? There nature, making light, as Fresnel said, of analytical difficulties, uses only simple means, and by combining them produces a hopelessly tangled skein. That is hidden simplicity, the kind that must be discovered.

4.9.33 Examples of the opposite abound. In the kinetic theory of gases, we consider molecules moving at great speeds, whose paths, distorted by incessant collisions, take the most capricious 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 restored simplicity in the average. Here the simplicity is only apparent, and only the

coarseness of our senses prevents us from perceiving the complexity.

4.9.34 Many phenomena obey a law of proportionality; but why? Because in these phenomena there is something that is very small. The simple law observed is then merely a translation of the general analytical rule according to which the infinitely small increment of a function is proportional to the increment of the variable. Since in reality our increments are not infinitely small but only very small, the law of proportionality is only approximate and the simplicity only apparent. What I have just said applies to the rule of the superposition of small motions, which is so fruitful in use and which is the foundation of optics.

4.9.35 And Newton's law itself? Its simplicity, so long hidden, is perhaps only apparent. Who knows whether it is not due to some complicated mechanism, to the impacts of some subtle matter in irregular motion? And who knows whether it has become simple only through the play of averages and large numbers? In any case it is hard not to suppose that the true law contains additional terms which would become appreciable at small distances. If in astronomy they are negligible compared with Newton's term, and the law thereby regains its simplicity, this would be solely because of the enormity of celestial distances.

4.9.36 No doubt, if our means of investigation became more and more penetrating, we should discover the simple beneath the complex, then the complex beneath the simple, then again the simple beneath the complex, and so on, without our being able to foresee what the last term will be.

4.9.37 We must stop somewhere, and for science to be possible we must stop when we have found simplicity. That is the only ground on which we can raise the edifice of our generalizations. But since this simplicity is only apparent, will the ground be solid enough? That is what we must look into.

4.9.38 To do so, let us see what role belief in simplicity plays in our generalizations. We have verified a simple law in a fairly large number of particular cases. We refuse to admit that this

agreement, so often repeated, is a mere effect of chance, and we conclude that the law must be true in the general case.

4.9.39 Kepler notices that the positions of a planet observed by Tycho all lie on one and the same ellipse. Not for a single moment does he imagine that, by a strange freak of chance, Tycho only ever looked at the sky at the moments when the planet's true path happened to cross that ellipse.[5]

4.9.40 What does it matter, then, whether the simplicity is real or whether it covers a complex truth? It may be due to the influence of large numbers, which levels out individual differences, or to the largeness or smallness of certain quantities, which allows certain terms to be neglected; in every case it is not due to chance. This simplicity, real or apparent, always has a cause. We can therefore always reason in the same way: if a simple law has been observed in several particular cases, we may legitimately suppose that it will still be true in analogous cases. To refuse to do so would be to give chance an inadmissible role.

4.9.41 There is, however, a difference. If the simplicity were real and deep, it would withstand the increasing precision of our means of measurement. So if we believed nature to be deeply simple, we ought to conclude from an approximate simplicity to a rigorous one. That is what used to be done; it is what we no longer have the right to do.

4.9.42 The simplicity of Kepler's laws, for example, is only apparent. That does not prevent them from applying, very nearly, to all systems analogous to the solar system, but it does prevent them from being rigorously exact.

THE ROLE OF HYPOTHESIS

Poincaré sorts hypotheses into natural, indifferent and genuine generalizations, and warns against unconscious ones.

4.9.43 The role of hypothesis.—Every generalization is a hypothesis; hypothesis therefore has a necessary role, which no one

5 Johannes Kepler (1571–1630) derived his laws of planetary motion from the observations of the Danish astronomer Tycho Brahe (1546–1601).

has ever disputed. Only, it must always be submitted to verification, as soon as possible and as often as possible. It goes without saying that if it does not stand this test, it must be abandoned without a second thought. That is indeed what is generally done, but sometimes with a certain irritation.

4.9.44 Well, even this irritation is unjustified. The physicist who has just given up one of his hypotheses ought, on the contrary, to be full of joy, for he has just found an unexpected opportunity for discovery. His hypothesis, I imagine, had not been adopted lightly; it took account of all the known factors that seemed capable of playing a part in the phenomenon. If the verification fails, it is because there is something unexpected, something extraordinary; it is because we are about to find something unknown and new.

4.9.45 Has the hypothesis thus overturned been sterile, then? Far from it; one may say it has rendered more service than a true hypothesis. Not only was it the occasion of the decisive experiment; had that experiment been made by chance, without the hypothesis, nothing would have come of it. No one would have seen anything extraordinary in it; one more fact would simply have been catalogued, without the slightest consequence being drawn from it.

4.9.46 Now, under what condition is the use of hypothesis free of danger?

4.9.47 A firm resolve to submit to experiment is not enough; there are still dangerous hypotheses—first and above all, those that are tacit and unconscious. Since we make them without knowing it, we are powerless to abandon them. Here again, then, is a service that mathematical physics can render us. By the precision that is proper to it, it compels us to state all the hypotheses that we should make without it, but without suspecting them.

4.9.48 Let us note, on the other hand, that it is important not to multiply hypotheses beyond measure, and to make them only one at a time. If we build a theory on multiple hypotheses and experiment condemns it, which of our premises must be

changed? It will be impossible to know. And conversely, if the experiment succeeds, shall we think we have verified all these hypotheses at once? Shall we think we have determined several unknowns with a single equation?

4.9.49 We must also take care to distinguish between the different kinds of hypotheses. First there are those that are quite natural and that one can scarcely avoid. It is hard not to suppose that the influence of very distant bodies is quite negligible, that small motions obey a linear law, that the effect is a continuous function of its cause. I would say as much of the conditions imposed by symmetry. All these hypotheses form, so to speak, the common stock of all the theories of mathematical physics. They are the last that should be abandoned.

4.9.50 There is a second category of hypotheses, which I shall call indifferent. In most questions the analyst supposes, at the outset of his calculation, either that matter is continuous or, conversely, that it is made up of atoms. Had he done the opposite, his results would have been no different; he would only have had more trouble obtaining them, that is all. If experiment then confirms his conclusions, will he think he has demonstrated, for example, the real existence of atoms?

4.9.51 Optical theories bring in two vectors, one of which is regarded as a velocity and the other as a vortex. This again is an indifferent hypothesis, since we should have reached the same conclusions by doing exactly the opposite. The success of the experiment cannot therefore prove that the first vector really is a velocity. It proves only one thing, namely that it is a vector; that is the only hypothesis that has really been introduced into the premises. To give it the concrete appearance that the weakness of our minds demands, we had to regard it either as a velocity or as a vortex, just as we had to represent it by a letter, either x or y. But the result, whatever it may be, will not prove that we were right or wrong to regard it as a velocity, any more than it will prove that we were right or wrong to call it x rather than y.

4.9.52 These indifferent hypotheses are never dangerous, provided their character is not misunderstood. They can be useful, either as devices of calculation or to support our understanding with concrete images—to fix our ideas, as the saying goes. There is therefore no reason to ban them.

4.9.53 Hypotheses of the third category are the true generalizations. It is these that experiment must confirm or refute. Verified or condemned, they may be fruitful. But for the reasons I have set out, they will be so only if they are not multiplied.

4.9.54 The origin of mathematical physics.—Let us go further and examine more closely the conditions that allowed mathematical physics to develop. We see at once that the efforts of scientists have always aimed at resolving the complex phenomenon given directly by experiment into a very large number of elementary phenomena.

4.9.55 And this in three different ways. First, in time. Instead of grasping the gradual development of a phenomenon as a whole, we seek simply to link each instant to the instant immediately before it. We assume that the present state of the world depends only on the most recent past, without being directly influenced, so to speak, by the memory of a distant past. Thanks to this postulate, instead of studying directly the whole succession of phenomena, we can confine ourselves to writing down its "differential equation"; for Kepler's laws we substitute Newton's.

4.9.56 Next, we seek to break the phenomenon down in space. What experiment gives us is a confused mass of facts occurring on a stage of some extent. We must try to discern the elementary phenomenon, which will, on the contrary, be localized in a very small region of space.

4.9.57 A few examples may make my meaning clearer. If we wanted to study, in all its complexity, the distribution of temperature in a cooling solid, we could never succeed. Everything becomes simple if we reflect that a point of the solid cannot give up heat directly to a distant point. It will give up heat directly only to the nearest points, and it is step by step that the flow of heat can reach other parts of the solid. The elementary phe-

nomenon is the exchange of heat between two adjacent points. It is strictly localized, and it is relatively simple if we assume, as is natural, that it is not influenced by the temperature of molecules at an appreciable distance.

4.9.58 I bend a rod. It will take on a very complicated shape whose direct study would be impossible. But I can tackle it nonetheless if I observe that its bending is simply the resultant of the deformation of very small elements of the rod, and that the deformation of each of these elements depends only on the forces applied directly to it, and not at all on those that may act on the other elements.

4.9.59 In all these examples, which I could easily multiply, it is assumed that there is no action at a distance, or at least none at a great distance. This is a hypothesis. It is not always true, as the law of gravitation proves; it must therefore be submitted to verification. If it is confirmed, even approximately, it is precious, for it will allow us to do mathematical physics, at least by successive approximations.

4.9.60 If it does not stand the test, we must look for something else of the same kind, for there are still other ways of arriving at the elementary phenomenon. If several bodies act simultaneously, it may happen that their actions are independent and simply add up, either in the manner of vectors or in the manner of scalar quantities. The elementary phenomenon is then the action of a single body. Or again, we may be dealing with small motions, or more generally with small variations, which obey the well-known law of superposition. The observed motion will then be broken down into simple motions—sound, for example, into its harmonics, and white light into its monochromatic components.

4.9.61 Once we have made out in which direction we should seek the elementary phenomenon, by what means can we reach it?

4.9.62 First, it will often happen that, in order to guess it, or rather to guess what of it is useful to us, we shall not need to penetrate its mechanism; the law of large numbers will suffice.

Let us return to the example of the propagation of heat. Each molecule radiates toward each neighboring molecule; according to what law, we need not know. If we supposed anything on that score, it would be an indifferent hypothesis, and therefore useless and unverifiable. And indeed, through the action of averages and thanks to the symmetry of the medium, all differences are leveled out, and whatever hypothesis is made, the result is always the same.

4.9.63 The same circumstance arises in the theory of elasticity and in that of capillarity. Neighboring molecules attract and repel one another; we need not know according to what law. It is enough for us that this attraction be appreciable only at small distances, that the molecules be very numerous, and that the medium be symmetrical; then we have only to let the law of large numbers do its work.

4.9.64 Here again the simplicity of the elementary phenomenon lay hidden beneath the complication of the resulting observable phenomenon; but this simplicity, in its turn, was only apparent and concealed a very complex mechanism.

4.9.65 The best way to arrive at the elementary phenomenon would obviously be experiment. We should, by experimental devices, take apart the complex bundle that nature offers to our inquiries and carefully study its elements, as purified as possible. For example, we shall break natural white light down into monochromatic lights with a prism, and into polarized lights with a polarizer.

4.9.66 Unfortunately, this is neither always possible nor always sufficient, and sometimes the mind must run ahead of experiment. I shall cite only one example, which has always struck me forcibly:

4.9.67 If I break down white light, I can isolate a small portion of the spectrum, but however small it is, it will keep a certain width. Likewise, the natural lights called *monochromatic* give us a very fine line, but one that is still not infinitely fine. One might suppose that by studying experimentally the properties of these natural lights, working with finer and finer spectral lines, and

finally passing to the limit, so to speak, we should come to know the properties of a strictly monochromatic light.

4.9.68 That would not be correct. Suppose that two rays come from the same source, that they are first polarized in two perpendicular planes, then brought back to the same plane of polarization, and that we try to make them interfere. If the light were *strictly* monochromatic, they would interfere. But with our nearly monochromatic lights there will be no interference, however narrow the line. For it to be otherwise, the line would have to be several million times narrower than the finest lines known.

4.9.69 Here, then, passing to the limit would have misled us. The mind had to run ahead of experiment, and if it did so successfully, it is because it let itself be guided by the instinct for simplicity.

4.9.70 Knowledge of the elementary fact allows us to put the problem into an equation; it then remains only to deduce from it, by combination, the complex fact that can be observed and verified. This is what is called *integration*; it is the mathematician's business.

4.9.71 One may ask why, in the physical sciences, generalization so readily takes mathematical form. The reason is now easy to see. It is not only because we have numerical laws to express; it is because the observable phenomenon is due to the superposition of a great number of elementary phenomena *all similar to one another*. That is how differential equations come in quite naturally.

4.9.72 It is not enough for each elementary phenomenon to obey simple laws; all those we have to combine must obey the same law. Only then can mathematics be of use, for mathematics teaches us to combine like with like. Its aim is to guess the result of a combination without having to go through that combination piece by piece. If we have to repeat the same operation several times, mathematics allows us to avoid the repetition by telling us the result in advance through a kind of induction. I explained this above, in the chapter on mathematical reasoning.

4.9.73 But for this, all these operations must be alike. Otherwise we should obviously have to resign ourselves to actually performing them one after another, and mathematics would become useless.

4.9.74 It is thus thanks to the approximate homogeneity of the matter studied by physicists that mathematical physics was able to come into being.

4.9.75 In the natural sciences these conditions are no longer found—homogeneity, the relative independence of distant parts, the simplicity of the elementary fact—and that is why naturalists are obliged to resort to other modes of generalization.

CHAPTER X: THE THEORIES OF MODERN PHYSICS

Physical theories pass away, but the true relations they express survive in their successors.

4.10.1 The meaning of physical theories.—Laypeople are struck by how short-lived scientific theories are. After a few years of prosperity, they see them abandoned one after another; they see ruins piled upon ruins. They foresee that the theories now in fashion must in turn soon succumb, and they conclude that these are utterly futile. This is what they call the *bankruptcy of science*.[6]

4.10.2 Their skepticism is superficial. They have no idea of the aim and role of scientific theories; otherwise they would understand that ruins may still be good for something.

4.10.3 No theory seemed more solid than Fresnel's, which attributed light to motions of the ether. Yet Maxwell's is now preferred to it. Does that mean Fresnel's work was in vain? No. Fresnel's aim was not to find out whether there really is an ether, whether or not it is made of atoms, whether these atoms really move in this or that direction; it was to predict optical phenomena.

4.10.4 Now Fresnel's theory still allows us to do that, today as well as before Maxwell. The differential equations are still true;

6 The phrase echoes the critic Ferdinand Brunetière, whose 1895 article "Après une visite au Vatican" proclaimed the "bankruptcy" of science.

they can still be integrated by the same methods, and the results of this integration still keep their full value.

4.10.5 And let no one say that we thereby reduce physical theories to the role of mere practical recipes. These equations express relations, and if the equations remain true, it is because these relations retain their reality. They teach us, now as before, that there is such and such a relation between something and something else. Only, that something we used to call *motion*, and now we call it *electric current*. But these names were merely images substituted for the real objects that nature will hide from us forever. The true relations between these real objects are the only reality we can attain. The only condition is that there be the same relations between these objects as between the images we are forced to put in their place. If these relations are known to us, what does it matter if we find it convenient to replace one image by another?

4.10.6 Whether some periodic phenomenon (an electrical oscillation, for example) is really due to the vibration of some atom that, behaving like a pendulum, truly moves in this or that direction—that is neither certain nor interesting. But that there is, between the electrical oscillation, the motion of the pendulum and all periodic phenomena, a close kinship that corresponds to a deep reality; that this kinship, this similarity, or rather this parallelism, extends into detail; that it is a consequence of more general principles, that of energy and that of least action—that is what we can affirm. That is the truth that will always remain the same under all the costumes in which we may see fit to dress it up.

4.10.7 Many theories of dispersion have been proposed. The first were imperfect and contained only a small share of truth. Then came Helmholtz's; it was then modified in various ways, and its author himself devised another, based on Maxwell's principles. But—a remarkable thing—all the scientists who came after Helmholtz arrived at the same equations, from points of departure that were apparently very far apart. I will venture to say that these theories are all true at once, not only because they

make us foresee the same phenomena, but because they bring to light a true relation, that between absorption and anomalous dispersion.[7] In the premises of these theories, what is true is what is common to all the authors: the assertion of such and such a relation between certain things that some call by one name and others by another.

4.10.8 The kinetic theory of gases has given rise to many objections, which would be hard to answer if one claimed to see in it the absolute truth. But all these objections do not alter the fact that it has been useful. In particular, it has revealed to us a true relation that without it would have stayed deeply hidden: that between gas pressure and osmotic pressure. In this sense, then, we may say that it is true.

4.10.9 When a physicist finds a contradiction between two theories that are equally dear to him, he sometimes says: Let us not worry about that, but hold fast to both ends of the chain, even though the intermediate links are hidden from us. This argument of an embarrassed theologian would be ridiculous if physical theories had to be given the meaning that laypeople give them. In case of contradiction, at least one of them would then have to be regarded as false. It is no longer so if we look in them only for what we ought to look for. It may be that both express true relations, and that the contradiction lies only in the images in which we have dressed reality.[8]

4.10.10 To those who find that we restrict too narrowly the domain accessible to the scientist, I shall reply: These questions, which we forbid you and whose loss you regret, are not only insoluble; they are illusory and devoid of meaning.

4.10.11 Some philosopher claims that all of physics can be explained by the mutual collisions of atoms. If he simply means that there are the same relations between physical phenomena as between the mutual collisions of a large number of billiard

7 Dispersion is the variation of a medium's refractive index with the wavelength of light, which makes a prism spread white light into colors.

8 The image of holding "both ends of the chain" comes from the theologian Bossuet (1627–1704), writing on how to reconcile free will with divine providence.

balls, well and good; that is verifiable, and perhaps it is true. But he means something more. And we think we understand him because we think we know what collision in itself is. Why? Simply because we have often watched games of billiards. Are we to understand that God, contemplating his work, has the same sensations that we have when watching a billiards match? If we do not wish to give his assertion this bizarre meaning, and if we also reject the restricted meaning I explained a moment ago, which is the right one, then it has no meaning at all.

4.10.12 Hypotheses of this kind therefore have only a metaphorical meaning. The scientist should no more forbid himself them than the poet forbids himself metaphors; but he must know what they are worth. They can be useful in giving satisfaction to the mind, and they will do no harm provided they are only indifferent hypotheses.

4.10.13 These considerations explain why certain theories, which were thought abandoned and definitively condemned by experiment, suddenly rise from their ashes and begin a new life. It is because they expressed true relations, and had not ceased to do so when, for one reason or another, we thought we had to state the same relations in another language. They had thus kept a sort of latent life.

4.10.14 Barely fifteen years ago, was there anything more ridiculous, more naively old-fashioned, than Coulomb's fluids? And yet here they are, reappearing under the name of *electrons*. In what way do these permanently electrified molecules differ from Coulomb's electric molecules? It is true that in electrons the electricity is carried by a little matter—but so little! In other words, they have mass (and now even that is being disputed). But Coulomb did not deny mass to his fluids, or if he did, it was only reluctantly.[9] It would be rash to assert that belief in electrons will never again suffer an eclipse; it was nonetheless curious to note this unexpected revival.

4.10.15 But the most striking example is Carnot's principle. Carnot established it starting from false hypotheses. When it

was realized that heat is not indestructible but can be transformed into work, his ideas were completely abandoned; then Clausius returned to them and made them triumph once and for all. Carnot's theory, in its original form, expressed true relations alongside other, inexact ones, remnants of old ideas; but the presence of the latter did not affect the reality of the former. Clausius had only to set them aside, as one prunes dead branches.[10]

4.10.16 The result was the second fundamental law of thermodynamics. It still expressed the same relations, although these relations no longer held, at least in appearance, between the same objects. That was enough for the principle to keep its value. And even Carnot's reasoning did not perish on that account. It was applied to subject matter tainted with error, but its form (that is, the essential thing) remained correct.

4.10.17 What I have just said throws light at the same time on the role of general principles such as the principle of least action or that of the conservation of energy.

4.10.18 These principles have a very high value. They were obtained by looking for what was common to the statements of numerous physical laws; they therefore represent something like the quintessence of countless observations.

4.10.19 However, from their very generality there follows a consequence to which I drew attention in Chapter VIII: they can no longer fail to 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. Well then, whatever new notions future experiments may give us about the world, we are sure in advance that there will be something that remains constant and that we can call *energy*.

4.10.20 Does this mean that the principle has no meaning and vanishes into a tautology? Not at all. It means that the different

9 Charles-Augustin de Coulomb (1736–1806), French physicist who treated electricity as consisting of two fluids made up of particles.

10 Sadi Carnot (1796–1832) reasoned in 1824 from the caloric theory, which held heat to be an indestructible fluid. Carnot's principle is the second law of thermodynamics.

things to which we give the name of *energy* are linked by a genuine kinship; it affirms a real relation between them. But then, if this principle has a meaning, it may be false. It may be that we have no right to extend its applications indefinitely, and yet it is certain in advance to be verified in the strict sense of the word. How, then, shall we be warned when it has reached the full extension that can legitimately be given to it? Quite simply, when it ceases to be useful to us—that is, to make us foresee new phenomena without misleading us. In such a case we shall be sure that the relation affirmed is no longer real, for otherwise it would be fruitful. Experiment, without directly contradicting a new extension of the principle, will nevertheless have condemned it.

4.10.21 Physics and mechanism.—Most theorists have a constant predilection for explanations borrowed from mechanics or dynamics. Some would be satisfied if they could account for all phenomena by the motions of molecules attracting one another according to certain laws. Others are more demanding: they would like to do away with attractions at a distance. Their molecules would follow straight-line paths, from which they could be deflected only by collisions. Others again, like Hertz, do away with forces as well, but suppose their molecules subject to geometric constraints analogous, for example, to those of our linkages; they thus want to reduce dynamics to a kind of kinematics.

4.10.22 In a word, all of them want to bend nature into a certain form, and outside that form their minds could not be satisfied. Will nature be flexible enough for that?

4.10.23 We shall examine the question in Chapter XII, in connection with Maxwell's theory. Whenever the principles of energy and of least action are satisfied, we shall see that a mechanical explanation is always possible, and also that there are always infinitely many. Thanks to a well-known theorem of Königs on linkages, one could show that everything can be explained, in infinitely many ways, by constraints in the manner of Hertz, or

else by central forces. No doubt one could just as easily prove that everything can always be explained by simple impacts.[11]

4.10.24 To do this, of course, we must not be satisfied with ordinary matter, the matter that falls within the reach of our senses and whose motions we observe directly. Either we shall suppose that this ordinary matter is made of atoms whose internal motions escape us, so that only the displacement of the whole remains accessible to our senses. Or else we shall imagine one of those subtle fluids which, under the name of *ether* or under other names, have always played so great a part in physical theories.

4.10.25 Often people go further and regard the ether as the only primitive matter, or even as the only true matter. The more moderate regard ordinary matter as condensed ether, which is not at all shocking. Others, however, reduce its importance still further and see in it no more than the geometric locus of the ether's singularities. For Lord Kelvin, for example, what we call *matter* is only the locus of the points where the ether is in vortex motion. For Riemann, it was the locus of the points where ether is constantly being destroyed. For other, more recent writers, Wiechert or Larmor, it is the locus of the points where the ether undergoes a kind of torsion of a quite special nature.[12] If we take one of these points of view, I wonder by what right we shall extend to the ether, on the pretext that it is true matter, the mechanical properties observed in ordinary matter, which is only false matter.

4.10.26 The old fluids—caloric, electricity, and so on—were abandoned when it was realized that heat is not indestructible. But they were abandoned for another reason too. By materializing them, we emphasized, so to speak, their individuality; we dug a kind of gulf between them. That gulf had to be filled when we gained a keener sense of the unity of nature and perceived the

11 Gabriel Koenigs (Königs) (1858–1931), French mathematician who worked on the kinematics of linkages.

12 Kelvin proposed his theory of vortex atoms in 1867. Emil Wiechert (1861–1928) was a German physicist; Joseph Larmor (1857–1942), an Irish physicist, wrote *Aether and Matter* (1900).

intimate relations that bind all its parts together. By multiplying fluids, the old physicists created entities without necessity, and they also broke real ties.

4.10.27 It is not enough for a theory not to assert false relations; it must not conceal true ones.

4.10.28 And our ether—does it really exist?

4.10.29 We know where our belief in the ether comes from. When light reaches us from a distant star, for several years it is no longer on the star and not yet on the earth. It must surely be somewhere in the meantime, held up, so to speak, by some material support.

4.10.30 The same idea can be expressed in a more mathematical and more abstract form. What we observe are the changes undergone by material molecules. We see, for example, that our photographic plate feels the consequences of phenomena that took place in the incandescent mass of the star several years earlier. Now, in ordinary mechanics, the state of the system under study depends only on its state at the immediately preceding instant; the system therefore satisfies differential equations. If, on the other hand, we did not believe in the ether, the state of the material universe would depend on the immediately preceding state and also on much earlier states; the system would satisfy finite-difference equations. It is to escape this departure from the general laws of mechanics that we invented the ether.

4.10.31 That would still oblige us only to fill interplanetary space with ether, not to make it penetrate into material media themselves. Fizeau's experiment goes further. Through the interference of rays that have passed through moving air or water, it seems to show us two different media interpenetrating and yet moving relative to each other. We seem to be touching the ether with our finger.[13]

4.10.32 Yet one can conceive of experiments that would let us touch it more closely still. Suppose that Newton's principle, the

13 In 1851 Hippolyte Fizeau measured the speed of light in flowing water and found that the moving water only partly drags the light along.

equality of action and reaction, were no longer true when applied to matter *alone*, and that this came to be established. The geometric sum of all the forces applied to all the material molecules would no longer be zero. If we did not want to change the whole of mechanics, we should then have to introduce the ether, so that the action that matter appeared to undergo would be balanced by the reaction of matter on something.

4.10.33 Or again, suppose it were found that optical and electrical phenomena are influenced by the motion of the earth. We should be led to conclude that these phenomena could reveal to us the relative motions of material bodies, and also what would seem to be their absolute motions. Here too there would have to be an ether, so that these so-called absolute motions would be displacements relative to something concrete rather than displacements relative to empty space.

4.10.34 Will we ever get that far? I have no such hope—I shall say why in a moment—and yet the hope is not so absurd, since others have had it.

4.10.35 For example, if Lorentz's theory, which I shall discuss in detail later, in Chapter XIII, were true, Newton's principle would not apply to matter *alone*, and the difference would not be very far from being accessible to experiment.[14]

4.10.36 On the other hand, much research has been done on the influence of the earth's motion. The results have always been negative. But if these experiments were undertaken, it was because no one was sure of the outcome in advance. Indeed, according to the prevailing theories, the compensation would be only approximate, and one ought to expect precise methods to give positive results.

4.10.37 I believe such a hope is illusory. It was nonetheless interesting to show that a success of this kind would open up, as it were, a new world for us.

4.10.38 And now I must be allowed a digression. I must explain why I do not believe, Lorentz notwithstanding, that more pre-

cise observations will ever be able to reveal anything but the relative displacements of material bodies. Experiments were made that should have detected the first-order terms, and the results were negative. Could that have been by chance? No one accepted that. A general explanation was sought, and Lorentz found it: he showed that the first-order terms must cancel, but that the same was not true of the second-order ones. Then more precise experiments were made, and they too were negative. This could not be the effect of chance either, so an explanation was needed. One was found—one always is; hypotheses are the one resource we never run short of.[15]

4.10.39 But that is not enough. Who does not feel that this still leaves too great a part to chance? Would it not also be chance, that strange coincidence by which one circumstance would arrive just in time to cancel the first-order terms, and another, quite different but just as opportune, would take care of canceling the second-order ones? No: we must find one and the same explanation for both. Everything then leads us to think that this explanation will hold equally for higher-order terms, and that the mutual cancellation of these terms will be rigorous and absolute.

THE PRESENT STATE OF SCIENCE

Physics moves both toward unity and toward complication; on balance it has drawn closer to unity.

4.10.40 The present state of science.—In the history of the development of physics two opposite tendencies can be distinguished. On the one hand, new links are constantly being discovered between objects that seemed destined to remain separate for

14 Hendrik Antoon Lorentz (1853–1928), Dutch physicist whose electron theory explained electrical and optical phenomena by charged particles moving through a stationary ether.

15 The more precise experiments are chiefly the Michelson–Morley experiment of 1887, explained by the contraction of moving bodies proposed by FitzGerald and Lorentz. The closing phrase echoes La Fontaine's fable "The Plowman and His Children": "c'est le fonds qui manque le moins."

ever. Scattered facts cease to be strangers to one another, and they tend to arrange themselves into an imposing synthesis. Science is moving toward unity and simplicity.

4.10.41 On the other hand, observation reveals new phenomena every day. They must wait a long time for their place, and sometimes, to make room for one, a corner of the building has to be torn down. Even in known phenomena, where our coarse senses showed us uniformity, we perceive details that grow more varied day by day. What we thought simple becomes complex again, and science seems to be moving toward variety and complication.

4.10.42 Of these two opposite tendencies, which seem to triumph by turns, which will win? If it is the first, science is possible. But nothing proves this *a priori*, and one may fear the following outcome. After vain efforts to bend nature against its will to our ideal of unity, and overwhelmed by the ever-rising flood of our new riches, we might have to give up classifying them, abandon our ideal, and reduce science to the recording of countless recipes.

4.10.43 This question we cannot answer. All we can do is observe the science of today and compare it with that of yesterday. From this examination we may no doubt draw some presumptions.

4.10.44 Half a century ago the highest hopes were entertained. The discovery of the conservation of energy and of its transformations had just revealed to us the unity of force. It thus showed that the phenomena of heat could be explained by molecular motions. What the nature of these motions was, no one knew exactly, but no one doubted that it would soon be known. For light, the task seemed completely accomplished. As for electricity, things were less advanced. Electricity had just annexed magnetism. That was a considerable step toward unity, and a definitive one. But how would electricity in its turn fit into the general unity? How would it be reduced to the universal mechanism? No one had any idea. Yet no one doubted that this reduction was possible; people had faith. Finally, as regards the molecular properties of material bodies, the reduction seemed

easier still, but all the detail remained in a fog. In a word, the hopes were vast, they were keen, but they were vague.

4.10.45 Today, what do we see?

4.10.46 First, an initial advance, and an immense one. The relations between electricity and light are now known. The three domains of light, electricity and magnetism, once separate, now form a single one, and this annexation seems definitive.

4.10.47 This conquest, however, has cost us some sacrifices. Optical phenomena now fall under electrical phenomena as special cases. As long as they stood alone, it was easy to explain them by motions we thought we knew in every detail; it all went smoothly. But now an explanation, to be acceptable, must extend effortlessly to the whole electrical domain. And that does not go without difficulty.

4.10.48 The most satisfactory thing we have is Lorentz's theory, which, as we shall see in the last chapter, explains electric currents by the motions of small electrified particles. It is undoubtedly the theory that best accounts for the known facts and brings to light the greatest number of true relations. It is also the one of which most traces will be found in the final construction. Nevertheless it still has a serious defect, which I pointed out above: it contradicts Newton's principle of the equality of action and reaction. Or rather, in Lorentz's eyes, this principle would not apply to matter alone. For it to be true, one would have to take into account the actions exerted by the ether on matter and the reaction of matter on the ether. Now, until further notice, it seems likely that things do not happen that way.

4.10.49 Be that as it may, Lorentz has linked several groups of results to one another and to the other properties of the ether: Fizeau's results on the optics of moving bodies, the laws of normal and anomalous dispersion, and those of absorption. These ties will undoubtedly never again be broken. See how easily the new Zeeman phenomenon found its place ready and waiting. It even helped to classify Faraday's magnetic rotation, which had resisted Maxwell's efforts. This ease clearly shows that Lorentz's

theory is not an artificial assemblage doomed to fall apart. It will probably have to be modified, but not destroyed.[16]

4.10.50 But Lorentz's only ambition was to embrace the whole of the optics and electrodynamics of moving bodies in a single framework; he made no claim to give a mechanical explanation of them. Larmor goes further. Keeping what is essential in Lorentz's theory, he grafts onto it, so to speak, MacCullagh's ideas on the direction of the motions of the ether. For him the velocity of the ether would have the same direction and the same magnitude as the magnetic force. This velocity is therefore known to us, since the magnetic force is accessible to experiment. However ingenious this attempt may be, the defect of Lorentz's theory remains, and is even aggravated. Action is not equal to reaction.[17] With Lorentz we did not know what the motions of the ether were. Thanks to this ignorance, we could suppose them to be such that, by compensating those of matter, they restored the equality of action and reaction. With Larmor we know the motions of the ether, and we can see that the compensation does not take place.

4.10.51 If Larmor has, in my opinion, failed, does that mean 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 admits infinitely many mechanical explanations. That is therefore the case with optical and electrical phenomena.

4.10.52 But that is not enough. For a mechanical explanation to be good, it must be simple. To choose it among all the possible ones, we must have reasons other than the necessity of making a choice. Well, a theory that meets this condition, and that can therefore be of some use, we do not yet have. Should we complain? That would be to forget what the goal is. The goal is not mechanism; the true, the only goal is unity.

16 The Zeeman effect (1896) is the splitting of spectral lines in a magnetic field. Faraday's magnetic rotation (1845) is the turning of the plane of polarization of light by a magnetic field.

17 James MacCullagh (1809–1847), Irish mathematician who developed a theory of the ether as an elastic medium.

4.10.53 We must therefore limit our ambition. Let us not try to formulate a mechanical explanation; let us be content to show that we could always find one if we wished. In this we have succeeded. The principle of the conservation of energy has received nothing but confirmations. A second principle has come to join it, that of least action, put into the form suited to physics. It too has always been verified, at least as far as reversible phenomena are concerned; these thus obey Lagrange's equations, that is, the most general laws of mechanics.

4.10.54 Irreversible phenomena are much more stubborn. Yet they too are falling into order and tending to fit into the unity; the light that has illuminated them has come to us from Carnot's principle. For a long time thermodynamics confined itself to the study of the expansion of bodies and their changes of state. For some time now it has grown bolder and has considerably widened its domain. To it we owe the theory of the battery and that of thermoelectric phenomena. There is no corner of physics it has not explored, and it has taken on chemistry itself. Everywhere the same laws reign. Everywhere, beneath the diversity of appearances, we find Carnot's principle again. Everywhere too we find that prodigiously abstract concept, entropy, which is as universal as that of energy and, like it, seems to cover a reality. Radiant heat seemed bound to escape it; recently it has been seen to bow to the same laws.

4.10.55 In this way new analogies are revealed to us, which often hold even in detail. Ohmic resistance resembles the viscosity of liquids; hysteresis would rather resemble the friction of solids. In every case, friction seems to be the model on which the most diverse irreversible phenomena are patterned, and this kinship is real and deep.

4.10.56 People have also sought a mechanical explanation, properly so called, of these phenomena. The phenomena hardly lent themselves to one. To find it, it was necessary to suppose that irreversibility is only an appearance, and that elementary phenomena are reversible and obey the known laws of dynamics. But the elements are extremely numerous and become more

and more intermingled. To our coarse eyes, therefore, everything seems to tend toward uniformity, that is, everything seems to move in the same direction, with no hope of return. Apparent irreversibility is thus only an effect of the law of large numbers. Only a being with infinitely subtle senses, like Maxwell's imaginary demon, could untangle this inextricable skein and turn the world back.[18]

4.10.57 This conception, which is connected with the kinetic theory of gases, has cost great efforts and has, on the whole, been rather unfruitful; it may become fruitful. This is not the place to examine whether it leads to contradictions, or whether it truly conforms to the real nature of things.

4.10.58 Let us mention, however, Gouy's original ideas on Brownian motion. According to this scientist, this strange motion would escape Carnot's principle. The particles it sets moving would be smaller than the meshes of that tightly woven skein. They would therefore be able to untangle it, and thereby make the world run against the current. One would think one saw Maxwell's demon at work.[19]

4.10.59 To sum up, phenomena known of old are being classified better and better, but new phenomena come claiming their place. Most of them, like Zeeman's phenomenon, have found it at once.

4.10.60 But we have cathode rays, X-rays, and the rays of uranium and radium. Here is a whole world that no one suspected. So many unexpected guests to find room for!

4.10.61 No one can yet foresee the place they will occupy. But I do not believe they will destroy the general unity; I believe rather that they will complete it. For, on the one hand, the new radiations seem linked to the phenomena of luminescence. They excite fluorescence, and they sometimes arise under the same conditions as fluorescence does.

18 In Maxwell's thought experiment (1867), a tiny being sorts fast molecules from slow ones and so would defeat the second law of thermodynamics.

19 Louis Georges Gouy (1854–1926), French physicist who in 1888 attributed Brownian motion to the agitation of molecules.

4.10.62 Nor are they unrelated to the causes that make a spark leap under the action of ultraviolet light.[20]

4.10.63 Finally, and above all, we think we can recognize in all these phenomena true ions—moving, it is true, at speeds incomparably greater than in electrolytes.

4.10.64 All this is very vague, but all of it will become clearer.

4.10.65 Phosphorescence and the action of light on the spark were somewhat isolated districts, and hence somewhat neglected by researchers. We may now hope that a new line will be built to improve their communications with science as a whole.

4.10.66 We are discovering new phenomena, and unforeseen aspects are also coming to light in those we thought we knew. In the free ether, the laws keep their majestic simplicity. But matter proper seems more and more complex. Whatever we say about it is only ever approximate, and at every moment our formulas demand new terms.

4.10.67 Nevertheless the frameworks are not broken. We had recognized relations between objects we thought simple, and these relations still hold between the same objects once we know their complexity; that alone is what matters. Our equations, it is true, become more and more complicated in order to follow more closely the complication of nature. But nothing has changed in the relations that allow us to deduce these equations from one another. In a word, the form of these equations has held out.

4.10.68 Take for example the laws of reflection. Fresnel had established them through a simple and attractive theory that experiment seemed to confirm. Since then, more precise research has shown that this verification was only approximate; it has revealed traces of elliptical polarization everywhere. But thanks to the support the first approximation gave us, the cause of these anomalies was found at once: the presence of a transition layer. And Fresnel's theory has survived in its essentials.

4.10.69 Only, one cannot help making a reflection: all these relations would have gone unnoticed if we had suspected from the

start the complexity of the objects they connect. It was said long ago: if Tycho had had instruments ten times more precise, there would never have been a Kepler, or a Newton, or astronomy. It is a misfortune for a science to be born too late, when the means of observation have become too perfect. That is what is happening today to physical chemistry. Its founders are hampered in their insights by the third and fourth decimal places; fortunately, they are men of robust faith.

4.10.70 The better we come to know the properties of matter, the more we see continuity reigning in it. Since the work of Andrews and van der Waals, we understand how the passage from the liquid to the gaseous state takes place, and we know that this passage is not abrupt. Likewise there is no gulf between the liquid and solid states. In the proceedings of a recent Congress, one could see a paper on the rigidity of liquids beside a memoir on the flow of solids.[21]

4.10.71 With this tendency simplicity no doubt loses. A phenomenon was represented by several straight lines, and these straight lines must now be joined by more or less complicated curves. In return, unity gains a great deal. Those clear-cut categories rested the mind, but they did not satisfy it.

4.10.72 Lastly, the methods of physics have invaded a new domain, that of chemistry, and physical chemistry has been born. It is still very young, but we can already see that it will allow us to link together phenomena such as electrolysis, osmosis and the motions of ions.

4.10.73 What shall we conclude from this rapid survey?

4.10.74 All things considered, we have drawn closer to unity. We have not gone as fast as was hoped fifty years ago, and we have not always taken the expected road; but in the end we have gained a great deal of ground.

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20 The effect of ultraviolet light on sparks was discovered by Heinrich Hertz in 1887; it is the photoelectric effect.

21 Thomas Andrews (1813–1885) discovered the critical point of gases (1869); Johannes Diderik van der Waals (1837–1923) gave an equation of state covering both liquids and gases.

CHAPTER XI: THE CALCULUS OF PROBABILITIES

Every scientific induction relies on probability, yet probability itself resists definition.

4.11.1 No doubt the reader will be surprised to find reflections on the calculus of probabilities at this point. What has it to do with the method of the physical sciences?

4.11.2 And yet the questions I am going to raise, without resolving them, arise naturally for the philosopher who wants to reflect on physics.

4.11.3 So much so that in the two preceding chapters I have several times been led to use the words probability and chance.

4.11.4 "Predicted facts," I said above, "can only be probable. However solidly founded a prediction may seem to us, we are never absolutely sure that experiment will not refute it. But the probability is often great enough for us to be content with it in practice."

4.11.5 And a little further on I added:

4.11.6 "Let us see what part the belief in simplicity plays in our generalizations. We have verified a simple law in a large number of particular cases; we refuse to admit that this agreement, so often repeated, is a mere effect of chance..."

4.11.7 Thus, in a host of circumstances, the physicist is in the same position as the gambler who reckons up his chances. Every time he reasons by induction, he makes use, more or less consciously, of the calculus of probabilities.

4.11.8 And that is why I am obliged to open a parenthesis and interrupt our study of method in the physical sciences. I must examine a little more closely what this calculus is worth and how much confidence it deserves.

4.11.9 The very name calculus of probabilities is a paradox. Probability, as opposed to certainty, is what we do not know—and how can we calculate what we do not know? Yet many eminent scientists have occupied themselves with this calculus, and

it cannot be denied that science has drawn some benefit from it. How are we to explain this apparent contradiction?

4.11.10 Has probability been defined? Can it even be defined? And if it cannot, how do we dare to reason about it? The definition, someone will say, is quite simple: the probability of an event is the ratio of the number of cases favorable to that event to the total number of possible cases.

4.11.11 A simple example will show how incomplete this definition is. I throw two dice. What is the probability that at least one of the two dice will come up six? Each die can show six different numbers, so the number of possible cases is 6×6=36. The number of favorable cases is 11, and the probability is 11/36.

4.11.12 That is the correct solution. But could I not just as well say this: the numbers shown by the two dice can form $(6 \times 7)/2 = 21$ different combinations? Of these combinations, 6 are favorable, and so the probability is 6/21.

4.11.13 Why is the first way of enumerating the possible cases more legitimate than the second? At any rate, our definition does not tell us.

4.11.14 We are therefore reduced to completing this definition by saying: "... to the total number of possible cases, provided these cases are equally probable." So here we are, reduced to defining the probable by the probable.

4.11.15 How shall we know that two possible cases are equally probable? Will it be by a convention? If we place an explicit convention at the start of each problem, all will be well. We shall then only have to apply the rules of arithmetic and algebra, and we shall carry the calculation through to the end without our result leaving any room for doubt. But as soon as we want to make the slightest application of it, we shall have to prove that our convention was legitimate. We shall then find ourselves once again facing the difficulty we thought we had evaded.

4.11.16 Will it be said that common sense is enough to tell us what convention to adopt? Alas! Bertrand amused himself by treating a simple problem: "What is the probability that a chord of a circle is longer than the side of the inscribed equilateral tri-

angle?" The illustrious mathematician adopted in turn two conventions that common sense seemed equally to impose. With one he found 1/2, with the other 1/3.[22]

4.11.17 The conclusion that seems to follow from all this is that the calculus of probabilities is a futile science. We would also have to distrust that obscure instinct we called common sense, which we asked to legitimize our conventions.

4.11.18 But we cannot subscribe to this conclusion either, for we cannot do without that obscure instinct. Without it science would be impossible; without it we could neither discover a law nor apply one. Have we the right, for example, to state Newton's law? No doubt many observations agree with it; but is that not a mere effect of chance? And how, besides, do we know whether this law, true for so many centuries, will still be true next year? To this objection you will find nothing to reply except: "That is highly improbable."

4.11.19 But let us grant the law. With its help I think I can calculate the position of Jupiter a year from now. Have I the right to? Who is to say that a gigantic mass, moving at enormous speed, will not pass near the solar system before then and produce unforeseen perturbations? Here again there is nothing to reply except: "That is highly improbable."

4.11.20 On this reckoning, all the sciences would be nothing but unconscious applications of the calculus of probabilities. To condemn this calculus would be to condemn the whole of science.

4.11.21 I shall dwell less on the scientific problems in which the intervention of the calculus of probabilities is more evident. Foremost among them is interpolation, in which, knowing a certain number of values of a function, we try to guess the intermediate values.

4.11.22 I shall also mention the celebrated theory of errors of observation, to which I shall return later. There is also the kinetic theory of gases, a well-known hypothesis in which each gas

22 Joseph Bertrand (1822–1900), French mathematician; the problem, from his *Calcul des probabilités* (1889), is now known as Bertrand's paradox.

molecule is supposed to describe an extremely complicated path. Yet in this theory, through the effect of large numbers, the average phenomena, which alone are observable, obey simple laws: those of Mariotte and Gay-Lussac.[23]

4.11.23 All these theories rest on the laws of large numbers, and if the calculus of probabilities fell, it would obviously bring them down with it. It is true that they are only of particular interest. Except for interpolation, these are sacrifices we might resign ourselves to.

4.11.24 But, as I said above, it would not be merely a matter of these partial sacrifices. It would be the legitimacy of science as a whole that would be called into question.

4.11.25 I can well see what might be said: "We are ignorant, and yet we must act. In order to act, we have no time to carry out an inquiry sufficient to dispel our ignorance; besides, such an inquiry would require infinite time. We must therefore decide without knowing. We have to do so haphazardly, and follow rules without believing in them too much. What I know is not that such and such a thing is true, but that the best thing for me is still to act as if it were true." The calculus of probabilities, and consequently science, would then have only a practical value.

4.11.26 Unfortunately the difficulty does not vanish so easily. A gambler wants to try a bet, and he asks my advice. If I give it, I shall draw on the calculus of probabilities, but I shall not guarantee him success. This is what I shall call *subjective probability*. In this case one could be content with the explanation I have just sketched. But suppose an observer watches the game and notes every play, and the game goes on for a long time. When he goes over his notebook, he will find that the events have been distributed in accordance with the laws of the calculus of probabilities. This is what I shall call *objective probability*, and it is this phenomenon that would have to be explained.

4.11.27 There are many insurance companies that apply the rules of the calculus of probabilities, and they distribute to their

23 Joseph Louis Gay-Lussac (1778–1850), French chemist; his law relates the volume of a gas to its temperature.

shareholders dividends whose objective reality cannot be disputed. To explain them, it is not enough to invoke our ignorance and the necessity of acting.

4.11.28 Thus absolute skepticism is out of place. We must be wary, but we cannot condemn wholesale; discussion is necessary.

4.11.29 I. Classification of problems of probability.—To classify the problems that arise in connection with probabilities, we can take several different points of view. The first is the *point of view of generality*. I said above that probability is the ratio of the number of favorable cases to the number of possible cases. What I call generality, for want of a better term, will increase with the number of possible cases. This number may be finite, as, for example, when we consider a throw of the dice in which the number of possible cases is 36. That is the first degree of generality.

4.11.30 But suppose we ask, for example, what the probability is that a point inside a circle is inside the inscribed square. There are then as many possible cases as there are points in the circle, that is, infinitely many. That is the second degree of generality. Generality can be pushed further still: we can ask the probability that a function satisfies a given condition. There are then as many possible cases as one can imagine different functions. That is the third degree of generality, to which we rise, for example, when we try to guess the most probable law from a finite number of observations.

4.11.31 We can take a quite different point of view. If we were not ignorant there would be no probability; there would be room only for certainty. But our ignorance cannot be absolute, for then there would be no probability either, since some light is still needed to arrive even at this uncertain science. Problems of probability can thus be classified according to the greater or lesser depth of this ignorance.

4.11.32 Even in mathematics we can set ourselves problems of probability. What is the probability that the fifth decimal of a logarithm taken at random from a table is a 9? No one will hesit-

ate to answer that this probability is 1/10. Here we possess all the data of the problem. We could calculate our logarithm without resorting to the table, but we do not want to take the trouble. That is the first degree of ignorance.

4.11.33 In the physical sciences our ignorance is already greater. The state of a system at a given instant depends on two things: its initial state and the law according to which that state varies. If we knew both this law and this initial state, we should have only a mathematical problem to solve, and we should fall back on the first degree of ignorance.

4.11.34 But it often happens that we know the law and do not know the initial state. We ask, for example, what the present distribution of the minor planets is. We know that they have always obeyed Kepler's laws, but we do not know what their initial distribution was.

4.11.35 In the kinetic theory of gases, we suppose that the gas molecules follow straight-line paths and obey the laws of impact of elastic bodies. But since we know nothing of their initial velocities, we know nothing of their present velocities.

4.11.36 Only the calculus of probabilities allows us to predict the average phenomena that will result from the combination of these velocities. That is the second degree of ignorance.

4.11.37 Finally, it is possible that the laws themselves are unknown, as well as the initial conditions. We then reach the third degree of ignorance, and in general we can no longer assert anything at all about the probability of a phenomenon.

4.11.38 It often happens that, instead of trying to guess an event from a more or less imperfect knowledge of the law, we know the events and try to guess the law. Instead of deducing effects from causes, we want to deduce causes from effects. These are the so-called problems of *probability of causes*, the most interesting from the point of view of their scientific applications.

4.11.39 I am playing écarté with a gentleman I know to be perfectly honest, and he is about to deal. What is the probability that he will turn up the king? It is 1/8; that is a problem of probability of effects. Now I am playing with a gentleman I do not

know. He has dealt 10 times and has turned up the king 6 times. What is the probability that he is a cardsharp? That is a problem of probability of causes.[24]

4.11.40 It may be called the essential problem of the experimental method. I have observed n values of x and the corresponding values of y; I have found that the ratio of the latter to the former is very nearly constant. That is the event; what is the cause?

4.11.41 Is it probable that there is a general law according to which y is proportional to x, and that the small discrepancies are due to errors of observation? This is the kind of question we are constantly led to ask, and we answer it unconsciously every time we do science.

4.11.42 I shall now review these different categories of problems, considering in turn what I called above subjective probability and what I called objective probability.

4.11.43 II. Probability in the mathematical sciences.—The impossibility of squaring the circle has been proved since 1883. But long before that recent date, all mathematicians considered this impossibility so "probable" that the Academy of Sciences rejected unexamined the papers, alas all too many, that a few unfortunate cranks sent it on the subject every year.[25]

4.11.44 Was the Academy wrong? Obviously not, and it knew perfectly well that in acting this way it ran no risk of stifling a serious discovery. It could not have proved that it was right, but it knew that its instinct was not deceiving it. Had you questioned the academicians, they would have answered: "We have compared the probability that an unknown scientist has found what has been sought in vain for so long with the probability that there is one more crank in the world; the second seemed to

24 Écarté is a two-player card game with a 32-card pack; the dealer turns up a card for trumps and scores a point if it is a king. Poincaré's word for a cheat, *grec*, was period slang for a cardsharp.

25 The proof came from Ferdinand von Lindemann, who showed in 1882 that π is transcendental, and hence that the circle cannot be squared with ruler and compass.

us greater." These are very good reasons, but there is nothing mathematical about them; they are purely psychological.

4.11.45 And had you pressed them further, they would have added: "Why should a particular value of a transcendental function be an algebraic number? And if π were a root of an algebraic equation, why should this root be a period of the function sin 2x, while the same is not true of the other roots of that equation?" In short, they would have invoked the principle of sufficient reason in its vaguest form.

4.11.46 But what could they draw from it? At most a rule of conduct for the use of their time, which was better spent on their ordinary work than on reading an elaborate effusion that rightly inspired their distrust. What I called above objective probability, however, has nothing to do with this first problem.

4.11.47 The second problem is another matter.

4.11.48 Let us consider the first 10,000 logarithms I find in a table. From these 10,000 logarithms I pick one at random; what is the probability that its third decimal is an even number? You will not hesitate to answer 1/2. And indeed, if you go through a table and note down the third decimals of these 10,000 numbers, you will find roughly as many even digits as odd ones.

4.11.49 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 to −1 otherwise. Then let us take the mean of these 10,000 numbers.

4.11.50 I shall not hesitate to say that the mean of these 10,000 numbers is probably zero, and if I actually worked it out, I would verify that it is very small.

4.11.51 But even this verification is unnecessary. I could have proved rigorously that this mean is less than 0.003. To establish this result I would have needed a rather long calculation, which has no place here; for it I simply refer the reader to an article I published in the *Revue générale des Sciences* on 15 April 1899. The only point to which I must draw attention is this: in that 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 considered, between certain limits.

4.11.52 Hence a first consequence: the property holds not only for the logarithm but for any continuous function whatever, since the derivatives of every continuous function are bounded.

4.11.53 If I was certain of the result in advance, it was first because I had often observed similar facts for other continuous functions; and second because, in my own mind, more or less unconsciously and imperfectly, I was carrying out the reasoning that led me to the inequalities above—like a practiced calculator who, before finishing a multiplication, realizes that "it's going to come to about so much."

4.11.54 Moreover, since what I called my intuition was only an incomplete glimpse of a genuine piece of reasoning, it is understandable that observation confirmed my predictions, and that objective probability agreed with subjective probability.

4.11.55 As a third example I shall choose the following problem. A number u is taken at random, and n is a given, very large integer; what is the probable value of sin nu? This problem has no meaning in itself. To give it one, a convention is needed: we shall *agree* that the probability that the number u lies between a and $a+da$ is equal to $\phi(a)da$; that it is therefore proportional to the size of the infinitely small interval da, and equal to that size multiplied by a function $\phi(a)$ depending only on a. As for this function, I choose it arbitrarily, but I must of course assume it to be continuous. Since the value of sin nu stays the same when u increases by 2π, I can, without loss of generality, assume that u lies between 0 and 2π, and I shall thus be led to assume that $\phi(a)$ is a periodic function with period 2π.

4.11.56 The probable value sought is easily 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 greatest value of the kth derivative of $\phi(u)$. We see, then, that if the kth derivative is finite, our probable value will tend to zero as n increases indefinitely, and will do so faster than $1/n^{k-1}$.

4.11.57 The probable value of sin *nu* for very large *n* is therefore zero. To define this value I needed a convention; but the result remains the same *whatever that convention may be*. I imposed only slight restrictions on myself in assuming that the function $\phi(a)$ is continuous and periodic, and these hypotheses are so natural that one wonders how they could be avoided.

4.11.58 Examining the three preceding examples, so different in every respect, 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 the physical sciences will lead us to the same result.

4.11.59 III. Probability in the physical sciences.—Let us now come to the problems that relate to what I called above the second degree of ignorance: those in which we know the law but do not know the initial state of the system. I could multiply examples; I shall take only one. What is the probable current distribution of the minor planets around the zodiac?[26]

4.11.60 We know that they obey Kepler's laws; we may even, without changing the nature of the problem, suppose that their orbits are all circular and lie in a single plane, and that we know this. On the other hand, we are completely ignorant of what their initial distribution was. Yet we do not hesitate to assert that today this distribution is roughly uniform. Why?

4.11.61 Let b be the longitude of a minor planet at the initial epoch, that is, at time zero; let a be its mean motion. Its longitude at the present epoch, that is, at time t, will be $at + b$. To say that the current distribution is uniform is to say that the mean value of the sines and cosines of the multiples of $at + b$ is zero. Why do we assert this?[27]

4.11.62 Let us represent each minor planet by a point in a plane, namely the point whose coordinates are precisely a and b. All these representative points will lie within a certain region of the

26 The minor planets are what we now usually call asteroids.

27 The mean motion is a planet's average angular speed along its orbit.

plane; but since they are very numerous, this region will appear peppered with points. Beyond that, we know nothing about the distribution of these points.

4.11.63 What do we do when we want to apply the calculus of probabilities to a question of this kind? What is the probability that one or more representative points lie in a given part of the plane? In our ignorance we are reduced to making an arbitrary hypothesis. To convey the nature of this hypothesis, let me use, instead of a mathematical formula, a crude but concrete image. Let us imagine that a fictitious substance has been spread over the surface of our plane, with a density that varies, but varies continuously. We shall then agree to say that the probable number of representative points on a part of the plane is proportional to the quantity of fictitious substance found there. If we then have two regions of the plane of equal size, the probabilities that a representative point of one of our minor planets lies in one or the other of these regions will be to each other as the mean densities of the fictitious substance in the two regions.

4.11.64 Here, then, are two distributions. One is real: in it the representative points are very numerous and very close together, but discrete, like the molecules of matter in the atomic hypothesis. The other is remote from reality: in it our representative points are replaced by a continuous fictitious substance. We know that the latter cannot be real, but our ignorance condemns us to adopt it.

4.11.65 If we at least had some idea of the real distribution of the representative points, we could arrange things so that, in a region of some size, the density of this continuous fictitious substance would be roughly proportional to the number of representative points—or, if you like, of atoms—contained in that region. Even that is impossible, and our ignorance is so great that we are forced to choose arbitrarily the function that defines the density of our fictitious substance. We shall be bound only by one hypothesis, which we could hardly escape: we shall assume that this function is continuous. That is enough, as we shall see, to allow us a conclusion.

4.11.66 What is the probable distribution of the minor planets at time t? Or again, what is the probable value of the sine of the longitude at time t, that is, of $\sin(at + b)$? At the outset we made an arbitrary convention; but if we adopt it, this probable value is completely defined. Let us divide the plane into surface elements. Let us take the value of $\sin(at + b)$ at the center of each of these elements, multiply this value by the area of the element and by the corresponding density of the fictitious substance, and then add up the results for all the elements of the plane. This sum will be, by definition, the probable mean value we are looking for, which will thus be expressed by a double integral.

4.11.67 One might think at first that this mean value will depend on the choice of the function ϕ that defines the density of the fictitious substance, and that, since this function ϕ is arbitrary, we could obtain any mean value we liked, depending on the arbitrary choice we made. Not at all.

4.11.68 A simple calculation shows that our double integral decreases very rapidly as t increases.

4.11.69 So I hardly knew what hypothesis to make about the probability of this or that initial distribution; but whatever hypothesis is made, the result will be the same, and that is what gets me out of my difficulty.

4.11.70 Whatever the function ϕ, the mean value tends to zero as t increases; and since the minor planets have certainly completed a very large number of revolutions, I can assert that this mean value is very small.

4.11.71 I may choose ϕ as I please, with one restriction, however: this function must be continuous. And indeed, from the standpoint of subjective probability, choosing a discontinuous function would have been unreasonable. What reason could I have, for example, for supposing that the initial longitude can be exactly 0°, but cannot lie between 0° and 1°?

4.11.72 But the difficulty reappears if we take the standpoint of objective probability—if we pass from our imaginary distribution, in which the fictitious substance was assumed continuous,

to the real distribution, in which our representative points form something like discrete atoms.

4.11.73 The mean value of sin(*at*+*b*) will be represented quite simply by: 1/n ∑ sin(at + b), *n* being the number of minor planets. Instead of a double integral over a continuous function, we have a sum of discrete terms. And yet no one will seriously doubt that this mean value is in fact very small.

4.11.74 The reason is that, since our representative points are very close together, our discrete sum will in general differ very little from an integral.

4.11.75 An integral is the limit toward which a sum of terms tends when the number of these terms increases indefinitely. If the terms are very numerous, the sum will differ very little from its limit, that is, from the integral, and what I have said of the integral will still hold for the sum itself.

4.11.76 There are exceptional cases, nevertheless. If, for example, we had for all the minor planets: b = π/2 − at, all the planets would have longitude π/2 at time *t*, and the mean value would obviously equal 1. For this to happen, the minor planets would all have had to be placed at epoch 0 on a sort of spiral of a peculiar shape, with extremely tightly wound turns. Everyone will judge such an initial distribution extremely improbable (and even supposing it had occurred, the distribution would not be uniform at the present epoch—on 1 January 1900, for example —but it would become uniform again a few years later).

4.11.77 Still, why do we judge this initial distribution improbable? This needs explaining, for if we had no reason to reject this outlandish hypothesis as implausible, everything would collapse, and we could no longer assert anything about the probability of this or that current distribution.

4.11.78 What we shall invoke is, once again, the principle of sufficient reason, to which we must always return. We could admit that the planets were originally distributed roughly in a straight line; we could admit that they were irregularly distributed. But it seems to us that there is no sufficient reason why the unknown cause that gave rise to them should have acted along a

curve so regular and yet so complicated—a curve that would seem to have been chosen precisely so that the current distribution would not be uniform.

4.11.79 IV. Red and black.—The questions raised by games of chance, such as roulette, are at bottom entirely analogous to those we have just dealt with.

4.11.80 For example, a dial is divided into a large number of equal sections, alternately red and black. A pointer is spun hard, and after making a great many turns, it stops in front of one of these sections. The probability that this section is red is obviously 1/2.

4.11.81 The pointer will turn through an angle θ amounting to several full turns. I do not know the probability that the pointer will be spun with such force that this angle lies between θ and θ + $d\theta$; but I can make a convention. I can suppose that this probability is $\phi(\theta)d\theta$. As for the function $\phi(\theta)$, I can choose it entirely arbitrarily; there is nothing to guide me in my choice. Yet I am naturally led to suppose this function continuous.

4.11.82 Let ε be the length (measured on the circle of radius 1) of each red or black section.

4.11.83 We must calculate the integral of $\phi(\theta)d\theta$, taking it on the one hand over all the red sections and on the other over all the black sections, and compare the results.

4.11.84 Consider an interval 2ε comprising a red section and the black section that follows it. Let M and m be the greatest and least values of the function $\phi(\theta)$ in this interval. The integral taken over the red sections will be less than $\sum M\varepsilon$; the integral taken over the black sections will be greater than $\sum m\varepsilon$; the difference will therefore be less than $\sum(M - m)\varepsilon$. But if the function φ is assumed continuous, and if, moreover, the interval ε is very small relative to the total angle traversed by the pointer, the difference $M - m$ will be very small. The difference between the two integrals will therefore be very small, and the probability will be very close to 1/2.

4.11.85 We can see why, without knowing anything about the function φ, I should act as if the probability were 1/2. We can

also see why, if I take the objective standpoint and observe a certain number of spins, observation will give me roughly as many black results as red ones.

4.11.86 All gamblers know this objective law; but it leads them into a curious error, one that has often been pointed out and into which they always fall again. When red has come up, say, six times in a row, they bet on black, thinking it a sure thing—because, they say, it is very rare for red to come up seven times in a row.

4.11.87 In reality their probability of winning remains 1/2. Observation does show, it is true, that runs of seven consecutive reds are very rare; but runs of six reds followed by a black are just as rare. They have noticed how rare runs of seven reds are; if they have not noticed how rare runs of six reds and a black are, it is solely because such runs are less striking.

THE PROBABILITY OF CAUSES

Inferring causes from effects requires a prior convention, and that convention rests on belief in continuity.

4.11.88 V. The probability of causes.—I come now to problems of the probability of causes, the most important from the standpoint of scientific applications. Suppose two stars are very close together on the celestial sphere. Is this apparent closeness a pure effect of chance—are these stars, though roughly on the same line of sight, at very different distances from the Earth, and therefore very far from each other? Or does it correspond to a real closeness? That is a problem of the probability of causes.

4.11.89 I recall first that at the start of all the problems of the probability of effects that have occupied us so far, we have always had to put a more or less justified convention. And if, more often than not, the result was to some extent independent of this convention, that was only on condition of certain hypotheses that allowed us to reject *a priori* discontinuous functions, for example, or certain outlandish conventions.

4.11.90 We shall find something similar when we deal with the probability of causes. An effect may be produced by cause A or by cause B. The effect has just been observed; we ask for the probability that it is due to cause A. This is the *a posteriori* probability of the cause. But I could not calculate it unless a more or less justified convention told me *in advance* the *a priori* probability that cause A would come into play—I mean the probability of that event for someone who had not yet observed the effect.

4.11.91 To explain myself better, I return to the example of the game of écarté mentioned above. My opponent deals for the first time and turns up the king; what is the probability that he is a cardsharp? The formulas usually taught give 8/9, an obviously very surprising result. If we look at them more closely, we see that the calculation is done as if, *before we sat down at the card table*, I had considered that there was one chance in two that my opponent was dishonest. An absurd hypothesis, since in that case I would certainly not have played with him; and that is what explains the absurdity of the conclusion.

4.11.92 The convention about the *a priori* probability was unjustified; that is why the calculation of the *a posteriori* probability led me to an unacceptable result. We see how important this prior convention is. I shall even add that if none were made, the problem of *a posteriori* probability would have no meaning; it must always be made, either explicitly or tacitly.

4.11.93 Let us turn to an example of a more scientific character. I want to determine an experimental law. This law, once I know it, can be represented by a curve. I make a certain number of separate observations; each of them will be represented by a point. Once I have obtained these various points, I draw a curve between them, trying to stay as close to them as possible while still giving my curve a regular shape—no corners, no overly sharp inflections, no abrupt changes in the radius of curvature. This curve will represent the probable law for me, and I take it that it gives me not only the values of the function intermediate between those observed, but also the observed values them-

selves more accurately than direct observation does (that is why I draw it near my points and not through the points themselves).

4.11.94 This is a problem of the probability of causes. The effects are the measurements I have recorded; they depend on the combination of two causes: the true law of the phenomenon and the errors of observation. Knowing the effects, we must find the probability that the phenomenon obeys a given law and that the observations were affected by a given error. The most probable law then corresponds to the curve drawn, and the most probable error of an observation is represented by the distance of the corresponding point from this curve.

4.11.95 But the problem would have no meaning if, before any observation, I did not form an *a priori* idea of the probability of this or that law, and of the chances of error to which I am exposed.

4.11.96 If my instruments are good (and I knew that before observing), I shall not allow my curve to stray far from the points that represent the raw measurements. If they are poor, I may move a little further away from them in order to get a less winding curve; I shall sacrifice more to regularity.

4.11.97 Why, then, do I try to draw a curve without twists and turns? Because I consider *a priori* a law represented by a continuous function (or by a function whose higher-order derivatives are small) as more probable than a law that does not satisfy these conditions. Without this belief, the problem we are discussing would have no meaning; interpolation would be impossible; no law could be deduced from a finite number of observations; science would not exist.

4.11.98 Fifty years ago, physicists considered a simple law 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. Today they have repudiated this belief; and yet how often are they obliged to act as if they still held it! Be that as it may, what remains of this tendency is the belief in

continuity, and we have just seen that if this belief disappeared in its turn, experimental science would become impossible.[28]

4.11.99 VI. The theory of errors.—We are thus led to speak of the theory of errors, which is directly connected with the problem of the probability of causes. Here again we observe *effects*, namely a certain number of discordant observations, and we try to guess the *causes*, which are, on the one hand, the true value of the quantity to be measured and, on the other, the error made in each separate observation. We would need to calculate the probable *a posteriori* magnitude of each error, and hence the probable value of the quantity to be measured.

4.11.100 But, as I have just explained, this calculation could not be undertaken unless we accepted *a priori*, that is, before any observation, a law of probability of errors. Is there a law of errors?

4.11.101 The law of errors accepted by all calculators is Gauss's law, which is represented by a certain transcendental curve known as the "bell curve."[29]

4.11.102 But first we should recall the classic distinction between systematic and accidental errors. If we measure a length with a meter stick that is too long, we shall always get a figure that is too small, and repeating the measurement several times will be of no use; that is a systematic error. If we measure it with an accurate meter stick, we may still make mistakes, but we shall err sometimes too high, sometimes too low, and when we take the mean of a large number of measurements, the error will tend to diminish. These are accidental errors.

4.11.103 It is evident, first, that systematic errors cannot satisfy Gauss's law; but do accidental errors satisfy it? A great many proofs have been attempted; almost all are crude fallacies. Gauss's law can nevertheless be proved starting from the following hypotheses: the error made is the resultant of a very large

28 Henri Victor Regnault (1810–1878), French physicist who measured how real gases depart from Mariotte's law.

29 Carl Friedrich Gauss (1777–1855), German mathematician; the law is today usually called the normal distribution.

number of partial and independent errors; each of the partial errors is very small and otherwise obeys any law of probability whatever, except that the probability of a positive error is the same as that of an equal error of opposite sign. It is evident that these conditions will often be met, but not always, and we may reserve the name accidental for the errors that satisfy them.

4.11.104 We see that the method of least squares is not legitimate in every case; in general, physicists distrust it more than astronomers do. This is no doubt because astronomers, besides the systematic errors they meet with just as physicists do, have to contend with an extremely important source of error that is entirely accidental: I mean atmospheric undulations. So it is very curious to hear a physicist arguing with an astronomer about a method of observation. The physicist, convinced that one good measurement is worth more than many bad ones, is concerned above all with eliminating, by dint of precautions, the last systematic errors; and the astronomer replies: "But that way you will be able to observe only a small number of stars; the accidental errors will not disappear."

4.11.105 What should we conclude? Should we go on applying the method of least squares? We must draw a distinction. We have eliminated all the systematic errors we could suspect; we know very well that some remain, but we cannot find them. Still, we have to make a decision and adopt a definitive value, which will be regarded as the probable value; and for that, it is evident that the best we can do is to apply Gauss's method. We have merely applied a practical rule relating to subjective probability. There is nothing to object to.

4.11.106 But people want to go further and assert not only that the probable value is so much, but that the probable error in the result is so much. *That is absolutely illegitimate.* It would be true only if we were sure that all systematic errors had been eliminated, and we know absolutely nothing about that. Suppose we have two series of observations; applying the rule of least squares, we find that the probable error for the first series is half as large as for the second. The second series may nonetheless be

better than the first, because the first may be affected by a large systematic error. All we can say is that the first series is *probably* better than the second, since its accidental error is smaller, and we have no reason to assert that the systematic error is greater for one series than for the other, our ignorance on this point being absolute.

4.11.107 VII. Conclusions.—In the preceding pages I have posed many problems without solving any of them. Yet I do not regret having written them, for they may perhaps invite the reader to reflect on these delicate questions.

4.11.108 Be that as it may, certain points seem well established. To undertake any calculation of probability whatever—and even for such a calculation to have a meaning—we must accept, as a starting point, a hypothesis or a convention that always involves a certain degree of arbitrariness. In choosing this convention we can be guided only by the principle of sufficient reason. Unfortunately, this principle is very vague and very elastic, and in the quick survey we have just made, we have seen it take many different forms. The form in which we have met it most often is the belief in continuity—a belief that would be hard to justify by apodictic reasoning, but without which all science would be impossible. Finally, the problems to which the calculus of probabilities can usefully be applied are those in which the result is independent of the hypothesis made at the outset, provided only that this hypothesis satisfies the condition of continuity.

CHAPTER XII: OPTICS AND ELECTRICITY

Fresnel's optics survives within Maxwell's theory, which proves that a mechanical explanation is possible.

4.12.1 Fresnel's theory.—The best example[30] we could choose is the theory of light and its relations with the theory of electricity.

30 POINCARÉ'S NOTE. This chapter partly reproduces the prefaces of two of my works: *Théorie mathématique de la lumière* (Paris, Naud, 1889) and *Électricité et optique* (Paris, Naud, 1901).

TRANSLATOR'S NOTE. The titles mean "Mathematical Theory of Light" and "Electricity and Optics."

Thanks to Fresnel, optics is the most advanced branch of physics; the so-called wave theory forms a whole that is truly satisfying to the mind. But we must not ask of it what it cannot give us.

4.12.2 The aim of mathematical theories is not to reveal to us the true nature of things; that would be an unreasonable pretension. Their sole purpose is to coordinate the physical laws that experiment makes known to us, but which, without the help of mathematics, we could not even state.

4.12.3 It matters little to us whether the ether really exists; that is the metaphysicians' business. What matters to us is that everything happens as if it existed, and that this hypothesis is convenient for explaining phenomena. After all, have we any other reason for believing in the existence of material objects? That too is only a convenient hypothesis; only it will never cease to be so, whereas a day will no doubt come when the ether is discarded as useless.

4.12.4 But even on that day, the laws of optics and the equations that express them analytically will remain true, at least as a first approximation. It will therefore always be useful to study a doctrine that links all these equations together.

4.12.5 The wave theory rests on a molecular hypothesis. For some, who believe they thereby uncover the cause beneath the law, this is an advantage; for others, it is a reason for distrust. But this distrust seems to me as little justified as the illusion of the former.

4.12.6 These hypotheses play only a secondary role. They could be sacrificed; this is not usually done because the exposition would lose in clarity, but that is the only reason.

4.12.7 Indeed, if we looked 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, which is the general law of small motions, as of all small variations.

4.12.8 This explains why most of Fresnel's conclusions survive unchanged when we adopt the electromagnetic theory of light.

4.12.9 Maxwell's theory.—It was Maxwell, as we know, who bound together closely two branches of physics until then entirely foreign to each other: optics and electricity. In merging thus into a larger whole, a higher harmony, Fresnel's optics has not ceased to be alive. Its various parts survive, and their mutual relations are still the same. Only the language we use to express them has changed; and, besides, Maxwell has revealed to us other relations, unsuspected before him, between the different parts of optics and the domain of electricity.

4.12.10 The first time a French reader opens Maxwell's book, a feeling of unease, and often even of distrust, mingles at first with his admiration. Only after prolonged acquaintance, and at the cost of much effort, does this feeling fade. Some eminent minds even retain it permanently.

4.12.11 Why do the English scientist's ideas have so much trouble taking root among us? No doubt because the education received by most educated French people disposes them to value precision and logic above every other quality.

4.12.12 The older theories of mathematical physics gave us complete satisfaction in this respect. All our masters, from Laplace to Cauchy, proceeded in the same way. Starting from clearly stated hypotheses, they deduced all their consequences with mathematical rigor, and then compared them with experiment. They seem to have wanted to give each branch of physics the same precision as celestial mechanics.[31]

4.12.13 For a mind accustomed to admiring such models, a theory is seldom satisfying. Not only will it not tolerate the slightest appearance of contradiction, but it will demand that the various parts be logically connected with one another, and that the number of distinct hypotheses be reduced to a minimum.

4.12.14 That is not all; it will make further demands, which seem to me less reasonable. Behind the matter that our senses reach and that experiment makes known to us, it will want to see another matter, the only true one in its eyes, which will have only purely geometric qualities, and whose atoms will be noth-

31 Augustin-Louis Cauchy (1789–1857), French mathematician.

ing but mathematical points subject solely to the laws of dynamics. And yet, by an unconscious contradiction, it will try to picture these invisible, colorless atoms, and so to bring them as close as possible to ordinary matter.

4.12.15 Only then will it be fully satisfied and imagine that it has penetrated the secret of the universe. If this satisfaction is deceptive, it is no less painful to give it up.

4.12.16 Thus, on opening Maxwell, a Frenchman expects to find a theoretical whole as logical and as precise as the physical optics founded on the ether hypothesis. He is thereby preparing for himself a disappointment that I should like to spare the reader by warning him at once of what he should look for in Maxwell, and of what he cannot find there.

4.12.17 Maxwell does not give a mechanical explanation of electricity and magnetism; he confines himself to proving that such an explanation is possible.

4.12.18 He also shows that optical phenomena are only a special case of electromagnetic phenomena. From any theory of electricity, then, a theory of light can be deduced immediately.

4.12.19 Unfortunately, the converse is not true: from a complete explanation of light it is not always easy to derive a complete explanation of electrical phenomena. It is not easy, in particular, if we want to start from Fresnel's theory. It would doubtless not be impossible; but we are nonetheless led to wonder whether we shall be forced to give up admirable results that we thought permanently secured. That looks like a step backward, and many sound minds refuse to resign themselves to it.

4.12.20 Once the reader has agreed to limit his hopes, he will still run into other difficulties. The English scientist does not try to build a single, definitive, well-ordered edifice. He seems rather to raise a great number of provisional and independent structures, between which communication is difficult and sometimes impossible.

4.12.21 Take as an example the chapter in which electrostatic attractions are explained by pressures and tensions supposed to prevail in the dielectric medium. This chapter could be removed

without the rest of the volume becoming any less clear or less complete. On the other hand, it contains a theory that is self-sufficient, and one could understand it without having read a single line of what precedes or follows. But it is not merely independent of the rest of the work; it is hard to reconcile with the book's fundamental ideas. Maxwell does not even attempt this reconciliation; he merely says: "I have not been able to make the next step, namely, to account by mechanical considerations for these stresses in the dielectric."[32]

4.12.22 This example will be enough to make my meaning clear; I could cite many others. Thus, reading the pages devoted to magnetic rotatory polarization, who would suspect that optical and magnetic phenomena are identical?

4.12.23 We must not, then, flatter ourselves that we can avoid all contradiction; we must simply accept it. Two contradictory theories can in fact both be useful instruments of research, provided we do not mix them and do not look in them for the underlying nature of things. And perhaps Maxwell would be less suggestive to read if he had not opened up so many new and divergent paths for us.

4.12.24 But in this way the fundamental idea is somewhat obscured. So much so that in most popular accounts it is the one point that is left out entirely.

4.12.25 To bring out its importance more clearly, I therefore think I ought to explain what this fundamental idea consists of. But for that a short digression is necessary.

MECHANICAL EXPLANATION

If a phenomenon satisfies the principle of least action, it admits infinitely many mechanical explanations.

4.12.26 On the mechanical explanation of physical phenomena.—In every physical phenomenon there are a certain number of para-

32 From Maxwell's *Treatise on Electricity and Magnetism* (1873), which Poincaré quotes in English.

meters that experiment reaches directly and allows us to measure. I shall call them the parameters q.

4.12.27 Observation then shows us the laws by which these parameters vary. These laws can generally be put in the form of differential equations linking the parameters q with one another and with time.

4.12.28 What must we do to give a mechanical interpretation of such a phenomenon?

4.12.29 We shall try to explain it either by the motions of ordinary matter or by those of one or more hypothetical fluids.

4.12.30 These fluids will be regarded as made up of a very large number of separate molecules m.

4.12.31 When, then, shall we say that we have a complete mechanical explanation of the phenomenon? It will be, on the one hand, when we know the differential equations satisfied by the coordinates of these hypothetical molecules m—equations which must, moreover, conform to the principles of dynamics. It will be, on the other hand, when we know the relations that define the coordinates of the molecules m as functions of the parameters q, which are accessible to experiment.

4.12.32 These equations, as I have said, must conform to the principles of dynamics, and in particular to the principle of the conservation of energy and the principle of least action.

4.12.33 The first of these two principles tells us that the total energy is constant and that this energy is divided into two parts:

4.12.34 1. The kinetic energy, or vis viva, which depends on the masses of the hypothetical molecules m and on their velocities, and which I shall call T;

4.12.35 2. And the potential energy, which depends only on the coordinates of these molecules and which I shall call U. It is the *sum* of the two energies T and U that is constant.

4.12.36 Now what does the principle of least action teach us? It teaches us that the system must pass from the initial situation it occupies at the instant t_0 to the final situation it occupies at the instant t_1 by a particular path. Over the interval of time between the two instants t_0 and t_1, this path must make the mean value of

the "action" (that is, of the *difference* between the two energies T and U) as small as possible. The first of the two principles is, moreover, a consequence of the second.

4.12.37 If we know the two functions T and U, this principle is enough to determine the equations of motion.

4.12.38 Among all the paths by which one can pass from one situation to another, there is obviously one for which the mean value of the action is smaller than for all the others. There is, moreover, only one. It follows that the principle of least action is enough to determine the path followed, and consequently the equations of motion.

4.12.39 In this way we obtain what are called Lagrange's equations.

4.12.40 In these equations the independent variables are the coordinates of the hypothetical molecules m. But now suppose we take as variables the parameters q that are directly accessible to experiment.

4.12.41 The two parts of the energy will then have to be expressed as functions of the parameters q and their derivatives; this will obviously be the form in which they appear to the experimenter. He will naturally try to define the potential energy and the kinetic energy by means of quantities he can observe directly[33].

4.12.42 This being granted, the system will always go from one situation to another by a path such that the mean action is a minimum.

4.12.43 It does not matter that T and U are now expressed by means of the parameters q and their derivatives. It does not matter that we also define the initial and final situations by means of these parameters. The principle of least action still holds.

4.12.44 Now here again, of all the paths leading from one situation to another, there is one for which the mean action is a minimum, and only one. The principle of least action is therefore

33 POINCARÉ'S NOTE. We may add that U will depend only on the q, and that T will depend on the q and their derivatives with respect to time and will be a homogeneous polynomial of the second degree in these derivatives.

enough to determine the differential equations that define the variations of the parameters q.

4.12.45 The equations so obtained are another form of Lagrange's equations.

4.12.46 To form these equations, we do not need to know the relations linking the parameters q to the coordinates of the hypothetical molecules. Nor do we need the masses of these molecules, or the expression of U as a function of their coordinates. All we need to know is the expression of U as a function of the q, and that of T as a function of the q and their derivatives. In other words, we need the expressions of the kinetic energy and the potential energy as functions of the experimental data.

4.12.47 Then one of two things holds. Either, for a suitable choice of the functions T and U, Lagrange's equations, constructed as we have just described, will be identical with the differential equations deduced from experiment. Or else there will exist no functions T and U for which this agreement occurs. In the latter case it is clear that no mechanical explanation is possible.

4.12.48 The *necessary* condition for a mechanical explanation to be possible is therefore that we can choose the functions T and U so as to satisfy the principle of least action, which entails that of the conservation of energy.

4.12.49 This condition is, moreover, *sufficient*. Suppose, in fact, that we have found a function U of the parameters q which represents one of the parts of the energy. Suppose that another part of the energy, which we shall represent by T, is a function of the q and their derivatives, and is a homogeneous polynomial of the second degree in these derivatives. And suppose, finally, that Lagrange's equations formed with these two functions T and U agree with the data of experiment.

4.12.50 What is needed to deduce a mechanical explanation from this? U must be capable of being regarded as the potential energy of a system, and T as the vis viva of that same system.

4.12.51 There is no difficulty as far as U is concerned; but can T be regarded as the vis viva of a material system?

4.12.52 It is easy to show that this is always possible, and even in infinitely many ways. For further details I shall merely refer to the preface of my book *Électricité et optique*.

4.12.53 Thus, if the principle of least action cannot be satisfied, no mechanical explanation is possible. If it can be satisfied, there is not just one explanation but infinitely many. It follows that as soon as there is one, there are infinitely many others.

4.12.54 One more remark.

4.12.55 Among the quantities that experiment lets us reach directly, we shall regard some as functions of the coordinates of our hypothetical molecules; these will be our parameters q. We shall consider the others as depending not only on the coordinates but also on the velocities—or, what comes to the same thing, on the derivatives of the parameters q. Alternatively, we shall treat them as combinations of these parameters and their derivatives.

4.12.56 And then a question arises: among all these experimentally measured quantities, which shall we choose to represent the parameters q? Which shall we prefer to regard as the derivatives of these parameters? This choice remains arbitrary to a very large extent. But for a mechanical explanation to be possible, it is enough that the choice can be made so as to stay in agreement with the principle of least action.

4.12.57 And so Maxwell asked himself whether he could make this choice, and that of the two energies T and U, so that electrical phenomena would satisfy this principle. Experiment shows us that the energy of an electromagnetic field breaks down into two parts, electrostatic energy and electrodynamic energy. Suppose we regard the first as representing the potential energy U and the second as representing the kinetic energy T. Suppose, moreover, that we consider the electrostatic charges of the conductors as parameters q and the intensities of the currents as derivatives of other parameters q. Under these conditions, I say, Maxwell recognized that electrical phenomena satisfy the principle of least action. From then on he was sure that a mechanical explanation was possible.

4.12.58 Had he set out this idea at the beginning of his book instead of relegating it to a corner of the second volume, it would not have escaped most readers.

4.12.59 If, then, a phenomenon admits of one complete mechanical explanation, it will admit of infinitely many others. All of them will account equally well for every particular revealed by experiment.

4.12.60 And this is confirmed by the history of every branch of physics. In optics, for example, Fresnel believes the vibration to be perpendicular to the plane of polarization; Neumann regards it as parallel to that plane. For a long time people looked for an "experimentum crucis" that would decide between these two theories, and none could be found.[34]

4.12.61 Likewise, without leaving the domain of electricity, we can see that the two-fluid theory and the one-fluid theory account equally satisfactorily for all the laws observed in electrostatics.

4.12.62 All these facts are easily explained by the properties of Lagrange's equations that I have just recalled.

4.12.63 It is now easy to understand what Maxwell's fundamental idea is.

4.12.64 To prove that a mechanical explanation of electricity is possible, we need not trouble ourselves to find that explanation itself. It is enough for us to know the expressions of the two functions T and U, which are the two parts of the energy. We then form Lagrange's equations from these two functions and compare these equations with the experimental laws.

4.12.65 Among all these possible explanations, how are we to choose, when experiment can give us no help? A day will perhaps come when physicists lose interest in these questions, which are beyond the reach of positive methods, and leave them to the metaphysicians. That day has not come; man does not so easily resign himself to remaining forever ignorant of the underlying nature of things.

4.12.66 Our choice, then, can be guided only by considerations in which personal judgment plays a very large part. There are, however, solutions that everyone will reject because of their oddity, and others that everyone will prefer because of their simplicity.

4.12.67 As regards electricity and magnetism, Maxwell refrains from making any choice. It is not that he systematically scorns everything beyond the reach of positive methods; the time he devoted to the kinetic theory of gases is proof enough of that. I will add that, although his great work develops no complete explanation, he had earlier attempted to give one in an article in the *Philosophical Magazine*. The strangeness and complexity of the hypotheses he had been obliged to make later led him to abandon it.[35]

4.12.68 The same spirit is found throughout the work. What is essential—that is, what must remain common to all theories—is brought to light. Whatever would suit only one particular theory is almost always passed over in silence. The reader thus finds himself facing a form almost empty of matter, which he is at first tempted to take for a fleeting, elusive shadow. But the efforts to which he is thus condemned force him to think. In the end he understands what was often a little artificial in the theoretical constructions he used to admire.

CHAPTER XIII: ELECTRODYNAMICS

The theories of Ampère and Helmholtz falter over open currents, which Maxwell abolishes.

4.13.1 The history of electrodynamics is especially instructive from our point of view.

4.13.2 Ampère entitled his immortal work "Théorie des phénomènes électrodynamiques, *uniquement* fondée sur l'expérience" (Theory of electrodynamic phenomena, founded

34 Franz Ernst Neumann (1798–1895), German physicist.

35 Maxwell's paper "On Physical Lines of Force" (1861–62), which proposed a mechanical model of the field built from vortices and "idle wheels."

solely on experiment). He imagined, then, that he had made *no* hypothesis. Yet he had made some, as we shall soon see; only he had made them without noticing.[36]

4.13.3 Those who came after him, on the contrary, did notice them, because their attention was drawn to the weak points of Ampère's solution. They made new hypotheses, and this time they were fully aware of them. But how many times these had to be changed before reaching today's classical system, which is perhaps not yet final—that is what we are about to see.

4.13.4 I. Ampère's theory.—When Ampère studied the mutual actions of currents experimentally, he worked, and could only work, with closed currents.

4.13.5 Not that he denied the possibility of open currents. If two conductors are charged with electricity of opposite kinds and are connected by a wire, a current is set up running from one to the other. It lasts until the two potentials have become equal. According to the ideas prevailing in Ampère's time, this was an open current: one could clearly see the current going from the first conductor to the second, but one did not see it come back from the second to the first.

4.13.6 Thus Ampère regarded currents of this kind as open—for example, the discharge currents of capacitors. But he could not make them the subject of his experiments, because they last too short a time.

4.13.7 One can also imagine another kind of open current. Suppose two conductors, A and B, connected by a wire AMB. Small moving conducting bodies first come into contact with conductor B and take a charge of electricity from it. They leave contact with B and set off along the path BNA. Carrying their charge with them, they come into contact with A and give up their charge to it. The charge then returns to B along the wire AMB.

4.13.8 In one sense we do indeed have a closed circuit here, since the electricity travels round the closed circuit BNAMB. But the two parts of this current are very different. In the wire AMB the electricity moves *through* a fixed conductor, as in a voltaic

36 Ampère's memoir appeared in 1827.

current, overcoming an ohmic resistance and generating heat; we say it moves *by conduction*. In the part BNA the electricity is *carried* by a moving conductor; we say it moves *by convection*.

4.13.9 If, then, the convection current is regarded as entirely analogous to the conduction current, the circuit BNAMB is closed. If, on the contrary, the convection current is not "a true current" and, for example, does not act on magnets, there remains only the conduction current AMB, which is *open*.

4.13.10 For example, if the two poles of a Holtz machine are joined by a wire, the charged rotating disk carries electricity by convection from one pole to the other. The electricity then returns to the first pole by conduction through the wire.[37]

4.13.11 But currents of this kind are very hard to produce with an appreciable intensity. With the means at Ampère's disposal, one may say it was impossible.

4.13.12 In short, Ampère could conceive of the existence of two kinds of open current, but he could work with neither, because they were too intense or because they lasted too short a time.[38]

4.13.13 Experiment could therefore show him only the action of a closed current on a closed current, or at most the action of a closed current on a portion of a current. This is because a current can be made to run through a *closed* circuit composed of a moving part and a fixed part. One can then study the displacements of the moving part under the action of another closed current.

4.13.14 On the other hand, Ampère had no means of studying the action of an open current either on a closed current or on another open current.

4.13.15 1. *The case of closed currents*.—In the case of the mutual action of two closed currents, experiment revealed to Ampère some remarkably simple laws.

37 An electrostatic influence machine devised by the German physicist Wilhelm Holtz in 1865.

38 The French reads "trop intenses" (too intense), evidently a slip for "trop peu intenses" (too weak), as the previous paragraph shows.

4.13.16 I shall briefly recall here those that will be useful to us later.

4.13.17 1. *Suppose the intensity of the currents is kept constant*, and the two circuits, after undergoing any displacements and deformations whatever, finally return to their initial positions. Then the total work of the electrodynamic actions will be zero.

4.13.18 In other words, there is an *electrodynamic potential* of the two circuits, proportional to the product of the intensities and depending on the shape and relative position of the circuits. The work of the electrodynamic actions is equal to the change in this potential;

4.13.19 2. The action of a closed solenoid is zero;

4.13.20 3. The action of a circuit C on another voltaic circuit C′ depends only on the "magnetic field" produced by this circuit C. At each point of space one can, in fact, define in magnitude and direction a certain force called the *magnetic force*, which has the following properties:

4.13.21 *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;

4.13.22 *b*) A very short magnetized needle tends to align itself with the direction of the magnetic force. The couple tending to bring it back to that direction is proportional to the product of the magnetic force, the magnetic moment of the needle, and the sine of the angle of deviation;

4.13.23 *c*) If the circuit C′ is displaced, the work of the electrodynamic action exerted by C on C′ will be equal to the increase in the "flux of magnetic force" passing through this circuit.

4.13.24 2. *Action of a closed current on a portion of a current*.—Since Ampère had been unable to produce an open current in the strict sense, he had only one way of studying the action of a closed current on a portion of a current.

4.13.25 This was to work with a circuit C′ made up of two parts, one fixed, the other movable. The movable part was, for example, a movable wire α β whose ends α and β could slide along a fixed wire. In one of the positions of the movable wire,

the end α rested on the point A of the fixed wire and the end β on the point B of the fixed wire. The current flowed from α to β, that is, from A to B along the movable wire. It then returned from B to A along the fixed wire. *This current was therefore closed*.

4.13.26 In a second position, once the movable wire had slid, the end α rested on another point A′ of the fixed wire and the end β on another point B′ of the fixed wire. The current then flowed from α to β, that is, from A′ to B′ along the movable wire. It then returned from B′ to B, then from B to A, and finally from A to A′, still following the fixed wire. So the current was still closed.

4.13.27 If such a circuit is subjected to the action of a closed current C, the movable part will be displaced as though a force were acting on it. This apparent force, to which the movable part AB thus seems subject, represents the action of C on the portion αβ of the current. Ampère *assumes* that it is the same as if αβ carried an open current stopping at α and at β. In reality, it carries a closed current which, after reaching β, returns to α through the fixed part of the circuit.

4.13.28 This hypothesis may seem fairly natural, and Ampère makes it without noticing. Nevertheless *it is not forced upon us*, since we shall see later that Helmholtz rejected it. However that may be, it allowed Ampère to state laws for the action of a closed current on an open current, or even on a current element, though he was never able to produce an open current.

4.13.29 The laws remain simple:

4.13.30 1. The force acting on a current element is applied to that element. It is normal to the element and to the magnetic force, and proportional to the component of that magnetic force which is normal to the element;

4.13.31 2. The action of a closed solenoid on a current element is still zero.

4.13.32 But there is no longer an electrodynamic potential. That is to say, when a closed current and an open current return to their initial positions, their intensities having been kept constant, the total work is not zero.

4.13.33 3. *Continuous rotations*.—Among electrodynamic experiments, the most curious are those in which continuous rotations have been produced; they are sometimes called experiments in *unipolar induction*. A magnet can turn about its axis. A current first runs through a fixed wire and enters the magnet at the N pole, say. It runs through half the magnet, leaves it through a sliding contact, and goes back into the fixed wire.

4.13.34 The magnet then goes into continuous rotation, without ever being able to reach a position of equilibrium. This is Faraday's experiment.[39]

4.13.35 How is this possible? Suppose we were dealing with two circuits of invariable shape, one, C, fixed, the other, C′, movable about an axis. The latter could never take up a continuous rotation. For there is an electrodynamic potential, and so there will necessarily be a position of equilibrium—the one in which this potential is a maximum.

4.13.36 Continuous rotations are therefore possible only if the circuit C′ consists of two parts, one fixed, the other movable about an axis, as is the case in Faraday's experiment. Even then a distinction must be made. The passage from the fixed part to the movable part, or the reverse, may take place in one of two ways. It may be by a simple contact (the same point of the movable part remaining constantly in contact with the same point of the fixed part). Or it may be by a sliding contact (the same point of the movable part coming successively into contact with various points of the fixed part).

4.13.37 Only in the second case can there be continuous rotation. What then happens is this: the system does tend to take up a position of equilibrium. But just as that position is about to be reached, the sliding contact puts the movable part in communication with a new point of the fixed part. It changes the connections, and so changes the conditions of equilibrium. The position of equilibrium thus flees, as it were, before the system that is trying to reach it, and the rotation can go on indefinitely.

39 Michael Faraday (1791–1867), English physicist and chemist, who first produced continuous electromagnetic rotations in 1821.

4.13.38 Ampère assumes that the action of the circuit on the movable part of C′ is the same as if the fixed part of C′ did not exist. That would mean the current flowing in the movable part was open.

4.13.39 He therefore concludes that the action of a closed current on an open current, or conversely that of an open current on a closed current, can give rise to a continuous rotation.

4.13.40 But this conclusion depends on the hypothesis I have just stated, which, as I said above, Helmholtz does not accept.

4.13.41 4. *Mutual action of two open currents.*—As regards the mutual action of two open currents, and in particular that of two current elements, there is no experiment at all. Ampère resorts to hypothesis. He supposes: 1. that the mutual action of two elements reduces to a force directed along the straight line joining them; 2. that the action of two closed currents is the resultant of the mutual actions of their various elements. These actions are, moreover, the same as if the elements were isolated.

4.13.42 What is remarkable is that here again Ampère makes these two hypotheses without noticing.

4.13.43 However that may be, these two hypotheses, together with the experiments on closed currents, are enough to determine completely the law of the mutual action of two elements.

4.13.44 But then most of the simple laws we met with in the case of closed currents are no longer true.

4.13.45 First, there is no electrodynamic potential. Nor, as we have seen, was there one in the case of a closed current acting on an open current.

4.13.46 Next, there is no longer, strictly speaking, any magnetic force.

4.13.47 And indeed, we gave above three different definitions of this force:

4.13.48 1. By the action undergone by a magnetic pole;

4.13.49 2. By the directing couple that orients the magnetized needle;

4.13.50 3. By the action undergone by a current element.

4.13.51 Now, in the case before us, not only do these three definitions no longer agree, but each of them is devoid of meaning. For:

4.13.52 1. A magnetic pole is no longer subject simply to a single force applied to that pole. We have seen, in fact, that the force due to the action of a current element on a pole is applied not to the pole but to the element. It can, moreover, be replaced by a force applied to the pole together with a couple;

4.13.53 2. The couple acting on the magnetized needle is no longer a simple directing couple, for its moment about the axis of the needle is not zero. It breaks down into a directing couple proper and an additional couple. This additional couple tends to produce the continuous rotation I spoke of above;

4.13.54 3. Finally, the force undergone by a current element is not normal to that element.

4.13.55 In other words, *the unity of the magnetic force has disappeared*.

4.13.56 Here is what this unity consists in. Take two systems that exert the same action on a magnetic pole. They will also exert the same action on an infinitely small magnetized needle, or on a current element, placed at the point of space where that pole was.

4.13.57 Well, this is true if the two systems contain only closed currents; according to Ampère, it would no longer be true if these systems contained open currents.

4.13.58 It is enough to note an example. Suppose a magnetic pole is placed at A and an element at B, with the direction of the element lying along the extension of the straight line AB. This element will exert no action on the pole. It will, on the contrary, exert an action either on a magnetized needle placed at point A or on a current element placed at point A.

4.13.59 5. *Induction*.—As is well known, the discovery of electrodynamic induction followed soon after Ampère's immortal work.

4.13.60 So long as only closed currents are concerned, there is no difficulty. Helmholtz even remarked that the principle of the

conservation of energy could suffice to deduce the laws of induction from Ampère's electrodynamic laws. There is one condition, however, as Bertrand clearly showed: one must also admit a certain number of hypotheses.

4.13.61 The same principle still allows this deduction in the case of open currents. Of course, the result cannot be submitted to the test of experiment, since such currents cannot be produced.

4.13.62 If we try to apply this mode of analysis to Ampère's theory of open currents, we arrive at results well calculated to surprise us.

4.13.63 First, induction cannot be deduced from the variation of the magnetic field by the formula well known to scientists and practical engineers. And indeed, as we have said, there is no longer, strictly speaking, any magnetic field.

4.13.64 But there is more. Suppose a circuit C is subject to induction from a variable voltaic system S. Suppose this system S is displaced and deformed in any way whatever, and that the intensity of its currents varies according to any law whatever. But suppose that after these variations the system finally returns to its initial situation. It seems natural to suppose that the *mean* electromotive force induced in the circuit C is zero.

4.13.65 That is true if the circuit C is closed and if the system S contains only closed currents. If we accept Ampère's theory, it would no longer be true as soon as there were open currents. Not only, then, will induction no longer be the variation of the flux of magnetic force in any of the usual senses of the word. It cannot be represented by the variation of anything whatever.

4.13.66 II. Helmholtz's theory.—I have dwelt on the consequences of Ampère's theory and of his way of understanding the action of open currents.

4.13.67 It is hard to overlook the paradoxical and artificial character of the propositions to which we are thus led; one is brought to think that "it can't really be like that."

4.13.68 It is understandable, then, that Helmholtz was led to look for something else.

4.13.69 Helmholtz rejects Ampère's fundamental hypothesis, namely that the mutual action of two current elements reduces to a force directed along the straight line joining them.

4.13.70 He assumes that a current element is subject not to a single force but to a force and a couple. Indeed, this is what gave rise to the famous controversy between Bertrand and Helmholtz.

4.13.71 Helmholtz replaces Ampère's hypothesis with the following. Two current elements always possess an electrodynamic potential, depending solely on their position and orientation. The work of the forces they exert on each other is equal to the change in this potential. Thus Helmholtz can no more do without hypothesis than Ampère could; but at least he does not make a hypothesis without stating it explicitly.

4.13.72 In the case of closed currents, the only one accessible to experiment, the two theories agree; in all other cases they differ.

4.13.73 First, contrary to what Ampère supposed, consider the force to which the movable portion of a closed current seems subject. It is not the same as the force this movable portion would undergo if it were isolated and formed an open current.

4.13.74 Let us go back to the circuit C′ we spoke of above, which was formed of a movable wire αβ sliding on a fixed wire. In the only experiment that can be carried out, the movable portion αβ is not isolated but forms part of a closed circuit. When it moves from AB to A′B′, the total electrodynamic potential changes for two reasons. 1. It undergoes a first increase because the potential of A′B′ with respect to the circuit C is not the same as that of AB. 2. It undergoes a second increase because the potentials of the elements AA′ and B′B with respect to C must be added to it.

4.13.75 It is this *double* increase that represents the work of the force to which the portion AB seems subject.

4.13.76 If, on the contrary, αβ were isolated, the potential would undergo only the first increase. It is this first increase alone that would measure the work of the force acting on AB.

4.13.77 Second, there can be no continuous rotation without a sliding contact. And indeed, as we saw in connection with

closed currents, this is an immediate consequence of the existence of an electrodynamic potential.

4.13.78 In Faraday's experiment, suppose the magnet is fixed and the part of the current outside the magnet runs through a movable wire. Then this movable part can undergo continuous rotation. But that does not mean the wire would still go into continuous rotation if we removed its contacts with the magnet and sent an *open* current through it.

4.13.79 For I have just said that an *isolated* element does not undergo the same action as a movable element forming part of a closed circuit.

4.13.80 Another difference: the action of a closed solenoid on a closed current is zero according to experiment and according to both theories. Its action on an open current would be zero according to Ampère; it would not be zero according to Helmholtz.

4.13.81 Hence an important consequence. We gave above three definitions of magnetic force. The third has no meaning here, since a current element is no longer subject to a single force. Nor does the first. What, after all, is a magnetic pole? It is the end of an indefinitely long linear magnet. This magnet can be replaced by an indefinitely long solenoid. For the definition of magnetic force to have a meaning, the action exerted by an open current on an indefinitely long solenoid would have to depend only on the position of the end of that solenoid. In other words, the action on a closed solenoid would have to be zero. Now we have just seen that this is not true.

4.13.82 On the other hand, nothing prevents us from adopting the second definition, the one based on measuring the directing couple that tends to orient a magnetized needle.

4.13.83 But if we adopt it, neither the effects of induction nor the electrodynamic effects will depend solely on the distribution of the lines of force of this magnetic field.

4.13.84 III. Difficulties raised by these theories.—Helmholtz's theory is an advance on Ampère's, yet it is far from smoothing out all the difficulties. In the one as in the other, the term "mag-

netic field" has no meaning. If it is given one by a more or less artificial convention, the ordinary laws, so familiar to all electricians, no longer apply. Thus the electromotive force induced in a wire is no longer measured by the number of lines of force cut by that wire.

4.13.85 And our reluctance does not come merely from the difficulty of giving up ingrained habits of language and thought. There is something more. If we do not believe in action at a distance, electrodynamic phenomena must be explained by a modification of the medium. It is precisely this modification that is called the magnetic field, and so electrodynamic effects ought to depend only on this field.

4.13.86 All these difficulties arise from the hypothesis of open currents.

4.13.87 IV. Maxwell's theory.—Such were the difficulties raised by the prevailing theories when Maxwell appeared and, with a stroke of the pen, made them all disappear. In his scheme, in fact, there are only closed currents.

4.13.88 Maxwell assumes that if the electric field in a dielectric happens to vary, the dielectric becomes the seat of a special phenomenon. This phenomenon acts on the galvanometer like a current, and he calls it the *displacement current*.

4.13.89 If, then, two conductors carrying opposite charges are connected by a wire, an open conduction current flows in the wire during the discharge. But at the same time displacement currents arise in the surrounding dielectric, and these close the conduction current.

4.13.90 As is well known, Maxwell's theory leads to an explanation of optical phenomena, which would be due to extremely rapid electrical oscillations.

4.13.91 At that time such a conception was only a bold hypothesis, without the support of any experiment.

4.13.92 Twenty years later, Maxwell's ideas were confirmed by experiment. Hertz succeeded in producing systems of electrical oscillations that reproduce all the properties of light and differ from it only in wavelength—that is, as violet differs from red. He

achieved, in a sense, the synthesis of light. It is from this, as everyone knows, that wireless telegraphy has come.

4.13.93 It might be said that Hertz did not directly prove Maxwell's fundamental idea, the action of the displacement current on the galvanometer. That is true in one sense; what he showed directly, in short, is that electromagnetic induction is not propagated instantaneously, as had been believed, but with the speed of light.

4.13.94 But to suppose that there is no displacement current and that induction is propagated with the speed of light, or to suppose that displacement currents produce induction effects and that induction is propagated instantaneously—*this is the same thing*.

4.13.95 This is not obvious at first sight, but it can be proved by an analysis that I cannot even think of summarizing here.

ROWLAND'S EXPERIMENTS AND LORENTZ'S ELECTRONS

Convection currents act like conduction currents, and Lorentz makes all currents motions of electrons.

4.13.96 V. Rowland's experiments.—But, as I said above, there are two kinds of open conduction currents. First, there are the discharge currents of a capacitor or of any conductor whatever.

4.13.97 There are also the cases where electric charges travel round a closed path, moving by conduction in one part of the circuit and by convection in the other.

4.13.98 For open currents of the first kind, the question could be regarded as solved: they were closed by displacement currents.

4.13.99 For open currents of the second kind, the solution seemed simpler still: if the current was closed, it could only be, it seemed, by the convection current itself. For that, it was enough to assume that a "convection current"—that is, a charged conductor in motion—could act on the galvanometer. But experimental confirmation was lacking. It seemed difficult, in fact, to obtain a sufficient intensity, even by increasing the charge and the speed of the conductors as much as possible.

4.13.100 It was Rowland, an extremely skillful experimenter, who first overcame these difficulties. A disk was given a strong electrostatic charge and a very high speed of rotation. An astatic magnetic system placed beside the disk was deflected.[40]

4.13.101 Rowland performed the experiment twice, once in Berlin and once in Baltimore; it was later repeated by Himstedt. These physicists even believed they could announce that they had made quantitative measurements.

4.13.102 This law of Rowland's was accepted without dispute by all physicists.

4.13.103 Everything, moreover, seemed to confirm it. The spark certainly produces a magnetic effect; now, does it not seem likely that discharge by spark is due to particles torn from one of the electrodes and carried, with their charge, to the other electrode? Is not the very spectrum of the spark, in which one recognizes the lines of the electrode's metal, a proof of this? The spark would then be a genuine convection current.

4.13.104 On the other hand, it is also accepted that in an electrolyte electricity is conveyed by moving ions. The current in an electrolyte would therefore also be a convection current; and it acts on the magnetic needle.

4.13.105 Likewise with cathode rays. Crookes attributes these rays to the effect of a very subtle matter, charged with negative electricity and moving at very great speed; in other words, he regards them as convection currents, and his view, disputed for a time, is now adopted everywhere. Now, these cathode rays are deflected by a magnet. By the principle of action and reaction, they must in turn deflect the magnetic needle.[41]

4.13.106 It is true that Hertz believed he had shown that cathode rays carry no negative electricity and do not act on the magnetic needle. But Hertz was mistaken. First, Perrin was able to collect the electricity carried by these rays, whose existence Hertz

40 Henry Augustus Rowland (1848–1901), American physicist at Johns Hopkins University in Baltimore; he first performed this experiment in Helmholtz's Berlin laboratory in 1876.

41 William Crookes (1832–1919), English chemist and physicist who studied discharges in evacuated tubes.

denied; the German scientist seems to have been misled by effects due to the action of X-rays, which had not yet been discovered. Second, and quite recently, the action of cathode rays on the magnetic needle has been demonstrated, and the cause of Hertz's error identified.[42]

4.13.107 Thus all these phenomena regarded as convection currents—sparks, electrolytic currents, cathode rays—act in the same way on the galvanometer, in accordance with Rowland's law.

4.13.108 VI. Lorentz's theory.—It was not long before people went further. According to Lorentz's theory, conduction currents themselves would be genuine convection currents: electricity would remain inseparably attached to certain material particles called *electrons*; it would be the circulation of these electrons through bodies that produced voltaic currents, and what would distinguish conductors from insulators is that the former let these electrons pass through, while the latter stopped their motion.

4.13.109 Lorentz's theory is very attractive. It gives a very simple explanation of certain phenomena that the older theories, even Maxwell's in its original form, could not account for satisfactorily: for example, the aberration of light, the partial dragging of light waves, magnetic polarization, and Zeeman's experiment.

4.13.110 Some objections still remained. The phenomena occurring in a system seemed bound to depend on the absolute speed of translation of that system's center of gravity, which is contrary to our idea of the relativity of space. At Crémieu's thesis defense, Lippmann put this objection in a striking form. Suppose two charged conductors moving with the same speed of translation. They are at rest relative to each other; yet, since each is equivalent to a convection current, they must attract

42 Jean Perrin (1870–1942), French physicist, who showed in 1895 that cathode rays carry negative charge.

each other, and by measuring this attraction one could measure their absolute speed.[43]

4.13.111 No, answered Lorentz's supporters: what would be measured in this way is not their absolute speed but their relative speed *with respect to the ether*, so that the principle of relativity is safe. Since then, moreover, Lorentz has found a subtler, but far more satisfactory, answer.

4.13.112 Whatever may be said of these last objections, the edifice of electrodynamics seems, at least in its main outlines, to be definitively built; everything looks as satisfactory as could be; the theories of Ampère and Helmholtz, made for open currents that no longer exist, now seem to have no more than a purely historical interest.

4.13.113 The history of these shifts will be no less instructive for that; it will teach us what traps the scientist is exposed to, and how he may hope to escape them.

CHAPTER XIV: THE END OF MATTER

Experiments on electrons suggest that mass may be borrowed from the ether, so that matter would not exist.

4.14.1 One of the most astonishing discoveries that physicists have announced in recent years is that matter does not exist. Let us hasten to add that this discovery is not yet final. The essential attribute of matter is its mass, its inertia. Mass is what remains constant everywhere and always, what persists when a chemical transformation has altered all the perceptible qualities of matter and seems to have turned it into another body. So if it were shown that the mass, the inertia of matter does not really belong to it, that it is borrowed finery with which matter adorns itself, that this mass, the constant par excellence, is itself liable to change, then one might well say that matter does not exist. Now, that is precisely what is being announced.

43 Victor Crémieu defended a doctoral thesis in Paris (1901) whose experiments seemed to contradict Rowland's; the physicist Gabriel Lippmann sat on the examining board.

[1] See *L'Évolution de la Matière*, by Gustave Le Bon.[44]

4.14.2 The speeds we had been able to observe until now were very small, since the heavenly bodies, which leave all our automobiles far behind, barely do 60 or 100 "kilometers" a second. Light, it is true, goes 3,000 times faster, but it is not matter that moves: it is a disturbance traveling through a relatively motionless substance, like a wave on the surface of the ocean. All observations made at these low speeds showed that mass is constant, and no one had asked whether the same would still hold at higher speeds.

4.14.3 It is the infinitely small that have beaten the record of Mercury, the fastest planet: I mean the corpuscles whose motions produce cathode rays and the rays of radium. These radiations are known to be due to a genuine molecular bombardment. The projectiles hurled in this bombardment are charged with negative electricity, as can be verified by collecting this electricity in a Faraday cylinder. Because of their charge they are deflected both by a magnetic field and by an electric field, and comparing these deflections can tell us their speed and the ratio of their charge to their mass.

4.14.4 Now, these measurements have shown us, on the one hand, that their speed is enormous—a tenth or a third of that of light, a thousand times that of the planets—and, on the other, that their charge is very large relative to their mass. Each moving corpuscle therefore amounts to an appreciable electric current. But we know that electric currents display a special kind of inertia called *self-induction*. A current, once established, tends to maintain itself; that is why, when one tries to break a current by cutting the conductor it flows through, a spark is seen to leap at the break. Thus the current tends to keep its intensity just as a moving body tends to keep its speed. So our cathode corpuscle will resist the causes that might alter its speed for two reasons:

44 Gustave Le Bon (1841–1931), French physician and popular writer on psychology and science; this book (1905) argued that matter is slowly dissolving into the ether.

first, by its inertia properly so called, and second, by its self-induction, because any change in its speed would at the same time be a change in the corresponding current. The corpuscle—the *electron*, as it is called—will therefore have two inertias: mechanical inertia and electromagnetic inertia.

4.14.5 Abraham and Kaufmann, one doing the calculations and the other the experiments, joined forces to determine the share of each. To do so they had to adopt a hypothesis: they assumed that all negative electrons are identical, that they carry the same charge, which is essentially constant, and that the differences observed among them come solely from the different speeds at which they move. When the speed varies, the real mass, the mechanical mass, remains constant—that is, so to speak, its very definition; but the electromagnetic inertia, which helps make up the apparent mass, increases with speed according to a certain law.[45] There must therefore be a relation between the speed and the ratio of mass to charge—quantities that can be calculated, as we have said, by observing how the rays are deflected by a magnet or an electric field; and studying this relation makes it possible to determine the share of the two inertias. The result is quite surprising: *the real mass is zero*. It is true that one has to accept the hypothesis made at the outset, but the agreement between the theoretical curve and the experimental curve is close enough to make that hypothesis very likely.

4.14.6 Thus these negative electrons have no mass properly so called; if they seem endowed with inertia, it is because they cannot change their speed without disturbing the ether. Their apparent inertia is only borrowed: it is not theirs, it belongs to the ether. But these negative electrons are not the whole of matter; one could therefore suppose that apart from them there is a true matter endowed with an inertia of its own. There are certain radiations—such as Goldstein's canal rays and the α rays of radium—that are also due to a rain of projectiles, but of posit-

45 Max Abraham (1875–1922), German theoretical physicist; Walter Kaufmann (1871–1947), German experimental physicist who measured the deflection of fast electrons from 1901 onward.

ively charged projectiles. Are these positive electrons also devoid of mass?[46] It is impossible to say, because they are much heavier and much slower than the negative electrons. So two hypotheses remain admissible. Either these electrons are heavier because, besides their borrowed electromagnetic inertia, they have a mechanical inertia of their own, and then it is they that are the true matter; or they are massless like the others, and if they appear heavier to us, it is because they are smaller. I do mean smaller, paradoxical as that may seem; for on this view the corpuscle would be nothing but a hollow in the ether, which alone is real and alone endowed with inertia.

4.14.7 So far matter is not too badly compromised; we can still adopt the first hypothesis, or even believe that apart from positive and negative electrons there are neutral atoms. Lorentz's recent research is going to deprive us of this last resource. We are carried along in the motion of the Earth, which is very rapid; will not optical and electrical phenomena be altered by this translation? This was long believed, and it was supposed that observations would reveal differences depending on how the apparatus was oriented relative to the Earth's motion. Nothing of the kind happened, and the most delicate measurements showed nothing of the sort. And in this the experiments justified a reluctance shared by all physicists; for if something had been found, one could have known not only the relative motion of the Earth with respect to the Sun but its absolute motion in the ether. Now, many people find it hard to believe that any experiment could give anything other than relative motion; they would more readily agree to believe that matter has no mass.

4.14.8 So no one was very surprised by the negative results obtained; they ran counter to the theories that were taught, but they gratified a deep instinct older than all those theories. Still, those theories had to be modified accordingly, to bring them into harmony with the facts. This is what FitzGerald did, by a surprising hypothesis: he supposes that all bodies undergo a

46 Canal rays: streams of positive ions, discovered by Eugen Goldstein in 1886, that pass through holes ("canals") in the cathode of a discharge tube.

contraction of about one hundred-millionth in the direction of the Earth's motion. A perfect sphere becomes a flattened ellipsoid, and if it is rotated, it deforms in such a way that the short axis of the ellipsoid always stays parallel to the Earth's velocity.[47] Since measuring instruments undergo the same deformations as the objects to be measured, nothing is noticed, unless one thinks of determining the time light takes to travel the length of the object.

4.14.9 This hypothesis accounts for the observed facts. But that is not enough. Some day still more precise observations will be made; will the results this time be positive? Will they enable us to determine the absolute motion of the Earth? Lorentz did not think so; he believes this determination will always be impossible, and the common instinct of all physicists and the failures met with so far are guarantee enough for him. Let us therefore regard this impossibility as a general law of nature; let us accept it as a postulate. What will its consequences be? That is what Lorentz set out to find, and he found that all atoms, all electrons, positive or negative, must have an inertia that varies with speed, and according to precisely the same laws. Thus every material atom would be made of positive electrons, small and heavy, and negative electrons, large and light; and if perceptible matter does not seem electrified to us, it is because the two kinds of electrons are roughly equal in number. Both kinds are devoid of mass and have only a borrowed inertia. In this system there is no true matter; there are only holes in the ether.

4.14.10 For Langevin, matter would be liquefied ether that has lost its properties. When matter moved, it would not be this liquefied mass traveling through the ether; rather, the liquefaction would spread step by step to new portions of the ether, while behind, the parts first liquefied would return to their original state. Matter, in moving, would not keep its identity.[48]

4.14.11 That is where the question stood a short while ago; but now Kaufmann announces new experiments. The negative elec-

47 George Francis FitzGerald (1851–1901), Irish physicist; Lorentz proposed the same contraction independently.

tron, whose speed is enormous, ought to undergo the FitzGerald contraction, and the relation between speed and mass would be modified as a result; but the recent experiments do not confirm this prediction. Everything would then collapse, and matter would regain its right to exist. But the experiments are delicate, and a definitive conclusion would be premature today.

48 Paul Langevin (1872–1946), French physicist, a former student of Pierre Curie.