The value of science

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THE VALUE ~~OF~~ LESSNESS SCIENCE

THE VALUE OF SCIENCE

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AUTHORIZED TRANSLATION WITH AN INTRODUCTION

BY

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WITH A SPECIAL PREFATORY ESSAY

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TRANSLATOR'S INTRODUCTION

1. *Does the Scientist create Science?*—Professor Rados of Budapest in his report to the Hungarian Academy of Science on the award to Poincaré of the Bolyai prize of ten thousand crowns, speaking of him as at the present moment unquestionably the most powerful investigator in the domain of mathematics and mathematical physics, recognizes in him the intuitive genius drawing the inspiration for his wide-reaching researches from the exhaustless fountain of geometric and physical intuition, yet working this inspiration out in detail with marvelous logical keenness. With his brilliant creative genius is combined the capacity for sharp and successful generalization, pushing far out the boundaries of thought in the most widely different domains, so that his works must be ranked with the greatest mathematical achievements of all time. "Finally," says Rados, "permit me to make especial mention of his last, his intensely interesting book, 'The Value of Science,' in which he in a way has laid down the scientist's creed." Now what is this creed?

Sense may act as stimulus, as suggestive, yet not to awaken a dormant depiction, or to educe the conception of an archetypal form, but rather to strike the hour for creation, to summon to work a sculptor capable of smoothing a Venus of Milo out of the formless clay. Knowledge is not a gift of bare experience, nor even made solely out of experience. The creative activity of mind is in mathematics particularly clear. The axioms of geometry are conventions, disguised definitions or unprovable hypotheses precreated by auto-active animal and human minds. Bertrand Russell says of projective geometry: "It takes nothing from experience, and has, like arithmetic, a creature of the pure intellect for its object. It deals with an object whose properties are logically deduced from its definition, not empirically discovered from data." Then does the scientist create science? This is a question Poincaré here dissects with master hand.

The physiologic-psychologic investigation of the space problem must give the meaning of the words *geometric fact, geometric reality.* Poincaré here subjects to the most successful analysis ever made the tridimensionality of our space.

2. *The Mind Dispelling Optical Illusions.*—Actual perception of

spatial properties is accompanied by movements corresponding to its character. In the case of optical illusions, with the so-called false perceptions eye-movements are closely related. But though the perceived object and its environment remain constant, the sufficiently powerful mind can, as we say, dispel these illusions, the perception itself being creatively changed. Photographs taken at intervals during the presence of these optical illusions, during the change, perhaps gradual and unconscious, in the perception, and after these illusions have, as the phrase is, finally disappeared, show quite clearly that changes in eye-movements corresponding to those internally created in perception itself successively occur. What is called accuracy of movement is created by what is called correctness of perception. The higher creation in the perception is the determining cause of an improvement, a precision in the motion. Thus we see correct perception in the individual helping to make that cerebral organization and accurate motor adjustment on which its possibility and permanence seem in so far to depend. So-called correct perception is connected with a long-continued process of perceptual education, motived and initiated from within. How this may take place is here illustrated at length by our author.

3. *Euclid not Necessary.*—Geometry is a construction of the intellect, in application not certain but convenient. As Schiller says, when we see these facts as clearly as the development of metageometry has compelled us to see them, we must surely confess that the Kantian account of space is hopelessly and demonstrably antiquated. As Royce says in 'Kant's Doctrine of the Basis of Mathematics,' "That very use of intuition which Kant regarded as geometrically ideal, the modern geometer regards as scientifically defective, because surreptitious. No mathematical exactness without explicit proof from assumed principles—such is the motto of the modern geometer. But suppose the reasoning of Euclid purified of this comparatively surreptitious appeal to intuition. Suppose that the principles of geometry are made quite explicit at the outset of the treatise, as Pieri and Hilbert or Professor Halsted or Dr. Veblen makes his principles explicit in his recent treatment of geometry. Then, indeed, geometry becomes for the modern mathematician a purely rational science. But very few students of the logic of mathematics at the present time can see any warrant in the analysis of geometrical truth for regarding just the Euclidean system of principles as possessing any discoverable necessity." Yet the environmental and perhaps heredi-

tary premiums on Euclid still make even the scientist think Euclid most convenient.

4. *Without Hypotheses, no Science.*—Nobody ever observed an equidistantial, but also nobody ever observed a straight line. Emerson's Uriel

> "Gave his sentiment divine
> Against the being of a line.
> Line in Nature is not found."

Clearly not, being an eject from man's mind. What is called 'a knowledge of facts' is usually merely a subjective realization that the old hypotheses are still sufficiently elastic to serve in some domain, that is, with a sufficiency of conscious or unconscious omissions and doctorings and fudgings more or less wilful. In the present book we see the very foundation rocks of science, the conservation of energy and the indestructibility of matter, beating against the bars of their cages, seemingly anxious to take wing away into the empyrean, to chase the once divine parallel postulate broken loose from Euclid and Kant.

5. *What Outcome?*—What now is the definite, the permanent outcome? What new islets raise their fronded palms in air within thought's musical domain? Over what age-gray barriers rise the fragrant floods of this new spring-tide, redolent of the wolf-haunted forests of Transylvania, of far Erdély's plunging river, Maros the bitter, or broad mother Volga at Kazan? What victory heralded the great rocket for which young Lobachevski, the widow's son, was cast into prison? What severing of age-old mental fetters symbolized young Bolyai's cutting-off with his Damascus blade the spikes driven into his door-post and strewing over the sod the thirteen Austrian cavalry officers? This book by the greatest living mathematician gives weightiest and most charming answer.

AUTHOR'S ESSAY PREFATORY TO THE TRANSLATION

The Choice of Facts

Tolstoi somewhere explains why 'science for its own sake' is in his eyes an absurd conception. We can not know *all* facts, since their number is practically infinite. It is necessary to choose; then we may let this choice depend on the pure caprice of our curiosity; would it not be better to let ourselves be guided by utility, by our practical and above all by our moral needs; have we nothing better to do than counting the number of lady-bugs on our planet?

It is clear the word utility has not for him the sense men of affairs give it, and following them most of our contemporaries. Little cares he for industrial applications, for the marvels of electricity or of automobilism, which he regards rather as obstacles to moral progress; utility for him is solely what can make man better.

For my part, it need scarce be said, I could never be content with either the one or the other ideal; I want neither that plutocracy grasping and mean, nor that democracy goody and mediocre, occupied solely in turning the other cheek, where would dwell sages without curiosity, who, shunning excess, would not die of disease, but would surely die of ennui. But that is a matter of taste and is not what I wish to discuss.

The question nevertheless remains and should fix our attention; if our choice can only be determined by caprice or by immediate utility, there can be no science for its own sake, and consequently no science. But is that true? That a choice must be made is incontestable; whatever be our activity, facts go quicker than we, and we can not catch them; while the scientist discovers one fact, there happen milliards of milliards in a cubic millimeter of his body. To wish to comprise nature in science would be to want to put the whole into the part.

But scientists believe there is a hierarchy of facts and that among them may be made a judicious choice. They are right, since otherwise there would be no science, yet science exists. One need only open the eyes to see that the conquests of industry which have enriched so many practical men would never have seen the light, if these practical men alone had existed and if they had not been preceded by unselfish devotees who died poor, who never thought of utility, and yet had a guide far other than caprice.

As Mach says, these devotees have spared their successors the trouble of thinking. Those who might have worked solely in view of an immediate application would have left nothing behind them, and, in face of a new need, all must have been begun over again. Now most men do not love to think, and this is perhaps fortunate when instinct guides them, for most often, when they pursue an aim which is immediate and ever the same, instinct guides them better than reason would guide a pure intelligence. But instinct is routine, and if thought did not fecundate it, it would no more progress in man than in the bee or ant. It is needful then to think for those who love not thinking and, as they are numerous, it is needful that each of our thoughts be as often useful as possible, and this is why a law will be the more precious the more general it is.

This shows us how we should choose: the most interesting facts are those which may serve many times; these are the facts which have a chance of coming up again. We have been so fortunate as to be born in a world where there are such. Suppose that instead of 60 chemical elements there were 60 milliards of them, that they were not, some common, the others rare, but that they were uniformly distributed. Then, every time we picked up a new pebble there would be great probability of its being formed of some unknown substance; all that we knew of other pebbles would be worthless for it; before each new object we should be as the new-born babe; like it we could only obey our caprices or our needs. Biologists would be just as much at a loss if there were only individuals and no species and if heredity did not make sons like their fathers.

In such a world there would be no science; perhaps thought and even life would be impossible, since evolution could not there develop the preservational instincts. Happily it is not so; like all good fortune to which we are accustomed, this is not appreciated at its true worth.

Which then are the facts likely to reappear? They are first the simple facts. It is clear that in a complex fact a thousand circumstances are united by chance, and that only a chance still much less probable could reunite them anew. But are there any simple facts? And if there are, how recognize them? What assurance is there that a thing we think simple does not hide a dreadful complexity? All we can say is that we ought to prefer the facts which *seem* simple to those where our crude eye discerns unlike elements. And then one of two things: either this simplicity is real, or else the elements are so intimately mingled as not to be distinguishable. In the first case there is chance of our meeting anew this same simple fact, either in all its purity or entering itself as element in a complex manifold. In the second case this intimate mixture has likewise more chances of recurring than a heterogeneous assemblage; chance knows how to mix, it knows not how to disentangle, and to make with multiple elements a well-ordered edifice in which something is distinguishable, it must be made expressly. The facts which appear simple, even if they are not so, will therefore be more easily revived by chance. This it is which justifies the method instinctively adopted by the scientist, and what justifies it still better, perhaps, is that oft-recurring facts appear to us simple, precisely because we are used to them.

But where is the simple fact? Scientists have been seeking it in the two extremes, in the infinitely great and in the infinitely small. The astronomer has found it because the distances of the stars are immense, so great that each of them appears but as a point, so great that the qualitative differences are effaced, and because a point is simpler than a body which has form and qualities. The physicist, on the other hand, has sought the elementary phenomenon in fictively cutting up bodies into infinitesimal cubes, because the conditions of the problem, which undergo slow and continuous variation in passing from one point of the body to another, may be regarded as constant in the interior of each of these little cubes. In the same way the biologist has been instinctively led to regard the cell as more interesting than the whole animal, and the outcome has shown his wisdom, since cells belonging to organisms the most different are more alike, for the one who can recognize their resemblances, than are these organisms themselves. The sociologist is more embarrassed; the elements, which for him are men, are too unlike, too variable, too capricious in a word, too complex; besides, history never begins over again. How then choose the interesting fact, which is that which begins again? Method is precisely the choice of facts; it is needful then to be occupied first with creating a method, and many have been imagined, since none imposes itself, so that sociology is the science which has the most methods and the fewest results.

Therefore it is by the regular facts that it is proper to begin; but after the rule is well established, after it is beyond all doubt, the facts in full conformity with it are ere long without interest since they no longer teach us anything new. It is then the exception which becomes important. We cease to seek resemblances; we devote ourselves above all to the differences, and among the differences are chosen first the most accentuated, not only because they are the most striking, but because they will be the most instructive. A simple example will make my thought plainer: Suppose one wishes to determine a curve by observing some of its points. The practician who concerns himself only with immediate utility would observe only the points he might need for some special object. These points would be badly distributed on the curve; they would be crowded in certain regions, rare in others, so that it would be impossible to join them by a continuous line, and they would be unavailable for other applications. The scientist will proceed differently; as he wishes to study the curve for itself, he will distribute regularly the points to be observed, and when enough are known he will join them by a regular line and then he will have the entire curve. But for that how does he proceed? If he has determined an extreme point of the curve, he does not stay near this extremity, but goes first to the other end; after the two extremities the most instructive point will be the mid-point and so on.

So when a rule is established we should first seek the cause where this rule has the greatest chance of failing. Thence, among other reasons, come the interest of astronomic facts and the interest of the geologic past; by going very far away in space or very far away in time, we may find our usual rules entirely overturned, and these grand overturnings aid us the better to see or the better to understand the little changes which may happen nearer to us, in the little corner of the world where we are called to live and act. We shall better know this corner for having traveled in distant countries with which we have nothing to do.

But what we ought to aim at is less the ascertainment of resemblances and differences than the recognition of likenesses hidden under apparent divergences. Particular rules seem at first discordant, but looking more closely we see in general that they resemble each other; different as to matter, they are alike as to form, as to the order of their parts. When we look at them with this bias, we shall see them enlarge and tend to embrace everything. And this it is which makes the value of certain facts which come to complete an assemblage and to show that it is the faithful image of other known assemblages.

I will not further insist, but these few words suffice to show that the scientist does not choose at random the facts he observes. He does not, as Tolstoi says, count the lady-bugs, because, however interesting lady-bugs may be, their number is subject to capricious variations. He seeks to condense much experience and much thought into a slender volume; and that is why a little book on physics contains so many past experiences and a thousand times as many possible experiences whose result is known beforehand.

But we have as yet looked at only one side of the question. The scientist does not study nature because it is useful; he studies it because he delights in it, and he delights in it because it is beautiful. If nature were not beautiful, it would not be worth knowing, and if nature were not worth knowing, life would not be worth living. Of course I do not here speak of that beauty which strikes the senses, the beauty of qualities and of appearances; not that I undervalue such beauty, far from it, but it has nothing to do with science; I mean that profounder beauty which comes from the harmonious order of the parts and which a pure intelligence can grasp. This it is which gives body, a structure so to speak, to the iridescent appearances which flatter our senses, and without this support the beauty of these fugitive dreams would be only imperfect, because it would be vague and always fleeting. On the contrary, intellectual beauty is sufficient unto itself, and it is for its sake, more perhaps than for the future good of humanity, that the scientist devotes himself to long and difficult labors.

It is, therefore, the quest of this especial beauty, the sense of the harmony of the cosmos, which makes us choose the facts most fitting to contribute to this harmony, just as the artist chooses from among the features of his model those which perfect the picture and give it character and life. And we need not fear that this instinctive and unavowed prepossession will turn the scientist aside from the search for the true. One may dream a harmonious world, but how far the real world will leave it behind! The greatest artists that ever lived, the Greeks, made their heavens; how shabby it is beside the true heavens, ours!

And it is because simplicity, because grandeur, is beautiful, that we preferably seek simple facts, sublime facts, that we delight now to follow the majestic course of the stars, now to examine with the microscope that prodigious littleness which is also a grandeur, now to seek in geologic time the traces of a past which attracts because it is far away.

We see too that the longing for the beautiful leads us to the same choice as the longing for the useful. And so it is that this economy of thought, this economy of effort, which is, according to Mach, the constant tendency of science, is at the same time a source of beauty and a practical advantage. The edifices that we admire are those where the architect has known how to proportion the means to the end, where the columns seem to carry gaily, without effort, the weight placed upon them, like the gracious cariatids of the Erechtheion.

Whence comes this concordance? Is it simply that the things which seem to us beautiful are those which best adapt themselves to our intelligence, and that consequently they are at the same time the implement this intelligence knows best how to use? Or is there here a play of evolution and natural selection? Have the peoples whose ideal most conformed to their highest interest exterminated the others and taken their place? All pursued their ideals without reference to consequences, but while this quest led some to destruction, to others it gave empire. One is tempted to believe it. If the Greeks triumphed over the barbarians and if Europe, heir of Greek thought, dominates the world, it is because the savages loved loud colors and the clamorous tones of the drum which occupied only their senses, while the Greeks loved the intellectual beauty which hides beneath sensuous beauty, and that this intellectual beauty it is which makes intelligence sure and strong.

Doubtless such a triumph would horrify Tolstoi, and he would not like to acknowledge that it might be truly useful. But this disinterested quest of the true for its own beauty is sane also and able to make man better. I well know that there are mistakes, that the thinker does not always draw thence the serenity he should find therein, and even that there are scientists of bad character. Must we, therefore, abandon science and study only morals? What! Do you think the moralists themselves are irreproachable when they come down from their pedestal?

THE VALUE OF SCIENCE

INTRODUCTION

THE search for truth should be the goal of our activities; it is the sole end worthy of them. Doubtless we should first bend our efforts to assuage human suffering, but why? Not to suffer is a negative ideal more surely attained by the annihilation of the world. If we wish more and more to free man from material cares, it is that he may be able to employ the liberty obtained in the study and contemplation of truth.

But sometimes truth frightens us. And in fact we know that it is sometimes deceptive, that it is a phantom never showing itself for a moment except to ceaselessly flee, that it must be pursued further and ever further without ever being attained. Yet to work one must stop, as some Greek, Aristotle or another, has said. We also know how cruel the truth often is, and we wonder whether illusion is not more consoling, yea, even more bracing, for illusion it is which gives confidence. When it shall have vanished, will hope remain and shall we have the courage to achieve? Thus would not the horse harnessed to his treadmill refuse to go, were his eyes not bandaged? And then to seek truth it is necessary to be independent, wholly independent. If on the contrary we wish to act, to be strong, we should be united. This is why many of us fear truth; we consider it a cause of weakness. Yet truth should not be feared, for it alone is beautiful.

When I speak here of truth, assuredly I refer first to scientific truth; but I also mean moral truth, of which what we call justice is only one aspect. It may seem that I am misusing words, that I combine thus under the same name two things having nothing in common; that scientific truth, which is demonstrated, can in no way be likened to moral truth, which is felt. And yet I can not separate them, and whosoever loves the one can not help loving the other. To find the one, as well as to find the other, it is necessary to free the soul completely from prejudice and from passion; it is necessary to attain absolute sincerity. These two sorts of truth when discovered give the same joy; each when perceived beams with the same splendor, so that we must see it or close our eyes. Lastly, both attract us and flee from us; they are never fixed: when we think to have reached them, we find that we have still to advance, and he who pursues them is condemned never to know repose. It must be added that those who fear the one will also fear the other; for they are the ones who in everything are concerned above all with consequences. In a word, I liken the two truths, because the same reasons make us love them and because the same reasons make us fear them.

If we ought not to fear moral truth, still less should we dread scientific truth. In the first place it can not conflict with ethics. Ethics and science have their own domains, which touch but do not interpenetrate. The one shows us to what goal we should aspire, the other, given the goal, teaches us how to attain it. So they can never conflict since they can never meet. There can no more be immoral science than there can be scientific morals.

But if science is feared, it is above all because it can not give us happiness. Of course it can not. We may even ask whether the beast does not suffer less than man. But can we regret that earthly paradise where man brute-like was really immortal in knowing not that he must die? When we have tasted the apple, no suffering can make us forget its savor. We always come back to it. Could it be otherwise? As well ask if one who has seen and is blind will not long for the light. Man, then, can not be happy through science, but to-day he can much less be happy without it.

But if truth be the sole aim worth pursuing, may we hope to attain it? It may well be doubted. Readers of my little book 'Science and Hypothesis' already know what I think about the question. The truth we are permitted to glimpse is not altogether what most men call by that name. Does this mean that our most legitimate, most imperative aspiration is at the same time the most vain? Or can we, despite all, approach truth on some side? This it is which must be investigated.

In the first place, what instrument have we at our disposal for this conquest? Is not human intelligence, more specifically the intelligence of the scientist, susceptible of infinite variation? Volumes could be written without exhausting this subject; I, in a few brief pages, have only touched it lightly. That the geometer's mind is not like the physicist's or the naturalist's, all the world would agree; but mathematicians themselves do not resemble each other; some recognize only implacable logic, others appeal to intuition and see in it the only source of discovery. And this would be a reason for distrust. To minds so unlike can the mathematical theorems themselves appear in the same light? Truth which is not the same for all, is it truth? But looking at things more closely, we see how these very different workers collaborate in a common task which could not be achieved without their cooperation. And that already reassures us.

Next must be examined the frames in which nature seems enclosed and which are called time and space. In 'Science and Hypothesis' I have already shown how relative their value is; it is not nature which imposes them upon us, it is we who impose them upon nature because we find them convenient. But I have spoken of scarcely more than space, and particularly quantitative space, so to say, that is of the mathematical relations whose aggregate constitutes geometry. I should have shown that it is the same with time as with space and still the same with 'qualitative space'; in particular, I should have investigated why we attribute three dimensions to space. I may be pardoned then for taking up again these important questions.

Is mathematical analysis then, whose principal object is the study of these empty frames, only a vain play of the mind? It can give to the physicist only a convenient language; is this not a mediocre service, which, strictly speaking, could be done without; and even is it not to be feared that this artificial language may be a veil interposed between reality and the eye of the physicist? Far from it; without this language most of the intimate analogies of things would have remained forever unknown to us; and we should forever have been ignorant of the internal harmony of the world, which is, we shall see, the only true objective reality.

The best expression of this harmony is law. Law is one of the most recent conquests of the human mind; there still are people who live in the presence of a perpetual miracle and are not astonished at it. On the contrary, we it is who should be astonished at nature's regularity. Men demand of their gods to prove their existence by miracles; but the eternal marvel is that there are not miracles without cease. The world is divine because it is a harmony. If it were ruled by caprice, what could prove to us it was not ruled by chance?

This conquest of law we owe to astronomy, and just this makes the grandeur of the science rather than the material grandeur of the objects it considers. It was altogether natural then that celestial mechanics should be the first model of mathematical physics; but since then this science has developed; it is still developing, even rapidly developing. And it is already necessary to modify in certain points the scheme I outlined in 1900 and from which I drew two chapters of 'Science and Hypothesis.' In an address at the St. Louis exposition in 1904, I sought to survey the road traveled; the result of this investigation the reader shall see farther on.

The progress of science has seemed to imperil the best established principles, those even which were regarded as fundamental. Yet nothing shows they will not be saved; and if this comes about only imperfectly, they will still subsist even though they are modified. The advance of science is not comparable to the changes of a city, where old edifices are pitilessly torn down to give place to new, but to the continuous evolution of zoologic types which develop ceaselessly and end by becoming unrecognizable to the common sight, but where an expert eye finds always traces of the prior work of the centuries past. One must not think then that the old-fashioned theories have been sterile and vain.

Were we to stop there, we should find in these pages some reasons for confidence in the value of science, but many more for distrusting it; an impression of doubt would remain; it is needful now to set things to rights.

Some people have exaggerated the rôle of convention in science; they have even gone so far as to say that law, that scientific fact itself, was created by the scientist. This is going much too far in the direction of nominalism. No, scientific laws are not artificial creations; we have no reason to regard them as accidental, though it be impossible to prove they are not.

Does the harmony the human intelligence thinks it discovers in nature exist outside of this intelligence? No, beyond doubt, a reality completely independent of the mind which conceives it, sees or feels it, is an impossibility. A world as exterior as that, even if it existed, would for us be forever inaccessible. But what we call objective reality is, in the last analysis, what is common to many thinking beings, and could be common to all; this common part, we shall see, can only be the harmony expressed by mathematical laws. It is this harmony then which is the sole objective reality, the only truth we can attain; and when I add that the universal harmony of the world is the source of all beauty, it will be understood what price we should attach to the slow and difficult progress which little by little enables us to know it better.

PART I

THE MATHEMATICAL SCIENCES

CHAPTER I.

Intuition and Logic in Mathematics

I

It is impossible to study the works of the great mathematicians, or even those of the lesser, without noticing and distinguishing two opposite tendencies, or rather two entirely different kinds of minds. The one sort are above all preoccupied with logic; to read their works, one is tempted to believe they have advanced only step by step, after the manner of a Vauban who pushes on his trenches against the place besieged, leaving nothing to chance. The other sort are guided by intuition and at the first stroke make quick but sometimes precarious conquests, like bold cavalrymen of the advance guard.

The method is not imposed by the matter treated. Though one often says of the first that they are *analysts* and calls the others *geometers,* that does not prevent the one sort from remaining analysts even when they work at geometry, while the others are still geometers even when they occupy themselves with pure analysis. It is the very nature of their mind which makes them logicians or intuitionalists, and they can not lay it aside when they approach a new subject.

Nor is it education which has developed in them one of the two tendencies and stifled the other. The mathematician is born, not made, and it seems he is born a geometer or an analyst. I should like to cite examples and there are surely plenty; but to accentuate the contrast I shall begin with an extreme example, taking the liberty of seeking it in two living mathematicians.

M. Méray wants to prove that a binomial equation always has a root, or, in ordinary words, that an angle may always be subdivided. If there is any truth that we think we know by direct intuition, it is this. Who could doubt that an angle may always be divided into any number of equal parts? M. Méray does not look at it that way; in his eyes this proposition is not at all evident and to prove it he needs several pages.

On the other hand, look at Professor Klein: he is studying one of the most abstract questions of the theory of functions to determine whether on a given Riemann surface there always exists a function admitting of given singularities. What does the celebrated German geometer do? He replaces his Riemann surface by a metallic surface whose electric conductivity varies according to certain laws. He connects two of its points with the two poles of a battery. The current, says he, must pass, and the distribution of this current on the surface will define a function whose singularities will be precisely those called for by the enunciation.

Doubtless Professor Klein well knows he has given here only a sketch: nevertheless he has not hesitated to publish it; and he would probably believe he finds in it, if not a rigorous demonstration, at least a kind of moral certainty. A logician would have rejected with horror such a conception, or rather he would not have had to reject it, because in his mind it would never have originated.

Again, permit me to compare two men, the honor of French science, who have recently been taken from us, but who both entered long ago into immortality. I speak of M. Bertrand and M. Hermite. They were scholars of the same school at the same time; they had the same education, were under the same influences; and yet what a difference! Not only does it blaze forth in their writings; it is in their teaching, in their way of speaking, in their very look. In the memory of all their pupils these two faces are stamped in deathless lines; for all who have had the pleasure of following their teaching, this remembrance is still fresh; it is easy for us to evoke it.

While speaking, M. Bertrand is always in motion; now he seems in combat with some outside enemy, now he outlines with a gesture of the hand the figures he studies. Plainly he sees and he is eager to paint, this is why he calls gesture to his aid. With M. Hermite, it is just the opposite; his eyes seem to shun contact with the world; it is not without, it is within he seeks the vision of truth.

Among the German geometers of this century, two names above all are illustrious, those of the two scientists who have founded the general theory of functions, Weierstrass and Riemann. Weierstrass leads everything back to the consideration of series and their analytic transformations; to express it better, he reduces analysis to a sort of prolongation of arithmetic; you may turn through all his books without finding a figure. Riemann, on the contrary, at once calls geometry to his aid; each of his conceptions is an image that no one can forget, once he has caught its meaning.

More recently, Lie was an intuitionalist; this might have been doubted in reading his books, no one could doubt it after talking with him; you saw at once that he thought in pictures. Madame Kovalevski was a logician.

Among our students we notice the same differences; some prefer to treat their problems 'by anaylsis,' others 'by geometry.' The first are incapable of 'seeing in space,' the others are quickly tired of long calculations and become perplexed.

The two sorts of minds are equally necessary for the progress of science; both the logicians and the intuitionalists have achieved great things that others could not have done. Who would venture to say whether he preferred that Weierstrass had never written or that there had never been a Riemann? Analysis and synthesis have then both their legitimate rôles. But it is interesting to study more closely in the history of science the part which belongs to each.

II

Strange! If we read over the works of the ancients we are tempted to class them all among the intuitionalists. And yet nature is always the same; it is hardly probable that it has begun in this century to create minds devoted to logic. If we could put ourselves into the flow of ideas which reigned in their time, we should recognize that many of the old geometers were in tendency analysts. Euclid, for example, erected a scientific structure wherein his contemporaries could find no fault. In this vast construction, of which each piece however is due to intuition, we may still to-day, without much effort, recognize the work of a logician.

It is not minds that have changed, it is ideas; the intuitional minds have remained the same; but their readers have required of them greater concessions.

What is the cause of this evolution? It is not hard to find. Intuition can not give us rigor, nor even certainty; this has been recognized more and more. Let us cite some examples. We know there exist continuous functions lacking derivatives. Nothing is more shocking to intuition than this proposition which is imposed upon us by logic. Our fathers would not have failed to say: "It is evident that every continuous function has a derivative, since every curve has a tangent."

How can intuition deceive us on this point? It is because when we seek to imagine a curve, we can not represent it to ourselves without width; just so, when we represent to ourselves a straight line, we see it under the form of a rectilinear band of a certain breadth. We well know these lines have no width; we try to imagine them narrower and narrower and thus to approach the limit; so we do in a certain measure, but we shall never attain this limit. And then it is clear we can always picture these two narrow bands, one straight, one curved, in a position such that they encroach slightly one upon the other without crossing. We shall thus be led, unless warned by a rigorous analysis, to conclude that a curve always has a tangent.

I shall take as second example Dirichlet's principle on which rest so many theorems of mathematical physics; to-day we establish it by reasonings very rigorous but very long; heretofore, on the contrary, we were content with a very summary proof. A certain integral depending on an arbitrary function can never vanish. Hence it is concluded that it must have a minimum. The flaw in this reasoning strikes us immediately, since we use the abstract term *function* and are familiar with all the singularities functions can present when the word is understood in the most general sense.

But it would not be the same had we used concrete images, had we, for example, considered this function as an electric potential; it would have been thought legitimate to affirm that electrostatic equilibrium can be attained. Yet perhaps a physical comparison would have awakened some vague distrust. But if care had been taken to translate the reasoning into the language of geometry, intermediate between that of analysis and that of physics. doubtless this distrust would not have been produced, and perhaps one might thus, even to-day, still deceive many readers not forewarned.

Intuition, therefore, does not give us certainty. This is why the evolution had to happen; let us now see how it happened.

It was not slow in being noticed that rigor could not be introduced in the reasoning unless first made to enter into the definitions. For the most part the objects treated of by mathematicians were long ill defined; they were supposed to be known because represented by means of the senses or the imagination; but one had only a crude image of them and not a precise idea on which reasoning could take hold. It was there first that the logicians had to direct their efforts.

So, in the case of incommensurable numbers. The vague idea of continuity, which we owe to intuition, resolved itself into a complicated system of inequalities referring to whole numbers.

By that means the difficulties arising from passing to the limit, or from the consideration of infinitesimals, are finally removed. To-day in analysis only whole numbers are left or systems, finite or infinite, of whole numbers bound together by a net of equality or inequality relations. Mathematics, as they say, is arithmetized.

III

A first question presents itself. Is this evolution ended? Have we finally attained absolute rigor? At each stage of the evolution our fathers also thought they had reached it. If they deceived themselves, do we not likewise cheat ourselves?

We believe that in our reasonings we no longer appeal to intuition; the philosophers will tell us this is an illusion. Pure logic could never lead us to anything but tautologies; it could create nothing new; not from it alone can any science issue. In one sense these philosophers are right; to make arithmetic, as to make geometry, or to make any science, something else than pure logic is necessary. To designate this something else we have no word other than *intuition*. But how many different ideas are hidden under this same word?

Compare these four axioms: (1) Two quantities equal to a third are equal to one another; (2) if a theorem is true of the number 1 and if we prove that it is true of $n+1$ if true for n, then will it be true of all whole numbers; (3). if on a straight the point C is between A and B and the point D between A and C, then the point D will be between A and B; (4) through a given point there is not more than one parallel to a given straight.

All four are attributed to intuition, and yet the first is the enunciation of one of the rules of formal logic; the second is a real synthetic *a priori* judgment, it is the foundation of rigorous mathematical induction; the third is an appeal to the imagination; the fourth is a disguised definition.

Intuition is not necessarily founded on the evidence of the senses; the senses would soon become powerless; for example, we can not represent to ourselves a chiliagon, and yet we reason by intuition on polygons in general, which include the chiliagon as a particular case.

You know what Poncelet understood by the *principle of continuity*. What is true of a real quantity, said Poncelet, should be true of an imaginary quantity; what is true of the hyperbola whose asymptotes are real, should then be true of the ellipse whose asymptotes are imaginary. Poncelet was one of the most intuitive minds of this century;

he was passionately, almost ostentatiously, so; he regarded the principle of continuity as one of his boldest conceptions, and yet this principle did not rest on the evidence of the senses. To assimilate the hyperbola to the ellipse was rather to contradict this evidence. It was only a sort of precocious and instinctive generalization which, moreover, I have no desire to defend.

We have then many kinds of intuition; first, the appeal to the senses and the imagination; next, generalization by induction, copied, so to speak, from the procedures of the experimental sciences; finally, we have the intuition of pure number, whence arose the second of the axioms just enunciated, which is able to create the real mathematical reasoning. I have shown above by examples that the first two can not give us certainty; but who will seriously doubt the third, who will doubt arithmetic?

Now in the analysis of to-day, when one cares to take the trouble to be rigorous, there can be nothing but syllogisms or appeals to this intuition of pure number, the only intuition which can not deceive us. It may be said that to-day absolute rigor is attained.

IV

The philosophers make still another objection: "What you gain in rigor," they say, "you lose in objectivity. You can rise toward your logical ideal only by cutting the bonds which attach you to reality. Your science is infallible, but it can only remain so by imprisoning itself in an ivory tower and renouncing all relation with the external world. From this seclusion it must go out when it would attempt the slightest application."

For example, I seek to show that some property pertains to some object whose concept seems to me at first indefinable, because it is intuitive. At first I fail or must content myself with approximate proofs; finally I decide to give to my object a precise definition, and this enables me to establish this property in an irreproachable manner.

"And then," say the philosophers, "it still remains to show that the object which corresponds to this definition is indeed the same made known to you by intuition; or else that some real and concrete object whose conformity with your intuitive idea you believe you immediately recognize corresponds to your new definition. Only then could you affirm that it has the property in question. You have only displaced the difficulty."

That is not exactly so; the difficulty has not been displaced, it has been divided. The proposition to be established was in reality composed of two different truths, at first not distinguished. The first was a mathematical truth, and it is now rigorously established. The second was an experimental verity. Experience alone can teach us that some real and concrete object corresponds or does not correspond to some abstract definition. This second verity is not mathematically demonstrated, but neither can it be, no more than can the empirical laws of the physical and natural sciences. It would be unreasonable to ask more.

Well, is it not a great advance to have distinguished what long was wrongly confused? Does this mean that nothing is left of this objection of the philosophers? That I do not intend to say; in becoming rigorous, mathematical science takes a character so artificial as to strike every one; it forgets its historical origins; we see how the questions can be answered, we no longer see how and why they are put.

This shows us that logic is not enough; that the science of demonstration is not all science and that intuition must retain its rôle as complement, I was about to say, as counterpoise or as antidote of logic.

I have already had occasion to insist on the place intuition should hold in the teaching of the mathematical sciences. Without it young minds could not make a beginning in the understanding of mathematics; they could not learn to love it and would see in it only a vain logomachy; above all, without intuition they would never become capable of applying mathematics. But now I wish before all to speak of the rôle of intuition in science itself. If it is useful to the student, it is still more so to the creative scientist.

V

We seek reality, but what is reality? The physiologists tell us that organisms are formed of cells; the chemists add that cells themselves are formed of atoms. Does this mean that these atoms or these cells constitute reality, or rather the sole reality? The way in which these cells are arranged and from which results the unity of the individual, is not it also a reality much more interesting than that of the isolated elements, and should a naturalist who had never studied the elephant except by means of the microscope think himself sufficiently acquainted with that animal?

Well, there is something analogous to this in mathematics. The logician cuts up, so to speak, each demonstration into a very great number of elementary operations; when we have examined these operations one after the other and ascertained that each is correct, are we to think we have grasped the real meaning of the demonstration? Shall we have understood it even when, by an effort of memory, we have become able to repeat this proof by reproducing all these elementary operations in just the order in which the inventor had arranged them? Evidently not; we shall not yet possess the entire reality; that I know not what which makes the unity of the demonstration will completely elude us.

Pure analysis puts at our disposal a multitude of procedures whose infallibility it guarantees; it opens to us a thousand different ways on which we can embark in all confidence; we are assured of meeting there no obstacles; but of all these ways, which will lead us most promptly to our goal? Who shall tell us which to choose? We need a faculty which makes us see the end from afar, and intuition is this faculty. It is necessary to the explorer for choosing his route; it is not less so to the one following his trail who wants to know why he chose it.

If you are present at a game af chess, it will not suffice, for the understanding of the game, to know the rules for moving the pieces. That will only enable you to recognize that each move has been made conformably to these rules, and this knowledge will truly have very little value. Yet this is what the reader of a book on mathematics would do if he were a logician only. To understand the game is wholly another matter; it is to know why the player moves this piece rather than that other which he could have moved without breaking the rules of the game. It is to perceive the inward reason which makes of this series of successive moves a sort of organized whole. This faculty is still more necessary for the player himself, that is, for the inventor.

Let us drop this comparison and return to mathematics. For example, see what has happened to the idea of continuous function. At the outset this was only a sensible image, for example, that of a continuous mark traced by the chalk on a blackboard. Then it became little by little more refined; ere long it was used to construct a complicated system of inequalities, which reproduced, so to speak, all the lines of the original image; this construction finished, the centering of the arch, so to say, was removed, that crude representation which had temporarily served as support and which was afterward useless was rejected; there remained only the construction itself, irreproachable in the eyes of the logician. And yet if the primitive image had totally disappeared from our recollection, how could we divine by what caprice all these inequalities were erected in this fashion one upon another?

Perhaps you think I use too many comparisons; yet pardon still another. You have doubtless seen those delicate assemblages of silicious needles which form the skeleton of certain sponges. When the organic matter has disappeared, there remains only a frail and elegant lace-work. True, nothing is there except silica, but what is interesting is the form this silica has taken, and we could not understand it if we did not know the living sponge which has given it precisely this form. Thus it is that the old intuitive notions of our fathers, even when we have abandoned them, still imprint their form upon the logical constructions we have put in their place.

This view of the aggregate is necessary for the inventor; it is equally necessary for whoever wishes really to comprehend the inventor. Can logic give it to us? No; the name mathematicians give it would suffice to prove this. In mathematics logic is called *analysis* and analysis means *division, dissection*. It can have, therefore, no tool other than the scalpel and the microscope.

Thus logic and intuition have each their necessary rôle. Each is indispensable. Logic, which alone can give certainty, is the instrument of demonstration; intuition is the instrument of invention.

VI

But at the moment of formulating this conclusion I am seized with scruples. At the outset I distinguished two kinds of mathematical minds, the one sort logicians and analysts, the others intuitionalists and geometers. Well, the analysts also have been inventors. The names I have just cited make my insistence on this unnecessary.

Here is a contradiction, at least apparently, which needs explanation. And first, do you think these logicians have always proceeded from the general to the particular, as the rules of formal logic would seem to require of them? Not thus could they have extended the boundaries of science; scientific conquest is to be made only by generalization.

In one of the chapters of 'Science and Hypothesis,' I have had occasion to study the nature of mathematical reasoning, and I have shown how this reasoning, without ceasing to be absolutely rigorous, could lift us from the particular to the general by a procedure I have called *mathematical induction.* It is by this procedure that the analysts have made science progress, and if we examine the detail itself of their demonstrations, we shall find it there at each instant beside the classic syllogism of Aristotle. We, therefore, see already that the analysts are not simply makers of syllogisms after the fashion of the scholastics.

Besides, do you think they have always marched step by step with no vision of the goal they wished to attain? They must have divined the way leading thither, and for that they needed a guide. This guide is, first, analogy. For example, one of the methods of demonstration dear to analysts is that founded on the employment of dominant functions. We know it has already served to solve a multitude of problems; in what consists then the rôle of the inventor who wishes to apply it to a new problem? At the outset he must recognize the analogy of this question with those which have already been solved by this method; then he must perceive in what way this new question differs from the others, and thence deduce the modifications necessary to apply to the method.

But how does one perceive these analogies and these differences? In the example just cited they are almost always evident, but I could have found others where they would have been much more deeply hidden; often a very uncommon penetration is necessary for their discovery. The analysts, not to let these hidden analogies escape them, that is, in order to be inventors, must, without the aid of the senses and imagination, have a direct sense of what constitutes the unity of a piece of reasoning, of what makes, so to speak, its soul and inmost life.

When one talked with M. Hermite, he never evoked a sensuous image, and yet you soon perceived that the most abstract entities were for him like living beings. He did not see them, but he perceived that they are not an artificial assemblage, and that they have some principle of internal unity.

But, one will say, that still is intuition. Shall we conclude that the distinction made at the outset was only apparent, that there is only one sort of mind and that all the mathematicians are intuitionalists, at least those who are capable of inventing?

No, our distinction corresponds to something real. I have said above that there are many kinds of intuition. I have said how much the intuition of pure number, whence comes rigorous mathematical induction, differs from sensible intuition to which the imagination, properly so called, is the principal contributor.

Is the abyss which separates them less profound than it at first appeared? Could we recognize with a little attention that this pure intuition itself could not do without the aid of the senses? This is the affair of the psychologist and the metaphysician and I shall not discuss the question. But the thing's being doubtful is enough to justify me in recognizing and affirming an essential difference between the two kinds of intuition; they have not the same object and seem to call into play two different faculties of our soul; one would think of two search-lights directed upon two worlds strangers to one another.

It is the intuition of pure number, that of pure logical forms, which illumines and directs those we have called *analysts*. This it is which enables them not alone to demonstrate, but also to invent. By it they perceive at a glance the general plan of a logical edifice, and that too without the senses appearing to intervene. In rejecting the aid of the imagination, which, as we have seen, is not always infallible, they can advance without fear of deceiving themselves. Happy, therefore, are those who can do without this aid! We must admire them; but how rare they are!

Among the analysts there will then be inventors, but they will be few. The majority of us, if we wished to see afar by pure intuition alone, would soon feel ourselves seized with vertigo. Our weakness has need of a staff more solid, and, despite the exceptions of which we have just spoken, it is none the less true that sensible intuition is in mathematics the most usual instrument of invention.

Apropos of these reflections, a question comes up that I have not the time either to solve or even to enunciate with the developments it would admit of. Is there room for a new distinction, for distinguishing among the analysts those who above all use this pure intuition and those who are first of all preoccupied with formal logic?

M. Hermite, for example, whom I have just cited, can not be classed among the geometers who make use of the sensible intuition; but neither is he a logician, properly so called. He does not conceal his aversion to purely deductive procedures which start from the general and end in the particular.

CHAPTER II

The Measure of Time

I

So long as we do not go outside the domain of consciousness, the notion of time is relatively clear. Not only do we distinguish without difficulty present sensation from the remembrance of past sensations or the anticipation of future sensations, but we know perfectly well what we mean when we say that, of two conscious phenomena which we remember, one was anterior to the other; or that, of two foreseen conscious phenomena, one will be anterior to the other.

When we say that two conscious facts are simultaneous, we mean that they profoundly interpenetrate, so that analysis can not separate them without mutilating them.

The order in which we arrange conscious phenomena does not admit of any arbitrariness. It is imposed upon us and of it we can change nothing.

I have only a single observation to add. For an aggregate of sensations to have become a remembrance capable of classification in time, it must have ceased to be actual, we must have lost the sense of its infinite complexity, otherwise it would have remained present. It must, so to speak, have crystallized around a center of associations of ideas which will be a sort of label. It is only when they thus have lost all life that we can classify our memories in time as a botanist arranges dried flowers in his herbarium.

But these labels can only be finite in number. On that score, psychologic time should be discontinuous. Whence comes the feeling that between any two instants there are others? We arrange our recollections in time, but we know that there remain empty compartments. How could that be, if time were not a form preexistent in our mind? How could we know there were empty compartments, if these compartments were revealed to us only by their content?

II

But that is not all; into this form we wish to put not only the phenomena of our own consciousness, but those of which other consciousnesses are the theater. But more, we wish to put there physical facts, these I know not what with which we people space and which no consciousness sees directly. This is necessary because without it science could not exist. In a word, psychologic time is given to us and must needs create scientific and physical time. There the difficulty begins, or rather the difficulties, for there are two.

Think of two consciousnesses, which are like two worlds impenetrable one to the other. By what do we strive to put them into the same mold, to measure them by the same standard? Is it not as if one strove to measure length with a gram or weight with a meter? And besides, why do we speak of measuring? We know perhaps that some fact is anterior to some other, but not *by how much* it is anterior.

Therefore two difficulties: (1) Can we transform psychologic time, which is qualitative, into a quantitative time? (2) Can we reduce to one and the same measure facts which transpire in different worlds?

III

The first difficulty has long been noticed; it has been the subject of long discussions and one may say the question is settled. *We have not a direct intuition of the equality of two intervals of time.* The persons who believe they possess this intuition are dupes of an illusion. When I say, from noon to one the same time passes as from two to three, what meaning has this affirmation?

The least reflection shows that by itself it has none at all. It will only have that which I choose to give it, by a definition which will certainly possess a certain degree of arbitrariness. Psychologists could have done without this definition; physicists and astronomers could not; let us see how they have managed.

To measure time they use the pendulum and they suppose by definition that all the beats of this pendulum are of equal duration. But this is only a first approximation; the temperature, the resistance of the air, the barometric pressure, make the pace of the pendulum vary. If we could escape these sources of error, we should obtain a much closer approximation, but it would still be only an approximation. New causes, hitherto neglected, electric, magnetic or others, would introduce minute perturbations.

In fact, the best chronometers must be corrected from time to time, and the corrections are made by the aid of astronomic observations; arrangements are made so that the sidereal clock marks the same hour when the same star passes the meridian. In other words, it is the sidereal day, that is, the duration of the rotation of the earth, which is the constant unit of time. It is supposed, by a new definition substituted for that based on the beats of the pendulum, that two complete rotations of the earth about its axis have the same duration.

However, the astronomers are still not content with this definition. Many of them think that the tides act as a check on our globe, and that the rotation of the earth is becoming slower and slower. Thus would be explained the apparent acceleration of the motion of the moon, which would seem to be going more rapidly than theory permits because our watch, which is the earth, is going slow.

IV

All this is unimportant, one will say; doubtless our instruments of measurement are imperfect, but it suffices that we can conceive a perfect instrument. This ideal can not be reached, but it is enough to have conceived it and so to have put rigor into the definition of the unit of time.

The trouble is that there is no rigor in the definition. When we use the pendulum to measure time, what postulate do we implicitly admit? *It is that the duration of two identical phenomena is the same;* or, if you prefer, that the same causes take the same time to produce the same effects.

And at first blush, this is a good definition of the equality of two durations. But take care. Is it impossible that experiment may some day contradict our postulate?

Let me explain myself. I suppose that at a certain place in the world the phenomenon α happens, causing as consequence at the end of a certain time the effect α'. At another place in the world very far away from the first, happens the phenomenon β, which causes as consequence the effect β'. The phenomena α and β are simultaneous, as are also the effects α' and β'.

Later, the phenomenon α is reproduced under approximately the same conditions as before, and *simultaneously* the phenomenon β is also reproduced at a very distant place in the world and almost under the same circumstances. The effects α' and β' also take place. Let us suppose that the effect α' happens perceptibly before the effect β'.

If experience made us witness such a sight, our postulate would be contradicted. For experience would tell us that the first duration $\alpha\alpha'$ is equal to the first duration $\beta\beta'$ and that the second duration $\alpha\alpha'$ is less than the second duration $\beta\beta'$. On the other hand, our postulate would require that the two durations $\alpha\alpha'$ should be equal to each other, as likewise the two durations $\beta\beta'$. The equality and the inequality deduced from experience would be incompatible with the two equalities deduced from the postulate.

Now can we affirm that the hypotheses I have just made are absurd? They are in no wise contrary to the principle of contradiction. Doubtless they could not happen without the principle of sufficient reason seeming violated. But to justify a definition so fundamental I should prefer some other guarantee.

V

But that is not all. In physical reality one cause does not produce a given effect, but a multitude of distinct causes contribute to produce it, without our having any means of discriminating the part of each of them.

Physicists seek to make this distinction; but they make it only approximately, and, however they progress, they never will make it except approximately. It is approximately true that the motion of the pendulum is due solely to the earth's attraction; but in all rigor every attraction, even of Sirius, acts on the pendulum.

Under these conditions, it is clear that the causes which have produced a certain effect will never be reproduced except approximately. Then we should modify our postulate and our definition. Instead of saying: 'The same causes take the same time to produce the same effects,' we should say: 'Causes almost identical take almost the same time to produce almost the same effects.'

Our definition therefore is no longer anything but approximate. Besides, as M. Calinon very justly remarks in a recent memoir:[2]

> One of the circumstances of any phenomenon is the velocity of the earth's rotation; if this velocity of rotation varies, it constitutes in the reproduction of this phenomenon a circumstance which no longer remains the same. But to suppose this velocity of rotation constant is to suppose that we know how to measure time.

Our definition is therefore not yet satisfactory; it is certainly not that which the astronomers of whom I spoke above implicitly adopt, when they affirm that the terrestrial rotation is slowing down.

What meaning according to them has this affirmation? We can only understand it by analyzing the proofs they give of their proposi-

[2] 'Etude sur les diverses grandeurs,' Paris, Gauthier-Villars, 1897.

tion. They say first that the friction of the tides producing heat must destroy *vis viva*. They invoke therefore the principle of *vis viva*, or of the conservation of energy.

They say next that the secular acceleration of the moon, calculated according to Newton's law, would be less than that deduced from observations unless the correction relative to the slowing down of the terrestrial rotation were made. They invoke therefore Newton's law. In other words, they define duration in the following way: time should be so defined that Newton's law and that of *vis viva* may be verified. Newton's law is an experimental truth; as such it is only approximate, which shows that we still have only a definition by approximation.

If now it be supposed that another way of measuring time is adopted, the experiments on which Newton's law is founded would none the less have the same meaning. Only the enunciation of the law would be different, because it would be translated into another language; it would evidently be much less simple. So that the definition implicitly adopted by the astronomers may be summed up thus: Time should be so defined that the equations of mechanics may be as simple as possible. In other words, there is not one way of measuring time more true than another; that which is generally adopted is only more *convenient*. Of two watches, we have no right to say that the one goes true, the other wrong; we can only say that it is advantageous to conform to the indications of the first.

The difficulty which has just occupied us has been, as I have said, often pointed out; among the most recent works in which it is considered, I may mention, besides M. Calinon's little book, the treatise on mechanics of M. Andrade.

VI

The second difficulty has up to the present attracted much less attention; yet it is altogether analogous to the preceding; and even, logically, I should have spoken of it first.

Two psychological phenomena happen in two different consciousnesses; when I say they are simultaneous, what do I mean? When I say that a physical phenomenon, which happens outside of every consciousness, is before or after a psychological phenomenon, what do I mean?

In 1572, Tycho Brahe noticed in the heavens a new star. An immense conflagration had happened in some far distant heavenly body; but it had happened long before; at least two hundred years were necessary for the light from that star to reach our earth. This conflagration therefore happened before the discovery of America. Well, when considering this gigantic phenomenon, which perhaps had no witness, since the satellites of that star were perhaps uninhabited, I say this phenomenon is anterior to the formation of the visual image of the isle of Española in the consciousness of Christopher Columbus, what do I mean?

A little reflection is sufficient to understand that all these affirmations have by themselves no meaning. They can have one only as the outcome of a convention.

VII

We should first ask ourselves how one could have had the idea of putting into the same frame so many worlds impenetrable to each other. We should like to represent to ourselves the external universe, and only by so doing could we feel that we understood it. We know we never can attain this representation: our weakness is too great. But at least we desire the ability to conceive an infinite intelligence for which this representation would be possible, a sort of great consciousness which should see all, and which should classify all *in its time,* as we classify, *in our time,* the little we see.

This hypothesis is indeed crude and incomplete, because this supreme intelligence would be only a demigod; infinite in one sense, it would be limited in another, since it would have only an imperfect recollection of the past; and it could have no other, since otherwise all recollections would be equally present to it and for it there would be no time. And yet when we speak of time, for all which happens outside of us, do we not unconsciously adopt this hypothesis; do we not put ourselves in the place of this imperfect god; and do not even the atheists put themselves in the place where god would be if he existed?

What I have just said shows us, perhaps, why we have tried to put all physical phenomena into the same frame. But that can not pass for a definition of simultaneity, since this hypothetical intelligence, even if it existed, would be for us impenetrable. It is therefore necessary to seek something else.

VIII

The ordinary definitions which are proper for psychologic time would suffice us no better. Two simultaneous psychologic facts are so closely bound together that analysis can not separate without mutilating them. Is it the same with two physical facts? Is not my present nearer my past of yesterday than the present of Sirius?

It has also been said that two facts should be regarded as simultaneous when the order of their succession may be inverted at will.[1] It is evident that this definition would not suit two physical facts which happen far from one another, and that, in what concerns them, we no longer even understand what this reversibility would be; besides, succession itself must first be defined.

IX

Let us then seek to give an account of what is understood by simultaneity or antecedence, and for this let us analyze some examples.

I write a letter; it is afterward read by the friend to whom I have addressed it. There are two facts which have had for their theater two different consciousnesses. In writing this letter I have had the visual image of it, and my friend has had in his turn this same visual image in reading the letter. Though these two facts happen in impenetrable worlds, I do not hesitate to regard the first as anterior to the second, because I believe it is its cause.

I hear thunder, and I conclude there has been an electric discharge; I do not hesitate to consider the physical phenomenon as anterior to the auditory image perceived in my consciousness, because I believe it is its cause.

Behold then the rule we follow, and the only one we can follow: when a phenomenon appears to us as the cause of another, we regard it as anterior. It is therefore by cause that we define time; but most often, when two facts appear to us bound by a constant relation, how do we recognize which is the cause and which the effect? We assume that the anterior fact, the antecedent, is the cause of the other, of the consequent. It is then by time that we define cause. How save ourselves from this *petitio principii?*

We say now *post hoc, ergo propter hoc;* now *propter hoc, ergo post hoc;* shall we escape from this vicious circle?

X

Let us see, not how we succeed in escaping, for we do not completely succeed, but how we try to escape.

I execute a voluntary act A and I feel afterward a sensation D, which I regard as a consequence of the act A; on the other hand, for whatever reason, I infer that this consequence is not immediate, but that outside my consciousness two facts B and C, which I have not witnessed, have happened, and in such a way that B is the effect of A, that C is the effect of B, and D of C.

But why? If I think I have reason to regard the four facts *A, B, C, D,* as bound to one another by a causal connection, why range them in the causal order *A B C D,* and at the same time in the chronologic order *A B C D,* rather than in any other order?

I clearly see that in the act *A* I have the feeling of having been active, while in undergoing the sensation *D,* I have that of having been passive. This is why I regard *A* as the initial cause and *D* as the ultimate effect; this is why I put *A* at the beginning of the chain and *D* at the end; but why put *B* before *C* rather than *C* before *B*?

If this question is put, the reply ordinarily is: we know that it is *B* which is the cause of *C* because we *always* see *B* happen before *C*. These two phenomena, when witnessed, happen in a certain order; when analogous phenomena happen without witness, there is no reason to invert this order.

Doubtless, but take care; we never know directly the physical phenomena *B* and *C*. What we know are sensations *B′* and *C′* produced respectively by *B* and *C*. Our consciousness tells us immediately that *B′* precedes *C′* and we *suppose* that *B* and *C* succeed one another in the same order.

This rule appears in fact very natural, and yet we are often led to depart from it. We hear the sound of the thunder only some seconds after the electric discharge of the cloud. Of two flashes of lightning, the one distant, the other near, can not the first be anterior to the second, even though the sound of the second comes to us before that of the first?

XI.

Another difficulty; have we really the right to speak of the cause of a phenomenon? If all the parts of the universe are interchained in a certain measure, any one phenomenon will not be the effect of a single cause, but the resultant of causes infinitely numerous; it is, one often says, the consequence of the state of the universe a moment before. How enunciate rules applicable to circumstances so complex? And yet it is only thus that these rules can be general and rigorous.

Not to lose ourselves in this infinite complexity let us make a simpler hypothesis. Consider three stars, for example, the sun, Jupiter and Saturn; but, for greater simplicity, regard them as reduced to material points and isolated from the rest of the world. The positions and the velocities of three bodies at a given instant suffice to determine their positions and velocities at the following instant, and consequently at any instant. Their positions at the instant *t* determine their positions at the instant $t+h$ as well as their positions at the instant $t-h$.

Even more; the position of Jupiter at the instant t, together with that of Saturn at the instant $t+a$, determines the position of Jupiter at any instant and that of Saturn at any instant.

The aggregate of positions occupied by Jupiter at the instant $t+e$ and Saturn at the instant $t+a+e$ is bound to the aggregate of positions occupied by Jupiter at the instant t and Saturn at the instant $t+a$, by laws as precise as that of Newton, though more complicated. Then why not regard one of these aggregates as the cause of the other, which would lead to considering as simultaneous the instant t of Jupiter and the instant $t+a$ of Saturn?

In answer there can only be reasons, very strong, it is true, of convenience and simplicity.

XII

But let us pass to examples less artificial; to understand the definition implicitly supposed by the savants, let us watch them at work and look for the rules by which they investigate simultaneity.

I will take two simple examples, the measurement of the velocity of light and the determination of longitude.

When an astronomer tells me that some stellar phenomenon, which his telescope reveals to him at this moment, happened nevertheless fifty years ago, I seek his meaning, and to that end I shall ask him first how he knows it, that is, how he has measured the velocity of light.

He has begun by *supposing* that light has a constant velocity, and in particular that its velocity is the same in all directions. That is a postulate without which no measurement of this velocity could be attempted. This postulate could never be verified directly by experiment; it might be contradicted by it if the results of different measurements were not concordant. We should think ourselves fortunate that this contradiction has not happened and that the slight discordances which may happen can be readily explained.

The postulate, at all events, resembling the principle of sufficient reason, has been accepted by everybody; what I wish to emphasize is that it furnishes us with a new rule for the investigation of simultaneity, entirely different from that which we have enunciated above.

This postulate assumed, let us see how the velocity of light has been measured. You know that Roemer used eclipses of the satellites of Jupiter, and sought how much the event fell behind its prediction.

But how is this prediction made? It is by the aid of astronomic laws, for instance Newton's law.

Could not the observed facts be just as well explained if we attributed to the velocity of light a little different value from that adopted, and supposed Newton's law only approximate? Only this would lead to replacing Newton's law by another more complicated. So for the velocity of light a value is adopted, such that the astronomic laws compatible with this value may be as simple as possible. When navigators or geographers determine a longitude, they have to solve just the problem we are discussing; they must, without being at Paris, calculate Paris time. How do they accomplish it? They carry a chronometer set for Paris. The qualitative problem of simultaneity is made to depend upon the quantitative problem of the measurement of time. I need not take up the difficulties relative to this latter problem, since above I have emphasized them at length.

Or else they observe an astronomic phenomenon, such as an eclipse of the moon, and they suppose that this phenomenon is perceived simultaneously from all points of the earth. That is not altogether true, since the propagation of light is not instantaneous; if absolute exactitude were desired, there would be a correction to make according to a complicated rule.

Or else finally they use the telegraph. It is clear first that the reception of the signal at Berlin, for instance, is after the sending of this same signal from Paris. This is the rule of cause and effect analyzed above. But how much after? In general, the duration of the transmission is neglected and the two events are regarded as simultaneous. But, to be rigorous, a little correction would still have to be made by a complicated calculation; in practise it is not made, because it would be well within the errors of observation; its theoretic necessity is none the less from our point of view, which is that of a rigorous definition. From this discussion, I wish to emphasize two things: (1) The rules applied are exceedingly various. (2) It is difficult to separate the qualitative problem of simultaneity from the quantitative problem of the measurement of time; no matter whether a chronometer is used, or whether account must be taken of a velocity of transmission, as that of light, because such a velocity could not be measured without *measuring* a time.

XIII

To conclude: We have not a direct intuition of simultaneity, nor of the equality of two durations. If we think we have this intuition, this is an illusion. We replace it by the aid of certain rules which we apply almost always without taking count of them.

But what is the nature of these rules? No general rule, no rigorous rule; a multitude of little rules applicable to each particular case.

These rules are not imposed upon us and we might amuse ourselves in inventing others; but they could not be cast aside without greatly complicating the enunciation of the laws of physics, mechanics and astronomy.

We therefore choose these rules, not because they are true, but because they are the most convenient, and we may recapitulate them as follows: " The simultaneity of two events, or the order of their succession, the equality of two durations, are to be so defined that the enunciation of the natural laws may be as simple as possible. In other words, all these rules, all these definitions are only the fruit of an unconscious opportunism."

CHAPTER III

The Notion of Space

1. *Introduction*

In the articles I have heretofore devoted to space I have above all emphasized the problems raised by non-Euclidean geometry, while leaving almost completely aside other questions more difficult of approach, such as those which pertain to the number of dimensions. All the geometries I considered had thus a common basis, that tridimensional continuum which was the same for all and which differentiated itself only by the figures one drew in it or when one aspired to measure it.

In this continuum, primitively amorphous, we may imagine a network of lines and surfaces, we may then convene to regard the meshes of this net as equal to one another, and it is only after this convention that this continuum, become measurable, becomes Euclidean or non-Euclidean space. From this amorphous continuum can therefore arise indifferently one or the other of the two spaces, just as on a blank sheet of paper may be traced indifferently a straight or a circle.

In space we know rectilinear triangles the sum of whose angles is equal to two right angles; but equally we know curvilinear triangles the sum of whose angles is less than two right angles. The existence of the one sort is not more doubtful than that of the other. To give the name of straights to the sides of the first is to adopt Euclidean geometry; to give the name of straights to the sides of the latter is to adopt the non-Euclidean geometry. So that to ask what geometry it is proper to adopt is to ask, to what line is it proper to give the name straight?

It is evident that experiment can not settle such a question; one would not ask, for instance, experiment to decide whether I should call *AB* or *CD* a straight. On the other hand, neither can I say that I have not the right to give the name of straights to the sides of non-Euclidean triangles because they are not in conformity with the eternal idea of straight which I have by intuition. I grant, indeed, that I have the intuitive idea of the side of the Euclidean triangle, but I have equally the intuitive idea of the side of the non-Euclidean triangle.

Why should I have the right to apply the name of straight to the first of these ideas and not to the second? Wherein does this syllable form an integrant part of this intuitive idea? Evidently when we say that the Euclidean straight is a *true* straight and that the non-Euclidean straight is not a true straight, we simply mean that the first intuitive idea corresponds to a *more noteworthy* object than the second. But how do we decide that this object is more noteworthy? This question I have investigated in 'Science and Hypothesis.'

It is here that we saw experience come in. If the Euclidean straight is more noteworthy than the non-Euclidean straight, it is so chiefly because it differs little from certain noteworthy natural objects from which the non-Euclidean straight differs greatly. But, it will be said, the definition of the non-Euclidean straight is artificial; if we for a moment adopt it, we shall see that two circles of different radius both receive the name of non-Euclidean straights, while of two circles of the same radius one can satisfy the definition without the other being able to satisfy it, and then if we transport one of these so-called straights without deforming it, it will cease to be a straight. But by what right do we consider as equal these two figures which the Euclidean geometers call two circles with the same radius? It is because by transporting one of them without deforming it we can make it coincide with the other. And why do we say this transportation is effected without deformation? It is impossible to give a good reason for it. Among all the motions conceivable, there are some of which the Euclidean geometers say that they are not accompanied by deformation; but there are others of which the non-Euclidean geometers would say that they are not accompanied by deformation. In the first, called Euclidean motions, the Euclidean straights remain Euclidean straights, and the non-Euclidean straights do not remain non-Euclidean straights; in the motions of the second sort, or non-Euclidean motions, the non-Euclidean straights remain non-Euclidean straights and the Euclidean straights do not remain Euclidean straights. It has, therefore, not been demonstrated that it was unreasonable to call straights the sides of non-Euclidean triangles; it has only been shown that that would be unreasonable if one continued to call the Euclidean motions motions without deformation; but it has at the same time been shown that it would be just as unreasonable to call straights the sides of Euclidean triangles if the non-Euclidean motions were called motions without deformation.

Now when we say that the Euclidean motions are the *true* motions without deformation, what do we mean? We simply mean that they are *more noteworthy* than the others. And why are they more noteworthy? It is because certain noteworthy natural bodies, the solid bodies, undergo motions almost similar.

And then when we ask: Can one imagine non-Euclidean space? that means: Can we imagine a world where there would be noteworthy natural objects affecting almost the form of non-Euclidean straights, and noteworthy natural bodies frequently undergoing motions almost similar to the non-Euclidean motions? I have shown in 'Science and Hypothesis' that to this question we must answer yes.

It has often been observed that if all the bodies in the universe were dilated simultaneously and in the same proportion, we should have no means of perceiving it, since all our measuring instruments would grow at the same time as the objects themselves which they serve to measure. The world, after this dilatation, would continue on its course without anything apprising us of so considerable an event. In other words, two worlds similar to one another (understanding the word similitude in the sense of Euclid, Book VI.) would be absolutely indistinguishable. But more; worlds will be indistinguishable not only if they are equal or similar, that is, if we can pass from one to the other by changing the axes of coordinates, or by changing the scale to which lengths are referred; but they will still be indistinguishable if we can pass from one to the other by any 'point-transformation' whatever. I will explain my meaning. I suppose that to each point of one corresponds one point of the other and only one, and inversely; and besides that the coordinates of a point are continuous functions, *otherwise altogether arbitrary,* of the corresponding point. I suppose besides that to each object of the first world corresponds in the second an object of the same nature placed precisely at the corresponding point. I suppose finally that this correspondence fulfilled at the initial instant is maintained indefinitely. We should have no means of distinguishing these two worlds one from the other. The relativity of space is not ordinarily understood in so broad a sense; it is thus, however, that it would be proper to understand it.

If one of these universes is our Euclidean world, what its inhabitants will call straight will be our Euclidean straight; but what the inhabitants of the second world will call straight will be a curve which will have the same properties in relation to the world they inhabit and in relation to the motions that they will call motions without deformation. Their geometry will, therefore, be Euclidean geometry, but their straight will not be our Euclidean straight. It will be its transform by the point-transformation which carries over from our world to theirs. The straights of these men will not be our straights, but they will have among themselves the same relations as our straights to one another. It is in this sense I say their geometry will be ours. If then we wish after all to proclaim that they deceive themselves, that their straight is not the true straight, if we still are unwilling to admit that such an affirmation has no meaning, at least we must confess that these people have no means whatever of recognizing their error.

2. *Qualitative Geometry*

All that is relatively easy to understand, and I have already so often repeated it that I think it needless to expatiate further on the matter. Euclidean space is not a form imposed upon our sensibility, since we can imagine non-Euclidean space; but the two spaces, Euclidean and non-Euclidean, have a common basis, that amorphous continuum of which I spoke in the beginning. From this continuum we can get either Euclidean space or Lobachevskian space, just as we can, by tracing upon it a proper graduation, transform an ungraduated thermometer into a Fahrenheit or a Réaumur thermometer.

And then comes a question: Is not this amorphous continuum that our analysis has allowed to survive a form imposed upon our sensibility? If so, we should have enlarged the prison in which this sensibility is confined, but it would always be a prison.

This continuum has a certain number of properties, exempt from all idea of measurement. The study of these properties is the object of a science which has been cultivated by many great geometers and in particular by Riemann and Betti and which has received the name of analysis situs. In this science abstraction is made of every quantitative idea and, for example, if we ascertain that on a line the point B is between the points A and C, we shall be content with this ascertainment and shall not trouble to know whether the line ABC is straight or curved, nor whether the length AB is equal to the length BC, or whether it is twice as great.

The theorems of analysis situs have, therefore, this peculiarity that they would remain true if the figures were copied by an inexpert draftsman who should grossly change all the proportions and replace the straights by lines more or less sinuous. In mathematical terms, they are not altered by any 'point-transformation' whatsoever. It has often been said that metric geometry was quantitative, while projective geometry was purely qualitative. That is not altogether true. The straight is still distinguished from other lines by properties which remain quantitative in some respects. The real qualitative geometry is, therefore, analysis situs.

The same questions which came up apropos of the truths of Euclidean geometry, come up anew apropos of the theorems of analysis situs. Are they obtainable by deductive reasoning? Are they disguised conventions? Are they experimental verities? Are they the characteristics of a form imposed either upon our sensibility or upon our understanding?

I wish simply to observe that the last two solutions exclude each other. We can not admit at the same time that it is impossible to imagine space of four dimensions and that experience proves to us that space has three dimensions. The experimenter puts to nature a question: Is it this or that? and he can not put it without imagining the two terms of the alternative. If it were impossible to imagine one of these terms, it would be futile and besides impossible to consult experience. There is no need of observation to know that the hand of a watch is not marking the hour 15 on the dial, because we know beforehand that there are only 12, and we could not look at the mark 15 to see if the hand is there, because this mark does not exist.

Note likewise that in analysis situs the empiricists are disembarrassed of one of the gravest objections that can be leveled against them, of that which renders absolutely vain in advance all their efforts to apply their thesis to the verities of Euclidean geometry. These verities are rigorous and all experimentation can only be approximate. In analysis situs approximate experiments may suffice to give a rigorous theorem and, for instance, if it is seen that space can not have either two or less than two dimensions, nor four or more than four, we are certain that it has exactly three, since it could not have two and a half or three and a half.

Of all the theorems of analysis situs, the most important is that which is expressed in saying that space has three dimensions. This it is that we are about to consider, and we shall put the question in these terms: When we say that space has three dimensions, what do we mean?

3. *The Physical Continuum of Several Dimensions*

I have explained in 'Science and Hypothesis' whence we derive the notion of physical continuity and how that of mathematical continuity has arisen from it. It happens that we are capable of distinguishing two impressions one from the other, while each is indistinguishable from a third. Thus we can readily distinguish a weight of 12 grams from a weight of 10 grams, while a weight of 11 grams could neither be distinguished from the one nor the other. Such a statement, translated into symbols, may be written:

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

This would be the formula of the physical continuum, as crude experience gives it to us, whence arises an intolerable contradiction that has been obviated by the introduction of the mathematical continuum. This is a scale of which the steps (commensurable or incommensurable numbers) are infinite in number, but are exterior to one another instead of encroaching on one another as do the elements of the physical continuum, in conformity with the preceding formula.

The physical continuum is, so to speak, a nebula not resolved; the most perfect instruments could not attain to its resolution. Doubtless if we measured the weights with a good balance instead of judging them by the hand, we could distinguish the weight of 11 grams from those of 10 and 12 grams, and our formula would become:

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

But we should always find between A and B and between B and C new elements D and E, such that

$$A = D, \quad D = B, \quad A < B; \quad B = E, \quad E = C, \quad B < C,$$

and the difficulty would only have receded and the nebula would always remain unresolved; the mind alone can resolve it and the mathematical continuum it is which is the nebula resolved into stars.

Yet up to this point we have not introduced the notion of the number of dimensions. What is meant when we say that a mathematical continuum or that a physical continuum has two or three dimensions?

First we must introduce the notion of cut, studying first physical continua. We have seen what characterizes the physical continuum. Each of the elements of this continuum consists of a manifold of impressions; and it may happen either that an element can not be discriminated from another element of the same continuum, if this new element corresponds to a manifold of impressions not sufficiently different, or, on the contrary, that the discrimination is possible; finally it may happen that two elements indistinguishable from a third, may, nevertheless, be distinguished one from the other.

That postulated, if A and B are two distinguishable elements of a continuum C, a series of elements may be found, $E_1, E_2, \cdots, E_n$, all belonging to this same continuum C and such that each of them is indistinguishable from the preceding, that E_1 is indistinguishable from A and E_n indistinguishable from B. Therefore we can go from A to B by a continuous route and without quitting C. If this condition is fulfilled for any two elements A and B of the continuum C, we may say that this continuum C is all in one piece. Now let us distinguish certain of the elements of C which may either be all distinguishable from one another, or themselves form one or several continua. The assemblage of the elements thus chosen arbitrarily among all those of C will form what I shall call the *cut* or the *cuts*.

Take on C any two elements A and B. Either we can also find a series of elements $E_1, E_2, \cdots, E_n$, such: (1) that they all belong to C; (2) that each of them is indistinguishable from the following, E_1 indistinguishable from A and E_n from B; (3) *and besides that none of the elements E is indistinguishable from any element of the cut.* Or else, on the contrary, in each of the series $E_1, E_2, \cdots, E_n$ satisfying the first two conditions, there will be an element E indistinguishable from one of the elements of the cut. In the first case we can go from A to B by a continuous route without quitting C and *without meeting the cuts;* in the second case that is impossible.

If then for any two elements A and B of the continuum C, it is always the first case which presents itself, we shall say that C remains all in one piece despite the cuts.

Thus, if we choose the cuts in a certain way, otherwise arbitrary, it may happen either that the continuum remains all in one piece or that it does not remain all in one piece; in this latter hypothesis we shall then say that it is *divided* by the cuts.

It will be noticed that all these definitions are constructed in setting out solely from this very simple fact, that two manifolds of impressions sometimes can be discriminated, sometimes can not be. That postulated, if, to *divide* a continuum, it suffices to consider as cuts a certain number of elements all distinguishable from one another, we say that this continuum *is of one dimension*; if, on the contrary, to divide a continuum, it is necessary to consider as cuts a system of elements themselves forming one or several continua, we shall say that this continuum is *of several dimensions.*

If to divide a continuum C, cuts forming one or several continua of one dimension suffice, we shall say that C is a continuum *of two dimensions;* if cuts suffice which form one or several continua of two dimensions at most, we shall say that C is a continuum *of three dimensions;* and so on.

To justify this definition it is proper to see whether it is in this way that geometers introduce the notion of three dimensions at the beginning of their works. Now, what do we see? Usually they begin by defining surfaces as the boundaries of solids or pieces of space, lines as the boundaries of surfaces, points as the boundaries of lines, and they affirm that the same procedure can not be pushed further.

This is just the idea given above: to divide space, cuts that are called surfaces are necessary; to divide surfaces, cuts that are called lines are necessary; to divide lines, cuts that are called points are necessary; we can go no further, the point can not be divided, so the point is not a continuum. Then lines which can be divided by cuts which are not continua will be continua of one dimension; surfaces which can be divided by continuous cuts of one dimension will be continua of two dimensions; finally space which can be divided by continuous cuts of two dimensions will be a continuum of three dimensions.

Thus the definition I have just given does not differ essentially from the usual definitions; I have only endeavored to give it a form applicable not to the mathematical continuum, but to the physical continuum, which alone is susceptible of representation, and yet to retain all its precision. Moreover, we see that this definition applies not alone to space; that in all which falls under our senses we find the characteristics of the physical continuum, which would allow of the same classification; that it would be easy to find there examples of continua of four, of five, dimensions, in the sense of the preceding definition; such examples occur of themselves to the mind.

I should explain finally, if I had the time, that this science, of which I spoke above and to which Riemann gave the name of analysis situs, teaches us to make distinctions among continua of the same number of dimensions and that the classification of these continua rests also on the consideration of cuts.

From this notion has arisen that of the mathematical continuum of several dimensions in the same way that the physical continuum of one dimension engendered the mathematical continuum of one dimension. The formula

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

which summed up the data of crude experience, implied an intolerable contradiction. To get free from it it was necessary to introduce a new notion while still respecting the essential characteristics of the physical continuum of several dimensions. The mathematical continuum of one dimension admitted of a scale whose divisions, infinite in number, corresponded to the different values, commensurable or not, of one same magnitude. To have the mathematical continuum of n dimensions, it will suffice to take n like scales whose divisions correspond to different values of n independent magnitudes called coordinates. We thus shall have an image of the physical continuum of n dimensions, and this image will be as faithful as it can be after the determination not to allow the contradiction of which I spoke above.

4. *The Notion of Point*

It seems now that the question we put to ourselves at the start is answered. When we say that space has three dimensions, it will be said, we mean that the manifold of points of space satisfies the definition we have just given of the physical continuum of three dimensions. To be content with that would be to suppose that we know what is the manifold of points of space, or even one point of space.

Now that is not as simple as one might think. Every one believes he knows what a point is, and it is just because we know it too well that we think there is no need of defining it. Surely we can not be required to know how to define it, because in going back from definition to definition a time must come when we must stop. But at what moment should we stop?

We shall stop first when we reach an object which falls under our senses or that we can represent to ourselves; definition then will become useless; we do not define the sheep to a child; we say to him: *See* the sheep.

So, then, we should ask ourselves if it is possible to represent to ourselves a point of space. Those who answer yes do not reflect that they represent to themselves in reality a white spot made with the chalk on a blackboard or a black spot made with a pen on white paper, and that they can represent to themselves only an object or rather the impressions that this object made on their senses.

When they try to represent to themselves a point, they represent the impressions that very little objects made them feel. It is needless to add that two different objects, though both very little, may produce extremely different impressions, but I shall not dwell on this difficulty, which would still require some discussion.

But it is not a question of that; it does not suffice to represent *one* point, it is necessary to represent *a certain* point and to have the means of distinguishing it from an *other* point. And in fact, that we may be able to apply to a continuum the rule I have above expounded and by which one may recognize the number of its dimensions, we must rely upon the fact that two elements of this continuum sometimes can and sometimes can not be distinguished. It is necessary therefore that we should in certain cases know how to represent to ourselves *a specific* element and to distinguish it from an *other* element.

The question is to know whether the point that I represented to myself an hour ago is the same as this that I now represent to myself, or whether it is a different point. In other words, how do we know whether the point occupied by the object A at the instant α is the same as the point occupied by the object B at the instant β, or still better, what this means?

I am seated in my room; an object is placed on my table; during a second I do not move, no one touches the object. I am tempted to say that the point A which this object occupied at the beginning of this second is identical with the point B which it occupies at its end. Not at all; from the point A to the point B is 30 kilometers, because the object has been carried along in the motion of the earth. We can not know whether an object, be it large or small, has not changed its absolute position in space, and not only can we not affirm it, but this affirmation has no meaning and in any case can not correspond to any representation.

But then we may ask ourselves if the relative position of an object with regard to other objects has changed or not, and first whether the relative position of this object with regard to our body has changed. If the impressions this object makes upon us have not changed, we shall be inclined to judge that neither has this relative position changed; if they have changed, we shall judge that this object has changed either in state or in relative position. It remains to decide which of the *two*. I have explained in 'Science and Hypothesis' how we have been led to distinguish the changes of position. Moreover, I shall return to that further on. We come to know, therefore, whether the relative position of an object with regard to our body has or has not remained the same.

If now we see that two objects have retained their relative position with regard to our body, we conclude that the relative position of these two objects with regard to one another has not changed; but we reach this conclusion only by indirect reasoning. The only thing that we know directly is the relative position of the objects with regard to our body. *A fortiori* it is only by indirect reasoning that we think we know (and, moreover, this belief is delusive) whether the absolute position of the object has changed.

In a word, the system of coordinate axes to which we naturally refer all exterior objects is a system of axes invariably bound to our body, and carried around with us.

It is impossible to represent to oneself absolute space; when I try to represent to myself simultaneously objects and myself in motion in absolute space, in reality I represent to myself my own self motionless and seeing move around me different objects and a man that is exterior to me, but that I convene to call me.

Will the difficulty be solved if we agree to refer everything to these axes bound to our body? Shall we know then what is a point thus defined by its relative position with regard to ourselves? Many persons will answer yes and will say that they 'localize' exterior objects.

What does this mean? To localize an object simply means to represent to oneself the movements that would be necessary to reach it. I will explain myself. It is not a question of representing the movements themselves in space, but solely of representing to oneself the muscular sensations which accompany these movements and which do not presuppose the preexistence of the notion of space.

If we suppose two different objects which successively occupy the same relative position with regard to ourselves, the impressions that these two objects make upon us will be very different; if we localize them at the same point, this is simply because it is necessary to make the same movements to reach them; apart from that, one can not just see what they could have in common.

But, given an object, we can conceive many different series of movements which equally enable us to reach it. If then we represent to ourselves a point by representing to ourselves the series of muscular sensations which accompany the movements which enable us to reach this point, there will be many ways entirely different of representing to oneself the same point. If one is not satisfied with this solution, but wishes, for instance, to bring in the visual sensations along with the muscular sensations, there will be one or two more ways of representing to oneself this same point and the difficulty will only be increased. In any case the following question comes up: Why do we think that all these representations so different from one another still represent the same point?

Another remark: I have just said that it is to our own body that we naturally refer exterior objects; that we carry about everywhere with us a system of axes to which we refer all the points of space, and that this system of axes seems to be invariably bound to our body. It should be noticed that rigorously we could not speak of axes invariably bound to the body unless the different parts of this body were themselves invariably bound to one another. As this is not the case, we ought, before referring exterior objects to these fictitious axes, to suppose our body brought back to the initial attitude.

5. *The Notion of Displacement*

I have shown in 'Science and Hypothesis' the preponderant rôle played by the movements of our body in the genesis of the notion of space. For a being completely immovable there would be neither space nor geometry; in vain would exterior objects be displaced about him, the variations which these displacements would make in his impressions would not be attributed by this being to changes of position, but to simple changes of state; this being would have no means of distinguishing these two sorts of changes, and this distinction, fundamental for us, would have no meaning for him.

The movements that we impress upon our members have as effect the varying of the impressions produced on our senses by external objects; other causes may likewise make them vary; but we are led to distinguish the changes produced by our own motions and we easily discriminate them for two reasons: (1) because they are voluntary; (2) because they are accompanied by muscular sensations.

So we naturally divide the changes that our impressions may undergo into two categories to which perhaps I have given an inappropriate designation: (1) the internal changes, which are voluntary and accompanied by muscular sensations; (2) the external changes, having the opposite characteristics.

We then observe that among the external changes are some which can be corrected, thanks to an internal change which brings everything back to the primitive state; others can not be corrected in this way (it is thus that when an exterior object is displaced, we may then by changing our own position replace ourselves as regards this object in the same relative position as before, so as to reestablish the original aggregate of impressions; if this object was not displaced, but changed its state, that is impossible). Thence comes a new distinction among external changes: those which may be so corrected we call changes of position; and the others, changes of state.

Think, for example, of a sphere with one hemisphere blue and the other red; it first presents to us the blue hemisphere, then it so revolves as to present the red hemisphere. Now think of a spherical vase containing a blue liquid which becomes red in consequence of a chemical reaction. In both cases the sensation of red has replaced that of blue; our senses have experienced the same impressions which have succeeded each other in the same order, and yet these two changes are regarded by us as very different; the first is a displacement, the second a change of state. Why? Because in the first case it is sufficient for me to go around the sphere to place myself opposite the blue hemisphere and reestablish the original blue sensation.

Still more; if the two hemispheres, in place of being red and blue, had been yellow and green, how should I have interpreted the revolution of the sphere? Before, the red succeeded the blue, now the green succeeds the yellow; and yet I say that the two spheres have undergone the same revolution, that each has turned about its axis; yet I can not say that the green is to yellow as the red is to blue; how then am I led to decide that the two spheres have undergone the *same* displacement? Evidently because, in one case as in the other, I am able to reestablish the original sensation by going around the sphere, by making the same movements, and I know that I have made the same movements because I have felt the same muscular sensations; to know it, I do not need, therefore, to know geometry in advance and to represent to myself the movements of my body in geometric space.

Another example: An object is displaced before my eye; its image was first formed at the center of the retina; then it is formed at the border; the old sensation was carried to me by a nerve fiber ending at the center of the retina; the new sensation is carried to me by *another* nerve fiber starting from the border of the retina; these two sensations are qualitatively different; otherwise, how could I distinguish them?

Why then am I led to decide that these two sensations, qualitatively different, represent the same image, which has been displaced? It is because I *can follow the object with the eye* and by a displacement of the eye, voluntary and accompanied by muscular sensations, bring back the image to the center of the retina and reestablish the primitive sensation.

I suppose that the image of a red object has gone from the center A to the border B of the retina, then that the image of a blue object goes in its turn from the center A to the border B of the retina; I shall decide that these two objects have undergone the *same* displacement. Why? Because in both cases I shall have been able to reestablish the primitive sensation, and that to do it I shall have had to execute the *same* movement of the eye, and I shall know that my eye has executed the same movement because I shall have felt the *same* muscular sensations.

If I could not move my eye, should I have any reason to suppose that the sensation of red at the center of the retina is to the sensation of red at the border of the retina as that of blue at the center is to that of blue at the border? I should only have four sensations qualitatively different, and if I were asked if they are connected by the proportion I have just stated, the question would seem to me ridiculous, just as if I were asked if there is an analogous proportion between an auditory sensation, a tactile sensation and an olfactory sensation.

Let us now consider the internal changes, that is, those which are produced by the voluntary movements of our body and which are accompanied by muscular changes. They give rise to the two following observations, analogous to those we have just made on the subject of external changes.

1. I may suppose that my body has moved from one point to another but that the same *attitude* is retained; all the parts of the body have therefore retained or resumed the same *relative* situation, although their absolute situation in space may have varied. I may suppose that not only has the position of my body changed, but that its attitude is no longer the same, that, for instance, my arms which before were folded are now stretched out.

I should therefore distinguish the simple changes of position without change of attitude, and the changes of attitude. Both would appear to me under form of muscular sensations. How then am I led to distinguish them? It is that the first may serve to correct an external change, and that the others can not, or at least can only give an imperfect correction.

This fact I proceed to explain as I would explain it to some one who already knew geometry, but it need not thence be concluded that it is necessary already to know geometry to make this distinction; before knowing geometry I ascertain the fact (experimentally, so to speak), without being able to explain it. But merely to make the distinction between the two kinds of change, I do not need to *explain* the fact, it suffices me *to ascertain* it.

However that may be, the explanation is easy. Suppose that an exterior object is displaced; if we wish the different parts of our body to resume with regard to this object their initial relative position, it is necessary that these different parts should have resumed likewise their initial relative position with regard to one another. Only the internal changes which satisfy this latter condition will be capable of correcting the external change produced by the displacement of that object. If, therefore, the relative position of my eye with regard to my finger has changed, I shall still be able to replace the eye in its initial relative situation with regard to the object and reestablish thus the primitive visual sensations, but then the relative position of the finger with regard to the object will have changed and the tactile sensations will not be reestablished.

2. We ascertain likewise that the same external change may be corrected by two internal changes corresponding to different muscular sensations. Here again I can ascertain this without knowing geometry: and I have no need of anything else; but I proceed to give the explanation of the fact employing geometrical language. To go from the position A to the position B I may take several routes. To the first of these routes will correspond a series S of muscular sensations; to a second route will correspond another series S'' of muscular sensations which generally will be completely different, since other muscles will be used.

How am I led to regard these two series S and S'' as corresponding to the same displacement AB? It is because these two series are capable of correcting the same external change. Apart from that, they have nothing in common.

Let us now consider two external changes: α and β, which shall be, for instance, the rotation of a sphere half blue, half red, and that of a sphere half yellow, half green; these two changes have nothing in common, since the one is for us the passing of blue into red and the other the passing of yellow into green. Consider, on the other hand, two series of internal changes S and S''; like the others, they will have nothing in common. And yet I say that α and β correspond to the same displacement, and that S and S'' correspond also to the same displacement. Why? Simply because S can correct β as well as α and because α can be corrected by S'' as well as by S. And then a question suggests itself: If I have ascertained that S corrects α and β and that S'' corrects α, am I certain that S'' likewise corrects β? Experiment alone can teach us whether this law is verified. If it were not verified, at least approximately, there would be no geometry, there would be no space, because we should have no more interest in classifying the internal and external changes as I have just done, and, for instance, in distinguishing changes of state from changes of position.

It is interesting to see what has been the rôle of experience in all this. It has shown me that a certain law is approximately verified. It has not told me *wherefore* space is, and that it satisfies the condition in question. I knew in fact, before all experience, that space satisfied this condition or that it would not be; nor have I any right to say that experience told me that geometry is possible; I very well see that geometry is possible, since it does not imply contradiction; experience only tells me that geometry is useful.

6. *Visual Space*

Although motor impressions have had, as I have just explained, an altogether preponderant influence in the genesis of the notion of space, which never would have taken birth without them, it will not be without interest to examine also the rôle of visual impressions and to investigate how many dimensions 'visual space' has, and for that purpose to apply to these impressions the definition of § 3.

A first difficulty presents itself: consider a red color sensation affecting a certain point of the retina; and on the other hand a blue color sensation affecting the same point of the retina. It is necessary that we have some means of recognizing that these two sensations, qualitatively different, have something in common. Now, according to the considerations expounded in the preceding paragraph, we have been able to recognize this only by the movements of the eye and the observations to which they have given rise. If the eye were immovable, or if we were unconscious of its movements, we should not have been able to recognize that these two sensations, of different quality, had something in common; we should not have been able to disengage from them what gives them a geometric character. The visual sensations, without the muscular sensations, would have nothing geometric, so that it may be said there is no pure visual space.

To do away with this difficulty, consider only sensations of the same nature, red sensations for instance, differing one from another only as regards the point of the retina that they affect. It is clear that I have no reason for making such an arbitrary choice among all the possible visual sensations, for the purpose of uniting in the same class all the sensations of the same color, whatever may be the point of the retina affected. I should never have dreamt of it, had I not before learned, by the means we have just seen, to distinguish changes of state from changes of position, that is, if my eye were immovable. Two sensations of the same color affecting two different parts of the retina would have appeared to me as qualitatively distinct, just as two sensations of different color.

In restricting myself to red sensations, I therefore impose upon myself an artificial limitation and I neglect systematically one whole side of the question; but it is only by this artifice that I am able to analyze visual space without mingling any motor sensation.

Imagine a line traced on the retina and dividing in two its surface; and set apart the red sensations affecting a point of this line, or those differing from them too little to be distinguished from them. The aggregate of these sensations will form a sort of cut that I shall call C, and it is clear that this cut suffices to divide the manifold of possible red sensations, and that if I take two red sensations affecting two points situated on one side and the other of the line, I can not pass from one of these sensations to the other in a continuous way without passing at a certain moment through a sensation belonging to the cut.

If, therefore, the cut has n dimensions, the total manifold of my red sensations, or, if you wish, the whole visual space, will have $n+1$.

Now, I distinguish the red sensations affecting a point of the cut C. The assemblage of these sensations will form a new cut C'. It is clear that this *will divide* the cut C, always giving to the word divide the same meaning.

If, therefore, the cut C' has n dimensions, the cut C will have $n+1$ and the whole of visual space $n+2$.

If all the red sensations affecting the same point of the retina were regarded as identical, the cut C' reducing to a single element would have 0 dimension, and visual space would have 2.

And yet most often it is said that the eye gives us the sense of a third dimension, and enables us in a certain measure to recognize the distance of objects. When we seek to analyze this feeling, we ascertain that it reduces either to the consciousness of the convergence of the eyes, or to that of the effort of accommodation which the ciliary muscle makes to focus the image.

Two red sensations affecting the same point of the retina will therefore be regarded as identical only if they are accompanied by the same sensation of convergence and also by the same sensation of effort of accommodation or at least by sensations of convergence and accommodation so slightly different as to be indistinguishable.

On this account the cut C' is itself a continuum and the cut C has more than one dimension.

But it happens precisely that experience teaches us that when two visual sensations are accompanied by the same sensation of convergence, they are likewise accompanied by the same sensation of accommodation. If then we form a new cut C'' with all those of the sensations of the cut C', which are accompanied by a certain sensation of convergence, in accordance with the preceding law they will all be indistinguishable and may be regarded as identical. Therefore C'' will not be a continuum and will have 0 dimension; and as C'' divides C' it will thence result that C' has one, C two and *the whole visual space three dimensions.*

But would it be the same if experience had taught us the contrary and if a certain sensation of convergence were not always accompanied by the same sensation of accommodation? In this case two sensations affecting the same point of the retina and accompanied by the same sense of convergence, two sensations which consequently would both appertain to the cut C'' could nevertheless be distinguished since they would be accompanied by two different sensations of accommodation. Therefore C'' would be in its turn a continuum and would have one dimension (at least); then C' would have two, C three and *the whole visual space would have four dimensions.*

Will it then be said that it is experience which teaches us that space has three dimensions, since it is in setting out from an experimental law that we have come to attribute three to it? But we have therein performed, so to speak, only an experiment in physiology; and as also it would suffice to fit over the eyes glasses of suitable construction to put an end to the accord between the feelings of convergence and of accommodation, are we to say that putting on spectacles is enough to make space have four dimensions and that the optician who constructed them has given one more dimension to space? Evidently not; all we can say is that experience has taught us that it is convenient to attribute three dimensions to space.

But visual space is only one part of space, and in even the notion of this space there is something artificial, as I have explained at the beginning. The real space is motor space and this it is that we shall examine in the following chapter.

CHAPTER IV

Space and its Three Dimensions

1. *The Group of Displacements*

Let us sum up briefly the results obtained. We proposed to investigate what was meant in saying that space has three dimensions and we have asked first what is a physical continuum and when it may be said to have n dimensions. If we consider different systems of impressions and compare them with one another, we often recognize that two of these systems of impressions are indistinguishable (which is ordinarily expressed in saying that they are too close to one another, and that our senses are too crude, for us to distinguish them) and we ascertain besides that two of these systems can sometimes be discriminated from one another though indistinguishable from a third system. In that case we say the manifold of these systems of impressions forms a physical continuum C. And each of these systems is called an *element* of the continuum C.

How many dimensions has this continuum? Take first two elements A and B of C, and suppose there exists a series Σ of elements, all belonging to the continuum C, of such a sort that A and B are the two extreme terms of this series and that each term of the series is indistinguishable from the preceding. If such a series Σ can be found, we say that A and B are joined to one another; and if any two elements of C are joined to one another, we say that C is all of one piece.

Now take on the continuum C a certain number of elements in a way altogether arbitrary. The aggregate of these elements will be called a *cut*. Among the various series Σ which join A to B, we shall distinguish those of which an element is indistinguishable from one of the elements of the cut (we shall say that these are they which *cut* the cut) and those of which *all* the elements are distinguishable from all those of the cut. If *all* the series Σ which join A to B cut the cut, we shall say that A and B are *separated* by the cut, and that the cut *divides* C. If we can not find on C two elements which are separated by the cut, we shall say that the cut *does not divide* C.

These definitions laid down, if the continuum C can be divided by cuts which do not themselves form a continuum, this continuum C has only one dimension; in the contrary case it has several. If a cut forming a continuum of 1 dimension suffices to divide C, C will have 2 dimensions; if a cut forming a continuum of 2 dimensions suffices, C will have 3 dimensions, etc. Thanks to these definitions, we can always recognize how many dimensions any physical continuum has. It only remains to find a physical continuum which is, so to speak, equivalent to space, of such a sort that to every point of space corresponds an element of this continuum, and that to points of space very near one another correspond indistinguishable elements. Space will have then as many dimensions as this continuum.

The intermediation of this physical continuum, capable of representation, is indispensable; because we can not represent space to ourselves, and that for a multitude of reasons. Space is a mathematical continuum, it is infinite, and we can represent to ourselves only physical continua and finite objects. The different elements of space, which we call points, are all alike, and, to apply our definition, it is necessary that we know how to distinguish the elements from one another, at least if they are not too close. Finally absolute space is nonsense, and it is necessary for us to begin by referring space to a system of axes invariably bound to our body (which we must always suppose put back in the initial attitude).

Then I have sought to form with our visual sensations a physical continuum equivalent to space; that certainly is easy and this example is particularly appropriate for the discussion of the number of dimensions; this discussion has enabled us to see in what measure it is allowable to say that 'visual space' has three dimensions. Only this solution is incomplete and artificial. I have explained why, and it is not on visual space, but on motor space that it is necessary to bring our efforts to bear. I have then recalled what is the origin of the distinction we make between changes of position and changes of state. Among the changes which occur in our impressions, we distinguish, first the *internal* changes, voluntary and accompanied by muscular sensations, and the *external* changes, having opposite characteristics. We ascertain that it may happen that an external change may be *corrected* by an internal change which reestablishes the primitive sensations. The external changes capable of being corrected by an internal change are called *changes of position,* those not capable of it are called *changes of state.* The internal changes capable of correcting an external change are called *displacements of the whole body;* the others are called *changes of attitude.*

Now let α and β be two external changes, α' and β' two internal changes. Suppose that α may be corrected either by α' or by β', and that α' can correct either α or β; experience tells us then that β' can likewise correct β. In this case we say that α and β correspond to the *same* displacement and also that α' and β' correspond to the *same* displacement. That postulated, we can imagine a physical continuum which we shall call *the continuum or group of displacements* and which we shall define in the following manner. The elements of this continuum shall be the internal changes capable of correcting an external change. Two of these internal changes α' and β' shall be regarded as indistinguishable: (1) if they are so naturally, that is, if they are too close to one another; (2) if α' is capable of correcting the same external change as a third internal change naturally indistinguishable from β'. In this second case, they will be, so to speak, indistinguishable by convention, I mean by agreeing to disregard circumstances which might distinguish them.

Our continuum is now entirely defined, since we know its elements and have fixed under what conditions they may be regarded as indistinguishable. We thus have all that is necessary to apply our definition and determine how many dimensions this continuum has. We shall recognize that it has *six*. The continuum of displacements is, therefore, not equivalent to space, since the number of dimensions is not the same; it is only related to space. Now how do we know that this continuum of displacements has six dimensions? We know it *by experience.*

It would be easy to describe the experiments by which we could arrive at this result. It would be seen that in this continuum cuts can be made which divide it and which are continua; that these cuts themselves can be divided by other cuts of the second order which yet are continua, and that this would stop only after cuts of the sixth order which would no longer be continua. From our definitions that would mean that the group of displacements has six dimensions.

That would be easy, I have said, but that would be rather long; and would it not be a little superficial? This group of displacements, we have seen, is related to space, and space could be deduced from it, but it is not equivalent to space, since it has not the same number of dimensions; and when we shall have shown how the notion of this continuum can be formed and how that of space may be deduced from it, it might always be asked why space of three dimensions is much more familiar to us than this continuum of six dimensions, and consequently doubted whether it was by this detour that the notion of space was formed in the human mind.

2. *Identity of Two Points*

What is a point? How do we know whether two points of space are identical or different? Or, in other words, when I say: The object A occupied at the instant α the point which the object B occupies at the instant β, what does that mean?

Such is the problem we set ourselves in the preceding chapter, § 4. As I have explained it, it is not a question of comparing the positions of the objects A and B in absolute space; the question then would manifestly have no meaning. It is a question of comparing the positions of these two objects with regard to axes invariably bound to my body, supposing always this body replaced in the same attitude.

I suppose that between the instants α and β I have moved neither my body nor my eye, as I know from my muscular sense. Nor have I moved either my head, my arm or my hand. I ascertain that at the instant α impressions that I attributed to the object A were transmitted to me, some by one of the fibers of my optic nerve, the others by one of the sensitive tactile nerves of my finger; I ascertain that at the instant β other impressions which I attribute to the object B are transmitted to me, some by this same fiber of the optic nerve, the others by this same tactile nerve.

Here I must pause for an explanation; how am I told that this impression which I attribute to A, and that which I attribute to B, impressions which are qualitatively different, are transmitted to me by the same nerve? Must we suppose, to take for example the visual sensations, that A produces two simultaneous sensations, a sensation purely luminous a and a colored sensation a', that B produces in the same way simultaneously a luminous sensation b and a colored sensation b', that if these different sensations are transmitted to me by the same retinal fiber, a is identical with b, but that in general the colored sensations a' and b' produced by different bodies are different? In that case it would be the identity of the sensation a which accompanies a' with the sensation b which accompanies b', which would tell that all these sensations are transmitted to me by the same fiber.

However it may be with this hypothesis and although I am led to prefer to it others considerably more complicated, it is certain that we are told in some way that there is something in common between these sensations $a + a'$ and $b + b'$, without which we should have no means of recognizing that the object B has taken the place of the object A.

Therefore I do not further insist and I recall the hypothesis I have just made: I suppose that I have ascertained that the impressions which I attribute to B are transmitted to me at the instant β by the same fibers, optic as well as tactile, which, at the instant α, had transmitted to me the impressions that I attributed to A. If it is so, we shall not hesitate to declare that the point occupied by B at the instant β is identical with the point occupied by A at the instant α.

I have just enunciated two conditions for these points being identical; one is relative to sight, the other to touch. Let us consider them separately. The first is necessary, but is not sufficient. The second is at once necessary and sufficient. A person knowing geometry could easily explain this in the following manner: Let O be the point of the retina where is formed at the instant α the image of the body A; let M be the point of space occupied at the instant α by this body A; let M' be the point of space occupied at the instant β by the body B. For this body B to form its image in O, it is not necessary that the points M and M' coincide; since vision acts at a distance, it suffices for the three points O M M' to be in a straight line. This condition that the two objects form their image on O is therefore necessary, but not sufficient for the points M and M' to coincide. Let now P be the point occupied by my finger and where it remains, since it does not budge. As touch does not act at a distance, if the body A touches my finger at the instant α, it is because M and P coincide; if B touches my finger at the instant β, it is because M' and P coincide. Therefore M and M' coincide. Thus this condition that if A touches my finger at the instant α, B touches it at the instant β, is at once necessary and sufficient for M and M' to coincide.

But we who, as yet, do not know geometry can not reason thus; all that we can do is to ascertain experimentally that the first condition relative to sight may be fulfilled without the second, which is relative to touch, but that the second can not be fulfilled without the first.

Suppose experience had taught us the contrary, as might well be; this hypothesis contains nothing absurd. Suppose, therefore, that we had ascertained experimentally that the condition relative to touch may be fulfilled without that of sight being fulfilled, and that, on the contrary, that of sight can not be fulfilled without that of touch being also. It is clear that if this were so we should conclude that it is touch which may be exercised at a distance, and that sight does not operate at a distance.

But this is not all; up to this time I have supposed that to determine the place of an object, I have made use only of my eye and a single finger; but I could just as well have employed other means, for example, all my other fingers.

I suppose that my first finger receives at the instant α a tactile impression which I attribute to the object A. I make a series of movements, corresponding to a series S of muscular sensations. After these movements, at the instant α, my *second* finger receives a tactile impression that I attribute likewise to A. Afterwards, at the instant β, without my having budged, as my muscular sense tells me, this same second finger transmits to me anew a tactile impression which I attribute this time to the object B; I then make a series of movements, corresponding to a series S' of muscular sensations. I know that this series S' is the inverse of the series S and corresponds to contrary movements. I know this because many previous experiences have shown me that if I made successively the two series of movements corresponding to S and to S', the primitive impressions would be reestablished, in other words, that the two series mutually compensate. That settled, should I expect that at the instant β', when the second series of movements is ended, my *first finger* would feel a tactile impression attributable to the object B?

To answer this question, those already knowing geometry would reason as follows: There are chances that the object A has not budged, between the instants α and α', nor the object B between the instants β and β'; assume this. At the instant α, the object A occupied a certain point M of space. Now at this instant it touched my first finger, and *as touch does not operate at a distance,* my first finger was likewise at the point M. I afterward made the series S of movements and at the end of this series, at the instant α', I ascertained that the object A touched my second finger. I thence conclude that this second finger was then at M, that is, that the movements S had the result of bringing the second finger to the place of the first. At the instant β the object B has come in contact with my second finger: as I have not budged, this second finger has remained at M; therefore the object B has come to M; by hypothesis it does not budge up to the instant β'. But between the instants β and β' I have made the movements S'; as these movements are the inverse of the movements S, they must have for effect bringing the first finger in the place of the second. At the instant β' this first finger will, therefore, be at M; and as the object B, is likewise at M, this object B will touch my first finger. To the question put, the answer should, therefore, be yes.

We who do not yet know geometry can not reason thus; but we ascertain that this anticipation is ordinarily realized; and we can always explain the exceptions by saying that the object A has moved between the instants α and α', or the object B between the instants β and β'.

But could not experience have given a contrary result? Would this contrary result have been absurd in itself? Evidently not. What should we have done then if experience had given this contrary result? Would all geometry thus have become impossible? Not the least in the world. We should have contented ourselves with concluding that *touch can operate at a distance.*

When I say, touch does not operate at a distance, but sight operates at a distance, this assertion has only one meaning, which is as follows: To recognize whether B occupies at the instant β the point occupied by A at the instant α, I can use a multitude of different criteria. In one my eye intervenes, in another my first finger, in another my second finger, etc. Well, it is sufficient for the criterion relative to one of my fingers to be satisfied in order that all the others should be satisfied, but it is not sufficient that the criterion relative to the eye should be. This is the sense of my assertion, I content myself with affirming an experimental fact which is ordinarily verified.

At the end of the preceding chapter we analyzed visual space; we saw that to engender this space it is necessary to bring in the retinal sensations, the sensation of convergence and the sensation of accommodation; that if these last two were not always in accord, visual space would have four dimensions in place of three; we also saw that if we brought in only the retinal sensations, we should obtain 'simple visual space,' of only two dimensions. On the other hand, consider tactile space, limiting ourselves to the sensations of a single finger, that is in sum the assemblage of positions this finger can occupy. This tactile space that we shall analyze in the following section and which consequently I ask permission not to consider further for the moment, this tactile space, I say, has three dimensions. Why has space properly so called as many dimensions as tactile space and more than simple visual space? It is because touch does not operate at a distance, while vision does operate at a distance. These two assertions have the same meaning and we have just seen what this is.

Now I return to a point over which I passed rapidly in order not to interrupt the discussion. How do we know that the impressions made on our retina by A at the instant α and B at the instant β are transmitted by the same retinal fiber, although these impressions are qualitatively different? I have suggested a simple hypothesis, while adding that other hypotheses, decidedly more complex, would seem to me more probably true. Here then are these hypotheses, of which I have already said a word. How do we know that the impressions produced by the red object A at the instant α, and by the blue object B at the instant β, if these two objects have been imaged on the same point of the retina, have something in common? The simple hypothesis above made may be rejected and we may suppose that these two impressions, qualitatively different, are transmitted by two different though contiguous nervous fibers. What means have I then of knowing that these fibers are contiguous? It is probable that we should have none, if the eye were immovable. It is the movements of the eye which have told us that there is the same relation between the sensation of blue at the point A and the sensation of blue at the point B of the retina as between the sensation of red at the point A and the sensation of red at the point B. They have shown us, in fact, that the same movements, corresponding to the same muscular sensations, carry us from the first to the second, or from the third to the fourth. I do not emphasize these considerations, which belong, as one sees, to the question of local signs raised by Lotze.

3. *Tactile Space*

Thus I know how to recognize the identity of two points, the point occupied by A at the instant α and the point occupied by B at the instant β, but only ***on one condition,*** namely, that I have not budged between the instants α and β. That does not suffice for our object. Suppose, therefore, that I have moved in any manner in the interval between these two instants, how shall I know whether the point occupied by A at the instant α is identical with the point occupied by B at the instant β? I suppose that at the instant α, the object A was in contact with my first finger and that in the same way, at the instant β, the object B touches this first finger; but at the same time, my muscular sense has told me that in the interval my body has moved. I have considered above two series of muscular sensations S and S', and I have said it sometimes happens that we are led to consider two such series S and S' as inverse one of the other, because we have often observed that when these two series succeed one another our primitive impressions are reestablished.

If then my muscular sense tells me that I have moved between the two instants α and β, but so as to feel successively the two series of muscular sensations S and S' that I consider inverses, I shall still conclude, just as if I had not budged, that the points occupied by A at the instant α and by B at the instant β are identical, if I ascertain that my first finger touches A at the instant α and B at the instant β.

This solution is not yet completely satisfactory, as one will see. Let us see, in fact, how many dimensions it would make us attribute to space. I wish to compare the two points occupied by A and B at the instants α and β, or (what amounts to the same thing since I suppose that my finger touches A at the instant α and B at the instant β) I wish to compare the two points occupied by my finger at the two instants α and β. The sole means I use for this comparison is the series Σ of muscular sensations which have accompanied the movements of my body between these two instants. The different imaginable series Σ form evidently a physical continuum of which the number of dimensions is very great. Let us agree, as I have done, not to consider as distinct the two series Σ and $\Sigma + S + S'$, when S and S' are inverses one of the other in the sense above given to this word; in spite of this agreement, the aggregate of distinct series Σ will still form a physical continuum and the number of dimensions will be less but still very great.

To each of these series Σ corresponds a point of space; to two series Σ and Σ' thus correspond two points M and M'. The means we have hitherto used enable us to recognize that M and M' are not distinct in two cases: (1) if Σ is identical with Σ'; (2) if $\Sigma' = \Sigma + S + S'$, S and S' being inverses one of the other. If in all the other cases we should regard M and M' as distinct, the manifold of points would have as many dimensions as the aggregate of distinct series Σ, that is, much more than three.

For those who already know geometry, the following explanation would be easily comprehensible. Among the imaginable series of muscular sensations, there are those which correspond to series of movements where the finger does not budge. I say that if one does not consider as distinct the series Σ and $\Sigma + \sigma$, where the series σ corresponds to movements where the finger does not budge, the aggregate of series will constitute a continuum of three dimensions, but that if one regards as distinct two series Σ and Σ' unless $\Sigma' = \Sigma + S + S'$, S

and S' being inverses, the aggregate of series will constitute a continuum of more than three dimensions.

In fact, let there be in space a surface A, on this surface a line B, on this line a point M. Let C_0 be the aggregate of all series Σ. Let C_1 be the aggregate of all the series Σ, such that at the end of corresponding movements the finger is found upon the surface A, and C_2 or C_3 the aggregate of series Σ such that at the end the finger is found on B, or at M. It is clear, first that C_1 will constitute a cut which will divide C_0, that C_2 will be a cut which will divide C_1, and C_3 a cut which will divide C_2. Thence it results, in accordance with our definitions, that if C_3 is a continuum of n dimensions, C_0 will be a physical continuum of $n + 3$ dimensions.

Therefore, let Σ and $\Sigma' = \Sigma + \sigma$ be two series forming part of C_3; for both, at the end of the movements, the finger is found at M; thence results that at the beginning and at the end of the series σ, the finger is at the same point M. This series σ is therefore one of those which correspond to movements where the finger does not budge. If Σ and $\Sigma + \sigma$ are not regarded as distinct, all the series of C_3 blend into one; therefore C_3 will have 0 dimension, and C_0 will have 3, as I wished to prove. If, on the contrary, I do not regard Σ and $\Sigma + \sigma$ as blending (unless $\sigma = S + S'$, S and S' being inverses), it is clear that C_3 will contain a great number of series of distinct sensations; because, without the finger budging, the body may take a multitude of different attitudes. Then C_3 will form a continuum and C_0 will have more than three dimensions, and this also I wished to prove.

We who do not yet know geometry can not reason in this way; we can only verify. But then a question arises; how, before knowing geometry, have we been led to distinguish from the others these series σ where the finger does not budge? It is, in fact, only after having made this distinction that we could be led to regard Σ and $\Sigma + \sigma$ as identical, and it is on this condition alone, as we have just seen, that we can arrive at space of three dimensions.

We are led to distinguish the series σ, because it often happens that when we have executed the movements which correspond to these series σ of muscular sensations, the tactile sensations which are transmitted to us by the nerve of the finger that we have called the first finger, persist and are not altered by these movements. Experience alone tells us that and it alone could tell us.

If we have distinguished the series of muscular sensations $S + S'$ formed by the union of two inverse series, it is because they preserve the totality of our impressions; if now we distinguish the series σ, it is because they preserve *certain* of our impressions. (When I say that a series of muscular sensations S 'preserves' one of our impressions A, I mean that we ascertain that if we feel the impression A, then the muscular sensations S, we *still* feel the impression A *after* these sensations S.)

I have said above it often happens that the series σ do not alter the tactile impressions felt by our first finger; I said *often*, I did not say *always*. This it is that we express in our ordinary language by saying that the tactile impressions would not be altered if the finger has not moved, *on the condition* that *neither has* the object A, which was in contact with this finger, moved. Before knowing geometry, we could not give this explanation; all we could do is to ascertain that the impression often persists, but not always.

But that the impression often continues is enough to make the series σ appear remarkable to us, to lead us to put in the same class the series Σ and $\Sigma + \sigma$, and hence not regard them as distinct. Under these conditions we have seen that they will engender a physical continuum of three dimensions.

Behold then a space of three dimensions engendered by my first finger. Each of my fingers will create one like it. It remains to consider how we are led to regard them as identical with visual space, as identical with geometric space.

But one reflection before going further; according to the foregoing, we know the points of space, or more generally the final situation of our body, only by the series of muscular sensations revealing to us the movements which have carried us from a certain initial situation to this final situation. But it is clear that this final situation will depend, on the one hand, upon these movements and, *on the other hand, upon the initial situation* from which we set out. Now these movements are revealed to us by our muscular sensations; but nothing tells us the initial situation; nothing can distinguish it for us from all the other possible situations. This puts well in evidence the essential relativity of space.

4. *Identity of the Different Spaces*

We are therefore led to compare the two continua C and C' engendered, for instance, one by my first finger D, the other by my second finger D'. These two physical continua both have three dimensions. To each element of the continuum C, or, if you prefer, to each point of the first tactile space, corresponds a series of muscular sensations Σ, which carry me from a certain initial situation to a certain final situation.[1] Moreover, the same point of this first space will correspond to Σ and to $\Sigma + \sigma$, if σ is a series of which we know that it does not make the finger D move.

Similarly to each element of the continuum C', or to each point of the second tactile space, corresponds a series of sensations Σ', and the same point will correspond to Σ' and to $\Sigma' + \sigma'$, if σ' is a series which does not make the finger D' move.

What makes us distinguish the various series designated σ from those called σ' is that the first do not alter the tactile impressions felt by the finger D and the second preserve those the finger D' feels.

Now see what we ascertain: in the beginning my finger D' feels a sensation A'; I make movements which produce muscular sensations S; my finger D feels the impression A; I make movements which produce a series of sensations σ; my finger D continues to feel the impression A, since this is the characteristic property of the series σ; I then make movements which produce the series S' of muscular sensations, *inverse* to S in the sense above given to this word. I ascertain then that my finger D' feels anew the impression A'. (It is of course understood that S has been suitably chosen.)

This means that the series $S + \sigma + S'$, preserving the tactile impressions of the finger D', is one of the series I have called σ'. Inversely, if one takes any series, σ', $S' + \sigma' + S$ will be one of the series that we call σ.

Thus if S is suitably chosen, $S + \sigma + S'$ will be a series σ', and by making σ vary in all possible ways, we shall obtain all the possible series σ'.

Not yet knowing geometry, we limit ourselves to verifying all that, but here is how those who know geometry would explain the fact. In the beginning my finger D' is at the point M, in contact with the object a, which makes it feel the impression A'. I make the movements corresponding to the series S; I have said that this series should be suitably chosen, I should so make this choice that these movements carry the finger D to the point originally occupied by the finger D', that is, to the point M; this finger D will thus be in contact with the object a, which will make it feel the impression A.

I then make the movements corresponding to the series σ; in these

[1] In place of saying that we refer space to axes rigidly bound to our body, perhaps it would be better to say, in conformity to what precedes, that we refer it to axes rigidly bound to the initial situation of our body.

movements, by hypothesis, the position of the finger D does not change, this finger therefore remains in contact with the object a and continues to feel the impression A. Finally I make the movements corresponding to the series S'. As S' is inverse to S, these movements carry the finger D' to the point previously occupied by the finger D, that is, to the point M. If, as may be supposed, the object a has not budged, this finger D' will be in contact with this object and will feel anew the impression A'. . . . *Q. E. D.*

Let us see the consequences. I consider a series of muscular sensations Σ. To this series will correspond a point M of the first tactile space. Now take again the two series s and s', inverses of one another, of which we have just spoken. To the series $S + \Sigma + S'$ will correspond a point N of the second tactile space, since to any series of muscular sensations corresponds, as we have said, a point, whether in the first space or in the second.

I am going to consider the two points N and M, thus defined, as corresponding. What authorizes me so to do? For this correspondence to be admissible, it is necessary that if two points M and M', corresponding in the first space to two series Σ and Σ', are identical, so also are the two corresponding points of the second space N and N', that is the two points which correspond to the two series $S + \Sigma + S'$ and $S + \Sigma' + S'$. Now we shall see that this condition is fulfilled.

First a remark. As S and S' are inverses of one another, we shall have $S + S' = 0$, and consequently $S + S' + \Sigma = \Sigma + S + S' = \Sigma$, or again $\Sigma + S + S' + \Sigma' = \Sigma + \Sigma'$; but it does not follow that we have $S + \Sigma + S' = \Sigma$; because, though we have used the addition sign to represent the succession of our sensations, it is clear that the order of this succession is not indifferent: we can not, therefore, as in ordinary addition, invert the order of the terms; to use abridged language, our operations are associative, but not commutative.

That fixed, in order that Σ and Σ' should correspond to the same point $M = M'$ of the first space, it is necessary and sufficient for us to have $\Sigma' = \Sigma + \sigma$. We shall then have: $S + \Sigma' + S' = S + \Sigma + \sigma + S' = S + \Sigma + S' + S + \sigma + S'$.

But we have just ascertained that $S + \sigma + S'$ was one of the series σ'. We shall therefore have: $S + \Sigma' + S' = S + \Sigma + S' + \sigma'$, which means that the series $S + \Sigma' + S'$ and $S + \Sigma + S'$ correspond to the same point $N = N'$ of the second space. *Q. E. D.*

Our two spaces therefore correspond point for point; they can be 'transformed' one into the other; they are isomorphic. How are we led to conclude thence that they are identical?

Consider the two series σ and $S + \sigma + S' = \sigma'$. I have said that often, but not always, the series σ preserves the tactile impression A felt by the finger D; and similarly it often happens, but not always, that the series σ' preserves the tactile impression A' felt by the finger D'. Now I ascertain that it happens *very often* (that is, much more often than what I have just called 'often') that when the series σ has preserved the impression A of the finger D, the series σ' preserves at the same time the impression A' of the finger D'; and, inversely, that if the first impression is altered, the second is likewise. That happens *very often,* but not always.

We interpret this experimental fact by saying that the unknown object a which gives the impression A to the finger D is identical with the unknown object a' which gives the impression A' to the finger D'. And in fact when the first object moves, which the disappearance of the impression A tells us, the second likewise moves, since the impression A' disappears likewise. When the first object remains motionless, the second remains motionless. If these two objects are identical, as the first is at the point M of the first space and the second at the point N of the second space, these two points are identical. This is how we are led to regard these two spaces as identical; or better this is what we mean when we say that they are identical.

What we have just said of the identity of the two tactile spaces makes unnecessary our discussing the question of the identity of tactile space and visual space, which could be treated in the same way.

5. *Space and Empiricism*

It seems that I am about to be led to conclusions in conformity with empiristic ideas. I have, in fact, sought to put in evidence the rôle of experience and to analyze the experimental facts which intervene in the genesis of space of three dimensions. But whatever may be the importance of these facts, there is one thing we must not forget and to which besides I have more than once called attention. These experimental facts are often verified but not always. That evidently does not mean that space has often three dimensions, but not always.

I know well that it is easy to save oneself and that, if the facts do not verify, it will be easily explained by saying that the exterior objects have moved. If experience succeeds, we say that it teaches us about space; if it does not succeed, we hie to exterior objects which we accuse of having moved; in other words, if it does not succeed, it is given a fillip.

These fillips are legitimate; I do not refuse to admit them; but they suffice to tell us that the properties of space are not experimental truths, properly so called. If we had wished to verify other laws, we could have succeeded also, by giving other analogous fillips. Should we not always have been able to justify these fillips by the same reasons? One could at most have said to us: 'Your fillips are doubtless legitimate, but you abuse them; why move the exterior objects so often?'

To sum up, experience does not prove to us that space has three dimensions; it only proves to us that it is convenient to attribute three to it, because thus the number of fillips is reduced to a minimum.

I will add that experience brings us into contact only with representative space, which is a physical continuum, never with geometric space, which is a mathematical continuum. At the very most it would appear to tell us that it is convenient to give to geometric space three dimensions, so that it may have as many as representative space.

The empiric question may be put under another form. Is it impossible to conceive physical phenomena, the mechanical phenomena for example, otherwise than in space of three dimensions? We should thus have an objective experimental proof, so to speak, independent of our physiology, of our modes of representation.

But it is not so; I shall not here discuss the question completely, I shall confine myself to recalling the striking example given us by the mechanics of Hertz. You know that the great physicist did not believe in the ~~existence of forces~~, properly so called; he supposed that visible material points are subjected to certain invisible bonds which join them to other invisible points and that it is the effect of these invisible bonds that we attribute to forces.

But that is only a part of his ideas. Suppose a system formed of n material points, visible or not; that will give in all $3n$ coordinates; let us regard them as the coordinates of a *single* point in space of $3n$ dimensions. This single point would be constrained to remain upon a surface (of any number of dimensions $< 3n$) in virtue of the bonds of which we have just spoken; to go on this surface from one point to another, it would always take the shortest way; this would be the single principle which would sum up all mechanics.

Whatever should be thought of this hypothesis, whether we be allured by its simplicity, or repelled by its artificial character, the simple fact that Hertz was able to conceive it, and to regard it as more convenient than our habitual hypotheses, suffices to prove that our ordinary ideas, and, in particular, the three dimensions of space, are in no wise imposed upon mechanics with an invincible force.

6. *Mind and Space*

Experience, therefore, has played only a single rôle, it has served as occasion. But this rôle was none the less very important; and I have thought it necessary to give it prominence. This rôle would have been useless if there existed an *a priori* form imposing itself upon our sensitivity, and which was space of three dimensions.

Does this form exist, or, if you choose, can we represent to ourselves space of more than three dimensions? And first what does this question mean? In the true sense of the word, it is clear that we can not represent to ourselves space of four, nor space of three, dimensions; we can not first represent them to ourselves empty, and no more can we represent to ourselves an object either in space of four, or in space of three, dimensions: (1) Because these spaces are both infinite and we can not represent to ourselves a figure *in* space, that is, the part *in* the whole, without representing the whole, and that is impossible, because it is infinite; (2) because these spaces are both mathematical continua and we can represent to ourselves only the physical continuum; (3) because these spaces are both homogeneous, and the frames in which we enclose our sensations, being limited, can not be homogeneous.

Thus the question put can only be understood in another manner; is it possible to imagine that, the results of the experiences related above having been different, we might have been led to attribute to space more than three dimensions; to imagine, for instance, that the sensation of accommodation might not be constantly in accord with the sensation of convergence of the eyes; or indeed that the experiences of which we have spoken in paragraph 2 and of which we express the result by saying 'that touch does not operate at a distance,' might have led us to an inverse conclusion.

And *then evidently yes* that is possible. From the moment one imagines an experience, one imagines just by that the two contrary results it may give. That is possible, but that is difficult, because we have to overcome a multitude of associations of ideas, which are the fruit of a long personal experience and of the still longer experience of the race. Is it these associations (or at least those of them that we have inherited from our ancestors), which constitute this *a priori* form of which it is said that we have pure intuition? Then I do not see why one should declare it refractory to analysis and should deny me the right of investigating its origin.

When it is said that our sensations are 'extended' only one thing can be meant, that is that they are always associated with the idea of certain muscular sensations, corresponding to the movements which enable us to reach the object which causes them, which enable us, in other words, to defend ourselves against it. And it is just because this association is useful for the defense of the organism, that it is so old in the history of the species and that it seems to us indestructible. Nevertheless, it is only an association and we can conceive that it may be broken; so that we may not say that sensation can not enter consciousness without entering in space, but that in fact it does not enter consciousness without entering in space, which means, without being entangled in this association.

No more can I understand one's saying that the idea of time is logically subsequent to space, since we can represent it to ourselves only under the form of a straight line; as well say that time is logically subsequent to the cultivation of the prairies, since it is usually represented armed with a scythe. That one can not represent to himself simultaneously the different parts of time, goes without saying, since the essential character of these parts is precisely not to be simultaneous. That does not mean that we have not the intuition of time. So far as that goes, no more should we have that of space, because neither can we represent it, in the proper sense of the word, for the reasons I have mentioned. What we represent to ourselves under the name of straight is a crude image which as ill resembles the geometric straight as it does time itself.

Why has it been said that every attempt to give a fourth dimension to space always carries this one back to one of the other three? It is easy to understand. Consider our muscular sensations and the 'series' they may form. In consequence of numerous experiences, the ideas of these series are associated together in a very complex woof, our series are *classed*. Allow me, for convenience of language, to express my thought in a way altogether crude and even inexact by saying that our series of muscular sensations are classed in three classes corresponding to the three dimensions of space. Of course this classification is much more complicated than that, but that will suffice to make my reasoning understood. If I wish to imagine a fourth dimension, I shall suppose another series of muscular sensations, making part of a fourth class. But as *all* my muscular sensations have already been classed in one of the three preexistent classes, I can only represent to myself a series belonging to one of these three classes, so that my fourth dimension is carried back to one of the other three.

What does that prove? This: that it would have been necessary first to destroy the old classification and replace it by a new one in which the series of muscular sensations should have been distributed into four classes. The difficulty would have disappeared.

It is presented sometimes under a more striking form. Suppose I am enclosed in a chamber between the six impassable boundaries formed by the four walls, the floor and the ceiling; it will be impossible for me to get out and to imagine my getting out. Pardon, can you not imagine that the door opens, or that two of these walls separate? But of course, you answer, one must suppose that these walls remain immovable. Yes, but it is evident that I have the right to move; and then the walls that we suppose absolutely at rest will be in motion with regard to me. Yes, but such a relative motion can not be anything; when objects are at rest, their relative motion with regard to any axes is that of a rigid solid; now, the apparent motions that you imagine are not in conformity with the laws of motion of a rigid solid. Yes, but it is experience which has taught us the laws of motion of a rigid solid; nothing would prevent our *imagining* them different. To sum up, for me to imagine that I get out of my prison, I have only to imagine that the walls seem to open, when I move.

I believe, therefore, that if by space is understood a mathematical continuum of three dimensions, were it otherwise amorphous, it is the mind which constructs it, but it does not construct it out of nothing; it needs materials and models. These materials, like these models, preexist within it. But there is not a single model which is imposed upon it; it has *choice;* it may choose, for instance, between space of four and space of three dimensions. What then is the rôle of experience? It gives the indications following which the choice is made.

Another thing: whence does space get its quantitative character? It comes from the rôle which the series of muscular sensations play in its genesis. These are series which may *repeat themselves,* and it is from their repetition that number comes; it is because they can repeat themselves indefinitely that space is infinite. And finally we have seen, at the end of section 3, that it is also because of this that space is relative. So it is repetition which has given to space its essential characteristics; now, repetition supposes time; this is enough to tell that time is logically anterior to space.

7. *Rôle of the Semicircular Canals*

I have not hitherto spoken of the rôle of certain organs to which the physiologists attribute with reason a capital importance, I mean the semicircular canals. Numerous experiments have sufficiently shown that these canals are necessary to our sense of orientation; but the physiologists are not entirely in accord; two opposing theories have been proposed, that of Mach-Delage and that of M. de Cyon.

M. de Cyon is a physiologist who has made his name illustrious by important discoveries on the innervation of the heart; I can not, however agree with his ideas on the question before us. Not being a physiologist, I hesitate to criticize the experiments he has directed against the adverse theory of Mach-Delage; it seems to me, however, that they are not convincing, because in many of them the *total* pressure was made to vary in one of the canals, while, physiologically, what varies is the *difference* between the pressures on the two extremities of the canal; in others the organs were subjected to profound lesions, which must alter their functions.

Besides, this is not important; the experiments, if they were irreproachable, might be convincing against the old theory. They would not be convincing *for* the new theory. In fact, if I have rightly understood the theory, my explaining it will be enough for one to understand that it is impossible to conceive of an experiment confirming it.

The three pairs of canals would have as sole function to tell us that space has three dimensions. Japanese mice have only two pairs of canals; they believe, it would seem, that space has only two dimensions, and they manifest this opinion in the strangest way; they put themselves in a circle, and, so ordered, they spin rapidly around. The lampreys, having only one pair of canals, believe that space has only one dimension, but their manifestations are less turbulent.

It is evident that such a theory is inadmissible. The sense-organs are designed to tell us of *changes* which happen in the exterior world. We could not understand why the Creator should have given us organs destined to cry without cease: Remember that space has three dimensions, since the number of these three dimensions is not subject to change.

We must, therefore, come back to the theory of Mach-Delage. What the nerves of the canals can tell us is the difference of pressure on the two extremities of the same canal, and thereby: (1) the direction of the vertical with regard to three axes rigidly bound to the head; (2)

the three components of the acceleration of translation of the center of gravity of the head; (3) the centrifugal forces developed by the rotation of the head; (4) the acceleration of the motion of rotation of the head.

It follows from the experiments of M. Delage that it is this last indication which is much the most important; doubtless because the nerves are less sensible to the difference of pressure itself than to the brusque variations of this difference. The first three indications may thus be neglected.

Knowing the acceleration of the motion of rotation of the head at each instant, we deduce from it, by an unconscious integration, the final orientation of the head, referred to a certain initial orientation taken as origin. The circular canals contribute, therefore, to inform us of the movements that we have executed, and that on the same ground as the muscular sensations. When, therefore, above we speak of the series S or of the series Σ, we should say, not that these were series of muscular sensations alone, but that they were series at the same time of muscular sensations due to the semicircular canals. Apart from this addition, we should have nothing to change in what precedes.

In the series S and Σ, these sensations of the semicircular canals evidently hold a very important place. Yet alone they would not suffice, because they can tell us only of the movements of the head; they tell us nothing of the relative movements of the body, or of the members in regard to the head. And more, it seems that they tell us only of the rotations of the head and not of the translations it may undergo.

PART II

THE PHYSICAL SCIENCES

CHAPTER V

Analysis and Physics

I

You have doubtless often been asked of what good are mathematics and whether these delicate constructions entirely mind-made are not artificial and born of our caprice.

Among those who put this question I should make a distinction; practical people ask of us only the means of money-making. These merit no reply; rather would it be proper to ask of them what is the good of accumulating so much wealth and whether, to get time to acquire it, we are to neglect art and science, which alone give us souls capable of enjoying it, 'and for life's sake to sacrifice all reasons for living.'

Besides, a science made solely in view of applications is impossible; truths are fecund only if bound together. If we devote ourselves solely to those truths whence we expect an immediate result, the intermediary links are wanting and there will no longer be a chain.

The men most disdainful of theory get from it, without suspecting it, their daily bread; deprived of this food, progress would quickly cease, and we should soon congeal into the immobility of China.

But enough of uncompromising practicians! Besides these, there are those who are only interested in nature and who ask us if we can enable them to know it better.

To answer these, we have only to show them the two monuments already rough-hewn, Celestial Mechanics and Mathematical Physics.

They would doubtless concede that these structures are well worth the trouble they have cost us. But this is not enough. Mathematics have a triple aim. They must furnish an instrument for the study of nature. But that is not all: they have a philosophic aim and, I dare maintain, an esthetic aim. They must aid the philosopher to fathom the notions of number, of space, of time. And above all, their adepts find therein delights analogous to those given by painting and music. They admire the delicate harmony of numbers and forms; they marvel when a new discovery opens to them an unexpected perspective; and has not the joy they thus feel the esthetic character, even though the senses take no part therein? Only a privileged few are called to enjoy it fully, it is true, but is not this the case for all the noblest arts?

This is why I do not hesitate to say that mathematics deserve to be cultivated for their own sake, and the theories inapplicable to physics as well as the others. Even if the physical aim and the esthetic aim were not united, we ought not to sacrifice either.

But more: these two aims are inseparable and the best means of attaining one is to aim at the other, or at least never to lose sight of it. This is what I am about to try to demonstrate in setting forth the nature of the relations between the pure science and its applications.

The mathematician should not be for the physicist a mere purveyor of formulas; there should be between them a more intimate collaboration. Mathematical physics and pure analysis are not merely adjacent powers, maintaining good neighborly relations; they mutually interpenetrate and their spirit is the same. This will be better understood when I have shown what physics gets from mathematics and what mathematics, in return, borrows from physics.

II

The physicist can not ask of the analyst to reveal to him a new truth; the latter could at most only aid him to foresee it. It is a long time since one still dreamt of forestalling experiment, or of constructing the entire world on certain premature hypotheses. Since all those constructions in which one yet took a naive delight it is an age, to-day only their ruins remain.

All laws are therefore deduced from experiment; but to enunciate them, a special language is needful; ordinary language is too poor, it is besides too vague, to express relations so delicate, so rich, and so precise.

This therefore is one reason why the physicist can not do without mathematics; it furnishes him the only language he can speak. And a well-made language is no indifferent thing; not to go beyond physics, the unknown man who invented the word *heat* devoted many generations to error. Heat has been treated as a substance, simply because it was designated by a substantive, and it has been thought indestructible.

On the other hand, he who invented the word *electricity* had the unmerited good fortune to implicitly endow physics with a *new* law, that of the conservation of electricity, which, by a pure chance, has been found exact, at least until now.

Well, to continue the simile, the writers who embellish a language, who treat it as an object of art, make of it at the same time a more supple instrument, more apt for rendering shades of thought.

We understand, then, how the analyst, who pursues a purely esthetic aim, helps create, just by that, a language more fit to satisfy the physicist.

But this is not all: law springs from experiment, but not immediately. Experiment is individual, the law deduced from it is general; experiment is only approximate, the law is precise, or at least pretends to be. Experiment is made under conditions always complex, the enunciation of the law eliminates these complications. This is what is called 'correcting the systematic errors.'

In a word, to get the law from experiment, it is necessary to generalize; this is a necessity imposed upon the most circumspect observer. But how generalize? Every particular truth may evidently be extended in an infinity of ways. Among these thousand routes opening before us, it is necessary to make a choice, at least provisional; in this choice, what shall guide us?

It can only be analogy. But how vague is this word! Primitive man knew only crude analogies, those which strike the senses, those of colors or of sounds. He never would have dreamt of likening light to radiant heat.

What has taught us to know the true, profound analogies, those the eyes do not see but reason divines?

It is the mathematical spirit, which disdains matter to cling only to pure form. This it is which has taught us to give the same name to things differing only in material, to call by the same name, for instance, the multiplication of quaternions and that of whole numbers.

If quaternions, of which I have just spoken, had not been so promptly utilized by the English physicists, many persons would doubtless see in them only a useless fancy, and yet, in teaching us to liken what appearances separate, they would have already rendered us more apt to penetrate the secrets of nature.

Such are the services the physicist should expect of analysis; but for this science to be able to render them, it must be cultivated in the broadest fashion without immediate expectation of utility—the mathematician must have worked as artist.

What we ask of him is to help us to see, to discern our way in the labyrinth which opens before us. Now, he sees best who stands highest. Examples abound, and I limit myself to the most striking.

The first will show us how to change the language suffices to reveal generalizations not before suspected.

When Newton's law has been substituted for Kepler's, we still know only elliptic motion. Now, in so far as concerns this motion, the two laws differ only in form; we pass from one to the other by a simple differentiation. And yet from Newton's law may be deduced by an immediate generalization all the effects of perturbations and the whole of celestial mechanics. If, on the other hand, Kepler's enunciation had been retained, no one would ever have regarded the orbits of the perturbed plants, those complicated curves of which no one has ever written the equation, as the natural generalizations of the ellipse. The progress of observations would only have served to create belief in chaos.

The second example is equally deserving of consideration.

When Maxwell began his work, the laws of electro-dynamics admitted up to his time accounted for all the known facts. It was not a new experiment which came to invalidate them. But in looking at them under a new bias, Maxwell saw that the equations became more symmetrical when a term was added, and besides, this term was too small to produce effects appreciable with the old methods.

You know that Maxwell's *a priori* views awaited for twenty years an experimental confirmation; or if you prefer, Maxwell was twenty years ahead of experiment. How was this triumph obtained?

It was because Maxwell was profoundly steeped in the sense of mathematical symmetry; would he have been so, if others before him had not studied this symmetry for its own beauty?

It was because Maxwell was accustomed to 'think in vectors,' and yet it was through the theory of imaginaries (neomonics) that vectors were introduced into analysis. And those who invented imaginaries hardly suspected the advantage which would be obtained from them for the study of the real world; of this the name given them is proof sufficient.

In a word, Maxwell was perhaps not an able analyst, but this ability would have been for him only a useless and bothersome baggage. On the other hand, he had in the highest degree the intimate sense of mathematical analogies. Therefore it is that he made good mathematical physics.

Maxwell's example teaches us still another thing.

How should the equations of mathematical physics be treated? Should we simply deduce all the consequences, and regard them as intangible realities? Far from it; what they should teach us above all is what can and what should be changed. It is thus that we get from them something useful.

The third example goes to show us how we may perceive mathematical analogies between phenomena which have physically no relation either apparent or real, so that the laws of one of these phenomena aid us to divine those of the other.

The very same equation, that of Laplace, is met in the theory of Newtonian attraction, in that of the motion of liquids, in that of the electric potential, in that of magnetism, in that of the propagation of heat and in still many others. What is the result? These theories seem images copied one from the other; they are mutually illuminating, borrowing their language from each other; ask electricians if they do not felicitate themselves on having invented the phrase flow of force, suggested by hydrodynamics and the theory of heat.

Thus mathematical analogies not only may make us foresee physical analogies, but besides do not cease to be useful when these latter fail.

To sum up, the aim of mathematical physics is not only to facilitate for the physicist the numerical calculation of certain constants or the integration of certain differential equations. It is besides, it is above all, to reveal to him the hidden harmony of things in making him see them in a new way.

Of all the parts of analysis, the most elevated, the purest, so to speak, will be the most fruitful in the hands of those who know how to use them.

III

Let us now see what analysis owes to physics.

It would be necessary to have completely forgotten the history of science not to remember that the desire to understand nature has had on the development of mathematics the most constant and happiest influence.

In the first place the physicist sets us problems whose solution he expects of us. But in proposing them to us, he has largely paid us in advance for the service we shall render him, if we solve them.

If I may be allowed to continue my comparison with the fine arts, the pure mathematician who should forget the existence of the exterior world would be like a painter who knew how to harmoniously combine colors and forms, but who lacked models. His creative power would soon be exhausted.

The combinations which numbers and symbols may form are an infinite multitude. In this multitude how shall we choose those which are worthy to fix our attention? Shall we let ourselves be guided solely by our caprice? This caprice, which itself would besides soon tire, would doubtless carry us very far apart and we should quickly cease to understand each other.

But this is only the smaller side of the question. Physics will doubtless prevent our straying, but it will also preserve us from a danger much more formidable; it will prevent our ceaselessly going around in the same circle.

History proves that physics has not only forced us to choose among problems which came in a crowd; it has imposed upon us such as we should without it never have dreamed of. However varied may be the imagination of man, nature is still a thousand times richer. To follow her we must take ways we have neglected, and these paths lead us often to summits whence we discover new countries. What could be more useful!

It is with mathematical symbols as with physical realities; it is in comparing the different aspects of things that we are able to comprehend their inner harmony, which alone is beautiful and consequently worthy of our efforts.

The first example I shall cite is so old we are tempted to forget it; it is nevertheless the most important of all.

The sole natural object of mathematical thought is the whole number. It is the external world which has imposed the continuum upon us, which we doubtless have invented, but which it has forced us to invent. Without it there would be no infinitesimal analysis; all mathematical science would reduce itself to arithmetic or to the theory of substitutions.

On the contrary, we have devoted to the study of the continuum almost all our time and all our strength. Who will regret it; who will think that this time and this strength have been wasted? Analysis unfolds before us infinite perspectives that arithmetic never suspects; it shows us at a glance a majestic assemblage whose array is simple and symmetric; on the contrary, in the theory of numbers, where reigns the unforeseen, the view is, so to speak, arrested at every step.

Doubtless it will be said that outside of the whole number there is no rigor, and consequently no mathematical truth; that the whole number hides everywhere, and that we must strive to render transparent the screens which cloak it, even if to do so we must resign ourselves to interminable repetitions. Let us not be such purists and let us be grateful to the continuum, which, if *all* springs from the whole number, was alone capable of making *so much* proceed therefrom.

Need I also recall that M. Hermite obtained a surprising advantage from the introduction of continuous variables into the theory of numbers? Thus the whole number's own domain is itself invaded, and this invasion has established order where disorder reigned.

See what we owe to the continuum and consequently to physical nature.

Fourier's series is a precious instrument of which analysis makes continual use, it is by this means that it has been able to represent discontinuous functions; Fourier invented it to solve a problem of physics relative to the propagation of heat. If this problem had not come up naturally, we should never have dared to give discontinuity its rights; we should still long have regarded continuous functions as the only true functions.

The notion of function has been thereby considerably extended and has received from some logician-analysts an unforeseen development. These analysts have thus adventured into regions where reigns the purest abstraction and have gone as far away as possible from the real world. Yet it is a problem of physics which has furnished them the occasion.

After Fourier's series, other analogous series have entered the domain of analysis; they have entered by the same door; they have been imagined in view of applications.

The theory of partial differential equations of the second order has an analogous history. It has been developed chiefly by and for physics. But it may take many forms, because such an equation does not suffice to determine the unknown function, it is necessary to adjoin to it complementary conditions which are called conditions at the limits; whence many different problems.

If the analysts had abandoned themselves to their natural tendencies, they would never have known but one, that which Madame Kovalevski has treated in her celebrated memoir. But there are a multitude of others which they would have ignored. Each of the theories of physics, that of electricity, that of heat, presents us these equations under a new aspect. It may therefore be said that without these theories we should not know partial differential equations.

It is needless to multiply examples. I have given enough to be able to conclude: when physicists ask of us the solution of a problem, it is not a duty-service they impose upon us, it is on the contrary we who owe them thanks.

IV

But this is not all; physics not only gives us the occasion to solve problems; it aids us to find the means thereto, and that in two ways. It makes us foresee the solution; it suggests arguments to us.

I have spoken above of Laplace's equation which is met in a multitude of diverse physical theories. It is found again in geometry, in the theory of conformal representation and in pure analysis, in that of imaginaries.

In this way, in the study of functions of complex variables, the analyst, alongside of the geometric image, which is his usual instrument, finds many physical images which he may make use of with the same success. Thanks to these images he can see at a glance what pure deduction would show him only successively. He masses thus the separate elements of the solution, and by a sort of intuition divines before being able to demonstrate.

To divine before demonstrating! Need I recall that thus have been made all the important discoveries? How many are the truths that physical analogies permit us to present and that we are not in condition to establish by rigorous reasoning!

For example, mathematical physics introduces a great number of developments in series. No one doubts that these developments converge; but the mathematical certitude is lacking. These are so many conquests assured for the investigators who shall come after us.

On the other hand, physics furnishes us not alone solutions; it furnishes us besides, in a certain measure, arguments. It will suffice to recall how Felix Klein, in a question relative to Riemann surfaces, has had recourse to the properties of electric currents.

It is true, the arguments of this species are not rigorous, in the sense the analyst attaches to this word. And here a question arises: How can a demonstration not sufficiently rigorous for the analyst suffice for the physicist? It seems there can not be two rigors, that rigor is or is not, and that, where it is not there can not be deduction.

This apparent paradox will be better understood by recalling under what conditions number is applied to natural phenomena. Whence come in general the difficulties encountered in seeking rigor? We strike them almost always in seeking to establish that some quantity tends to some limit, or that some function is continuous, or that it has a derivative.

Now the numbers the physicist measures by experiment are never known except approximately; and besides, any function always differs as little as you choose from a discontinuous function, and at the same time it differs as little as you choose from a continuous function. The physicist may, therefore, at will suppose that the function studied is continuous, or that it is discontinuous; that it has or has not a derivative; and may do so without fear of ever being contradicted, either by present experience or by any future experiment. We see that with such liberty he makes sport of difficulties which stop the analyst. He may always reason as if all the functions which occur in his calculations were entire polynomials.

Thus the sketch which suffices for physics is not the deduction which analysis requires. It does not follow thence that one can not aid in finding the other. So many physical sketches have already been transformed into rigorous demonstrations that to-day this transformation is easy. There would be plenty of examples did I not fear in citing them to tire the reader.

I hope I have said enough to show that pure analysis and mathematical physics may serve one another without making any sacrifice one to the other, and that each of these two sciences should rejoice in all which elevates its associate.

CHAPTER VI

Astronomy

Governments and parliments must find that astronomy is one of the sciences which cost most dear: the least instrument costs hundreds of thousands of dollars, the least observatory costs millions; each eclipse carries with it supplementary appropriations. And all that for stars which are so far away, which are complete strangers to our electoral contests, and in all probability will never take any part in them. It must be that our politicians have retained a remnant of idealism, a vague instinct for what is grand; truly, I think they have been calumniated; they should be encouraged and shown that this instinct does not deceive them, that they are not dupes of that idealism.

We might indeed speak to them of navigation, of which no one can underestimate the importance, and which has need of astronomy. But this would be to take the question by its smaller side.

Astronomy is useful because it raises us above ourselves; it is useful because it is grand; that is what we should say. It shows us how small is man's body, how great his mind, since his intelligence can embrace the whole of this dazzling immensity, where his body is only an obscure point, and enjoy its silent harmony. Thus we attain the consciousness of our power, and this is something which can not cost too dear, since this consciousness makes us mightier.

But what I should wish before all to show is, to what point astronomy has facilitated the work of the other sciences, more directly useful, since it has given us a soul capable of comprehending nature.

Think how diminished humanity would be if, under heavens constantly overclouded, as Jupiter's must be, it had forever remained ignorant of the stars. Do you think that in such a world we should be what we are? I know well that under this somber vault we should have been deprived of the light of the sun, necessary to organisms like those which inhabit the earth. But if you please we shall assume that these clouds are phosphorescent and emit a soft and constant light. Since we are making hypotheses, another will cost no more. Well! I repeat my question: Do you think that in such a world we should be what we are?

Damned Right.

The stars send us not only that visible and gross light which strikes our bodily eyes, but from them also comes to us a light far more subtile, which illuminates our minds and whose effects I shall try to show you. You know what man was on the earth some thousands of years ago, and what he is to-day. Isolated amidst a nature where everything was a mystery to him, terrified at each unexpected manifestation of incomprehensible forces, he was incapable of seeing in the conduct of the universe anything but caprice; he attributed all phenomena to the action of a multitude of little genii, fantastic and exacting, and to act on the world he sought to conciliate them by means analogous to those employed to gain the good graces of a minister or a deputy. Even his failures did not enlighten him, any more than to-day a beggar refused is discouraged to the point of ceasing to beg.

To-day we no longer beg of nature; we command her, because we have discovered certain of her secrets and shall discover others each day. We command her in the name of laws she can not challenge because they are hers; these laws we do not madly ask her to change, we are the first to submit to them. Nature can only be governed by obeying her.

What a change must our souls have undergone to pass from the one state to the other! Does any one believe that, without the lessons of the stars, under the heavens perpetually overclouded that I have just supposed, they would have changed so quickly? Would the metamorphosis have been possible, or at least would it not have been much slower?

And first of all, astronomy it is which taught that there are laws. The Chaldeans, who were the first to observe the heavens with some attention, saw that this multitude of luminous points is not a confused crowd wandering at random, but rather a disciplined army. Doubtless the rules of this discipline escaped them, but the harmonious spectacle of the starry night sufficed to give them the impression of regularity, and that was in itself already a great thing. Besides, these rules were discerned by Hipparchus, Ptolemy, Copernicus, Kepler, one after another, and finally, it is needless to recall that Newton it was who enunciated the oldest, the most precise, the most simple, the most general of all natural laws.

And then, taught by this example, we have seen our little terrestrial world better and, under the apparent disorder, there also we have found again the harmony that the study of the heavens had revealed to us. It also is regular, it also obeys immutable laws, but they are more complicated, in apparent conflict one with another, and an eye untrained by other sights would have seen there only chaos and the reign of chance or caprice. If we had not known the stars, some bold spirits might perhaps have sought to foresee physical phenomena; but their failures would have been frequent, and they would have excited only the derision of the vulgar; do we not see, that even in our day the meteorologists sometimes deceive themselves, and that certain persons are inclined to laugh at them.

How often would the physicists, disheartened by so many checks, have fallen into discouragement, if they had not had, to sustain their confidence, the brilliant example of the success of the astronomers! This success showed them that nature obeys laws; it only remained to know what laws; for that they only needed patience, and they had the right to demand that the sceptics should give them credit.

This is not all: astronomy has not only taught us that there are laws, but that from these laws there is no escape, that with them there is no possible compromise. How much time should we have needed to comprehend that fact, if we had known only the terrestrial world, where each elemental force would always seem to us in conflict with other forces? Astronomy has taught us that the laws are infinitely precise, and that if those we enunciate are approximative, it is because we do not know them well. Aristotle, the most scientific mind of antiquity, still accorded a part to accident, to chance, and seemed to think that the laws of nature, at least here below, determine only the large features of phenomena. How much has the ever-increasing precision of astronomical predictions contributed to correct such an error, which would have rendered nature unintelligible!

But are these laws not local, varying in different places, like those which men make; does not that which is truth in one corner of the universe, on our globe for instance, or in our little solar system, become error a little farther away? And then could it not be asked whether laws depending on space do not also depend upon time, whether they are not simple habitudes, transitory, therefore, and ephemeral? Again it is astronomy that answers this question. Consider the double stars; all describe conics; thus, as far as the telescope carries, it does not reach the limits of the domain which obeys Newton's law.

Even the simplicity of this law is a lesson for us; how many complicated phenomena are contained in the two lines of its enunciation;

persons who do not understand celestial mechanics may form some idea of it at least from the size of the treatises devoted to this science; and then it may be hoped that the complication of physical phenomena likewise hides from us some simple cause still unknown.

It is therefore astronomy which has shown us what are the general characteristics of natural laws; but among these characteristics there is one, the most subtile and the most important of all, which I shall ask leave to stress.

How was the order of the universe understood by the ancients; for instance, by Pythagoras, Plato or Aristotle? It was either an immutable type fixed once for all, or an ideal to which the world sought to approach. Kepler himself still thought thus when, for instance, he sought whether the distances of the planets from the sun had not some relation to the five regular polyhedrons. This idea contained nothing absurd, but it was sterile, since nature is not so made. Newton has shown us that a law is only a necessary relation between the present state of the world and its immediately subsequent state. All the other laws since discovered are nothing else; they are in sum, differential equations; but it is astronomy which furnished the first model for them, without which we should doubtless long have erred.

Astronomy has also taught us to set at naught appearances. The day Copernicus proved that what was thought the most stable was in motion, that what was thought moving was fixed, he showed us how deceptive could be the infantile reasonings which spring directly from the immediate data of our senses. True, his ideas did not easily triumph, but since this triumph there is no longer a prejudice so inveterate that we can not shake it off. How can we estimate the value of the new weapon thus won?

The ancients thought everything was made for man, and this illusion must be very tenacious, since it must ever be combated. Yet it is necessary to divest oneself of it; or else one will be only an eternal myope, incapable of seeing the truth. To comprehend nature one must be able to get out of self, so to speak, and to contemplate her from many different points of view; otherwise we never shall know more than one side. Now, to get out of self is what he who refers everything to himself can not do. Who delivered us from this illusion? It was those who showed us that the earth is only one of the smallest planets of the solar system, and that the solar system itself is only an imperceptible point in the infinite spaces of the stellar universe.

At the same time astronomy taught us not to be afraid of big numbers. This was needful, not only for knowing the heavens, but to know the earth itself; and was not so easy as it seems to us to-day. Let us try to go back and picture to ourselves what a Greek would have thought if told that red light vibrates four hundred millions of millions of times per second. Without any doubt, such an assertion would have appeared to him pure madness, and he never would have lowered himself to test it. To-day an hypothesis will no longer appear absurd to us because it obliges us to imagine objects much larger or smaller than those our senses are capable of showing us, and we no longer comprehend those scruples which arrested our predecessors and prevented them from discovering certain truths simply because they were afraid of them. But why? It is because we have seen the heavens enlarging and enlarging without cease; because we know that the sun is 150 millions of kilometers from the earth and that the distances of the nearest stars are hundreds of thousands of times greater yet. Habituated to the contemplation of the infinitely great, we have become apt to comprehend the infinitely small. Thanks to the education it has received, our imagination, like the eagle's eye that the sun does not dazzle, can look truth in the face.

Was I wrong in saying that it is astronomy which has made us a soul capable of comprehending nature; that under heavens always overcast and starless, the earth itself would have been for us eternally unintelligible; that we should there have seen only caprice and disorder; and that, not knowing the world, we should never have been able to subdue it? What science could have been more useful? And in thus speaking I put myself at the point of view of those who only value practical applications. Certainly, this point of view is not mine; as for me, on the contrary, if I admire the conquests of industry, it is above all because if they free us from material cares, they will one day give to all the leisure to contemplate nature. I do not say: Science is useful, because it teaches us to construct machines. I say: Machines are useful, because in working for us, they will some day leave us more ~~time to make~~ science. But finally it is worth remarking that between the two points of view there is no antagonism, and that man having pursued a disinterested aim, all else has been added unto him.

Auguste Comte has said somewhere, that it would be idle to seek to know the composition of the sun, since this knowledge would be of no use to sociology. How could he be so short-sighted? Have we not just seen that it is by astronomy that, to speak his language, humanity has passed from the theological to the positive state? He found an explanation for that because it had happened. But how has he not understood that what remained to do was not less considerable and would be not less profitable? Physical astronomy, which he seems to condemn, has already begun to bear fruit, and it will give us much more, for it only dates from yesterday.

First was discovered the nature of the sun, what the founder of positivism wished to deny us, and there bodies were found which exist on the earth, but had here remained undiscovered; for example, helium, that gas almost as light as hydrogen. That already contradicted Comte. But to the spectroscope we owe a lesson precious in a quite different way; in the most distant stars, it shows us the same substances. It might have been asked whether the terrestrial elements were not due to some chance which had brought together more tenuous atoms to construct of them the more complex edifice that the chemists call atoms; whether, in other regions of the universe, other fortuitous meetings had not engendered edifices entirely different. Now we know that this is not so, that the laws of our chemistry are the general laws of nature, and that they owe nothing to the chance which caused us to be born on the earth.

But, it will be said, astronomy has given to the other sciences all it can give them, and now that the heavens have procured for us the instruments which enable us to study terrestrial nature, they could without danger veil themselves forever. After what we have just said, is there still need to answer this objection? One could have reasoned the same in Ptolemy's time; then also men thought they knew everything, and they still had almost everything to learn.

The stars are majestic laboratories, gigantic crucibles, such as no chemist could dream. There reign temperatures impossible for us to realize. Their only defect is being a little far away; but the telescope will soon bring them near to us, and then we shall see how matter acts there. What good fortune for the physicist and the chemist!

Matter will there exhibit itself to us under a thousand different states, from those rarefied gases which seem to form the nebula and which are luminous with I know not what glimmering of mysterious origin, even to the incandescent stars and to the planets so near and yet so different.

Perchance even, the stars will some day teach us something about life; that seems an insensate dream and I do not at all see how it can be realized; but, a hundred years ago, would not the chemistry of the stars have also appeared a mad dream?

But limiting our views to horizons less distant, there still will remain to us promises less contingent and yet sufficiently seductive. If the past has given us much, we may rest assured that the future will give us still more.

After all, it could scarce be believed how useful belief in astrology has been to humanity. If Kepler and Tycho Brahe made a living, it was because they sold to naïve kings predictions founded on the conjunctions of the stars. If these princes had not been so credulous, we should perhaps continue to believe that nature obeys caprice, and we should still wallow in ignorance.

CHAPTER VII

The History of Mathematical Physics

What is the present state of mathematical physics? What are the problems it is led to set itself? What is its future? Is its orientation about to be modified?

Ten years hence will the aim and the methods of this science appear to our immediate successors in the same light as to ourselves; or, on the contrary, are we about to witness a profound transformation? Such are the questions we are forced to raise in entering to-day upon our investigation.

If it is easy to propound them: to answer is difficult. If we felt tempted to risk a prediction, we should easily resist this temptation, by thinking of all the stupidities the most eminent savants of a hundred years ago would have uttered, if some one had asked them what the science of the nineteenth century would be. They would have thought themselves bold in their predictions, and after the event, how very timid we should have found them. Do not, therefore, expect of me any prophecy.

But if, like all prudent physicians, I shun giving a prognosis, yet I can not dispense with a little diagnostic; well, yes, there are indications of a serious crisis, as if we might expect an approaching transformation. Still, be not too anxious: we are sure the patient will not die of it, and we may even hope that this crisis will be salutary, for the history of the past seems to guarantee us this. This crisis, in fact, is not the first, and to understand it, it is important to recall those which have preceded. Pardon then a brief historical sketch.

The Physics of Central Forces

Mathematical physics, as we know, was born of celestial mechanics, which gave birth to it at the end of the eighteenth century, at the moment when it itself attained its complete development. During its first years especially the infant strikingly resembled its mother.

The astronomic universe is formed of masses, very great, no doubt, but separated by intervals so immense that they appear to us only as material points. These points attract each other inversely as the square of the distance, and this attraction is the sole force which influences their movements. But if our senses were sufficiently keen to show us all the details of the bodies which the physicist studies, the spectacle thus disclosed would scarcely differ from the one the astronomer contemplates. There also we should see material points, separated from one another by intervals, enormous in comparison with their dimensions, and describing orbits according to regular laws. These infinitesimal stars are the atoms. Like the stars proper, they attract or repel each other, and this attraction or this repulsion following the straight line which joins them, depends only on the distance. The law according to which this force varies as function of the distance is perhaps not the law of Newton, but it is an analogous law; in place of the exponent —2, we have probably a different exponent, and it is from this change of exponent that arises all the diversity of physical phenomena, the variety of qualities and of sensations, all the world, colored and sonorous, which surrounds us; in a word, all nature.

Such is the primitive conception in all its purity. It only remains to seek in the different cases what value should be given to this exponent in order to explain all the facts. It is on this model that Laplace, for example, constructed his beautiful theory of capillarity; he regards it only as a particular case of attraction, or, as he says, of universal gravitation, and no one is astonished to find it in the middle of one of the five volumes of the 'Mécanique céleste.' More recently Briot believes he penetrated the final secret of optics in demonstrating that the atoms of ether attract each other in the inverse ratio of the sixth power of the distance; and Maxwell, Maxwell himself, does he not say somewhere that the atoms of gases repel each other in the inverse ratio of the fifth power of the distance? We have the exponent —6, or —5, in place of the exponent —2, but it is always an exponent.

Among the theories of this epoch, one alone is an exception, that of Fourier; in it are indeed atoms acting at a distance one upon the other; they mutually transmit heat, but they do not attract, they never budge. From this point of view, Fourier's theory must have appeared to the eyes of his contemporaries, to those of Fourier himself, as imperfect and provisional.

This conception was not without grandeur; it was seductive, and many among us have not finally renounced it; they know that one will attain the ultimate elements of things only by patiently disentangling the complicated skein that our senses give us; that it is necessary to advance step by step, neglecting no intermediary; that our fathers were wrong in wishing to skip stations; but they believe that when one shall have arrived at these ultimate elements, there again will be found the majestic simplicity of celestial mechanics.

Neither has this conception been useless; it has rendered us an inestimable service, since it has contributed to make precise the fundamental notion of the physical law.

I will explain myself; how did the ancients understand law? It was for them an internal harmony, static, so to say, and immutable; or else it was like a model that nature tried to imitate. For us a law is something quite different; it is a constant relation between the phenomenon of to-day and that of to-morrow; in a word, it is a differential equation.

Behold the ideal form of physical law; well, it is Newton's law which first clothed it forth. If then one has acclimated this form in physics, it is precisely by copying as far as possible this law of Newton, that is by imitating celestial mechanics. This is, moreover, the idea I have tried to bring out in chapter VI.

The Physics of the Principles

Nevertheless, a day arrived when the conception of central forces no longer appeared sufficient, and this is the first of those crises of which I just now spoke.

What was done then? The attempt to penetrate into the detail of the structure of the universe, to isolate the pieces of this vast mechanism, to analyze one by one the forces which put them in motion, was abandoned, and we were content to take as guides certain general principles the express object of which is to spare us this minute study. How so? Suppose we have before us any machine; the initial wheel work and the final wheel work alone are visible, but the transmission, the intermediary machinery by which the movement is communicated from one to the other, is hidden in the interior and escapes our view; we do not know whether the communication is made by gearing or by belts, by connecting-rods or by other contrivances. Do we say that it is impossible for us to understand anything about this machine so long as we are not permitted to take it to pieces? You know well we do not, and that the principle of the conservation of energy suffices to determine for us the most interesting point. We easily ascertain that the final wheel turns ten times less quickly than the initial wheel, since these two wheels are visible; we are able thence to conclude that a couple applied to the one will be balanced by a couple ten times greater applied to the other. For that there is no need to penetrate the mechanism of this equilibrium and to know how the forces compensate each other in the interior of the machine; it suffices to be assured that this compensation can not fail to occur.

Well, in regard to the universe, the principle of the conservation of energy is able to render us the same service. The universe is also a machine, much more complicated than all those of industry, of which almost all the parts are profoundly hidden from us; but in observing the motion of those that we can see, we are able, by the aid of this principle, to draw conclusions which remain true whatever may be the details of the invisible mechanism which animates them.

The principle of the conservation of energy, or Mayer's principle, is certainly the most important, but it is not the only one; there are others from which we can derive the same advantage. These are:

Carnot's principle, or the principle of the degradation of energy.

Newton's principle, or the principle of the equality of action and reaction.

The principle of relativity, according to which the laws of physical phenomena must be the same for a stationary observer as for an observer carried along in a uniform motion of translation; so that we have not and can not have any means of discerning whether or not we are carried along in such a motion.

The principle of the conservation of mass, or Lavoisier's principle.

I will add the principle of least action.

The application of these five or six general principles to the different physical phenomena is sufficient for our learning of them all that we could reasonably hope to know of them. The most remarkable example of this new mathematical physics is, beyond question, Maxwell's electromagnetic theory of light.

We know nothing as to what the ether is, how its molecules are disposed, whether they attract or repel each other; but we know that this medium transmits at the same time the optical perturbations and the electrical perturbations; we know that this transmission must take place in conformity with the general principles of mechanics, and that suffices us for the establishment of the equations of the electromagnetic field.

These principles are results of experiments boldly generalized; but they seem to derive from their very generality a high degree of certainty. In fact, the more general they are, the more frequent are the opportunities to check them, and the verifications multiplying, taking the most varied, the most unexpected forms, end by no longer leaving place for doubt.

Utility of the Old Physics.—Such is the second phase of the history of mathematical physics and we have not yet emerged from it. Shall we say that the first has been useless? that during fifty years science went the wrong way, and that there is nothing left but to forget so many accumulated efforts that a vicious conception condemned in advance to failure? Not the least in the world. Do you think the second phase could have come into existence without the first? The hypothesis of central forces contained all the principles; it involved them as necessary consequences; it involved both the conservation of energy and that of masses, and the equality of action and reaction, and the law of least action, which appeared, it is true, not as experimental truths, but as theorems; the enunciation of which had at the same time something more precise and less general than under their present form.

It is the mathematical physics of our fathers which has familiarized us little by little with these various principles; which has habituated us to recognize them under the different vestments in which they disguise themselves. They have been compared with the data of experience, it has been seen how it was necessary to modify their enunciation to adapt them to these data; thereby they have been extended and consolidated. Thus they came to be regarded as experimental truths; the conception of central forces became then a useless support, or rather an embarrassment, since it made the principles partake of its hypothetical character.

The frames then have not broken, because they are elastic; but they have enlarged; our fathers, who established them, did not labor in vain, and we recognize in the science of to-day the general traits of the sketch which they traced.

CHAPTER VIII

The Present Crisis of Mathematical Physics

The New Crisis.—Are we now about to enter upon a third period? Are we on the eve of a second crisis? These principles on which we have built all, are they about to crumble away in their turn? This has been for some time a pertinent question.

When I speak thus, you no doubt think of radium, that grand revolutionist of the present time, and in fact I shall come back to it presently; but there is something else. It is not alone the conservation of energy which is in question; all the other principles are equally in danger, as we shall see in passing them successively in review.

Carnot's Principle.—Let us commence with the principle of Carnot. This is the only one which does not present itself as an immediate consequence of the hypothesis of central forces; more than that, it seems, if not to directly contradict that hypothesis, at least not to be reconciled with it without a certain effort. If physical phenomena were due exclusively to the movements of atoms whose mutual attraction depended only on the distance, it seems that all these phenomena should be reversible; if all the initial velocities were reversed, these atoms, always subjected to the same forces, ought to go over their trajectories in the contrary sense, just as the earth would describe in the retrograde sense this same elliptic orbit which it describes in the direct sense, if the initial conditions of its motion had been reversed. On this account, if a physical phenomenon is possible, the inverse phenomenon should be equally so, and one should be able to reascend the course of time. Now, it is not so in nature, and this is precisely what the principle of Carnot teaches us; heat can pass from the warm body to the cold body; it is impossible afterwards to make it take the inverse route and to reestablish differences of temperature which have been effaced. Motion can be wholly dissipated and transformed into heat by friction; the contrary transformation can never be made except partially.

We have striven to reconcile this apparent contradiction. If the world tends toward uniformity, this is not because its ultimate parts, at first unlike, tend to become less and less different; it is because, shifting at random, they end by blending. For an eye which should distinguish all the elements, the variety would remain always as great; each grain of this dust preserves its originality and does not model itself on its neighbors; but as the blend becomes more and more intimate, our gross senses perceive only the uniformity. This is why, for example, temperatures tend to a level, without the possibility of going backwards.

A drop of wine falls into a glass of water; whatever may be the law of the internal motion of the liquid, we shall soon see it colored of a uniform rosy tint, and however much from this moment one may shake it afterwards, the wine and the water do not seem capable of again separating. Here we have the type of the irreversible physical phenomenon: to hide a grain of barley in a heap of wheat, this is easy; afterwards to find it again and get it out, this is practically impossible. All this Maxwell and Boltzmann have explained; but the one who has seen it most clearly, in a book too little read because it is a little difficult to read, is Gibbs, in his 'Elementary Principles of Statistical Mechanics.'

For those who take this point of view, Carnot's principle is only an imperfect principle, a sort of concession to the infirmity of our senses; it is because our eyes are too gross that we do not distinguish the elements of the blend; it is because our hands are too gross that we can not force them to separate; the imaginary demon of Maxwell, who is able to sort the molecules one by one, could well constrain the world to return backward. Can it return of itself? That is not impossible; that is only infinitely improbable. The chances are that we should wait a long time for the concourse of circumstances which would permit a retrogradation; but sooner or later they will occur, after years whose number it would take millions of figures to write. These reservations, however, all remained theoretic; they were not very disquieting, and Carnot's principle retained all its principal value. But here the scene changes. The biologist, armed with his microscope, long ago noticed in his preparations irregular movements of little particles in suspension; this is the Brownian movement. He first thought this was a vital phenomenon, but soon he saw that the inanimate bodies danced with no less ardor than the others; then he turned the matter over to the physicists. Unhappily, the physicists remained long uninterested in this question; one concentrates the light to illuminate the microscopic preparation, thought they; with light goes heat; thence inequalities of temperature and in the liquid interior currents which produce the movements referred to.

It occurred to M. Gouy to look more closely, and he saw, or thought he saw, that this explanation is untenable, that the movements become brisker as the particles are smaller, but that they are not influenced by the mode of illumination. If then these movements never cease, or rather are reborn without cease, without borrowing anything from an external source of energy, what ought we to believe? To be sure, we should not on this account renounce our belief in the conservation of energy, but we see under our eyes now motion transformed into heat by friction, now inversely heat changed into motion, and that without loss since the movement lasts forever. This is the contrary of Carnot's principle. If this be so, to see the world return backward, we no longer have need of the infinitely keen eye of Maxwell's demon; our microscope suffices. Bodies too large, those, for example, which are a tenth of a millimeter, are hit from all sides by moving atoms, but they do not budge, because these shocks are very numerous and the law of chance makes them compensate each other; but the smaller particles receive too few shocks for this compensation to take place with certainty and are incessantly knocked about. And behold already one of our principles in peril.

The Principle of Relativity.—Let us pass to the principle of relativity: this not only is confirmed by daily experience, not only is it a necessary consequence of the hypothesis of central forces, but it is irresistibly imposed upon our good sense, and yet it also is assailed. Consider two electrified bodies; though they seem to us at rest, they are both carried along by the motion of the earth; an electric charge in motion, Rowland has taught us, is equivalent to a current; these two charged bodies are, therefore, equivalent to two parallel currents of the same sense and these two currents should attract each other. In measuring this attraction, we shall measure the velocity of the earth; not its velocity in relation to the sun or the fixed stars, but its absolute velocity.

I well know what will be said: It is not its absolute velocity that is measured, it is its velocity in relation to the ether. How unsatisfactory that is! Is it not evident that from the principle so understood we could no longer infer anything? It could no longer tell us anything just because it would no longer fear any contradiction. If we succeed in measuring anything, we shall always be free to say that this is not the absolute velocity, and if it is not the velocity in relation to the ether, it might always be the velocity in relation to some new unknown fluid with which we might fill space.

Indeed, experiment has taken upon itself to ruin this interpretation of the principle of relativity; all attempts to measure the velocity of the earth in relation to the ether have led to negative results. This time experimental physics has been more faithful to the principle than mathematical physics; the theorists, to put in accord their other general views, would not have spared it; but experiment has been stubborn in confirming it. The means have been varied; finally Michelson pushed precision to its last limits; nothing came of it. It is precisely to explain this obstinacy that the mathematicians are forced to-day to employ all their ingenuity.

Their task was not easy, and if Lorentz has got through it, it is only by accumulating hypotheses.

The most ingenious idea was that of local time. Imagine two observers who wish to adjust their timepieces by optical signals; they exchange signals, but as they know that the transmission of light is not instantaneous, they are careful to cross them. When station B perceives the signal from station A, its clock should not mark the same hour as that of station A at the moment of sending the signal, but this hour augmented by a constant representing the duration of the transmission. Suppose, for example, that station A sends its signal when its clock marks the hour *O*, and that station B perceives it when its clock marks the hour *t*. The clocks are adjusted if the slowness equal to *t* represents the duration of the transmission, and to verify it, station B sends in its turn a signal when its clock marks *O;* then station A should perceive it when its clock marks *t*. The timepieces are then adjusted.

And in fact they mark the same hour at the same physical instant, but on the one condition, that the two stations are fixed. Otherwise the duration of the transmission will not be the same in the two senses, since the station A, for example, moves forward to meet the optical perturbation emanating from B, whereas the station B flees before the perturbation emanating from A. The watches adjusted in that way will not mark, therefore, the true time; they will mark what may be called the *local time*, so that one of them will gain on the other. It matters little, since we have no means of perceiving it. All the phenomena which happen at A, for example, will be late, but all will be equally so, and the observer will not perceive it, since his watch is slow; so, as the principle of relativity would have it, he will have no means of knowing whether he is at rest or in absolute motion.

Unhappily, that does not suffice, and complementary hypotheses are necessary; it is necessary to admit that bodies in motion undergo a uniform contraction in the sense of the motion. One of the diameters of the earth, for example, is shrunk by one two-hundred-millionth in consequence of our planet's motion, while the other diameter retains its normal length. Thus the last little differences are compensated. And then, there is still the hypothesis about forces. Forces, whatever be their origin, gravity as well as elasticity, would be reduced in a certain proportion in a world animated by a uniform translation; or, rather, this would happen for the components perpendicular to the translation; the components parallel would not change. Resume, then, our example of two electrified bodies; these bodies repel each other, but at the same time if all is carried along in a uniform translation, they are equivalent to two parallel currents of the same sense which attract each other. This electrodynamic attraction diminishes, therefore, the electrostatic repulsion, and the total repulsion is feebler than if the two bodies were at rest. But since to measure this repulsion we must balance it by another force, and all these other forces are reduced in the same proportion, we perceive nothing. Thus, all seems arranged, but are all the doubts dissipated? What would happen if one could communicate by non-luminous signals whose velocity of propagation differed from that of light? If, after having adjusted the watches by the optical procedure, we wished to verify the adjustment by the aid of these new signals, we should observe discrepancies which would render evident the common translation of the two stations. And are such signals inconceivable, if we admit with Laplace that universal gravitation is transmitted a million times more rapidly than light?

Thus, the principle of relativity has been valiantly defended in these latter times, but the very energy of the defense proves how serious was the attack.

Newton's Principle.—Let us speak now of the principle of Newton, on the equality of action and reaction. This is intimately bound up with the preceding, and it seems indeed that the fall of the one would involve that of the other. Thus we must not be astonished to find here the same difficulties.

Electrical phenomena, according to the theory of Lorentz, are due to the displacements of little charged particles, called electrons, immersed in the medium we call ether. The movements of these electrons produce perturbations in the neighboring ether; these perturbations propagate themselves in every direction with the velocity of light, and in turn other electrons, originally at rest, are made to vibrate when the perturbation reaches the parts of the ether which touch them. The electrons, therefore, act on one another, but this action is not direct, it is accomplished through the ether as intermediary. Under these conditions can there be compensation between action and reaction, at least for an observer who should take account only of the movements of matter, that is, of the electrons, and who should be ignorant of those of the ether that he could not see? Evidently not. Even if the compensation should be exact, it could not be simultaneous. The perturbation is propagated with a finite velocity; it, therefore, reaches the second electron only when the first has long ago entered upon its rest. This second electron, therefore, will undergo, after a delay, the action of the first, but will certainly not at that moment react upon it, since around this first electron nothing any longer budges.

The analysis of the facts permits us to be still more precise. Imagine, for example, a Hertzian oscillator, like those used in wireless telegraphy; it sends out energy in every direction; but we can provide it with a parabolic mirror, as Hertz did with his smallest oscillators, so as to send all the energy produced in a single direction. What happens then according to the theory? The apparatus recoils, as if it were a cannon and the projected energy a ball; and that is contrary to the principle of Newton, since our projectile here has no mass, it is not matter, it is energy. The case is still the same, moreover, with a beacon light provided with a reflector, since light is nothing but a perturbation of the electromagnetic field. This beacon light should recoil as if the light it sends out were a projectile. What is the force that should produce this recoil? It is what is called the Maxwell-Bartholdi pressure. It is very minute, and it has been difficult to put it in evidence even with the most sensitive radiometers; but it suffices that it exists.

If all the energy issuing from our oscillator falls on a receiver, this will act as if it had received a mechanical shock, which will represent in a sense the compensation of the oscillator's recoil; the reaction will be equal to the action, but it will not be simultaneous; the receiver will move on, but not at the moment when the oscillator recoils. If the energy propagates itself indefinitely without encountering a receiver, the compensation will never occur.

Shall we say that the space which separates the oscillator from the receiver and which the perturbation must pass over in going from the one to the other is not void, that it is full not only of ether, but of air, or even in the interplanetary spaces of some fluid subtile but still ponderable; that this matter undergoes the shock like the receiver at the moment when the energy reaches it, and recoils in its turn when the perturbation quits it? That would save Newton's principle, but that is not true. If energy in its diffusion remained always attached to some material substratum, then matter in motion would carry along light with it, and Fizeau has demonstrated that it does nothing of the sort, at least for air. Michelson and Morley have since confirmed this. It might be supposed also that the movements of matter proper are exactly compensated by those of the ether; but that would lead us to the same reflections as before now. The principle so understood will explain everything, since, whatever might be the visible movements, we always could imagine hypothetical movements which compensate them. But if it is able to explain everything, this is because it does not enable us to foresee anything; it does not enable us to decide between the different possible hypotheses, since it explains everything beforehand. It therefore becomes useless.

And then the suppositions that it would be necessary to make on the movements of the ether are not very satisfactory. If the electric charges double, it would be natural to imagine that the velocities of the diverse atoms of ether double also, and for the compensation, it would be necessary that the mean velocity of the ether quadruple.

This is why I have long thought that these consequences of theory, contrary to Newton's principle, would end some day by being abandoned, and yet the recent experiments on the movements of the electrons issuing from radium seem rather to confirm them.

Lavoisier's Principle.—I arrive at the principle of Lavoisier on the conservation of mass. Certainly, this is one not to be touched without unsettling all mechanics. And now certain persons think that it seems true to us only because in mechanics merely moderate velocities are considered, but that it would cease to be true for bodies animated by velocities comparable to that of light. Now these velocities, it is believed at present, have been realized; the cathode rays or those of radium may be formed of very minute particles or of electrons which are displaced with velocities smaller no doubt than that of light, but which might be its one tenth or one third.

These rays can be deflected, whether by an electric field, or by a magnetic field, and we are able, by comparing these deflections, to measure at the same time the velocity of the electrons and their mass (or rather the relation of their mass to their charge). But when it was seen that these velocities approached that of light, it was decided that a correction was necessary. These molecules, being electrified, can not be displaced without agitating the ether; to put them in motion it is necessary to overcome a double inertia, that of the molecule itself and that of the ether. The total or apparent mass that one measures is composed, therefore, of two parts: the real or mechanical mass of the molecule and the electrodynamic mass representing the inertia of the ether.

The calculations of Abraham and the experiments of Kaufmann have then shown that the mechanical mass, properly so called, is null, and that the mass of the electrons, or, at least, of the negative electrons, is of exclusively electrodynamic origin. This is what forces us to change the definition of mass; we can not any longer distinguish mechanical mass and electrodynamic mass, since then the first would vanish; there is no mass other than electrodynamic inertia. But in this case the mass can no longer be constant; it augments with the velocity, and it even depends on the direction, and a body animated by a notable velocity will not oppose the same inertia to the forces which tend to deflect it from its route, as to those which tend to accelerate or to retard its progress.

There is still a resource; the ultimate elements of bodies are electrons, some charged negatively, the others charged positively. The negative electrons have no mass, this is understood; but the positive electrons, from the little we know of them, seem much greater. Perhaps they have, besides their electrodynamic mass, a true mechanical mass. The real mass of a body would, then, be the sum of the mechanical masses of its positive electrons, the negative electrons not counting; mass so defined might still be constant.

Alas! this resource also evades us. Recall what we have said of the principle of relativity and of the efforts made to save it. And it is not merely a principle which it is a question of saving, it is the indubitable results of the experiments of Michelson.

Well, as was above seen, Lorentz, to account for these results, was obliged to suppose that all forces, whatever their origin, were reduced in the same proportion in a medium animated by a uniform translation; this is not sufficient; it is not enough that this take place for the real forces, it must also be the same for the forces of inertia; it is therefore necessary, he says, that *the masses of all the particles be influenced by a translation to the same degree as the electromagnetic masses of the electrons.*

So the mechanical masses must vary in accordance with the same laws as the electrodynamic masses; they can not, therefore, be constant.

Need I point out that the fall of Lavoisier's principle involves that of Newton's? This latter signifies that the center of gravity of an isolated system moves in a straight line; but if there is no longer a constant mass, there is no longer a center of gravity, we no longer know even what this is. This is why I said above that the experiments on the cathode rays appeared to justify the doubts of Lorentz concerning Newton's principle.

From all these results, if they were confirmed, would arise an entirely new mechanics, which would be, above all, characterized by this fact, that no velocity could surpass that of light,[1] any more than any temperature can fall below absolute zero.

No more for an observer, carried along himself in a translation he does not suspect, could any apparent velocity surpass that of light; and this would be then a contradiction, if we did not recall that this observer would not use the same clocks as a fixed observer, but, indeed, clocks marking 'local time.'

Here we are then facing a question I content myself with stating. If there is no longer any mass, what becomes of Newton's law? Mass has two aspects: it is at the same time a coefficient of inertia and an attracting mass entering as factor into Newtonian attraction. If the coefficient of inertia is not constant, can the attracting mass be? That is the question.

Mayer's Principle.—At least, the principle of the conservation of energy yet remained to us, and this seemed more solid. Shall I recall to you how it was in its turn thrown into discredit? This event has made more noise than the preceding, and it is in all the memoirs. From the first works of Becquerel, and, above all, when the Curies had discovered radium, it was seen that every radioactive body was an inexhaustible source of radiation. Its activity seemed to subsist without alteration throughout the months and the years. This was in itself a strain on the principles; these radiations were in fact energy, and from the same morsel of radium this issued and forever issued. But these

[1] Because bodies would oppose an increasing inertia to the causes which would tend to accelerate their motion; and this inertia would become infinite when one approached the velocity of light.

quantities of energy were too slight to be measured; at least that was the belief and we were not much disquieted.

The scene changed when Curie bethought himself to put radium in a calorimeter; it was then seen that the quantity of heat incessantly created was very notable.

The explanations proposed were numerous; but in such case we can not say, the more the better. In so far as no one of them has prevailed over the others, we can not be sure there is a good one among them. Since some time, however, one of these explanations seems to be getting the upper hand and we may reasonably hope that we hold the key to the mystery.

Sir W. Ramsay has striven to show that radium is in process of transformation, that it contains a store of energy enormous but not inexhaustible. The transformation of radium then would produce a million times more heat than all known transformations; radium would wear itself out in 1,250 years; this is quite short, and you see that we are at least certain to have this point settled some hundreds of years from now. While waiting, our doubts remain.

CHAPTER IX

The Future of Mathematical Physics

The Principles and Experiment.—In the midst of so much ruin, what remains standing? The principle of least action is hitherto intact, and Larmor appears to believe that it will long survive the others; in reality, it is still more vague and more general.

In presence of this general collapse of the principles, what attitude will mathematical physics take? And first, before too much excitement, it is proper to ask if all that is really true. All these derogations to the principles are encountered only among infinitesimals; the microscope is necessary to see the Brownian movement; electrons are very light; radium is very rare, and one never has more than some milligrams of it at a time. And, then, it may be asked whether, besides the infinitesimal seen, there was not another infinitesimal unseen counterpoise to the first.

So there is an interlocutory question, and, as it seems, only experiment can solve it. We shall, therefore, only have to hand over the matter to the experimenters, and, while waiting for them to finally decide the debate, not to preoccupy ourselves with these disquieting problems, and to tranquilly continue our work as if the principles were still uncontested. Certes, we have much to do without leaving the domain where they may be applied in all security; we have enough to employ our activity during this period of doubts.

The Rôle of the Analyst.—And as to these doubts, is it indeed true that we can do nothing to disembarrass science of them? It must indeed be said, it is not alone experimental physics that has given birth to them; mathematical physics has well contributed. It is the experimenters who have seen radium throw out energy, but it is the theorists who have put in evidence all the difficulties raised by the propagation of light across a medium in motion; but for these it is probable we should not have become conscious of them. Well, then, if they have done their best to put us into this embarrassment, it is proper also that they help us to get out of it.

They must subject to critical examination all these new views I have just outlined before you, and abandon the principles only after having made a loyal effort to save them. What can they do in this sense? That is what I will try to explain.

It is a question before all of endeavoring to obtain a more satisfactory theory of the electrodynamics of bodies in motion. It is there especially, as I have sufficiently shown above, that difficulties accumulate. It is useless to heap up hypotheses, we can not satisfy all the principles at once; so far, one has succeeded in safeguarding some only on condition of sacrificing the others; but all hope of obtaining better results is not yet lost. Let us take, then, the theory of Lorentz, turn it in all senses, modify it little by little, and perhaps everything will arrange itself.

Thus in place of supposing that bodies in motion undergo a contraction in the sense of the motion, and that this contraction is the same whatever be the nature of these bodies and the forces to which they are otherwise subjected, could we not make a more simple and natural hypothesis? We might imagine, for example, that it is the ether which is modified when it is in relative motion in reference to the material medium which penetrates it, that, when it is thus modified, it no longer transmits perturbations with the same velocity in every direction. It might transmit more rapidly those which are propagated parallel to the motion of the medium, whether in the same sense or in the opposite sense, and less rapidly those which are propagated perpendicularly. The wave surfaces would no longer be spheres, but ellipsoids, and we could dispense with that extraordinary contraction of all bodies.

I cite this only as an example, since the modifications that might be essayed would be evidently susceptible of infinite variation.

Aberration and Astronomy.—It is possible also that astronomy may some day furnish us data on this point; she it was in the main who raised the question in making us acquainted with the phenomenon of the aberration of light. If we make crudely the theory of aberration, we reach a very curious result. The apparent positions of the stars differ from their real positions because of the earth's motion, and as this motion is variable, these apparent positions vary. The real position we can not ascertain, but we can observe the variations of the apparent position. The observations of the aberration show us, therefore, not the earth's motion, but the variations of this motion; they can not, therefore, give us information about the absolute motion of the earth.

At least this is true in first approximation, but the case would be no longer the same if we could appreciate the thousandths of a second. Then it would be seen that the amplitude of the oscillation depends not alone on the variation of the motion, a variation which is well known, since it is the motion of our globe on its elliptic orbit, but on the mean value of this motion, so that the constant of aberration would not be quite the same for all the stars, and the differences would tell us the absolute motion of the earth in space.

This, then, would be, under another form, the ruin of the principle of relativity. We are far, it is true, from appreciating the thousandth of a second, but, after all, say some, the earth's total absolute velocity is perhaps much greater than its relative velocity with respect to the sun. If, for example, it were 300 kilometers per second in place of 30, this would suffice to make the phenomenon observable.

I believe that in reasoning thus one admits a too simple theory of aberration. Michelson has shown us, I have told you, that the physical procedures are powerless to put in evidence absolute motion; I am persuaded that the same will be true of the astronomic procedures, however far precision be carried.

However that may be, the data astronomy will furnish us in this regard will some day be precious to the physicist. Meanwhile, I believe that the theorists, recalling the experience of Michelson, may anticipate a negative result, and that they would accomplish a useful work in constructing a theory of aberration which would explain this in advance.

Electrons and Spectra.—This dynamics of electrons can be approached from many sides, but among the ways leading thither is one which has been somewhat neglected, and yet this is one of those which promise us the most surprises. It is movements of electrons which produce the lines of the emission spectra; this is proved by the Zeeman effect; in an incandescent body what vibrates is sensitive to the magnet, therefore electrified. This is a very important first point, but no one has gone farther. Why are the lines of the spectrum distributed in accordance with a regular law? These laws have been studied by the experimenters in their least details; they are very precise and comparatively simple. A first study of these distributions recalls the harmonics encountered in acoustics; but the difference is great. Not only are the numbers of vibrations not the successive multiples of a single number, but we do not even find anything analogous to the roots of those transcendental equations to which we are led by so many problems of mathematical physics: that of the vibrations of an elastic body of any form, that of the Hertzian oscillations in a generator of any form, the problem of Fourier for the cooling of a solid body.

The laws are simpler, but they are of wholly other nature, and to cite only one of these differences, for the harmonics of high order, the number of vibrations tends toward a finite limit, instead of increasing indefinitely.

That has not yet been accounted for, and I believe that there we have one of the most important secrets of nature. A Japanese physicist, M. Nagaoka, has recently proposed an explanation; according to him, atoms are composed of a large positive electron surrounded by a ring formed of a very great number of very small negative electrons. Such is the planet Saturn with its rings. This is a very interesting attempt, but not yet wholly satisfactory; this attempt should be renewed. We will penetrate, so to speak, into the inmost recess of matter. And from the particular point of view which we to-day occupy, when we know why the vibrations of incandescent bodies differ thus from ordinary elastic vibrations, why the electrons do not behave like the matter which is familiar to us, we shall better comprehend the dynamics of electrons and it will be perhaps more easy for us to reconcile it with the principles.

Conventions Preceding Experiment.—Suppose, now, that all these efforts fail, and, after all, I do not believe they will, what must be done? Will it be necessary to seek to mend the broken principles by giving what we French call a *coup de pouce?* That evidently is always possible, and I retract nothing of what I have said above.

Have you not written, you might say if you wished to seek a quarrel with me—have you not written that the principles, though of experimental origin, are now unassailable by experiment because they have become conventions? And now you have just told us that the most recent conquests of experiment put these principles in danger.

Well, formerly I was right and to-day I am not wrong. Formerly I was right, and what is now happening is a new proof of it. Take, for example, the calorimetric experiment of Curie on radium. Is it possible to reconcile it with the principle of the conservation of energy? This has been attempted in many ways; but there is among them one I should like you to notice; this is not the explanation which tends to-day to prevail, but it is one of those which have been proposed. It has been conjectured that radium was only an intermediary, that it only stored radiations of unknown nature which flashed through space in every direction, traversing all bodies, save radium, without being altered by this passage and without exercising any action upon them. Radium alone took from them a little of their energy and afterward gave it out to us in various forms.

What an advantageous explanation, and how convenient! First, it is unverifiable and thus irrefutable. Then again it will serve to account for any derogation whatever to Mayer's principle; it answers in advance not only the objection of Curie, but all the objections that future experimenters might accumulate. This new and unknown energy would serve for everything.

This is just what I said, and therewith we are shown that our principle is unassailable by experiment.

But then, what have we gained by this stroke? The principle is intact, but thenceforth of what use is it? It enabled us to foresee that in such or such circumstance we could count on such a total quantity of energy; it limited us; but now that this indefinite provision of new energy is placed at our disposal, we are no longer limited by anything; and, as I have written in 'Science and Hypothesis,' if a principle ceases to be fecund, experiment without contradicting it directly will nevertheless have condemned it.

Future Mathematical Physics. This, therefore, is not what would have to be done; it would be necessary to rebuild anew. If we were reduced to this necessity, we could moreover console ourselves. It would not be necessary thence to conclude that science can weave only a Penelope's web, that it can raise only ephemeral structures, which it is soon forced to demolish from top to bottom with its own hands.

As I have said, we have already passed through a like crisis. I have shown you that in the second mathematical physics, that of the principles, we find traces of the first, that of central forces; it will be just the same if we must know a third. Just so with the animal that exuviates, that breaks its too narrow carapace and makes itself a fresh one, under the new envelope one will recognize the essential traits of the organism which have persisted.

We can not foresee in what way we are about to expand; perhaps it is the kinetic theory of gases which is about to undergo development and serve as model to the others. Then the facts which first appeared to us as simple thereafter would be merely resultants of a very great number of elementary facts which only the laws of chance would make cooperate for a common end. Physical law would then assume an entirely new aspect; it would no longer be solely a differential equation, it would take the character of a statistical law.

Perhaps, too, we shall have to construct an entirely new mechanics that we only succeed in catching a glimpse of, where, inertia increasing with the velocity, the velocity of light would become an impassable limit. The ordinary mechanics, more simple, would remain a first approximation, since it would be true fcr velocities not too great, so that the old dynamics would still be found under the new. We should not have to regret having believed in the principles, and even, since velocities too great for the old formulas would always be only exceptional, the surest way in practise would be still to act as if we continued to believe in them. They are so useful, it would be necessary to keep a place for them. To determine to exclude them altogether would be to deprive oneself of a precious weapon. I hasten to say in conclusion that we are not yet there, and as yet nothing proves that the principles will not come forth from out the fray victorious and intact.[1]

[1] These considerations on mathematical physics are borrowed from my St. Louis address.

PART III

THE OBJECTIVE VALUE OF SCIENCE

CHAPTER X

Is Science Artificial?

1. *The Philosophy of M. LeRoy*

There are many reasons for being sceptics; should we push this scepticism to the very end or stop on the way? To go to the end is the most tempting solution, the easiest, and that which many have adopted, despairing of saving anything from the shipwreck.

Among the writings inspired by this tendency it is proper to place in the first rank those of M. LeRoy. This thinker is not only a philosopher and a writer of the greatest merit, but he has acquired a deep knowledge of the exact and physical sciences, and even has shown rare powers of mathematical invention. Let us recapitulate in a few words his doctrine, which has given rise to numerous discussions.

Science consists only of conventions, and to this circumstance solely does it owe its apparent certitude; the facts of science and, *a fortiori*, its laws are the artificial work of the scientist; science therefore can teach us nothing of the truth; it can only serve us as rule of action.

Here we recognize the philosophic theory known under the name of nominalism; all is not false in this theory; its legitimate domain must be left it, but out of this it should not be allowed to go.

This is not all; M. LeRoy's doctrine is not only nominalistic; it has besides another characteristic which it doubtless owes to M. Bergson, it is anti-intellectualistic. According to M. LeRoy, the intellect deforms all it touches, and that is still more true of its necessary instrument 'discourse.' There is reality only in our fugitive and changing impressions, and even this reality, when touched, vanishes.

And yet M. LeRoy is not a sceptic; if he regards the intellect as incurably powerless, it is only to give more scope to other sources of knowledge, to the heart for instance, to sentiment, to instinct or to faith.

However great my esteem for M. LeRoy's talent, whatever the ingenuity of this thesis, I can not wholly accept it. Certes, I am in accord on many points with M. LeRoy, and he has even cited, in support of his view, various passages of my writings which I am by no means disposed to reject. I think myself only the more bound to explain why I can not go with him all the way.

M. LeRoy often complains of being accused of scepticism. He could not help being, though this accusation is probably unjust. Are not appearances against him? Nominalist in doctrine, but realist at heart, he seems to escape absolute nominalism only by a desperate act of faith.

The fact is that anti-intellectualistic philosophy in rejecting analysis and 'discourse,' just by that condemns itself to being intransmissible, it is a philosophy essentially internal, or, at the very least, only its negations can be transmitted; what wonder then that for an external observer it takes the shape of scepticism?

Therein lies the weak point of this philosophy; if it strives to remain faithful to itself, its energy is spent in a negation and a cry of enthusiasm. Each author may repeat this negation and this cry, may vary their form, but without adding anything.

And yet, would it not be more logical in remaining silent? See, you have written long articles; for that, it was necessary to use words. And therein have you not been much more 'discursive' and consequently much farther from life and truth than the animal who simply lives without philosophizing? Would not this animal be the true philosopher?

However, because no painter has made a perfect portrait, should we conclude that the best painting is not to paint? When a zoologist dissects an animal, certainly he 'alters it.' Yes, in dissecting it, he condemns himself to never know all of it; but in not dissecting it, he would condemn himself to never know anything of it and consequently to never see anything of it.

Certes, in man are other forces besides his intellect, no one has ever been mad enough to deny that. The first comer makes these blind forces act or lets them act; the philosopher must *speak* of them; to speak of them, he must know of them the little that can be known, he should therefore *see* them act. How? With what eyes, if not with his intellect? Heart, instinct, may guide it, but not render it useless; they may direct the look, but not replace the eye. It may be granted that the heart is the workman, and the intellect only the instrument. Yet is it an instrument not to be done without, if not for action, at least for philosophizing. Therefore a philosopher really anti-intellectualistic is impossible. Perhaps we shall have to declare for the supremacy of action; always it is our intellect which will thus conclude; in allowing precedence to action it will thus retain the superiority of the thinking reed. This also is a supremacy not to be disdained.

Pardon these brief reflections and pardon also their brevity, scarcely skimming the question. The process of intellectualism is not the subject I wish to treat: I wish to speak of science, and about it there is no doubt; by definition, so to speak, it will be intellectualistic or it will not be at all. Precisely the question is, whether it will be.

2. *Science, Rule of Action*

For M. LeRoy, science is only a rule of action. We are powerless to know anything and yet we are launched, we must act, and at all hazards we have established rules. It is the aggregate of these rules that is called science.

It is thus that men, desirous of diversion, have instituted rules of play, like those of tric-trac for instance, which, better than science itself, could rely upon the proof by universal consent. It is thus likewise that, unable to choose, but forced to choose, we toss up a coin, head or tail to win.

The rule of tric-trac is indeed a rule of action like science, but does any one think the comparison just and not see the difference? The rules of the game are arbitrary conventions, and the contrary convention might have been adopted, *which would have been none the less good.* On the contrary, science is a rule of action which is successful, generally at least, and I add, while the contrary rule would not have succeeded.

If I say, to make hydrogen cause an acid to act on zinc, I formulate a rule which succeeds; I could have said, make distilled water act on gold; that also would have been a rule, only it would not have succeeded. If, therefore, scientific 'recipes' have a value, as rule of action, it is because we know they succeed, generally at least. But to know this is to know something and then why tell us we can know nothing?

Science foresees, and it is because it foresees, that it can be useful and serve as rule of action. I well know that its previsions are often contradicted by the event; that shows that science is imperfect and if I add that it will always remain so, I am certain that this is a prevision which, at least, will never be contradicted. Always the scientist is less often mistaken than a prophet who should predict at random. Besides the progress though slow is continuous, so that scientists, though more and more bold, are less and less misled. This is little, but it is enough.

I well know that M. LeRoy has somewhere said that science was mistaken oftener than one thought, that comets sometimes played tricks on astronomers, that scientists, who apparently are men, did not willingly speak of their failures and that, if they should speak of them, they would have to count more defeats than victories.

That day, M. LeRoy evidently overreached himself. If science did not succeed, it could not serve as rule of action; whence would it get its value? Because it is 'lived,' that is, because we love it and believe in it? The alchemists had recipes for making gold, they loved them and had faith in them, and yet our recipes are the good ones, although our faith be less lively, because they succeed.

There is no escape from this dilemma; either science does not enable us to foresee, and then it is valueless as rule of action; or else it enables us to foresee in a fashion more or less imperfect, and then it is not without value as means of knowledge.

It should not even be said that action is the goal of science; should we condemn studies of the star Sirius, under pretext that we shall probably never exercise any influence on that star? To my eyes, on the contrary, it is the knowledge which is the end, and the action which is the means. If I felicitate myself on the industrial development, it is not alone because it furnishes a facile argument to the advocates of science; it is above all because it gives to the scientist faith in himself and also because it offers an immense field of experience where clash forces too colossal to be interfered with. Without this ballast, who knows whether it would not quit the earth, seduced by the mirage of some scholastic novelty, or whether it would not despair, believing it had fashioned only a dream?

3. *The Crude Fact and the Scientific Fact*

What was most paradoxical in M. LeRoy's thesis was that affirmation that *the scientist creates the fact;* this was at the same time its essential point and it is one of those which have been most discussed.

Perhaps, says he (I well believe that this was a concession), it is not the scientist that creates the fact in the rough; it is at least he who creates the scientific fact.

This distinction between the fact in the rough and the scientific fact does not by itself appear to me illegitimate. But I complain first that the boundary has not been traced either exactly or precisely; and then that the author has seemed to suppose that the crude fact, not being scientific, is outside of science.

Finally, I can not admit that the scientist creates without restraint the scientific fact since it is the crude fact which imposes it upon him.

The examples given by M. LeRoy have greatly astonished me. The first is taken from the notion of atom. The atom chosen as example of fact! I avow that this choice has so disconcerted me that I prefer to say nothing about it. I have evidently misunderstood the author's thought and I could not fruitfully discuss it.

The second case taken as example is that of an eclipse where the crude phenomenon is a play of light and shadow, but where the astronomer can not intervene without introducing two foreign elements, to wit, a clock and Newton's law.

Finally, M. LeRoy cites the rotation of the earth; it has been answered: but this is not a fact, and he has replied: it was one for Galileo, who affirmed it, as for the inquisitor, who denied it. It always remains that this is not a fact in the same sense as those just spoken of and that to give them the same name is to expose one's self to many confusions.

Here then are four degrees:

1°. It grows dark, says the clown.

2°. The eclipse happened at nine o'clock, says the astronomer.

3°. The eclipse happened at the time deducible from the tables constructed according to Newton's law, says he again.

4°. That results from the earth's turning around the sun, says Galileo finally.

Where then is the boundary between the fact in the rough and the scientific fact? To read M. LeRoy one would believe that it is between the first and the second stage, but who does not see that there is a greater distance from the second to the third, and still more from the third to the fourth.

Allow me to cite two examples which perhaps will enlighten us a little.

I observe the deviation of a galvanometer by the aid of a movable mirror which projects a luminous image or spot on a divided scale. The crude fact is this: I see the spot displace itself on the scale, and the scientific fact is this: a current passes in the circuit.

Or again: when I make an experiment I should subject the result to certain corrections, because I know I must have made errors. These errors are of two kinds, some are accidental and these I shall correct by taking the mean; the others are systematic and I shall be able to correct those only by a thorough study of their causes. The first result obtained is then the fact in the rough, while the scientific fact is the final result after the finished corrections.

Reflecting on this latter example, we are led to subdivide our second stage, and in place of saying:

2. The eclipse happened at nine o'clock, we shall say:

2*a*. The eclipse happened when my clock pointed to nine, and

2*b*. My clock being ten minutes slow, the eclipse happened at ten minutes past nine.

And this is not all: the first stage also should be subdivided, and not between these two subdivisions will be the least distance; it is necessary to distinguish between the impression of obscurity felt by one witnessing an eclipse, and the affirmation; it grows dark, which this impression extorts from him. In a sense it is the first which is the only true fact in the rough, and the second is already a sort of scientific fact.

Now then our scale has six stages, and even though there is no reason for halting at this figure, there we shall stop.

What strikes me at the start is this. At the first of our six stages, the fact, still completely in the rough, is, so to speak, individual, it is completely distinct from all other possible facts. From the second stage, already it is no longer the same. The enunciation of the fact would suit an infinity of other facts. So soon as language intervenes, I have at my command only a finite number of terms to express the shades, in number infinite, that my impressions might cover. When I say: It grows dark, that well expresses the impressions I feel in being present at an eclipse; but even in obscurity a multitude of shades could be imagined, and if, instead of that actually realized, had happened a slightly different shade, yet I should still have enunciated this *other* fact by saying: It grows dark.

Second remark: even at the second stage, the enunciation of a fact can only be *true or false*. This is not so of any proposition; if this proposition is the enunciation of a convention, it can not be said that this enunciation is *true*, in the proper sense of the word, since it could not be true apart from me and is true only because I wish it to be.

When, for instance, I say the unit for length is the meter, this is a decree that I promulgate, it is not something ascertained which forces itself upon me. It is the same, as I think I have elsewhere shown, when it is a question for example of Euclid's postulate.

When I am asked: Is it growing dark? I always know whether I ought to reply yes or no. Although an infinity of possible facts may be susceptible of this same enunciation: it grows dark, I shall always know whether the fact realized belongs or does not belong among those which answer to this enunciation. Facts are classed in categories, and if I am asked whether the fact that I ascertain belongs or does not belong in such a category, I shall not hesitate.

Doubtless this classification is sufficiently arbitrary to leave a large part to man's freedom or caprice. In a word, this classification is a convention. *This convention being given*, if I am asked: Is such a fact true? I shall always know what to answer, and my reply will be imposed upon me by the witness of my senses.

If, therefore, during an eclipse, it is asked: Is it growing dark? all the world will answer yes. Doubtless those speaking a language where bright was called dark, and dark bright, would answer no. But of what importance is that?

In the same way, in mathematics, *when I have laid down the definitions, and the postulates which are conventions*, a theorem henceforth can only be true or false. But to answer the question: Is this theorem true? it is no longer to the witness of my senses that I shall have recourse, but to reasoning.

A statement of fact is always verifiable, and for the verification we have recourse either to the witness of our senses, or to the memory of this witness. This is properly what characterizes a fact. If you put the question to me: Is such a fact true? I shall begin by asking you, if there is occasion, to state precisely the conventions, by asking you, in other words, what language you have spoken; then once settled on this point, I shall interrogate my senses and shall answer yes or no. But it will be my senses that will have made answer, it will not be *you* when you say to me: I have spoken to you in English or in French.

Is there something to change in all that when we pass to the following stages? When I observe a galvanometer, as I have just said, if I ask an ignorant visitor: Is the current passing? he looks at the wire to try to see something pass; but if I put the same question to my assistant who understands my language, he will know I mean: Does the spot move? and he will look at the scale.

What difference is there then between the statement of a fact in the rough and the statement of a scientific fact? The same difference as between the statement of the same crude fact in French and in German. The scientific statement is the translation of the crude statement into a language which is distinguished above all from the common German or French, because it is spoken by a very much smaller number of people.

Yet let us not go too fast. To measure a current I may use a very great number of types of galvanometers or besides an electrodynamometer. And then when I shall say there is running in this circuit a current of so many amperes, that will mean: if I adapt to this circuit such a galvanometer I shall see the spot come to the division a; but that will mean equally: if I adapt to this circuit such an electrodynamometer, I shall see the spot go to the division b. And that will mean still many other things, because the current can manifest itself not only by mechanical effects, but by effects chemical, thermal, luminous, etc.

Here then is one same statement which suits a very great number of facts absolutely different. Why? It is because I assume a law according to which, whenever such a mechanical effect shall happen, such a chemical effect will happen also. Previous experiments, very numerous, have never shown this law to fail, and then I have understood that I could express by the same statement two facts so invariably bound one to the other.

When I am asked: Is the current passing? I can understand that that means: Will such a mechanical effect happen? But I can understand also: Will such a chemical effect happen? I shall then verify either the existence of the mechanical effect, or that of the chemical effect; that will be indifferent, since in both cases the answer must be the same.

And if the law should one day be found false? If it was perceived that the concordance of the two effects, mechanical and chemical, is not constant? That day it would be necessary to change the scientific language to free it from a grave ambiguity.

And after that? Is it thought that ordinary language by aid of which are expressed the facts of daily life is exempt from ambiguity?

Shall we thence conclude that the facts of daily life are the work of the grammarians?

You ask me: Is there a current? I try whether the mechanical effect exists, I ascertain it and I answer: Yes, there is a current. You understand at once that that means that the mechanical effect exists, and that the chemical effect, that I have not investigated, exists likewise. Imagine now, supposing an impossibility, the law we believe true, not to be, and the chemical effect not to exist. Under this hypothesis there will be two distinct facts, the one directly observed and which is true, the other inferred and which is false. It may strictly be said that we have created the second. So that error is the part of man's personal collaboration in the creation of the scientific fact.

But if we can say that the fact in question is false, is this not just because it is not a free and arbitrary creation of our mind, a disguised convention, in which case it would be neither true nor false. And in fact it was verifiable; I had not made the verification, but I could have made it. If I answered amiss, it was because I chose to reply too quickly, without having asked nature, who alone knew the secret.

When, after an experiment, I correct the accidental and systematic errors to bring out the scientific fact, the case is the same; the scientific fact will never be anything but the crude fact translated into another language. When I shall say: It is such an hour, that will be a short way of saying: There is such a relation between the hour indicated by my clock, and the hour it marked at the moment of the passing of such a star and such another star across the meridian. And this convention of language once adopted, when I shall be asked: Is it such an hour? it will not depend upon me to answer yes or no.

Let us pass to the stage before the last: the eclipse happened at the hour given by the tables deduced from Newton's laws. This is still a convention of language which is perfectly clear for those who know celestial mechanics or simply for those who have the tables calculated by the astronomers. I am asked: Did the eclipse happen at the hour predicted? I look in the nautical almanac, I see that the eclipse was announced for nine o'clock and I understand that the question means: Did the eclipse happen at nine o'clock? There still we have nothing to change in our conclusions. *The scientific fact is only the crude fact translated into a convenient language.*

It is true that at the last stage things change. Does the earth rotate? Is this a verifiable fact? Could Galileo and the Grand Inquisitor, to settle the matter, appeal to the witness of their senses? On the contrary, they were in accord about the appearances, and, whatever had been the accumulated experiences, they would have remained in accord with regard to the appearances without ever agreeing on their interpretation. It is just on that account that they were obliged to have recourse to procedures of discussion so unscientific.

This is why I think they did not disagree about a *fact:* we have not the right to give the same name to the rotation of the earth, which was the object of their discussion, and to the facts crude or scientific we have hitherto passed in review.

After what precedes, it seems superfluous to investigate whether the fact in the rough is outside of science, because there can neither be science without scientific fact, nor scientific fact without fact in the rough, since the first is only the translation of the second.

And then, has one the right to say that the scientist creates the scientific fact? First of all, he does not create it from nothing, since he makes it with the fact in the rough. Consequently he does not make it freely and *as he chooses.* However able the worker may be, his freedom is always limited by the properties of the raw material on which he works.

After all, what do you mean when you speak of this free creation of the scientific fact and when you take as example the astronomer who intervenes actively in the phenomenon of the eclipse by bringing his clock? Do you mean: The eclipse happened at nine o'clock; but if the astronomer had wished it to happen at ten, that depended only on him, he had only to advance his clock an hour?

But the astronomer, in perpetrating that bad joke, would evidently have been guilty of an equivocation. When he tells me: The eclipse happened at nine, I understand that nine is the hour deduced from the crude indication of the pendulum by the usual series of corrections. If he has given me solely that crude indication, or if he has made corrections contrary to the habitual rules, he has changed the language agreed upon without forewarning me. If, on the contrary, he took care to forewarn me, I have nothing to complain of, but then it is always the same fact expressed in another language.

In sum, *all the scientist creates in a fact is the language in which he enunciates it.* If he predicts a fact, he will employ this language, and for all those who can speak and understand it, his prediction is free from ambiguity. Moreover, this prediction once made, it evidently does not depend upon him whether it is fulfilled or not.

What then remains of M. LeRoy's thesis? This remains: the scientist intervenes actively in choosing the facts worth observing.

An isolated fact has by itself no interest; it becomes interesting if one has reason to think that it may aid in the prediction of other facts; or better, if, having been predicted, its verification is the confirmation of a law. Who shall choose the facts which, corresponding to these conditions, are worthy the freedom of the city in science? This is the free activity of the scientist.

And that is not all. I have said that the scientific fact is the translation of a crude fact into a certain language; I should add that every scientific fact is formed of many crude facts. This is sufficiently shown by the examples cited above. For instance, for the hour of the eclipse my clock marked the hour α at the instant of the eclipse; it marked the hour β at the moment of the last transit of the meridian of a certain star that we take as origin of right ascensions; it marked the hour γ at the moment of the preceding transit of this same star. There are three distinct facts (still it will be noticed that each of them results itself from two simultaneous facts in the rough; but let us pass this over). In place of that I say: The eclipse happened at the hour $24\ (\alpha-\beta)/(\beta-\gamma)$, and the three facts are combined in a single scientific fact. I have concluded that the three readings α, β, γ made on my clock at three different moments lacked interest and that the only thing interesting was the combination $(\alpha-\beta)/(\beta-\gamma)$ of the three. In this conclusion is found the free activity of my mind.

But I have thus used up my power; I can not make this combination $(\alpha-\beta)/(\beta-\gamma)$ have such a value and not such another, since I can not influence either the value of α, or that of β, or that of γ, which are imposed upon me as crude facts.

In sum, facts are facts, and *if it happens that they satisfy a prediction, this is not an effect of our free activity.* There is no precise frontier between the fact in the rough and the scientific fact; it can only be said that such an enunciation of fact is *more crude* or, on the contrary, *more scientific* than such another.

4. '*Nominalism*' and '*the Universal Invariant*'

If from facts we pass to laws, it is clear that the part of the free activity of the scientist will become much greater. But did not M. LeRoy make it still too great? This is what we are about to examine.

Recall first the examples he has given. When I say: Phosphorus melts at 44°, I think I am enunciating a law; in reality it is just the definition of phosphorus; if one should discover a body which, possessing otherwise all the properties of phosphorus, did not melt at 44°, we should give it another name, that is all, and the law would remain true.

Just so when I say: Heavy bodies falling freely pass over spaces proportional to the squares of the times, I only give the definition of free fall. Whenever the condition shall not be fulfilled, I shall say that the fall is not free, so that the law will never be wrong.

It is clear that if laws were reduced to that, they could not serve in prediction; then they would be good for nothing, either as means of knowledge, or as principle of action.

When I say: Phosphorus melts at 44°, I mean by that: All bodies possessing such or such a property (to wit, all the properties of phosphorus, save fusing-point) fuse at 44°. So understood, my proposition is indeed a law, and this law may be useful to me, because if I meet a body possessing these properties I shall be able to predict that it will fuse at 44°.

Doubtless the law may be found to be false. Then we shall read in the treatises on chemistry: "There are two bodies which chemists long confounded under the name of phosphorus; these two bodies differ only by their points of fusion." That would evidently not be the first time for chemists to attain to the separation of two bodies they were at first not able to distinguish; such, for example, are neodymium and praseodymium, long confounded under the name of didymium.

I do not think the chemists much fear that a like mischance will ever happen to phosphorus. And if, to suppose the impossible, it should happen, the two bodies would probably not have *identically* the same density, *identically* the same specific heat, etc., so that, after having determined with care the density, for instance, one could still foresee the fusion point.

It is, moreover, unimportant; it suffices to remark that there is a law, and that this law, true or false, does not reduce to a tautology.

Will it be said that if we do not know on the earth a body which does not fuse at 44° while having all the other properties of phosphorus, we can not know whether it does not exist on other planets? Doubtless that may be maintained, and it would then be inferred that the law in question, which may serve as a rule of action to us who inhabit the earth, has yet no general value from the point of view of knowledge, and owes its interest only to the chance which has placed us on this globe. This is possible, but, if it were so, the law would be valueless, not because it reduced to a convention, but because it would be false.

The same is true in what concerns the fall of bodies. It would do me no good to have given the name of free fall to falls which happen in conformity with Galileo's law, if I did not know that elsewhere, in such circumstances, the fall will be *probably* free or *approximately* free. That then is a law which may be true or false, but which does not reduce to a convention.

Suppose the astronomers discover that the stars do not exactly obey Newton's law. They will have the choice between two attitudes; they may say that gravitation does not vary exactly as the inverse of the square of the distance, or else they may say that gravitation is not the only force which acts on the stars and that there is in addition a different sort of force.

In the second case, Newton's law will be considered as the definition of gravitation. This will be the nominalist attitude. The choice between the two attitudes is free, and is made from considerations of convenience, though these considerations are most often so strong that there remains practically little of this freedom.

We can break up this proposition: (1) The stars obey Newton's law, into two others; (2) gravitation obeys Newton's law; (3) gravitation is the only force acting on the stars. In this case proposition (2) is no longer anything but a definition and is beyond the test of experiment; but then it will be on proposition (3) that this check can be exercised. This is indeed necessary, since the resulting proposition (1) predicts verifiable facts in the rough.

It is thanks to these artifices that by an unconscious nominalism the scientists have elevated above the laws what they call principles. When a law has received a sufficient confirmation from experiment, we may adopt two attitudes: either we may leave this law in the fray; it will then remain subjected to an incessant revision, which without any doubt will end by demonstrating that it is only approximative. Or else we may elevate it into a *principle* by adopting conventions such that the proposition may be certainly true. For that the procedure is always the same. The primitive law enunciated a relation between two facts in the rough, A and B; between these two crude facts is introduced an abstract intermediary C, more or less fictitious (such was in the preceding example the impalpable entity, gravitation). And then we have a relation between A and C that we may suppose rigorous and which is the *principle;* and another between C and B which remains a *law* subject to revision.

The principle, henceforth crystallized, so to speak, is no longer subject to the test of experiment. It is not true or false, it is convenient.

Great advantages have often been found in proceeding in that way, but it is clear that if *all* the laws had been transformed into principles *nothing* would be left of science. Every law may be broken up into a principle and a law, but thereby it is very clear that, however far this partition be pushed, there will always remain laws.

Nominalism has therefore limits, and this is what one might fail to recognize if one took to the very letter M. LeRoy's assertions.

A rapid review of the sciences will make us comprehend better what are these limits. The nominalist attitude is justified only when it is convenient; when is it so?

Experiment teaches us relations between bodies; this is the fact in the rough; these relations are extremely complicated. Instead of envisaging directly the relation of the body A and the body B, we introduce between them an intermediary, which is space, and we envisage three distinct relations: that of the body A with the figure A' of space, that of the body B with the figure B' of space, that of the two figures A' and B' to each other. Why is this detour advantageous? Because the relation of A and B was complicated, but differed little from that of A' and B', which is simple; so that this complicated relation may be replaced by the simple relation between A' and B' and by two other relations which tell us that the differences between A and A', on the one hand, between B and B', on the other hand, are *very small*. For example, if A and B are two natural solid bodies which are displaced with slight deformation, we envisage two movable *rigid* figures A' and B'. The laws of the relative displacements of these figures A' and B' will be very simple; they will be those of geometry. And we shall afterwards add that the body A, which always differs very little from A', dilates from the effect of heat and bends from the effect of elasticity. These dilatations and flexions, just because they are very small, will be for our mind relatively easy to study. Just imagine to what complexities of language it would have been necessary to be resigned if we had wished to comprehend in the same enunciation the displacement of the solid, its dilatation and its flexure?

The relation between A and B was a rough law, and was broken up; we now have two laws which express the relations of A and A', of B and B', and a principle which expresses that of A' with B'. It is the aggregate of these principles that is called geometry.

Two other remarks. We have a relation between two bodies A

and B, which we have replaced by a relation between two figures A' and B'; but this same relation between the same two figures A' and B' could just as well have replaced advantageously a relation between two other bodies A'' and B'', entirely different from A and B. And that in many ways. If the principles and geometry had not been invented, after having studied the relation of A and B, it would be necessary to begin again *ab ovo* the study of the relation of A'' and B'' That is why geometry is so precious. A geometrical relation can advantageously replace a relation which, considered in the rough state, should be regarded as mechanical, it can replace another which should be regarded as optical, etc.

Yet let no one say: But that proves geometry an experimental science; in separating its principles from laws whence they have been drawn, you artificially separate it itself from the sciences which have given birth to it. The other sciences have likewise principles, but that does not preclude our having to call them experimental.

It must be recognized that it would have been difficult not to make this separation that is pretended to be artificial. We know the rôle that the kinematics of solid bodies has played in the genesis of geometry; should it then be said that geometry is only a branch of experimental kinematics? But the laws of the rectilinear propagation of light have also contributed to the formation of its principles. Must geometry be regarded both as a branch of kinematics and as a branch of optics? I recall besides that our Euclidean space which is the proper object of geometry has been chosen, for reasons of convenience, from among a certain number of types which preexist in our mind and which are called groups.

If we pass to mechanics, we still see great principles whose origin is analogous, and, as their 'radius of action,' so to speak, is smaller, there is no longer reason to separate them from mechanics proper and to regard this science as deductive.

In physics, finally, the rôle of the principles is still more diminished. And in fact they are only introduced when it is of advantage. Now they are advantageous precisely because they are few, since each of them very nearly replaces a great number of laws. Therefore it is not of interest to multiply them. Besides an outcome is necessary, and for that it is needful to end by leaving abstraction to take hold of reality.

Such are the limits of nominalism, and they are narrow.

M. LeRoy has insisted, however, and he has put the question under another form.

Since the enunciation of our laws may vary with the conventions that we adopt, since these conventions may modify even the natural relations of these laws, is there in the manifold of these laws something independent of these conventions and which may, so to speak, play the rôle of *universal invariant?* For instance, the fiction has been introduced of beings who, having been educated in a world different from ours, would have been led to create a non-Euclidean geometry. If these beings were afterward suddenly transported into our world, they would observe the same laws as we, but they would enunciate them in an entirely different way. In truth there would still be something in common between the two enunciations, but this is because these beings do not yet differ enough from us. Beings still more strange may be imagined, and the part common to the two systems of enunciations will shrink more and more. Will it thus shrink in convergence toward zero, or will there remain an irreducible residue which will then be the universal invariant sought?

The question calls for precise statement. Is it desired that this common part of the enunciations be expressible in words? It is clear then that there are not words common to all languages, and we can not pretend to construct I know not what universal invariant which should be understood both by us and by the fictitious non-Euclidean geometers of whom I have just spoken; no more than we can construct a phrase which can be understood both by Germans who do not understand French and by French who do not understand German. But we have fixed rules which permit us to translate the French enunciations into German, and inversely. It is for that that grammars and dictionaries have been made. There are also fixed rules for translating the Euclidean language into the non-Euclidean language, or, if there are not, they could be made.

And even if there were neither interpreter nor dictionary, if the Germans and the French, after having lived centuries in separate worlds, found themselves all at once in contact, do you think there would be nothing in common between the science of the German books and that of the French books? The French and the Germans would certainly end by understanding each other, as the American Indians ended by understanding the language of their conquerors after the arrival of the Spanish.

But, it will be said, doubtless the French would be capable of understanding the Germans even without having learned German, but this is because there remains between the French and the Germans something in common, since both are men. We should still attain to an understanding with our hypothetical non-Euclideans, though they be not men, because they would still retain something human. But in any case a minimum of humanity is necessary.

This is possible, but I shall observe first that this little humanness which would remain in the non-Euclideans would suffice not only to make possible the translation of *a little* of their language, but to make possible the translation of *all* their language.

Now, that there must be a minimum is what I concede; suppose there exists I know not what fluid which penetrates between the molecules of our matter, without having any action on it and without being subject to any action coming from it. Suppose beings sensible to the influence of this fluid and insensible to that of our matter. It is clear that the science of these beings would differ absolutely from ours and that it would be idle to seek an 'invariant' common to these two sciences. Or again, if these beings rejected our logic and did not admit, for instance, the principle of contradiction.

But truly I think it without interest to examine such hypotheses.

And then, if we do not push whimsicality so far, if we introduce only fictitious beings having senses analogous to ours and sensible to the same impressions, and moreover admitting the principles of our logic, we shall then be able to conclude that their language, however different from ours it may be, would always be capable of translation. Now the possibility of translation implies the existence of an invariant. To translate is precisely to disengage this invariant. Thus, to decipher a cryptogram is to seek what in this document remains invariant, when the letters are permuted.

What now is the nature of this invariant it is easy to understand, and a word will suffice us. The invariant laws are the relations between the crude facts, while the relations between the 'scientific facts' remain always dependent on certain conventions.

CHAPTER XI

Science and Reality

5. *Contingence and Determinism* what a poor translation

I do not intend to treat here the question of the contingence of the laws of nature, which is evidently insoluble, and on which so much has already been written. I only wish to call attention to what different meanings have been given to this word, contingence, and how advantageous it would be to distinguish them.

If we look at any particular law, we may be certain in advance that it can only be approximative. It is, in fact, deduced from experimental verifications, and these verifications were and could be only approximate. We should always expect that more precise measurements will oblige us to add new terms to our formulas; this is what has happened, for instance, in the case of Marriotte's law.

Moreover the statement of any law is necessarily incomplete. This enunciation should comprise the enumeration of *all* the antecedents in virtue of which a given consequent can happen. I should first describe *all* the conditions of the experiment to be made and the law would then be stated: If all the conditions are fulfilled, the phenomenon will happen.

But we shall be sure of not having forgotten *any* of these conditions only when we shall have described the state of the entire universe at the instant t; all the parts of this universe may, in fact, exercise an influence more or less great on the phenomenon which must happen at the instant $t + dt$. = delirium tremens .

Now it is clear that such a description could not be found in the enunciation of the law; besides, if it were made, the law would become incapable of application; if one required so many conditions, there would be very little chance of their ever being all realized at any moment.

Then as one can never be certain of not having forgotten some essential condition, it can not be said: If such and such conditions are realized, such a phenomenon will occur; it can only be said: If such and such conditions are realized, it is probable that such a phenomenon will occur, very nearly.

Take the law of gravitation, which is the least imperfect of all known laws. It enables us to foresee the motions of the planets. When I use it, for instance, to calculate the orbit of Saturn, I neglect the action of the stars, and in doing so, I am certain of not deceiving myself, because I know that these stars are too far away for their action to be sensible.

B.S.

I announce, then, with a quasi-certitude that the coordinates of Saturn at such an hour will be comprised between such and such limits. Yet is that certitude absolute? Could there not exist in the universe some gigantic mass, much greater than that of all the known stars and whose action could make itself felt at great distances? That mass might be animated by a colossal velocity, and after having circulated from all time at such distances that its influence had remained hitherto insensible to us, it might come all at once to pass near us. Surely it would produce in our solar system enormous perturbations that we could not have foreseen. All that can be said is that such an event is wholly improbable, and then, instead of saying: Saturn will be near such a point of the heavens, we must limit ourselves to saying: Saturn will probably be near such a point of the heavens. Although this probability may be practically equivalent to certainty, it is only a probability.

For all these reasons, no particular law will ever be more than approximate and probable. Scientists have never failed to recognize this truth; only they believe, right or wrong, that every law may be replaced by another closer and more probable, that this new law will itself be only provisional, but that the same movement can continue indefinitely, so that science in progressing will possess laws more and more probable, that the approximation will end by differing as little as you choose from exactitude and the probability from certitude.

If the scientists who think thus were right, must it still be said that *the* laws of nature are contingent, even though *each* law, taken in particular, may be qualified as contingent? Or must one require, before concluding the contingence *of the* natural laws, that this progress have an end, that the scientist finish some day by being arrested in his search for a closer and closer approximation and that, beyond a certain limit, he thereafter meet in nature only caprice?

In the conception of which I have just spoken (and which I shall call the scientific conception), every law is only a statement, imperfect and provisional, but it must one day be replaced by another, a superior law, of which it is only a crude image. No place therefore remains for the intervention of a free will.

It seems to me that the kinetic theory of gases will furnish us a striking example.

You know that in this theory all the properties of gases are explained by a simple hypothesis; it is supposed that all the gaseous molecules move in every direction with great velocities and that they follow rectilineal paths which are disturbed only when one molecule passes very near the sides of the vessel or another molecule. The effects our crude senses enable us to observe are the mean effects, and in these means, the great deviations compensate, or at least it is very improbable that they do not compensate; so that the observable phenomena follow simple laws such as that of Mariotte or of Gay-Lussac. But this compensation of deviations is only probable. The molecules incessantly change place and in these continual displacements the figures they form pass successively through all possible combinations. Singly these combinations are very numerous; almost all are in conformity with Marictte's law, only a few deviate from it. These also will happen, only it would be necessary to wait a long time for them. If a gas were observed during a sufficiently long time, it would certainly be finally seen to deviate, for a very short time, from Mariotte's law. How long would it be necessary to wait? If it were desired to calculate the probable number of years, it would be found that this number is so great that to write only the number of places of figures employed would still require half a score places of figures. No matter; enough that it may be done.

I do not care to discuss here the value of this theory. It is evident that if it be adopted, Mariotte's law will thereafter appear only as contingent, since a day will come when it will not be true. And yet, think you the partisans of the kinetic theory are adversaries of determinism? Far from it; they are the most ultra of mechanists. Their molecules follow rigid paths, from which they depart only under the influence of forces which vary with the distance, following a perfectly determinate law. There remains in their system not the smallest place either for freedom, or for an evolutionary factor, properly so-called, or for anything whatever that could be called contingence. I add, to avoid mistake, that neither is there any evolution of Mariotte's law itself; it ceases to be true after I know not how many centuries; but at the end of a fraction of a second it again becomes true and that for an incalculable number of centuries.

And since I have pronounced the word evolution, let us clear away another mistake. It is often said: Who knows whether the laws do not evolve and whether we shall not one day discover that they were not at the Carboniferous epoch what they are to-day? What are we to understand by that? What we think we know about the past state of our globe, we deduce from its present state. And how is this deduction made? It is by means of laws supposed known. The law being a relation between the antecedent and the consequent, enables us equally well to deduce the consequent from the antecedent, that is, to foresee the future, and to deduce the antecedent from the consequent, that is, to conclude from the present to the past. The astronomer who knows the present situation of the stars can from it deduce their future situation by Newton's law, and this is what he does when he constructs ephemerides; and he can equally deduce from it their past situation. The calculations he thus can make can not teach him that Newton's law will cease to be true in the future, since this law is precisely his point of departure; not more can they tell him it was not true in the past. Still in what concerns the future, his ephemerides can one day be tested and our descendants will perhaps recognize that they were false. But in what concerns the past, the geologic past which had no witnesses, the results of his calculation, like those of all speculations where we seek to deduce the past from the present, escape by their very nature every species of test. So that if the laws of nature were not the same in the Carboniferous age as at the present epoch, we shall never be able to know it, since we can know nothing of this age only what we deduce from the hypothesis of the permanence of these laws.

Perhaps it will be said that this hypothesis might lead to contradictory results and that we shall be obliged to abandon it. Thus, in what concerns the origin of life, we may conclude that there have always been living beings, since the present world shows us always life springing from life; and we may also conclude that there have not always been, since the application of the existent laws of physics to the present state of our globe teaches us that there was a time when this globe was so warm that life on it was impossible. But contradictions of this sort can always be removed in two ways; it may be supposed that the actual laws of nature are not exactly what we have assumed; or else it may be supposed that the laws of nature actually are what we have assumed, but that it has not always been so.

It is evident that the actual laws will never be sufficiently well known for us not to be able to adopt the first of these two solutions and for us to be constrained to infer the evolution of natural laws.

On the other hand, suppose such an evolution; assume, if you wish, that humanity lasts sufficiently long for this evolution to have witnesses. The *same* antecedent shall produce, for instance, different consequents at the Carboniferous epoch and at the Quaternary. That evidently means that the antecedents are closely alike; if all the circumstances were identical, the Carboniferous epoch would be indistinguishable from the Quaternary. Evidently this is not what is supposed. What remains is that such antecedent, accompanied by such accessory circumstance, produces such consequent; and that the same antecedent, accompanied by such other accessory circumstance, produces such other consequent. Time does not enter into the affair.

The law, such as ill-informed science would have stated it, and which would have affirmed that this antecedent always produces this consequent, without taking account of the accessory circumstances, this law, which was only approximate and probable, must be replaced by another law more approximate and more probable, which brings in these accessory circumstances. We always come back, therefore, to that same process which we have analyzed above, and if humanity should discover something of this sort, it would not say that it is the laws which have evoluted, but the circumstances which have changed.

Quaint

Here, therefore, are several different senses of the word contingence. M. LeRoy retains them all and he does not sufficiently distinguish them, but he introduces a new one. Experimental laws are only approximate, and if some appear to us as exact, it is because we have artificially transformed them into what I have above called a principle. We have made this transformation freely, and as the caprice which has determined us to make it is something eminently contingent, we have communicated this contingence to the law itself. It is in this sense that we have the right to say that determinism supposes freedom, since it is freely that we become determinists. Perhaps it will be found that this is to give large scope to nominalism and that the introduction of this new sense of the word contingence will not help much to solve all those questions which naturally arise and of which we have just been speaking.

I do not at all wish to investigate here the foundations of the principle of induction; I know very well that I shall not succeed; it is as difficult to justify this principle as to get on without it. I only wish to show how scientists apply it and are forced to apply it.

When the same antecedent recurs, the same consequent must likewise recur; such is the ordinary statement. But reduced to these terms this principle could be of no use. For one to be able to say that the same antecedent recurred, it would be necessary for the circumstances *all* to be reproduced, since no one is absolutely indifferent, and for them to be *exactly* reproduced. And, as that will never happen, the principle can have no application.

We should therefore modify the enunciation and say: If an antecedent A has once produced a consequent B, an antecedent A', slightly different from A, will produce a consequent B', slightly different from B. But how shall we recognize that the antecedents A and A' are 'slightly different'? If some one of the circumstances can be expressed by a number, and this number has in the two cases values very near together, the sense of the phrase "slightly different" is relatively clear; the principle then signifies that the consequent is a continuous function of the antecedent. And as a practical rule, we reach this conclusion that we have the right to interpolate. This is in fact what scientists do every day, and without interpolation all science would be impossible.

Yet observe one thing. The law sought may be represented by a curve. Experiment has taught us certain points of this curve. In virtue of the principle we have just stated, we believe these points may be connected by a continuous graph. We trace this graph with the eye. New experiments will furnish us new points of the curve. If these points are outside of the graph traced in advance, we shall have to modify our curve, but not to abandon our principle. Through any points, however numerous they may be, a continuous curve may always be passed. Doubtless, if this curve is too capricious, we shall be shocked (and we shall even suspect errors of experiment), but the principle will not be directly put at fault.

Furthermore, among the circumstances of a phenomenon, there are some that we regard as negligible, and we shall consider A and A' as slightly different if they differ only by these accessory circumstances. For instance, I have ascertained that hydrogen unites with oxygen under the influence of the electric spark, and I am certain that these two gases will unite anew, although the longitude of Jupiter may have changed considerably in the interval. We assume, for instance, that the state of distant bodies can have no sensible influence on terrestrial phenomena, and that seems in fact requisite, but there are cases where the choice of these practically indifferent circumstances admits of more arbitrariness or, if you choose, requires more tact.

One more remark: The principle of induction would be inapplicable if there did not exist in nature a great quantity of bodies like one another, or almost alike, and if we could not infer, for instance, from one bit of phosphorus to another bit of phosphorus.

If we reflect on these considerations, the problem of determinism and of contingence will appear to us in a new light.

Suppose we were able to embrace the series of all phenomena of the universe in the whole sequence of time. We could envisage what might be called the *sequences,* I mean relations between antecedent and consequent. I do not wish to speak of constant relations or laws, I envisage separately (individually, so to speak) the different sequences realized.

We should then recognize that among these sequences there are no two altogether alike. But, if the principle of induction, as we have just stated it, is true, there will be those almost alike and that can be classed alongside one another. In other words, it is possible to make a classification of sequences.

It is to the possibility and the legitimacy of such a classification that determinism, in the end, reduces. This is all that the preceding analysis leaves of it. Perhaps under this modest form it will seem less appalling to the moralist.

It will doubtless be said that this is to come back by a detour to M. LeRoy's conclusion which a moment ago we seemed to reject: we are determinists voluntarily. And in fact all classification supposes the active intervention of the classifier. I agree that this may be maintained, but it seems to me that this detour will not have been useless and will have contributed to enlighten us a little.

6. *Objectivity of Science*

I arrive at the question set by the title of this article: What is the objective value of science? And first what should we understand by objectivity?

What guarantees the objectivity of the world in which we live is that this world is common to us with other thinking beings. Through the communications that we have with other men, we receive from them ready-made reasonings; we know that these reasonings do not come from us and at the same time we recognize in them the work of reasonable beings like ourselves. And as these reasonings appear to fit the world of our sensations, we think we may infer that these reasonable beings have seen the same thing as we; thus it is we know we have not been dreaming.

Such, therefore, is the first condition of objectivity; what is objective must be common to many minds and consequently transmissible from one to the other, and as this transmission can only come about by that "discourse" which inspires so much distrust in M. LeRoy, we are even forced to conclude: no discourse, no objectivity.

The sensations of others will be for us a world eternally closed. We have no means of verifying that the sensation I call red is the same as that which my neighbor calls red.

Suppose that a cherry and a red poppy produce on me the sensation A and on him the sensation B and that, on the contrary, a leaf produces on me the sensation B and on him the sensation A. It is clear we shall never know anything about it; since I shall call red the sensation A and green the sensation B, while he will call the first green and the second red. In compensation, what we shall be able to ascertain is that, for him as for me, the cherry and the red poppy produce the *same* sensation, since he gives the same name to the sensations he feels and I do the same.

Sensations are therefore intransmissible, or rather all that is pure quality in them is intransmissible and forever impenetrable. But it is not the same with relations between these sensations.

From this point of view, all that is objective is devoid of all quality and is only pure relation. Certes, I shall not go so far as to say that objectivity is only pure quantity (this would be to particularize too far the nature of the relations in question), but we understand how some one could have been carried away into saying that the world is only a differential equation.

With due reserve regarding this paradoxical proposition, we must nevertheless admit that nothing is objective which is not transmissible, and consequently that the relations between the sensations can alone have an objective value.

Perhaps it will be said that the esthetic emotion, which is common to all mankind, is proof that the qualities of our sensations are also the same for all men and hence are objective. But if we think about this, we shall see that the proof is not complete; what is proved is that this emotion is aroused in John as in James by the sensations to which James and John give the same name or by the corresponding combinations of these sensations; either because this emotion is associated in John with the sensation A, which John calls red, while parallelly it is associated in James with the sensation B, which James calls red; or better because this emotion is aroused, not by the qualities themselves of the sensations, but by the harmonious combination of their relations of which we undergo the unconscious impression.

Such a sensation is beautiful, not because it possesses such a quality, but because it occupies such a place in the woof of our associations of ideas, so that it can not be excited without putting in motion the 'receiver' which is at the other end of the thread and which corresponds to the artistic emotion.

Whether we take the moral, the esthetic or the scientific point of view, it is always the same thing. Nothing is objective except what is identical for all; now we can only speak of such an identity if a comparison is possible, and can be translated into a 'money of exchange' capable of transmission from one mind to another. Nothing, therefore, will have objective value except what is transmissible by 'discourse,' that is, intelligible.

But this is only one side of the question. An absolutely disordered aggregate could not have objective value since it would be unintelligible, but no more can a well-ordered assemblage have it, if it does not correspond to sensations really experienced. It seems to me superfluous to recall this condition, and I should not have dreamed of it, if it had not lately been maintained that physics is not an experimental science. Although this opinion has no chance of being adopted either by physicists or by philosophers, it is well to be warned so as not to let oneself slip over the declivity which would lead thither. Two conditions are therefore to be fulfilled, and if the first separates reality[2] from the dream, the second distinguishes it from the romance.

Now what is science? I have explained in the preceding article, it is before all a classification, a manner of bringing together facts which appearances separate, though they were bound together by some natural and hidden kinship. Science, in other words, is a system of relations. Now we have just said, it is in the relations alone that objectivity must be sought; it would be vain to seek it in beings considered as isolated from one another.

To say that science can not have objective value since it teaches us only relations, this is to reason backwards, since, precisely, it is relations alone which can be regarded as objective.

External objects, for instance, for which the word *object* was invented, are really *objects* and not fleeting and fugitive appearances,

[2] I here use the word real as a synonym of objective; I thus conform to common usage; perhaps I am wrong, our dreams are real, but they are not objective.

because they are not only groups of sensations, but groups cemented by a constant bond. It is this bond, and this bond alone, which is the object in itself, and this bond is a relation.

Therefore, when we ask what is the objective value of science, that does not mean: Does science teach us the true nature of things? but it means: Does it teach us the true relations of things?

To the first question, no one would hesitate to reply, no; but I think we may go farther; not only science can not teach us the nature of things; but nothing is capable of teaching it to us and if any god knew it, he could not find words to express it. Not only can we not divine the response, but if it were given to us, we could understand nothing of it; I ask myself even whether we really understand the question.

When, therefore, a scientific theory pretends to teach us what heat is, or what is electricity, or life, it is condemned beforehand; all it can give us is only a crude image. It is, therefore, provisional and crumbling.

The first question being out of reason, the second remains. Can science teach us the true relations of things? What it joins together should that be put asunder, what it puts asunder should that be joined together?

To understand the meaning of this new question, it is needful to refer to what was said above on the conditions of objectivity. Have these relations an objective value? That means: Are these relations the same for all? Will they still be the same for those who shall come after us?

It is clear that they are not the same for the scientist and the ignorant person. But that is unimportant, because if the ignorant person does not see them all at once, the scientist may succeed in making him see them by a series of experiments and reasonings. The thing essential is that there are points on which all those acquainted with the experiments made can reach accord.

The question is to know whether this accord will be durable and whether it will persist for our successors. It may be asked whether the unions that the science of to-day makes will be confirmed by the science of to-morrow. To affirm that it will be so we can not invoke any *a priori* reason; but this is a question of fact, and science has already lived long enough for us to be able to find out by asking its history whether the edifices it builds stand the test of time, or whether they are only ephemeral constructions.

Now what do we see? At the first blush it seems to us that the theories last only a day and that ruins upon ruins accumulate. To-day the theories are born, to-morrow they are the fashion, the day after to-morrow they are classic, the fourth day they are superannuated, and the fifth they are forgotten. But if we look more closely, we see that what thus succumb are the theories, properly so called, those which pretend to teach us what things are. But there is in them something which usually survives. If one of them has taught us a true relation, this relation is definitively acquired, and it will be found again under a new disguise in the other theories which will successively come to reign in place of the old.

Take only a single example: The theory of the undulations of the ether taught us that light is a motion; to-day fashion favors the electromagnetic theory which teaches us that light is a current. We do not consider whether we could reconcile them and say that light is a current, and that this current is a motion. As it is probable in any case that this motion would not be identical with that which the partisans of the old theory presume, we might think ourselves justified in saying that this old theory is dethroned. And yet something of it remains, since between the hypothetical currents which Maxwell supposes there are the same relations as between the hypothetical motions that Fresnel supposed. There is, therefore, something which remains over and this something is the essential. This it is which explains how we see the present physicists pass without any embarrassment from the language of Fresnel to that of Maxwell. Doubtless many connections that were believed well established have been abandoned, but the greatest number remain and it would seem must remain.

And for these, then, what is the measure of their objectivity? Well, it is precisely the same as for our belief in external objects. These latter are real in this, that the sensations they make us feel appear to us as united to each other by I know not what indestructible cement and not by the hazard of a day. In the same way science reveals to us between phenomena other bonds finer but not less solid; these are threads so slender that they long remained unperceived, but once noticed there remains no way of not seeing them; they are therefore not less real than those which give their reality to external objects; small matter that they are more recently known since neither can perish before the other.

It may be said, for instance, that the ether is no less real than any external body; to say this body exists is to say there is between the color of this body, its taste, its smell, an intimate bond, solid and persistent; to say the ether exists is to say there is a natural kinship between all the optical phenomena, and neither of the two propositions has less value than the other.

And the scientific syntheses have in a sense even more reality than those of the ordinary senses, since they embrace more terms and tend to absorb in them the partial syntheses.

It will be said that science is only a classification and that a classification can not be true, but convenient. But it is true that it is convenient, it is true that it is so not only for me, but for all men; it is true that it will remain convenient for our descendants; it is true finally that this can not be by chance.

In sum, the sole objective reality consists in the relations of things whence results ~~the~~ universal harmony. Doubtless these relations, this harmony, could not be conceived outside of a mind which conceives them. But they are nevertheless objective because they are, will become, or will remain, common to all thinking beings.

This will permit us to revert to the question of the rotation of the earth which will give us at the same time a chance to make clear what precedes by an example.

7. *The Rotation of the Earth*

". . . Therefore," have I said in *Science and Hypothesis,* "this affirmation, the earth turns round, has no meaning . . . or rather these two propositions, the earth turns round, and, it is more convenient to suppose that the earth turns round, have one and the same meaning."

These words have given rise to the strangest interpretations. Some have thought they saw in them the rehabilitation of Ptolemy's system, and perhaps the justification of Galileo's condemnation.

Those who had read attentively the whole volume could not, however, delude themselves. This truth, the earth turns round, was put on the same footing as Euclid's postulate, for example. Was that to reject it? But better; in the same language it may very well be said: These two propositions, the external world exists, or, it is more convenient to suppose that it exists, have one and the same meaning. So the hypothesis of the rotation of the earth would have the same degree of certitude as the very existence of external objects.

But after what we have just explained in the fourth part, we may go farther. A physical theory, we have said, is by so much the more true, as it puts in evidence more true relations. In the light of this new principle, let us examine the question which occupies us.

No, there is no absolute space; these two contradictory propositions: 'The earth turns round' and 'The earth does not turn round' are, therefore, neither of them more true than the other. To affirm one while denying the other, *in the kinematic sense,* would be to admit the existence of absolute space.

But if the one reveals true relations that the other hides from us, we can nevertheless regard it as physically more true than the other, since it has a richer content. Now in this regard no doubt is possible.

Behold the apparent diurnal motion of the stars, and the diurnal motion of the other heavenly bodies, and besides, the flattening of the earth, the rotation of Foucault's pendulum, the gyration of cyclones, the trade-winds, what not else? For the Ptolemaist all these phenomena have no bond between them; for the Copernican they are produced by the one same cause. In saying, the earth turns round, I affirm that all these phenomena have an intimate relation, and *that is true,* and that remains true, although there is not and can not be absolute space.

So much for the rotation of the earth upon itself; what shall we say of its revolution around the sun? Here again, we have three phenomena which for the Ptolemaist are absolutely independent and which for the Copernican are referred back to the same origin; they are the apparent displacements of the planets on the celestial sphere, the aberration of the fixed stars, the parallax of these same stars. Is it by chance that all the planets admit an inequality whose period is a year, and that this period is precisely equal to that of aberration, precisely equal besides to that of parallax? To adopt Ptolemy's system is to answer, yes; to adopt that of Copernicus is to answer, no; this is to affirm that there is a bond between the three phenomena and that also is true although there is no absolute space.

In Ptolemy's system, the motions of the heavenly bodies can not be explained by the action of central forces, celestial mechanics is impossible. The intimate relations that celestial mechanics reveals to us between all the celestial phenomena are true relations; to affirm the immobility of the earth would be to deny these relations, that would be to fool ourselves. 3

The truth for which Galileo suffered remains, therefore, the truth, although it has not altogether the same meaning as for the vulgar, and its true meaning is much more subtile, more profound and more rich.

8. *Science for Its Own Sake*

Not against M. LeRoy do I wish to defend science for its own sake; may be this is what he condemns, but this is what he cultivates, since he loves and seeks truth and could not live without it. But I have some thoughts to express.

We can not know all facts and it is necessary to choose those which are worthy of being known. According to Tolstoi, scientists make this choice at random, instead of making it, which would be reasonable, with a view to practical applications. On the contrary, scientists think that certain facts are more interesting than others, because they complete an unfinished harmony, or because they make one foresee a great number of other facts. If they are wrong, if this hierarchy of facts that they implicitly postulate is only an idle illusion, there could be no science for its own sake, and consequently there could be no science. As for me, I believe they are right, and, for example, I have shown above what is the high value of astronomical facts, not because they are capable of practical applications, but because they are the most instructive of all.

It is only through science and art that civilization is of value. Some have wondered at the formula: science for its own sake; and yet it is as good as life for its own sake, if life is only misery; and even as happiness for its own sake, if we do not believe that all pleasures are of the same quality, if we do not wish to admit that the goal of civilization is to furnish alcohol to people who love to drink.

Every act should have an aim. We must suffer, we must work, we must pay for our place at the game, but this is for seeing's sake; or at the very least that others may one day see.

All that is not thought is pure nothingness; since we can think only thought and all the words we use to speak of things can express only thoughts, to say there is something other than thought, is therefore an affirmation which can have no meaning.

And yet—strange contradiction for those who believe in time—geologic history shows us that life is only a short episode between two eternities of death, and that, even in this episode, conscious thought has lasted and will last only a moment. Thought is only a gleam in the midst of a long night.

But it is this gleam which is everything.

PS.

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