HENRI POINCARE

Mathematics and Science: Last Essays

(Dernières Pensées)

Translated from the French by

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TRANSLATOR'S NOTE

Just as the poet must seek the appropriate word to convey an idea with sufficient vigor and yet obtain the cadence and rhyme necessary for the finished product, so also the translator must achieve the proper expression in one language to convey accurately and with equal vigor the author's ideas as expressed in his original language. In this process the languages—in the translator's mind—tend to lose their identity and one language easily assumes the idiosyncrasies of the other.

I am therefore particularly grateful to Dr. Wallace L. Goldstein for his assistance in indicating flaws in grammatical constructions which would have resulted from the merging of the two languages. His help was equally important in proof-reading the manuscript and in the preparation of the index. Any errors in the final product, however, are my own.

J. W. B.

FOREWORD

Under the title of *Last Essays* are gathered the various articles and lectures which Mr. Henri Poincaré himself had intended should form the fourth volume of his writings on the philosophy of science. All previous essays and articles had already been included in that series.

It would be superfluous to point to the amazing success of the first three volumes. In these Poincaré, as the most illustrious modern mathematician, proved to be an eminent philosopher and an author whose writings profoundly influence human thought.

It is very likely that if Henri Poincaré had published this volume himself, he would have modified certain details, and eliminated some repetitions. But it seemed to us that the respect due to the memory of this great man should forbid any editing of his text.

It seemed equally superfluous to preface this volume with commentaries on the works of Henri Poincaré. These have been evaluated by scholars and any commentary could not possibly increase the glory of this great genius.

G. L. B.

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Chapter I

THE EVOLUTION OF LAWS

Mr. Boutroux, in his writings on the contingency of the laws of Nature, queried, whether natural laws are not susceptible to change and if the world evolves continuously, whether the laws themselves which govern this evolution are alone exempt from all variation. Such a concept has no chance of ever being accepted by the scientist; in the sense in which he would understand it, the scientist could not accept it without denying the justness and the very possibility of science. But the philosopher reserves the right to ask such a question, to consider the various solutions which it entails, to examine their consequences, and to try to reconcile them to the reasonable demands of the scientist. I should like to consider a few of the aspects which the problem can assume. I shall thus arrive not at so-called conclusions, but at various reflections which may perhaps not be devoid of interest. If, in the process, I take the liberty of considering somewhat at length certain related questions, I hope that the reader will bear with me.

I

First, let us assume the point of view of the mathematician. Let us grant for a moment that the physical laws have undergone variations during the course of the ages, and let us ask ourselves whether we would have the means of noticing these variations. Let us not forget, first of all, that the few centuries during which man has lived and thought were preceded by periods of incomparably longer duration when man did not exist as yet; they will no doubt be followed by eras when the human species will have ceased to exist. If we wish to believe in an evolution of laws, unquestionably it can only have been very slow, so that, during the few years that man has been able to reason, the laws of nature could have undergone only insignificant changes. If the laws did evolve in the past, we must understand by that the geological past. Were the laws of former eras those of today? Will the laws of tomorrow still be the same? When we ask such a question, what meaning must we attach to the terms "formerly," "today," and "tomorrow?" By "today" we mean those periods which are recorded in history; by "formerly" we mean the millions of years which preceded recorded history and during which the ichthyosauri lived quietly without philosophizing; "tomorrow" means the millions of years which will follow, during which the earth will cool and man will have neither eyes to see nor brain with which to think.

With this in mind, what is a law? It is a constant link between the antecedent and the consequent, between the current state of the world and the immediately succeeding state. Knowing the current state of each part of the universe, the ideal scientist who would know all the laws of nature would possess fixed rules by which to deduce the state in which these same parts shall be tomorrow. It is conceivable that this process could be carried on indefinitely. Knowing the state of the world on Monday, we can foretell its state on Tuesday; knowing that of Tuesday, we can deduce its state on Wednesday by the same process; and so on. But this is not all; if there is a constant link between the state of the world on Monday and that of Tuesday, it is possible to infer the second from the first. But it will also be possible to perform the reverse; that is, if the state of the world that exists on Tuesday is known, it will be possible to infer that of Monday; from the state on Monday we shall infer that of Sunday; and so on. It is thus possible to trace the course of time backward as well as forward. Knowing both the present and the laws, we can foretell the future, but we can understand the past as well. The process is essentially reversible.

Since we are assuming at this juncture the point of view of the mathematician, we must give to this concept all the precision that it requires, even if it becomes necessary to use mathematical language. We should then say that the body of laws is equivalent to a system of differential equations which link the speed of variations of the different elements of the universe to the present values of these elements.

Such a system involves, as we know, an infinite number of solutions. But if we take the initial values of all the elements, that is, their values at the instant $t=0$ (which would correspond in ordinary language to the "present"), the solution is completely determined, so that we can calculate the values of all the elements at any period whatever, whether we suppose $t>0$, which corresponds to the "future," or whether we suppose $t<0$, which corresponds to the "past." What is important to remember is that the manner of inferring the past from the present does not differ from that of inferring the future from the present.

What means do we therefore have of knowing the geological past; that is, the history of those former eras during which the laws could have varied? This past could not have been observed directly and we know it only by the traces which it has left in the present. We know it only through the present, and we can draw inferences about the past only by the process which we have just described, and which will permit us in the same manner to draw inferences about the future. But is this process capable of revealing changes in the laws? Obviously not; for we can apply this principle precisely only by supposing that the laws have not changed; we know directly merely the state of the world on Monday, for example, and the rules which link the state on Sunday with that on Monday. The application of these rules will therefore enable us to know the state on Sunday; but if we wish to pursue this further and to deduce the state of the world on Saturday, it is absolutely necessary that we admit that the same rules which permitted us to proceed from Monday to Sunday were still valid between Sunday and Saturday. Without this, the only conclusion which we would be permitted to draw is that it is impossible to know what occurred on Saturday. If then the immutability of the laws plays a part in the premises of all our reasoning processes, it is bound to occur in our conclusions.

Leverrier, knowing the present orbits of the planets, can calculate by means of Newton's law what these orbits will be 10,000 years hence. Whatever method he uses in his calculations, he will never be able to find Newton's law to be false in a few millennia. He could have calculated, simply by changing the sign for the time factor in his formulae, what these orbits were 10,000 years ago. But he is certain beforehand not to discover that Newton's law has not always been true.

In summary, we can know nothing of the past unless we admit that the laws have not changed; if we do admit this, the question of the evolution of the laws is meaningless; if we do not admit this condition, the question is impossible of solution, just as with all questions which relate to the past.

II

But, it may be asked, is it not possible that the application of the process just described may lead to a contradiction, or, if we wish, that our differential equations admit of no solution? Since the hypothesis of the immutability of the laws, posited at the beginning of our argument would lead to an absurd consequence, we would have demonstrated *per absurdum* that laws have changed, while at the same time we would forever be unable to know in what sense.

Since this process is reversible, what we have just said applies to the future as well, and there would seem to be cases in which we would be able to state that before a particular date the world would have to come to an end or change its laws; if, for example, our calculations indicate that on that date one of the quantities which we have to consider is due to become infinite or to assume a value which is physically impossible. To perish or to change its laws is just about the same thing; a world which would no longer have the same laws as ours would no longer be our world but another one.

Is it possible that the study of the present world and of its laws would lead us to formulae liable to such contradictions? Laws are derived through experience; if they teach us that condition A on Sunday leads us to condition B on Monday, it is because we have observed both conditions A and B. It is therefore because neither of these two conditions is physically impossible. If we pursue this process further, and if we end up proceeding each time from one day to the following day, from condition A to condition B, then from condition B to condition C, then from condition C to condition D, etc., it is because all these conditions are physically possible. For, if condition D, for example, were not physically possible, we would never have been able to gain the experience proving that condition C generates condition D at the end of one day. No matter how far the deductions are pursued, we will therefore never reach a condition which is physically impossible, that is, a contradiction. If one of our formulae were not free from contradiction, we would have gone beyond the bounds of experience; we would have extrapolated. Let us suppose for example that we had observed that under a given circumstance the temperature of a body decreased by one degree per day. If its present temperature is 20°C, for instance, we should conclude that in 300 days, the temperature will be −280°C.; and that would be absurd and physically impossible, since absolute zero is −273°C. How can this be explained? Have we observed the temperature falling in one day from −279° to −280°? Of course not, since these two temperatures cannot be observed. We had seen, for example, that the law was valid, at least very nearly so, between the temperatures of 0° and 20°, and we had improperly concluded that it would be equally valid at −273° and even beyond that. We had been guilty of an unwarranted extrapolation. But there is an infinite number of ways to extrapolate an empirical formula, and among these it is always possible to choose one which excludes the states which are physically impossible.

We know the laws only imperfectly. Experience merely limits our choice; and from among all the laws which experience permits us to choose, it is always possible to find some which will not lead us to a contradiction of the kind we have just mentioned and which could force us to conclude that the laws were not immutable. The means by which such an evolution of the laws can be demonstrated still escapes us, whether it concerns demonstrating that the laws will change or that they have changed.

III

At this point, we can be confronted with this factual argument. "You say that in the attempt to argue from the present to the past, made possible by the understanding of the laws, we shall never encounter a contradiction. And yet scientists have encountered such contradictions, which cannot be circumvented quite so easily as you think. I grant that they may merely seem to be contradictions or that we may continue to hope to resolve them; but according to your reasoning, even an apparent contradiction should be impossible."

Let us cite an example immediately. If we calculate according to the laws of thermodynamics the length of time the sun has been able to give off heat, we determine this to be about 50 million years. This length of time would not be enough for the geologist. Not only was it impossible for the evolution of organic life to take place so rapidly—this is a point which we could debate—but the deposit of the strata in which we find remains of plants or animals which could not have thrived without sunlight requires a period of time perhaps ten times longer.

What has made the contradiction possible is that the reasoning on which the geological evidence rests differs very much from that of the mathematician. When we observe identical effects, we infer an identity of causes. For example, when we discover fossils from animals which belonged to a family now living, we conclude that the conditions necessary for these animals to thrive were *all* realized at the same time during the epoch in which the stratum containing these fossils was deposited.

At first sight, that is the same method used by the mathematician, whose point of view we assumed in the preceding sections. He too concluded that, since the laws had not changed, identical effects could only have been produced by identical causes. There is an essential difference nevertheless. Let us consider the state of the world at a given instant and also at an earlier instant. The state of the world, or even the state of a very small part of the world, is something which is extremely complex and which depends upon a very large number of elements. In order to simplify the explanation, I shall assume only two elements, so that two given quantities are sufficient to define this state or condition. At the later instant these known quantities will be, for example, A and B; at the earlier instant A' and B'.

The mathematician's formula, derived from the collection of the empirical laws, teaches him that the state AB could have been generated only by the preceding state $A'B'$. But if he knows only one of the given quantities, such as A, without knowing whether A is accompanied by the other given quantity B, his formula does not permit him to arrive at any conclusion. At most, if the phenomena A and A' seem to him to be related to one another but relatively independent of B and B', he will argue from A to A'; in any case, he will not infer the double condition A' and B' from condition A alone. The geologist, on the contrary, observing effect A alone, will conclude that this effect could have been produced only by the *convergence* of the causes A' and B' which often give rise to it in plain view. For, in many cases, this effect A is so special that any other convergence of causes resulting in the same effect would be absolutely improbable.

If two organisms are identical or merely similar, this similarity cannot be due to chance, and we can affirm that they have existed under similar conditions. Discovering their remains, we shall be certain not only that there existed a seed similar to that from which we see similar beings evolved, but that the exterior temperature was not too great for the seed to develop. Otherwise, these remains could be only a *ludus naturae*, as it was believed during the seventeenth century. Needless to say, such a conclusion is absolutely contrary to reason. The existence of organic remains is furthermore merely an extreme case more striking than the others. We could have confined ourselves to the mineral world and still have cited cases of the same kind.

The geologist can therefore draw conclusions in cases in which the mathematician would be powerless. But we notice that the geologist is no longer assured against contradictions as was the mathematician. If from a sole circumstance he draws conclusions about multiple preceding circumstances, if the scope of the conclusion is in some way more extensive than that of the premises, it is possible that the deduction from a particular observation will not agree with an inference drawn from another. Each isolated fact becomes so to speak a center of irradiation: from each of these the mathematician deduced a single fact; the geologist deduces multiple facts from them. From the given point of light, he derives a luminous disc of greater or lesser extent. Two points of light will then give him two discs which may overlap; hence, the possibility of a conflict. For instance, if he finds in a stratum molluscs which cannot thrive at temperatures less than 20°C., he will conclude that the seas during that era were warm. But if later, one of his colleagues discovered in the same stratum other animals which would be killed by temperatures greater than 5°, he would conclude that these seas were cold.

There can be reasons to hope that the observations will not be contradictions in fact, or that the contradictions will not be unresolvable. But we are no more guaranteed, so to speak, against the risk of a contradiction by the very rules of formal logic. And so we may wonder if, by reasoning as the geologist does, we shall not some day be led to an absurd consequence which will oblige us to admit the mutability of the laws.

IV

Let me digress here for a moment. We have just seen that the geologist possesses an instrument which the mathematician lacks and which permits the geologist to draw conclusions about the past from the present. Why does not the same instrument permit us to make inferences about the future from the present? If I meet a man who is twenty years old, I am sure that he has gone through all the stages from childhood to adulthood and that consequently there has not been on the earth during the past twenty years a cataclysm which has destroyed all forms of life. But this does not prove to me in any way that there shall not be one within the next twenty years. We have means for knowing the past which fail us when the future is concerned, and that is the reason that the future seems to us more mysterious than the past.

I cannot help but refer to an article which I wrote about chance. In it I called attention to the opinion of Mr. Lalande who had said, on the contrary, that if the future is determined by the past, the past is not determined by the future. According to him, one cause can produce only one effect, whereas the same effect can be produced by several different causes. If that were so, the past would be mysterious and the future would be easy to know.

I could not accept this opinion, but I showed what its origin could have been. Carnot's principle tells us that energy, which cannot be destroyed, can be dissipated. Temperatures tend to become uniform, the world tends to uniformity, that is, toward death. Therefore, great differences in the causes produce only slight differences in the effects. As soon as the differences in the effects become too small to be noticed, we no longer have any means of knowing the differences which existed in the past between the causes which gave rise to them, no matter how great these differences might have been.

But it is exactly because all things tend toward death that life is an exception which it is necessary to explain.

Let rolling pebbles be left subject to chance on the side of a mountain, and they will all end by falling into the valley. If we find one of them at the foot, it will be a commonplace effect which will teach us nothing about the previous history of the pebble; we will not be able to know its original position on the mountain. But if, by accident, we find a stone near the summit, we can assert that it has always been there, since, if it had been on the slope, it would have rolled to the very bottom. And we will make this assertion with the greater certainty, the more exceptional the event is and the greater the chances were that the situation would not have occurred.

V

I have raised this question only in passing; it deserves more thought, but I do not wish to stray too far afield. Is it possible that the contradictions of the geologists will ever lead scientists to decide in favor of the evolution of laws? First, let us note that it is only in their first stages that sciences use the method of reasoning by analogy with which present geology must content itself. As sciences develop, they approach the state which astronomy and physics seem to have attained already, in which the laws are capable of being enunciated in mathematical language. In that event, what we said at the beginning of this work will again be considered true unreservedly. But many persons think that all the sciences must undergo more or less rapidly the same evolution, and one after the other. If that were the case, the difficulties which could arise would be only temporary and would be destined to vanish once the sciences had progressed beyond the stage of infancy.

But we need not wait for this uncertain future. Of what does the geologist's method of reasoning by analogy consist? A geological fact seems to him to be so similar to a present fact that he would not think of attributing this similarity to chance. He believes that he can explain this only if he supposes that these two facts were produced under entirely identical conditions. And he would imagine that the conditions were identical, except for this circumstance: if the natural laws had varied in the meantime, the entire world would have changed to the extent of becoming unrecognizable. He would affirm for one thing that the temperature must have remained the same, whereas, as a consequence of the overthrow of all physics, the effects of temperature would have become altogether different, so that even the word temperature would have lost all meaning. Obviously, whatever happens, he will never accept this point of view. The manner in which he views logic is absolutely opposed to it.

VI

And what if mankind should last for a longer time than we supposed, long enough to see the laws change noticeably? Or then again, what if mankind were to acquire instruments sensitive enough for this variation, however slow it might be, to become discernible after a few generations? We would then no longer learn of the changes in the laws by induction and by inference but rather by direct observation. Would not the preceding arguments lose their value completely? The memoirs in which the experiences of our forefathers were recorded would then be nothing but relics of the past which would afford us only an indirect knowledge of this past. Old documents are to the historian what fossils are to the geologist, and the achievements of former scientists would be merely old documents. They would reveal nothing as to the trend of thought of those scientists except to the extent to which people of former periods were similar to us. If the laws of the world were to change, all parts of the universe would feel the repercussions and mankind could not escape the effects. Even if we grant that mankind could thrive in a new surrounding, it would necessarily have to change in order to become adapted to it. And then the language of the people of former ages would become incomprehensible to us; the words which those people used would no longer have any meaning for us or else would have a different meaning for them. Is that not what happens after a few centuries even though the laws of physics have remained immutable?

And then we keep returning to the same dilemma: either the documents of old have remained entirely clear to us and therefore the world will have remained the same, and the documents will not be able to teach us anything different; or else the documents will have become undecipherable riddles and will not be capable of teaching us anything at all, not even that the laws have evolved. We know well enough that much less can occur to make documents become a dead letter.

Moreover, people of old, just as ourselves, would never have had more than a fragmentary knowledge of the natural laws. We would always find some means of linking these two fragments even if they had remained intact; all the more reason, if we are left with only a vague, uncertain, and half-forgotten image of the most ancient fragment.

VII

Let us take another point of view. The laws derived by direct observation are never anything more than resultants. Let us take Mariotte's law as an example. To most physicists, it is merely a consequence of the kinetic theory of gases; the gas molecules move with considerable velocities; they describe complicated trajectories whose exact equations we could write if we knew the laws by which they attract or repel each other. By analyzing these trajectories according to the rules of the calculus of probabilities, we succeed in proving that the density of a gas is proportional to its pressure.

The laws which govern visible bodies would then be merely consequences of the molecular laws.

Their simplicity would then be merely apparent and would conceal an extremely complex reality since its complexity would be measured by the very number of molecules. But it is precisely because this number is very large that the divergences in details would be mutually compensatory and we would therefore believe that there was harmony.

And molecules themselves may be miniature worlds; their laws may also be only resultants and in order to discover the cause, we would have to continue down to the molecules of molecules, never knowing when the process would end.

The observable laws, therefore, depend on two things: the molecular laws and the arrangement of the molecules. It is the molecular laws which enjoy immutability since they are the true laws and since the others are merely apparent laws. But the arrangement of the molecules can change and with it the observable laws. And this would be one reason for believing in the evolution of laws.

VIII

I imagine a world in which the various parts can conduct heat so perfectly that they maintain a constant equilibrium of temperature. The inhabitants of such a world would have no conception of what we call temperature difference; in their treatises on physics there would be no chapter devoted to thermometry. Other than that, these treatises could be fairly complete and would teach many laws, much simpler even than ours.

Now let us imagine that this world cools slowly through radiation; the temperature will remain everywhere uniform, but will diminish with time. I imagine also that one of the inhabitants falls into a state of lethargy and awakens after a few centuries. Let us grant, since we have already assumed so many things, that he is able to live in a cooler world and that he can remember previous experiences. He will notice that his descendants still write treatises on physics, that they still make no mention of thermometry, but that the laws which they teach are very different from those which he knew. For example, he had been told that water boils at a pressure of 10 millimeters of mercury, and the new physicists observe that in order for water to boil the pressure must be decreased to 5 millimeters. A body which he had known in the liquid state will now be found only in the solid state, and so forth. The mutual relations among the various parts of the universe all depend on temperature, and as soon as the temperature changes everything is upset.

Well, do we know whether there is not such a physical entity, as unknown to us as temperature was for the inhabitants of that world of fantasy? Do we know whether this entity does not vary constantly like the temperature of a sphere which loses its heat through radiation, and whether this variation does not bring about a variation of all the laws?

IX

Let us return to our imaginary world and let us ask ourselves whether its inhabitants could not, without repeating the story of the sleepers of Ephesus,* notice this evolution. No doubt, no matter how perfectly heat could be conducted on this planet, the conductivity could not be absolute, and extremely slight temperature differences would still be possible. These would escape observation for a long time but there would probably come a day when more sensitive measuring instruments would be devised and some gifted physicist would reveal evidences of these almost imperceptible differences. A theory would be developed and it would be seen that these differences in temperature influence all physical phenomena. Finally, some philosopher, whose views would seem hazardous and rash to most of his contemporaries, would assert that the mean temperature of the universe could have varied in the past and with it all known laws.

Could we not also do something similar? For example, the fundamental laws of mechanics have long been considered as absolute. Today some physicists say that they should be modified, or rather, be made more inclusive; that they are approximately true only for the velocities to which we are accustomed; that they would cease to be true at velocities comparable to that of light. These physicists base their point of view on certain experiments conducted with radium. The older laws of dynamics are no less actually true in our ordinary physical surroundings. But could we not say with some semblance of logic that as a result of the constant loss of energy the velocities of bodies must have tended to decrease, since their main force tended to be transformed into heat; that by tracing the process far enough into the past, we may discover an era during which velocities comparable to that of light were not exceptional, and when, as a result, the classical laws of dynamics were not yet true?

On the other hand, let us suppose that observable laws are nothing but resultants, dependent both on the molecular laws and on the arrangement of the molecules. When scientific progress has familiarized us with this dependence, we will no doubt be able to infer that precisely by virtue of the molecular laws, the arrangement of the molecules must once have been different from that of today, and consequently that the observable laws were not always the same. We would therefore conclude that the laws are variable, but we must note carefully that this would be by virtue of the principle of their immutability. We would assert that the apparent laws have changed, but only because the molecular laws which we had regarded formerly as the true laws were considered unalterable.

* [Translator's Note: For the significance of this reference to the sleepers of Ephesus, see the Epistle of St. Paul to the Ephesians and "The Seven Sleepers" in Padraic Colum's *The Forge in the Forest*, pp. 295–302 (The Macmillan Company).]

X

Thus there is not a single law which we can enunciate with the certainty that it has always been true in the past with the same approximation as today; in fact, not even with the certainty that we will never be able to demonstrate that it has been false in the past. And yet, there is nothing in this to prevent the scientist from maintaining his belief in the principle of immutability, since no law will ever be relegated to the rank of being transitory, only to be replaced by another law more general and more comprehensive; since that law will owe its disgrace merely to the advent of this new law so that there will have been no interregnum and the principles will remain intact; and since it will be through these principles that the changes will be made and these very changes will seem to be an obvious confirmation of them.

It will not even happen that we shall observe variations either empirically or inductively, nor shall we explain them after their occurrence by trying to fit everything in a more or less artificial synthesis. No, the synthesis will come first, and if we allow any variations, the purpose will be to prevent disturbing it.

XI

Up to this point we did not seem to worry whether the laws really vary but only whether man can consider them variable. Are laws, considered as existing outside the mind that creates or observes them, unalterable *in themselves*? Not only is the question impossible of solution but it is meaningless. What is the use of wondering whether in the world of intrinsic things the laws can vary with time whereas in a similar world the word "time" is perhaps meaningless? What this world consists of, we cannot say nor conjecture; we can only conjecture what it seems, or might seem to be to minds not too different from ours.

So stated, the question admits of a solution. If we imagine two minds similar to ours observing the universe on two occasions differing for example by millions of years, each of these minds will construct a science which will be a system of laws deduced from observed facts. It is probable that these sciences will be very different and in that sense it could be said that the laws have evolved. But however great the difference may be, it will always be possible to conceive of an intellect, of the same nature as ours but of much greater scope or endowed with a much longer life, which will be able to complete the synthesis and to combine in a single and perfectly coherent formula the two fragmentary and related formulae which the two ephemeral researchers had reached in the short time they had at their disposal. To this intellect, the laws will not have changed, science will be unalterable; the scientists will merely have been imperfectly informed.

Taking a comparison from geometry, let us suppose that we can represent the variations of the world by an analytic curve. Each one of us can see only a very small arc of this curve; if we had an exact knowledge of the arc, this would be sufficient to determine the equation of the curve, and to be able to extend it indefinitely. But we have merely a limited knowledge of this arc, and we can be mistaken about this equation. If we try to extend the curve, the graph will deviate from the real curve in inverse proportion to the length of the arc and to the extent to which we wish to extend it. Another observer would know only another arc and would know it only imperfectly also.

If ever these two workers be far from each other, their two extensions of the curve will not meet; but this does not prove that another observer from a more distant vantage point, who could observe directly a greater length of the curve in a manner so as to encompass these two arcs at the same time, would not be in a position to write a more exact equation which would reconcile their divergent formulae. Also, no matter how irregular the real curve may be, there will always be an analytic curve which, when extended as far as we may wish, will deviate from the real curve to as slight an extent as we shall wish.

No doubt many readers will be dismayed to note that I seem constantly to substitute for the world a system of simple symbols. This is not due simply to a professional habit of a mathematician; the nature of my subject made this approach absolutely necessary. The Bergsonian world has no laws; what can have laws is simply the more or less distorted image which the scientists make of it. When we say that nature is governed by laws, it is understood that this portrait is still rather lifelike. It is therefore according to this description and this description only that we must reason, or else we run the risk of losing the very idea of law which was the object of our study. For this image can be taken apart; we can dissect it into its elements, distinguish instants differing one from another, and recognize independent parts. If sometimes I have simplified excessively and reduced these elements to too small a number, this is only a matter of degree; this did not change in any way the nature of my arguments and their import; it merely made the exposition more concise.

Chapter II

SPACE AND TIME

One of the reasons which has induced me to return to one of the questions which I have discussed most frequently is the revolution which has recently taken place in our ideas concerning mechanics. Will not the principle of relativity, as conceived by Lorentz, impose upon us an entirely new conception of space and time and thus force us to abandon some conclusions which might have seemed established? Have we not said that geometry was devised by the mind as a result of experience, no doubt, but without having been imposed upon us by experience, so that, once constituted, it is secure from all revision and beyond the reach of new assaults from experience? And yet do not the experiments on which the new mechanics is based seem to have shaken it? In order to see what we should think about it, I must recall succinctly a few of the fundamental ideas which I have tried to make evident in my previous writings.

I shall exclude first of all the idea of an alleged sense of space which would locate our sensations in a ready-made space whose notion would pre-exist all experience, and which prior to all experience would have all the properties of the space of the geometer. As a matter of fact, what is this alleged sense of space? When we wish to know whether an animal possesses it, what experiment do we conduct? We place objects near it which it craves, and we observe if it knows how to execute without trial and error the movements which will permit it to reach them. How do we notice that other persons are endowed with this precious sense of space? It is because they too are capable of contracting their muscles purposefully in order to reach the objects whose presence is revealed to them by certain sensations. What more is there when we observe the sense of space in our own consciousness? Here again, in the presence of varied sensations, we know that we could go through movements which would enable us to reach the objects which we regard as the cause of these sensations, and thus to act upon these sensations, to make them disappear or to make them more intense. The only difference is that in order to be aware of it, we do not need to go through the movements actually; it is sufficient that we call them to mind. This sense of space which the intellect is incapable of conveying could only be some force residing in the very depths of the unconscious, and therefore this force could be known to us only through the acts which it provokes; and these acts are precisely the movements of which I have been speaking. The sense of space is therefore reduced to a constant association between certain sensations and certain movements or to the representation of these movements. (In order to avoid a constantly recurring equivocation, in spite of my oft-repeated explanations, is it necessary to repeat once again that by this I do not mean the representation of those movements in space, but the representation of the sensations which accompany them?)

Now, why and to what extent is space relative? It is clear that if all the objects which surround us and our body itself, as well as our measuring instruments, were transported to another region of space without their respective distances varying, we would not notice it. And that is what happens actually since we are carried along without suspecting it by the movement of the Earth. If all objects were enlarged in the same proportion, and also our measuring instruments, we would not notice it any more. Thus not only can we not know the absolute position of an object in space, so that this phrase "absolute position of an object" has no meaning and we agree to speak only of its position relative to other objects; but the phrases "absolute size of an object" and "absolute distance between two points" have no meaning; we must speak only of the relation of two sizes, of the relation of two distances. But there is more to it: let us suppose that all objects be deformed according to some law more complicated than the preceding laws, according to any law whatever, and that our measuring instruments be deformed according to the same law. We will not be able to notice this, either; so, space is much more relative than we ordinarily believe. We can only be aware of modifications of form of the objects which differ from simultaneous modifications of form of our measuring instruments.

Our measuring instruments are solid bodies; or else they are made up of several solid bodies movable with relation to one another and whose relative displacements are indicated by marks on these bodies, by pointers moving along graduated scales; and it is precisely by reading these scales that we make use of the instrument. We therefore know whether our instrument has or has not changed position in the same way as an invariable solid since in this case the indications in question have not changed. Our instruments also include telescopes with which we take sightings so that it is possible to say that a ray of light is also one of our instruments.

Will our intuitive idea of space teach us more? We have just seen that it is reduced to a constant association between certain sensations and certain movements. This is the same as saying that the members with which we make these movements also play the role, so to speak, of measuring instruments. These instruments, which are less precise than those of the scientist, are sufficient for everyday life, and it is with these that the child, like primitive man, has measured space or, to be more correct, has constructed a space which fulfills the needs of his daily life. Our body is our first measuring instrument. Like the others, it is composed of many solid pieces movable with relation to one another, and certain sensations inform us of the relative displacement of these pieces, so that, just as in the case of artificial instruments, we know whether or not our body has changed position as an invariable solid. In summary, our instruments, those which the child owes to nature, and those which the scientist owes to his genius, have as fundamental elements the solid body and the ray of light.

Under these conditions does space possess geometric properties independent of the instruments used to measure it? It can, we have said, undergo any deformation whatever without our being made aware of it if our instruments undergo the same deformation. In reality, space is therefore amorphous, a flaccid form, without rigidity, which is adaptable to everything; it has no properties of its own. To geometrize is to study the properties of our instruments, that is, of solid bodies.

But then, since our instruments are imperfect, geometry must be modified each time they are improved. The builders ought to be able to include in their prospectus: "I provide a space far superior to that of my competitors, far simpler, much more convenient, much more comfortable." We know that this is not so; we would be tempted to say that geometry is the study of the properties which the instruments would possess if they were perfect. But for this to be so it would be necessary to know what a perfect instrument is (and we do not know, since there is none) and to be able to define an ideal instrument only by means of geometry; which is a vicious circle. And then we shall say that geometry is the study of a set of laws hardly different from those which our instruments really obey but much more simple, laws which do not effectively govern any natural object but which can be conceived by the mind. In this sense, geometry is a convention, a sort of rough compromise between our love for simplicity and our desire not to go too far astray from what our instruments teach us. This convention defines both space and the perfect instrument.

What we have said of space is applicable to time. I do not wish to speak of time here as conceived by Bergson's disciples, of that duration which, far from being a pure quantity devoid of all quality, is, so to speak, quality itself and whose different parts, which otherwise penetrate each other mutually in parts, can be distinguished qualitatively from one another. This duration could not be an instrument for the scientist; it was able to play this role only by undergoing a profound transformation, only by spatializing itself, as Bergson says. In fact, it had to become measurable; that which cannot be measured cannot be an object of science. Thus, time which can be measured is also essentially relative. If all phenomena were to slow down and so also our clocks, we would not be aware of it; and this would be so whatever might be the law governing this slowing down, provided it were the same for all kinds of phenomena and for all clocks. The properties of time are therefore merely those of our clocks just as the properties of space are merely those of the measuring instruments.

That is not all; psychological time, the Bergsonian duration, from which the scientist's time had its origin, serves to classify the phenomena which take place in the same consciousness. It is incapable of classifying two psychological phenomena taking place in two different conscious settings or *a fortiori* two physical phenomena. One event takes place on Earth, another on Sirius; how shall we know whether the first occurs before, at the same time, or after the second? This can be so only as the result of a convention.

But we can consider the relativity of time and space from an altogether different point of view. Let us consider the laws which the world obeys; they can be expressed by differential equations. We observe that these equations are not falsified if the rectangular coordinate axes are changed, these axes remaining fixed; nor if we change the origin of time, nor if we replace the fixed rectangular axes with rectangular axes which move, but whose movement is a rectilinear and uniform translation. Permit me to designate relativity as *psychological* if it is considered from the first point of view and *physical* if it is considered from the second point of view. You see immediately that physical relativity is much more restricted than psychological relativity. We have said, for instance, that nothing would be changed if we multiplied all lengths by the same constant, provided that the multiplication applied at the same time to all objects and to all instruments. But if we multiply all the coordinates by the same constant, it is possible that the differential equations could be falsified. They would be if the system were placed in relation with movable, *rotating* axes since it would be necessary to introduce the ordinary centrifugal force and the composite centrifugal force. It was thus that Foucault's experiment demonstrated the rotation of the Earth. There is in this something which somewhat jars our ideas about the relativity of space, ideas which are based on psychological relativity, and this disagreement has seemed embarrassing to many philosophers.

Let us examine the question a little more closely. All parts of the world are interdependent; and however far Sirius may be, doubtless it cannot be absolutely without effect on what takes place on this Earth. If therefore we wish to write the differential equations which govern the world, either these equations will be inaccurate or they will depend on the condition of the entire world. There cannot be a system of equations for the Earth and another for Sirius; there must be only one which will apply to the entire universe.

Thus, we do not take note directly of the differential equations; what we do note are the finite equations which are the immediate translation of observable phenomena and from which the differential equations are derived by differentiation. The differential equations are not falsified when a change of axes is carried out as we have described; but the same is not true for finite equations. A change of axes would, in fact, oblige us to change the constants of integration. The principle of relativity is consequently not applicable to finite equations which are observed directly, but to the differential equations.

Thus, how can we proceed from finite equations to differential equations of which they are the integrals? It is necessary to know *several* particular integrals differing from one another by the values assigned to the constants of integration, then to eliminate these constants by differentiation. Only one of these solutions is effected in nature although there are infinitely many which are possible. In order to form the differential equations it would be necessary not only to know the one which is effected but also all those which are possible.

Thus, if we have only one system of laws applicable to the entire universe, observation will provide us with only one solution, that which is effected; for, there was only one copy of the universe ever printed; and that is a prime difficulty.

Besides, as a consequence of the psychological relativity of space, we can observe only what our instruments can measure; they will give us, for example, the distances of the stars, or of the various bodies which we need to consider. They will not give us their coordinates with relation to fixed or movable axes whose existence is purely conventional. If our equations contain these coordinates, it is through a fiction which can be convenient, but a fiction nevertheless. If we wish our equations to express directly what we observe, it will be necessary for the distances to appear among our independent variables, and then the other variables will disappear by themselves. That will then be our principle of relativity, but it no longer has any meaning. It signifies merely that we had introduced in our equations auxiliary variables—parasites—which represent nothing tangible, and that it is possible to eliminate them.

These difficulties will vanish if we do not insist on an absolute rigor. The various parts of the world are interdependent, but if the distance is at all great, the force of attraction is so weak that it is permissible to disregard it; and then our equations will break up into separate systems, one applicable only to the terrestrial world, another to that of the sun, another to that of Sirius, or even to much smaller worlds, like that of a laboratory table.

And then it will no longer be correct to say that there is only one copy of the universe; there can be many tables in a laboratory. It will be possible to begin an experiment again by varying the conditions. Nor will we know a single solution only, the only one which had been effected, but a great number of possible solutions, and it will become easy to proceed from finite equations to differential equations.

Moreover, we will not only know the respective distances between the various bodies of one of these smaller worlds but also their distances from the bodies of the neighboring small worlds. We can arrange it so that only the second distances will vary while the first distances remain constant. It will then be as if we had changed the axes to which the first small world had reference. The stars are too distant to affect perceptibly our terrestrial world but we see them, and thanks to them we can relate this terrestrial world to axes linked to these stars; we have the means to measure both the respective distances between the terrestrial bodies and the coordinates of these bodies with relation to this system of axes which is alien to the terrestrial world. The principle of relativity thus assumes a meaning; it becomes verifiable.

Let us observe nevertheless that we have obtained this result only by neglecting certain forces and yet we do not consider our principle as simply approximated; we assign an absolute value to it. Seeing, in fact, that it remains true however far apart our small worlds may be from one another, we agree to say that it is true for the exact equations of the universe; and this convention will never be found to be in error since, when applied to the entire universe, the principle is unverifiable.

Let us return now to the case mentioned a short while ago. A system is related now to fixed axes, now to rotating axes. Will the equations which govern it change? Yes, according to ordinary mechanics. Is this exact? What we observe is not the coordinates of the bodies but their respective distances. We could then try to form the equations which these distances obey by eliminating the other quantities which are mere parasitic variables, inaccessible to observation. This elimination is always possible; the only thing is that, if we had retained the coordinates, we would have obtained differential equations of the second order; those which we shall derive after having eliminated all that is not observable will be, on the contrary, of the third order, so that they will give way to a greater number of possible equations. According to this reckoning, the principle of relativity will still be applicable in this case. When we proceed from fixed axes to rotating axes these equations of the third order will not vary. What will vary will be the equations of the second order which define the coordinates; but the latter are, so to speak, integrals of the first equations, and just as in all integrals of differential equations, a constant of integration is involved; it is this constant which does not remain the same when we proceed from fixed axes to rotating axes. But, since we assume our system to be completely isolated in space, considering it as the entire universe, we have no means of knowing whether it rotates. It is therefore really the equations of the third order which express what we observe.

Instead of considering the entire universe, let us now consider our small separate worlds without mechanical force on one another but visible to one another. If one of these worlds rotates, we shall then see that it rotates. We shall recognize that the value which we must assign to the constant which we just mentioned depends on the speed of rotation and it is thus that the convention habitually adopted by the students of mechanics will be justified.

We therefore realize the meaning of the principle of physical relativity; it is no longer a simple convention. It is verifiable, and consequently it might not be verified. It is an experimental truth, and what is the meaning of this truth? It is easy to infer it from the preceding considerations. It signifies that the mutual attraction of two bodies tends to zero when the distance between these two bodies increases indefinitely. It signifies that two distant worlds behave as if they were independent; and we can understand better why the principle of physical relativity is less extensive than the principle of psychological relativity. It is no longer a necessity due to the very nature of our intellect; it is an experimental truth to which experiment imposes limits.

This principle of physical relativity can serve to define space; it provides us, so to speak, with a new measuring instrument. To make myself clear: how could a solid body enable us to measure or rather to construct space? By moving a solid body from one position to another we recognized that it was possible to apply it first to one figure and then to another, and we agreed to consider these two figures as equal. Geometry originated as a result of this convention. To each possible shifting of the solid body there thus corresponded a transformation of space in itself, without changing the form and size of the figures. Geometry is merely the knowledge of the mutual relations of these transformations, or to use mathematical language, the study of the structure of the group formed by these transformations, that is, the group of movements of solid bodies.

With this posited, there is another group, that of the transformations which do not falsify our differential equations; and this is another way of defining the equality of two figures. We will no longer say: two figures are equal when the same solid body can be made to coincide first with one and then with the other. We will say: two figures are equal when the same mechanical system, far enough from the neighboring systems to be considered as isolated and placed first in such a way that its different material points reproduce the first figure and again in such a way that they reproduce the second figure, behaves in the same manner.

Do the two conceptions differ essentially from one another? No; a solid body takes its form under the influence of the mutual attractions and repulsions of its various molecules; and this system of forces must be in equilibrium. To define space in such a way that a solid retains its form when its position is changed is to define it in such a way that the equations describing the equilibrium of this body are not falsified by a change of axes; for these equations of equilibrium are merely a particular case of the general equations of dynamics, which, according to the principle of physical relativity, must not be modified by this change of axes.

A solid body is a mechanical system just like any other; the only difference between our former definition of space and the new one is that the new one is more inclusive in the sense that it permits replacing the solid body by any other mechanical system. Moreover, the new convention not only defines space, it also defines time. It teaches us what two simultaneous instants are, what two equal intervals of time are or what double an interval of time is of another.

One last remark: the principle of physical relativity, as we have said, is an experimental fact for the same reason as the properties of the natural solids; as such, it is susceptible to constant revision; and geometry must escape this revision. For this to be so, it must again become a convention, and the principle of relativity must be regarded as a convention. We have mentioned what its experimental meaning is; it signifies that the mutual attraction between two very distant systems tends toward zero when their distance increases indefinitely. Experience teaches us that this is approximately true; it cannot teach us that this is completely true since the distance between the two systems will always remain finite. But there is nothing to prevent us from supposing that this is entirely true; nothing would prevent us even if experience seemed to belie the principle. Let us suppose that the mutual attraction, after decreasing when the distance increased, then begins to increase. Nothing would prevent us from admitting that for a still greater distance it would decrease anew and finally approach zero. Only then does the principle present itself as a convention, and this removes it from the attacks of experience. It is a convention which is suggested to us by experience, but which we adopt freely.

What, then, is the revolution which is due to the recent progress of physics? The principle of relativity, in its former aspect, has had to be abandoned; it is replaced by the principle of relativity according to Lorentz. It is the transformations of "the group of Lorentz" which do not falsify the differential equations of dynamics. If we suppose that the system no longer relates to fixed axes but to axes which are characterized by varying transformations, we must admit that all bodies become deformed, that a sphere, for example, is transformed into an ellipsoid in which the minor axis is parallel to the translation of the axes. Time itself must be profoundly modified. Here are two observers, the first linked to the fixed axes, and the second to the rotating axes, but each believing the other to be at rest. Not only will such a figure, which the first one considers as a sphere, appear to the second as an ellipsoid; but two events which the first will consider as simultaneous will not be so for the second.

Everything happens as if time were a fourth dimension of space, and as if four-dimensional space resulting from the combination of ordinary space and of time could rotate not only around an axis of ordinary space in such a way that time were not altered, but around any axis whatever. For the comparison to be mathematically accurate, it would be necessary to assign purely imaginary values to this fourth coordinate of space. The four coordinates of a point in our new space would not be x, y, z, and t, but x, y, z, and $t\sqrt{-1}$. But I do not insist on this point; the essential thing is to notice that in the new conception space and time are no longer two entirely distinct entities which can be considered separately, but two parts of the same whole, two parts which are so closely knit that they cannot be easily separated.

Another remark: I attempted before to define the relation between two events which occurred in two different settings by saying that one will be considered as anterior to the other if it can be considered as the cause of the other. This definition becomes inadequate. In this new mechanics there is no effect which is transmitted instantaneously; the maximum speed of transmission is that of light. Under these conditions it can happen that event *A* (as a consequence of the mere consideration of space and time) could be neither the effect nor the cause of event *B* if the distance between the places where they take place is such that light cannot travel in sufficient time from place *B* to place *A* nor from place *A* to place *B*.

What shall be our position in view of these new conceptions? Shall we be obliged to modify our conclusions? Certainly not; we had adopted a convention because it seemed convenient and we had said that nothing could constrain us to abandon it. Today some physicists want to adopt a new convention. It is not that they are constrained to do so; they consider this new convention more convenient; that is all. And those who are not of this opinion can legitimately retain the old one in order not to disturb their old habits. I believe, just between us, that this is what they shall do for a long time to come.

Chapter III

WHY SPACE HAS THREE DIMENSIONS

I. "ANALYSIS SITUS" AND THE CONTINUUM

Geometers distinguish ordinarily between two types of geometry and characterize the first as metric and the second as projective. Metric geometry is based on the notion of distance; in it, two figures are considered as equivalent when they are "equal" in the sense which mathematicians assign to this word. Projective geometry is based on the notion of the straight line. For two figures to be considered as equivalent in projective geometry it is not necessary that they be equal; it is sufficient that they correspond to each other by means of a projective transformation; that is, that one be the projection of the other. This second body of doctrine has often been called qualitative geometry; it is quite so if contrasted with the first. It is clear that measure and quantity play a less important role in it. It is not entirely so, however. The straightness of a line is not purely qualitative; it would not be possible to ascertain that a line is straight without making some measurement or without sliding along this line an instrument called a ruler which is a sort of measuring instrument.

But there is a third geometry in which quantity is completely excluded and which is purely qualitative: *analysis situs*. In this discipline two figures are equivalent every time it is possible to have one correspond to the other by means of a continuous deformation, whatever the law governing the deformation may be, provided the continuity is maintained. Thus, a circle is equivalent to an ellipse or even to any type of closed curve but it is not equivalent to a line segment because the segment is not a closed figure. A sphere is equivalent to any convex surface whatever, but it is not equivalent to a torus because in the torus there is a hole and there is none in a sphere. Let us imagine a pattern of any kind and the copy of this pattern drawn by a clumsy draftsman. The proportions are distorted, straight lines drawn by a trembling hand have undergone distressing deviations and result in disproportional curves. From the point of view of metric geometry, and even from that of projective geometry, the two figures are not equivalent; but on the contrary they are equivalent from the point of view of *analysis situs.*

Analysis situs is a very important science for the geometer. It gives rise to a series of theorems just as closely interconnected as those of Euclid; and it is from this set of propositions that Riemann constructed one of the most remarkable and abstract theories of pure analysis. I shall cite two of these theorems in order to explain their nature: (1) two closed curves in a plane intersect in an even number of points; and (2) if a polyhedron is convex, that is, if it is not possible to trace a closed curve on its surface without cutting it in two, the number of edges is equal to the number of vertices plus the number of faces less two; and this remains true when the faces and the edges of the polyhedron are curves.

And this is what makes *analysis situs* so interesting to us: it is in this discipline that geometric intuition truly plays a role. When use is made of this intuition in a theorem of metric geometry, it is because it is impossible to study the metric properties of a figure while disregarding its qualitative properties, that is to say, those properties which properly belong to *analysis situs.* It has often been said that geometry is the art of reasoning correctly about figures which are poorly constructed. This is not a quip but a truth which deserves reflection. But what is a poorly constructed figure? It is the type which can be drawn by the clumsy draftsman mentioned a short while ago. He distorts the proportions more or less flagrantly; his straight lines are disturbing zigzags; his circles appear as ungraceful humps. But all this does not matter; it will in no way trouble the geometer; this will not prevent him from reasoning correctly.

But the inexperienced artist must not represent a closed curve by an open curve, or three lines which intersect in a single point by three lines which have no point in common or a surface containing holes by a surface which is intact. In that case this artist's drawing could not be used and reasoning would become impossible. Intuition would not have been impeded by defects in drawing which are of interest only in metric or projective geometry. But intuition will become impossible as soon as these defects involve *analysis situs.*

This very simple observation indicates the true role of geometric intuition; it is to facilitate this intuition that the geometer needs to draw figures or at least to form a mental image of them. Now, if he minimizes the importance of the metric or projective properties of these figures, if he concentrates only on their purely qualitative properties, it is because herein only does geometric intuition truly play a role. I do not mean that metric geometry is based on pure logic or that no intuitive truth has a place in it. But they are intuitive ideas of another kind, analogous to those which play the essential role in arithmetic and in algebra.

The fundamental proposition of *analysis situs* is that space is a three-dimensional continuum. I have already discussed in other writings the origin of this proposition, but in a very succinct manner, and it does not seem pointless to me to consider it again with a few more details in order to clarify certain points.

Space is relative; by that I mean not only that it would be possible for us to be transported to another region of space without our noticing it (and that is what really happens to us since we do not notice the translation of the Earth); not only that all the dimensions of all objects could be increased proportionally without our being able to know it, provided our measuring instruments underwent the same change; but I also mean that space could be deformed according to some arbitrary law provided our measuring instruments were deformed according to this very same law.

This could be any deformation whatever; but it would have to be continuous; that is, it would have to be one of those which transform a figure into another figure which is equivalent from the point of view of *analysis situs*. Space, when considered independently of our measuring instruments, has therefore neither metric nor projective properties; it has only topological properties (that is, those studied in *analysis situs*). It is *amorphous*, that is, it does not differ from any space which one can derive from it by any continuous deformation whatever. I shall explain by using mathematical language. Here are two spaces E and E'; the point M in E corresponds to point M' in E'; point M has x, y, and z as rectangular coordinates; point M' has as rectangular coordinates three continuous functions whatever of x, y, and z. These two spaces do not differ from the point of view with which we are concerned.

How the functioning of our measuring instruments and in particular how the role of solid bodies provides the mind with the opportunity to determine and to organize this amorphous space more completely, how it permits projective geometry to plot a network of straight lines and metric geometry to measure the distances between these points, what essential role the fundamental notion of group plays in this process, I have explained at length in other writings. I consider all these points as established, and I do not need to refer to them again.

Our only concern here is the amorphous space considered in *analysis situs*, the only space which is independent of our measuring instruments; and its fundamental property—I was about to say its only property—is that of being a three-dimensional continuum.

2. CONTINUUM AND CUTS

But what is a continuum of n dimensions; how does it differ from a continuum in which the number of dimensions is greater or less? Let us first of all recall some of the results obtained recently by Cantor's students. It is possible to establish a one-to-one correspondence between the points of a straight line and those of a plane, or more generally between those of a continuum of n dimensions and those of a continuum of p dimensions. This is possible provided we are not bound by the condition that to two points infinitely near on a straight line there correspond two points infinitely near on the plane, that is, by the condition of continuity.

It is therefore possible to *deform* a plane in such a way as to obtain a straight line, provided that this deformation is not continuous. This would be impossible, on the other hand, with a continuous deformation. Thus the question of the number of dimensions is closely linked to the notion of continuity and it would have no meaning for anyone who wished to exclude this notion.

In order to define the continuum of n dimensions, we first of all have the analytic definition: a continuum of n dimensions is a set of n coordinates, that is, a set of n quantities capable of varying *independently* one from the other and of assuming all the real values which satisfy certain inequalities. This definition, flawless from the point of view of mathematics, nevertheless could not be entirely satisfactory to us. In a continuum the diverse coordinates are not, so to speak, juxtaposed one to the other; they are linked among themselves so as to form the various aspects of a whole. At each instant in the study of space, we carry out what is called a change of coordinates. For example, we carry out a change of rectangular axes, or else we change to curvilinear coordinates. In studying another continuum, we also carry out changes of coordinates; that is, we replace the n coordinates by n continuous functions whatever of the n coordinates. For those of us who derive the notion of the continuum of n dimensions not from the analytic definition already mentioned but from some more profound source, this operation is very natural; we feel that it does not change what is essential in the continuum. For those, on the other hand, who would know the continuum only from the analytic definition, the operation would be legal, no doubt, but baroque and not justified.

Finally, this definition minimizes the importance of the intuitive origin of the notion of continuum and of all the rich ideas which this notion contains. It recurs in the type of those definitions which have become so frequent in mathematics since the tendency to "arithmetize" this science. These definitions, we have stated, are flawless from the point of view of mathematics but they could not satisfy the philosopher. They substitute the object to be defined and the intuitive notion of this object with a construction made up of simpler materials. It is then easily seen that it is possible to form this construction effectively with these materials, but we see at the same time that it would be just as possible to make many more. What it does not reveal is the profound reason for which these materials have been assembled in this fashion rather than in another. I do not mean that this "arithmetization" of mathematics is undesirable; I say that it is not everything.

I shall base the determination of the number of dimensions on the notion of *cuts*. Let us consider first of all a closed curve, that is, a continuum of *one* dimension. If on this curve we take any two points through which we shall not permit ourselves to pass, the curve will be cut into two parts, and it will become impossible to go from one to the other, still remaining on the curve, but not passing through the excluded points. Let us consider, on the other hand, a closed surface which forms a continuum of *two* dimensions. It will be possible to take on this surface one, two, or any number of excluded points whatever. The surface will not be divided into two parts because of this; it will be possible to go from one point to another on this surface without encountering any obstacle because it will always be possible to *go around* the excluded points.

But if we trace on the surface one or many closed curves and if we consider them as *cuts* which may not be crossed, the surface can then be cut into several parts.

Let us consider now the case of space. It cannot be divided into several parts either by forbidding the passage through certain points or by forbidding the crossing of certain lines; these obstacles could always be circumvented. It will be necessary to forbid the crossing of certain surfaces; that is, certain two-dimensional cuts. And that is why we say that space has three dimensions.

We now know what a continuum of n dimensions is. A continuum has n dimensions when it is possible to divide it into many regions by means of one or more cuts which are themselves continua of $n-1$ dimensions. The continuum of n dimensions is thus defined by the continuum of $n-1$ dimensions. This is a definition by recurrence.

What gives me confidence in this definition and indicates to me that this is really how ideas arise naturally in one's mind is, first of all, that many authors of elementary treatises, who meant no mischief, have done something analogous at the beginning of their works. They define volumes as portions of space, surfaces as boundaries of volumes, lines as boundaries of surfaces, points as boundaries of lines; after which they pause and the analogy is evident. Following this, we rediscover the important role of cuts in other parts of *analysis situs*; everything rests on the notion of cuts. According to Riemann, what distinguishes the torus, for example, from the sphere? It is the fact that we cannot trace a closed curve on a sphere without cutting this surface in two, while there are closed curves which do not cut the torus in two, and it is necessary to execute two closed cuts with no point in common in order to be sure one has divided it.

There remains another point to consider. The continua which we have just considered are mathematical continua; each of their points is an individual thing absolutely distinct from the others and, moreover, absolutely indivisible. The continua directly revealed by our senses and which I have called physical continua are altogether different. The law governing these continua is that of Fechner, which I shall strip of the pompous mathematical array which usually surrounds it in order to reduce it to the simple terms of the experimental data on which it is based. It is possible to tell the difference between a 10-gram weight and a 12-gram weight at a guess; it would not be possible to tell an 11-gram from either a 10-gram or a 12-gram weight. More generally, there can be two sets of sensations which we can tell apart without being able to tell either one set or the other from a third set. With this posited, we can imagine a continuous chain of sets of sensations such that each of them cannot be distinguished from the next one although the two extremities of the chain can easily be told apart. This will be a physical continuum of one dimension. We can also imagine more complex physical continua. The *elements* of these physical continua will again be sets of sensations (but I prefer to use the word element which is simpler). When shall we say, then, that a system S of similar elements is a physical continuum? Whenever we can consider any two of its elements as the extremities of a continuous chain analogous to the one I have just described and all whose elements belong to S. It is thus that a surface is continuous—if it is possible to join any two of its points by a continuous line which does not leave the surface.

Can we extend the notion of cuts to physical continua and thereby determine their number of dimensions? Obviously we can. Let us exclude certain elements of S, and all those which cannot be distinguished from them. These restricted elements can well be of a finite number or else form one or many continua by their union. The set of these limited elements will constitute a *cut*; and it can happen that, after executing this cut, we may have divided the continuum S into several others such that it is no longer possible to go from any element whatever of S to any other element whatever by a continuous chain, no element of this chain being distinguishable from any other element of the cut.

Therefore a physical continuum which can thus be cut up by limiting ourselves to a finite number of elements will have *one* dimension; a physical continuum will have n dimensions if it is possible to subdivide it by means of cuts which are themselves physical continua of $n-1$ dimensions.

3. SPACE AND THE SENSES

The question seems resolved; we need only, it seems, apply this rule either to the physical continuum which is a rough image of space or to the corresponding mathematical continuum which is its refined image and which is the space of the geometer. But that is an illusion; all would be well if the physical continuum from which we derive space were directly revealed to us by the senses; but it is far from being so.

Let us see, as a matter of fact, how it is possible to deduce a physical continuum from our numerous sensations. Each element of a physical continuum is a set of sensations; and it is simplest to consider first of all a set of simultaneous sensations, a state of consciousness. But each of our states of consciousness is something exceedingly complex, so much so that we cannot hope ever to see two states of consciousness become indistinguishable. And yet, in order to construct a physical continuum, it is essential from what has been said before that two of their elements can, in certain cases, be considered as indistinguishable. Now, we will never be able to say: I cannot distinguish my present mental state from my mental state of the day before yesterday at the same hour.

It is therefore necessary that by an active operation of the mind we agree to consider two states of consciousness as identical by *disregarding* their differences. We shall be able, for example, and this will be simplest, to disregard the perceptions of certain senses. I have said that I could not tell the difference between a 10-gram weight and an 11-gram weight. And yet it is probable that, if I ever experimented, the sensation of pressure caused by the 10-gram weight was accompanied by diverse olfactory or auditory sensations and that when the 10-gram weight was replaced by the 11-gram weight, these diverse sensations had varied. It is because I disregard these strange sensations that I can say that the two states of consciousness are indistinguishable.

It is possible to lay down more complicated conditions; it is also possible to consider as elements of our continuum not only sets of simultaneous sensations but also sets of successive sensations, that is, *series* of sensations. It will next be necessary to lay down the fundamental condition and to indicate the common characteristics which two elements of the continuum must possess (whether they are sets of simultaneous or of successive sensations) in order to be considered identical.

Thus, in the case of the definition of a physical continuum, it is necessary to make a double choice: first, to choose the sets of simultaneous or successive sensations which are to serve as elements of this continuum; and secondly, to choose the fundamental condition which will define the cases in which two elements must be considered identical.

How must this double choice be made in order to obtain space? Can we be satisfied to consider a set of simultaneous sensations or is it necessary to consider a series of sensations? Can we, in particular, be content with the simplest and the most natural fundamental condition which would consist in disregarding the perceptions of certain senses? No.

Such a denial is impossible; we cannot choose from among our senses those which will convey for us the notion of space and only that. There is no sense which can convey the notion of space without the aid of the others; nor is there one which does not convey a great number of things which have nothing to do with space.

If we analyze, for example, the perceptions of so-called touch, this is what we perceive. Experience teaches us that if we touch our skin with two pin-points, our consciousness can distinguish between these two pin-points if they are sufficiently far apart, and ceases to distinguish one from the other if they are brought very close to each other. The minimum distance for telling them apart varies, moreover, according to the regions of the body. Ordinarily we say that the skin is divided into regions each of which is the domain of the same sensory nerve; and if the two pin-points fall in the same region, a single nerve is stimulated and we are aware of only one pin-point; but that we perceive two pin-points, on the other hand, if they fall in two regions and consequently affect two nerves. This is not entirely satisfactory; we could not discover the characteristics of the physical continuum in this manner. Let us suppose that we change the position of the two pin-points, with their distance, already very small, being kept constant. Since this distance is very small, it can happen that they will fall in the same region and that only one perception will result. But if we vary their position little by little without changing their distance, at some moment it will occur that one of them will fall outside the region while the other will still be located within it. At that instant we ought to feel two pin-points; but that is not what we observe. We would not derive in this manner the notion of a physical continuum, but that of a discrete set formed by as many distinct cases as there are regions. It is better to grant that the contact of a pin-point affects not only the nearest nerve but also the neighboring nerves, and with an intensity which decreases when the distance increases. Let us suppose therefore that we are comparing the effects of the contact of two pin-points. If the distance between the two is small, the same nerves are affected; the intensity of the stimulus on the same nerve by one or the other pin-point will doubtless be different, but this difference will be too small to be discernible according to Fechner's general rule. If a nerve is stimulated by pin-point *A* without being stimulated by *B*, it will be stimulated only very slightly by pin-point *A* and the stimulus will be below the "threshold of consciousness." The effects of the two pin-points will consequently be indistinguishable.

We have therein all that we need in order to construct a physical continuum; we have only to move the two pin-points along the surface of our skin and to note in which cases our consciousness differentiates between them. We have disregarded (and that is what I referred to above as our fundamental condition) a multitude of circumstances: the intensity of the stimulus of each sensory network, the greater or lesser pressure brought to bear upon the skin by the pin-point, and the nature of the contact. All these circumstances are revealed by the sense of touch, but we have eliminated them in order to conserve only those whose character is geometric. Do we thus derive the notion of space? No; first of all, the continuum so constructed has only two dimensions, like the surface of the skin itself. Next, we know very well that our skin is movable and that a particular point on the skin does not always correspond to a particular point in space; that the distance between two points on the skin varies when our body is deformed. It is doubtless in this manner that mollusks conceive of space, but this has no relation to ours.

The same is true of sight; two beams of light striking two points of the retina will give us the impression either of two luminous spots or of only one according to whether these two points are more or less distant. This is equivalent to the two pin-points above; we can use them to construct a physical continuum by disregarding the color and the intensity of the light; and this physical continuum will have two dimensions just like the surface of the retina. The third dimension will be introduced by bringing into play the convergence of the eyes in binocular vision; and that is what has been called visual space. It is superior to tactile space, first of all because with a little good will we can give it three dimensions, and secondly because the retina is no doubt movable, but in the manner of a solid body whereas the skin is pliant in all directions. We are then tempted to say that true space resides where we seek to localize all of our other sensations. This is not yet satisfactory. Not only is the eye movable, so that to a particular point of the retina and to a particular degree of convergence of the eyes there does not always correspond a particular point in space; but also this does not explain why a third dimension so manifestly heterogeneous to the other two has been introduced, nor why the geometry of the blind is the same as ours.

If we wish to combine visual space with tactile space, there will be 5 dimensions instead of 3 or 2; and there will remain the task of explaining by what process these 5 dimensions can be reduced to 3; and the number of dimensions will be increased again if we wish to introduce other senses into the combination.

There remains to explain in a few words why tactile space and visual space are one and the same space.

4. SPACE AND MOTION

It therefore seems that we cannot construct space by considering sets of simultaneous sensations, and that we must consider series of sensations. It is always necessary to refer again to what I have said formerly. Why is it that certain changes appear as changes of position and that others appear as changes of state without a geometric nature? For this, we must distinguish first of all between the external changes which are involuntary and which are not accompanied by muscular sensations and the internal changes which are the movements of our body and which we distinguish from the others because they are voluntary and are accompanied by muscular sensations. An external change can be *corrected* by an internal change, for example, when we follow with our eyes an object in motion in such a way as to bring back its image always to the same point on the retina. An external change susceptible to such a correction is a change of position; if it is not susceptible to this, it is a change of state.

Two external changes, which are completely different from the qualitative point of view, are considered as corresponding to the *same* change of position if they can be corrected by the same internal change. So also, two internal changes can be made up of series of muscular sensations which have nothing in common and yet correspond to the *same* change of position, if they can correct the same external change. This is what we mean in ordinary parlance when we say that there are many paths which can lead from one point to another.

What is important, therefore, are the movements which must be made in order to attain a specific object. Consciousness of these movements is nothing more for us than the set of muscular sensations which accompany them.

With this posited, a certain object is in contact with one of my fingers; say, the index finger of my right hand. From this fact I experience a tactile sensation T; at the same time, I experience from this object the visual sensations V. As the object is withdrawn, sensation T fades away, the sensations V are replaced by the new visual sensations V'; this is an external change. Suppose that I wish to correct partially this external change by renewing the sensation T, that is, by again bringing my index finger into contact with the object. In order to do this, I must execute certain movements which, for me, are expressed by a certain series of muscular sensations S. This I know because numerous experiences which I or my ancestors have had have taught me that when sensation T disappeared and the visual sensations changed from V to V', it was possible to renew sensation T by the movements corresponding to the series S. I know equally well that I could have obtained the same result by other movements expressing themselves for me not by the series S but by another series S' or S''.

All these series of muscular sensations S, S', S'',... have, perhaps, no element in common; I compare them because I know that any one of them can renew sensation T every time that the sensations V have become V'. In our usual language, we who already know geometry shall say that the diverse series of movements which correspond to the series of muscular sensations S, S', S'', have this in common: in any of them, the initial position as well as the final position of my index finger remain the same. Everything else can differ.

I am thus led not to differentiate between these diverse series S, S', S'',..., and to consider them as one single sensation. Nor will I differentiate between the series of muscular sensations which differ too little from these. I will then have the means for constructing a physical continuum. And I have, in fact, chosen the elements of this continuum, which are series of muscular sensations, and I have the "fundamental condition," which teaches me in which cases two of these elements must be considered as identical and *it is this continuum which has three dimensions.*

But this is not all. We have just defined a continuum which is a true space; it is the space which is considered as described by one of my fingers. But I have several fingers (and from the point of view which concerns me, all the points on my skin could serve as fingers). Will my different fingers describe the same space? Yes, without doubt, but what does this mean? This implies a set of properties which would not be easy to express in ordinary language, but which I can attempt to explain if I am permitted to use certain symbols. I shall consider two fingers which I shall call α and β; finger α shall be, for example, the index finger of the right hand which we used in order to define the series S, S', S'',.... We shall then write

$$S \equiv S' \pmod{\alpha}$$

and this shall mean that if the movements corresponding to S renew the tactile sensation experienced by finger α, the same will be true for the movements corresponding to S', and inversely. Similarly, I shall write

$$S_1 \equiv S_1' \pmod{\beta}$$

to express the fact that if the movements corresponding to S_1 renew the tactile sensation experienced by finger β, the same will be true for the movements corresponding to S_1'.

With this posited, I shall assume that there exist two particular series of muscular sensations s and s_1 which will be defined in the following manner: I shall suppose that finger β experiences a tactile sensation due to contact with an object. By executing the movements corresponding to s, this sensation will disappear. But, finally, it will be finger α which will experience a sensation of contact. I know through experience that this will happen every time that prior to these movements finger β felt a contact; or at least almost every time. (I say *almost* because, in order to succeed, this requires that the object has not moved in the interval.) In our ordinary language (which would be clearer for us, but which I dare not use since I speak of beings who do not yet possess any knowledge of geometry) we would say that the movements corresponding to s have brought finger α into the position originally occupied by finger β. For s_1 the contrary will be true; the corresponding movements will bring finger β into the position originally occupied by finger α.

If these two series s and s_1 exist, the relation

$$S \equiv S' \pmod{\alpha}$$

will bring about as a consequence the relation:

$$s+S+s_1 \equiv s+S'+s_1 \pmod{\beta}$$

of which we are immediately convinced if we recall the meaning of the symbols, and we could infer from it without difficulty that the two spaces generated by α and by β are isomorphic and, in particular, that they have the same number of dimensions.

The same would not be true if the series s and s_1 did not exist. Let us suppose, in fact, that it is not possible to find a series of movements which, upon a sensation of contact of finger β with an object, will bring about a sensation of contact of finger α with the same object—and with certainty or at least near certainty—how then should we reason? We would say that finger β feels the object without being at the same point of space, that it feels it at a distance; otherwise, every time that finger β felt the object, it would be because it was at the same point A in space. There would then have to be a series of movements which would bring finger α to point A. And since the object is at point A, finger α should be able to feel the object and this should always happen. If, therefore, we suppose that there is no series of movements endowed with this property, we must admit that finger β feels the contact at a distance; that is, that it is not sufficient for the object to be felt by the finger in order to determine the position of the object in space; that is, finally, that space must possess *more* dimensions than the physical continuum generated by finger β in the manner we have described.

I shall assume for example that space has four dimensions, and I shall designate the four coordinates by x, y, z, and t. I shall suppose that finger β feels the contact with the object every time that the 3 coordinates x, y, z are the same for both the finger and for the object, whatever the fourth coordinate may be; moreover, that finger α feels the contact with the object every time that the 3 coordinates x, y, t are the same for the object and for this finger, whatever the coordinate z may be. Under these conditions, let us apply our rules for the construction of the physical continuum generated by β; we will find that it has only 3 dimensions, which will correspond to the three coordinates x, y, z, coordinate t playing no role whatever. In the same way the physical continuum generated by α would have 3 dimensions corresponding to x, y, and t. But we would not be able to find a series of movements corresponding to the series of muscular sensations s such that the sensation of contact for α follows with certainty the sensation of contact for β.

In fact, let x_1, y_1, z_1, t_1 be the coordinates of the object; x_0, y_0, z_0, t_0 the coordinates of finger β before the movement; and x'_0, y'_0, z'_0, t'_0 the coordinates of finger α after the movement. We shall express the fact that finger β feels the contact before the movement by writing:

$$x_0 = x_1, \qquad y_0 = y_1, \qquad z_0 = z_1. \tag{1}$$

We shall express the fact that α feels the contact after the movement by writing:

$$x'_0 = x_1, \qquad y'_0 = y_1, \qquad t'_0 = t_1. \tag{2}$$

For s to exist, it would be necessary for us to be able to choose x_0, y_0, z_0, t_0 and x'_0, y'_0, z'_0, t'_0 in such a way that the relations (1) should bring about the relations (2) whatever x_1, y_1, z_1, t_1 might be. It is clear that this is impossible. It is precisely the impossibility of forming s which would reveal to us in such a case that space would have 4 dimensions and not 3 like the physical continuum generated by β.

Moreover, we actually observe something analogous if we introduce the sense of sight. Let us consider a point on the retina; we can assign to it the same role as that of our fingers α and β. We can consider the series of movements necessary to bring back the image of an object to this point γ on the retina or the corresponding series S of muscular sensations. We can make use of this series in order to define a physical continuum analogous to that which was generated by α or by β. *This continuum will have only two dimensions.* But we cannot construct a series analogous to s, that is, a series of movements which bring about with certainty the tactile sensation felt by finger α as a result of the visual sensation felt at point γ. In other words, it is not sufficient for us to observe that the image of the object takes place at γ for us to be able to determine the movements necessary to bring our finger into contact with this object. We lack one item of information, which is the distance of the object. And that is why we say that sight functions at a distance, and that space has three dimensions—one more than that of the continuum generated by γ.

We see from this brief account what are the experimental facts which have led us to attribute three dimensions to space. In view of these facts, it was more convenient for us to attribute to it three rather than four or two dimensions. But this word "convenient" may not be forceful enough. A being that had attributed two or four dimensions to space would have found himself at a disadvantage in his struggle for life in a world such as ours. What does this really mean? Let me refer again to my symbols and for example to the congruences

$$S \equiv S' \pmod{\alpha}$$

whose meaning I have explained above. To attribute two dimensions to space would be to admit similar congruences, which we ourselves do not admit. We would then be led to substitute for the movements S, which work out well, the movements S' which would not succeed. To attribute four dimensions to space would be, on the contrary, to reject congruences which we ourselves admit. We would therefore deprive ourselves of the possibility of substituting for the movements S other movements S' which would work out just as well and which could offer, in certain circumstances, particular advantages.

5. SPACE AND NATURE

But the question can be posed from an altogether different point of view. We have assumed up to now a point of view which is purely subjective, purely psychological or, if we wish, physiological. We have considered only the relations of space to our senses. We could adopt, on the other hand, the point of view of physics and ask ourselves if it would be possible to localize natural phenomena in a space other than our own, for example, in a space of two or of four dimensions. The laws which physics reveals to us are expressed by differential equations, and in these equations are involved the *three* coordinates of certain material points. Is it impossible to express the same laws by other equations in which, this time, other material points having *four* coordinates would be involved? Or would this be possible, but the equations thus obtained be less simple? Or, finally, would they be just as simple and would we reject them simply because they disturb our mental habits?

What do we mean when we speak of expressing the *same* laws by *other* equations? Let us consider two worlds M and M'. We can establish between the phenomena which take place or which could take place in these two worlds a correspondence such that to every phenomenon φ of the first there corresponds a phenomenon φ' perfectly determined by the other which would be, so to speak, an image of it. Thus, if I assume that the necessary effect of phenomenon φ, in keeping with the laws which govern the world M, is a certain phenomenon φ_1, and that the necessary effect of phenomenon φ', which is the image of φ, in keeping with the laws which govern the world M', is precisely the image φ_1' of phenomenon φ_1, we shall be able to say that the two worlds obey the same laws. The qualitative nature of the phenomena φ and φ' are of little importance to us; it is sufficient that the "parallelism" be possible.

And, in fact, this qualitative nature of the phenomena is of concern only to our senses, and we have agreed to assume an extra-psychological point of view, consequently to disregard the perceptions of our senses and to pay attention only to the mutual relations of the phenomena. This is, in fact, what the physicist does when, for example, he replaces gases we know through experience, which produce sensations of pressure and heat, with the gases of the kinetic theory, in which we see only material points in motion, or replaces the lights which we experience and the sensations of color which it generates with the vibrations of the ether.

It will suffice to consider a simple case, the one of the astronomical phenomena and of Newton's Law. What we observe is not the coordinates of the celestial bodies but only their distances. The natural expression of the laws of their movements is therefore differential equations of these distances and time. Now, the distance between two points in space is a known and simple function of the coordinates of these two points. Let us transform our differential equations by substituting in them this function instead of each distance. We shall then have these equations in their usual form, the form in which the very coordinates of the celestial bodies are involved.

But we could have substituted for these distances other functions, and we would have thus obtained other forms of these equations. All these forms would have been equally legitimate from the point of view which concerns us since they would have obeyed the "parallelism" among the phenomena. Let us imagine the celestial bodies as situated in a four-dimensional space in such a way that the position of each of them is no longer defined by three but by four coordinates. Let us next, in our equations, replace the quantity which we considered up to now as representing the distance between two celestial bodies, by any function *whatever* of the eight coordinates of these two bodies. It is entirely unnecessary that this function be the one which represents the distance between two points in the ordinary four-dimensional space; it can be any function whatever since the "parallelism" will not be violated.

We will thus obtain a form of our equations in which are involved the coordinates of the celestial bodies in the four-dimensional space. This will be a new expression of the astronomical laws based on the hypothesis of a four-dimensional space and this expression will not be counter to the laws since the condition of "parallelism" is obeyed.

It is clear, however, that the equations thus obtained will be much less simple than our usual equations.

And the same would no doubt be true of the laws of physics. Is there a general reason that it should be so, and that in all branches of physics it should be the hypothesis concerning three-dimensionality which gives the equations their simplest form? Does this reason have any relation with that which was developed in the first part of this work and which absolutely obliged all living beings to believe in three-dimensionality or to act as if they believed in it under the penalty of being at a disadvantage in the struggle for life?

Here, a brief digression is necessary. Let us refer again, for an instant, to our former, ordinary space. We say that it is relative and this means that the laws of physics are the same in all parts of this space; or, in mathematical language, that the differential equations which express these laws do not depend on the choice of the coordinate axes.

If we consider a system which is entirely isolated, this has no meaning; it will not be possible to observe the coordinates of the points of this system, but only their respective distances. Observation will not teach us whether the properties of this system depend on the absolute position of the system in space, since this position is inobservable.

If the system is not isolated, this will not do either (if we wish to reason with strict rigor) since it will become impossible to express the laws which govern this system without taking into account the action of the exterior bodies. But there are systems which are *nearly* isolated, surrounded by bodies which are close enough to be *seen*, but too distant for their force to be felt. This is what happens to our terrestrial world with relation to the stars. We can therefore state the laws of this terrestrial world as if the stars did not exist, and yet relate this world to a system of coordinate axes perfectly defined and unvaryingly linked to these stars. Experience teaches us therefore that the choice of axes does not matter and that the equations are not falsified when a change of axes is made. The set of possible changes of axes forms, as we know, a six-dimensional group.

Let us put aside our ordinary space, and let us substitute for our equations others which will be equivalent in the sense that they shall obey the "parellelism" of the phenomena. Every time that we shall be concerned with a system which is nearly isolated, there will be an extremely general fact, a property of invariance which will remain; there will be a group of transformations which will not falsify our equations. These transformations will no longer have the signification of a change of axes, their signification can be any whatever, but the group formed by these transformations must always remain isomorphic to the six-dimensional group which we have just mentioned. Without this there would no longer be any parallelism.

Because this group plays an important role in all cases, because it is isomorphic to the group of changes of axes in ordinary space, and because it is so closely related to our three-dimensional space, for these reasons our equations will assume their simplest form when this group is put forward in the most natural manner, that is, by introducing a three-dimensional space.

And since this group itself is isomorphic to the group of the changes of position of each of the members considered as a solid body, and since this property of solid bodies of moving in obedience to the laws of this group is merely, in the final analysis, a particular case of this property of invariance to which I have just drawn attention, we see that there is no essential difference between the *physical* reason which leads us to attribute three dimensions to space and the psychological reasons developed in the first paragraphs of this chapter.

6. "ANALYSIS SITUS" AND INTUITION

I should like to add a comment which is only indirectly related to what I have already said. We saw above the importance of *analysis situs* and I explained that therein is the true domain of geometric intuition. Does this intuition exist? I shall recall that there have been attempts to get along without it and that Mr. Hilbert sought to establish a geometry which was called rational because it is free from any appeal to intuition. It rests on a certain number of axioms or of postulates which are considered, not as intuitive truths, but as disguised definitions. These axioms are divided into five groups. Concerning four of these groups, I have had occasion to mention to what extent it is legitimate to consider them as containing only disguised definitions.

I should like to stress here one of these groups; the second, that of the "axioms of order." In order to explain fully what is involved, I shall cite one of them. If on any line whatever point C is between A and B, and point D between A and C, point D will be between A and B. According to Mr. Hilbert, there is no intuitive truth in this; we agree to say that in certain cases C is between A and B, but we do not know what this means any more than we know what a point or a line is. We can, according to our rules, use this expression *between* in order to designate any relation whatever among three points, provided that this relation satisfy the axioms of order. These axioms thus seem to us the definition of the word *between.*

It is therefore possible to use these axioms with the condition that it has been proved that they are not contradictory, and it will be possible to base on them a geometry in which figures will not be needed, and which could be understood by a man who possesses neither sight, nor touch, nor muscular sense, nor any of the senses, and which would be reduced to pure understanding.

Yes, this man would probably understand in the sense that he would very well realize that the propositions are logically deduced one from the other; but the collection of these propositions would seem artificial and baroque to him, and he would not understand why this collection is preferred instead of a multitude of other possible collections.

If we do not experience the same astonishment, it is because for us axioms are not really simple definitions and arbitrary conventions, but truly justified conventions. As to the axioms of the other groups, I maintain that they are justified because they are the ones which agree most nearly with certain experimental facts which are familiar to us and are thereby the most convenient for us. As to the axioms of order, it seems to me that there is something more; that they are true intuitive propositions, relating to *analysis situs.* We see that the fact that a point C is *between* two other points on a line is connected with the method of *cutting up* a one-dimensional continuum with the aid of *cuts* formed by impassable points.

But then a question arises: these truths, such as the axioms of order, are revealed to us by intuition; but is this a matter of the intuition of space itself, or of the intuition of the mathematical or physical continuum in general? What would favor the first solution is that we can reason easily about space but with much more difficulty about more complicated continua, about continua of more than three dimensions not capable of being represented in space.

And if this first solution were adopted, this whole discussion would become useless; we would simply attribute three-dimensionality to space because the three-dimensional continuum would be the only one about which we would have a clear intuition.

But there is an *analysis situs* of more than three dimensions. I do not say that it is an easy science; I have devoted too much effort to it not to have taken account of the difficulties which are encountered in it. But nevertheless this science is possible and it does not rest exclusively on analysis. It could not be pursued successfully without continual appeal to intuition. Therefore, there is surely an intuition about the continua of more than three dimensions and if it demands more sustained attention than ordinary geometric intuition it is doubtless a matter of habit and also the effect of the rapidly increasing complication of the properties of the continua when the number of dimensions increases. Do we not see in our high schools pupils who do well in plane geometry but who "cannot visualize space"? It is not that their intuition of three-dimensional space is lacking, but they are not in the habit of using it and they need to make an effort to do so. And moreover, in order to visualize a figure in space, do we not all visualize successively the various possible perspectives of this figure?

I shall conclude that there is in all of us an intuitive notion of the continuum of any number of dimensions whatever because we possess the capacity to construct a physical and mathematical continuum; and that this capacity exists in us before any experience because, without it, experience properly speaking would be impossible and would be reduced to brute sensations, unsuitable for any organization; and because this intuition is merely the awareness that we possess this faculty. And yet this faculty could be used in different ways; it could enable us to construct a space of four just as well as a space of three dimensions. It is the exterior world, it is experience which induces us to make use of it in one sense rather than in the other.

Chapter IV

THE LOGIC OF INFINITY

I. WHAT A CLASSIFICATION MUST BE

May the ordinary rules of logic be applied without change whenever we consider collections comprising an infinite number of objects? This is a question which had not been asked at first, but which we were led to examine when the mathematicians who have specialized in the study of infinity suddenly encountered certain seeming contradictions. Do these contradictions arise from the fact that the rules of logic have been incorrectly applied, or from the fact that they cease to be valid outside their proper domain, namely, the collections formed only of a finite number of objects? I believe that it will not be pointless to say a few words on this subject, and to give my readers an idea of the debates to which this problem has given rise.

Formal logic is nothing but the study of the properties common to all classifications; it teaches us that two soldiers who are members of the same regiment belong by this very fact to the same brigade, and consequently to the same division; and the whole theory of the syllogism is reduced to this. What is, then, the condition necessary for the rules of this logic to be valid? It is that the classification which is adopted be *immutable*. We learn that two soldiers are members of the same regiment, and we want to conclude that they are members of the same brigade; we have the right to do this provided that during the time spent carrying on our reasoning one of the two men has not been transferred from one regiment to another.

The antinomies which have been revealed all arise from forgetting this very simple condition: a classification was relied on which was not immutable and which could not be so; the precaution was taken to *proclaim* it as immutable; but this precaution was insufficient. It was necessary to render it immutable in fact, and there are cases in which this is not possible.

Permit me to refer again to an example cited by Mr. Russell. After all, it was to refute me that he referred to it. He wanted to prove that the difficulties did not arise from the introduction of the actual infinity, since they can be encountered even when only finite numbers are considered. I shall come back to this point later, but this is not the subject under consideration at present, and I choose this example because it is amusing and it emphasizes the fact which I have just indicated.

What is the smallest integer which cannot be defined by a sentence with fewer than one hundred French words? And furthermore does this number exist?

Yes; for, with one hundred French words, we can construct only a finite number of sentences, since the number of words in the French dictionary is limited. Among these sentences, there will be some which will have no meaning or which will not define any integer. But each of these will be capable of defining *at most* a single integer. The number of integers capable of being defined in this manner is therefore limited; consequently, there are certainly some integers which cannot be so defined; and among these integers, there is certainly one which is smaller than all the others.

No; for, if this integer did exist, its existence would imply a contradiction, since it would be defined by a sentence with fewer than one hundred French words; namely, by that very sentence which affirms that it cannot be defined.

This reasoning rests on a classification of integers into two categories: those which can be defined by a sentence with fewer than one hundred French words and those which cannot be. In asking the question, we proclaim implicitly that this classification is immutable and that we begin our reasoning only after having established it definitively. But that is not possible. The classification can be conclusive only when we have reviewed all the sentences with fewer than one hundred words, when we have rejected those which have no meaning, and when we have definitively fixed the meaning of those which possess a meaning. But among these sentences, there are some which can have meaning only after the classification is fixed; they are those in which the classification itself is concerned. In summary the classification of the numbers can be fixed only *after* the selection of the sentences is completed, and this selection can be completed only *after* the classification is determined, so that neither the classification nor the selection can *ever* be terminated.

These difficulties will be encountered even more frequently when infinite collections are involved. Let us suppose that it is desired to classify the elements of one of these collections and that the principle of the classification rests on some relation of the element to be classified with the entire collection. Can such a classification ever be considered as determined? There is no actual infinity, and when we speak of an infinite collection, we understand a collection to which we can add new elements unceasingly (similar to a subscription list which would never end, waiting for new subscribers). For the classification could not properly be completed except when the list was ended; every time that new elements are added to the collection, this collection is modified; it is therefore possible to modify the relation of this collection with the elements already classified; and since it is in accordance with this relation that these elements have been arranged in this or that drawer, it can happen that, once this relation is modified, these elements will no longer be in the correct drawer and that it will be necessary to shift them. As long as there are new elements to be introduced, it is to be feared that the work may have to be begun all over again; for it will never happen that there will not be new elements to be introduced; the classification will therefore never be fixed.

From this we draw a distinction between two types of classifications applicable to the elements of infinite collections: the *predicative* classifications, which cannot be disordered by the introduction of new elements; the *non-predicative* classifications in which the introduction of new elements necessitates constant modification.

Let us suppose for example that we classify the integers into two families according to their size. We can recognize whether a number is greater or less than 10 without having to consider the relations of this number with the set of the other integers. Presumably, after the first 100 numbers have been defined, we shall know which among them are less than and which are greater than 10. When we then introduce the number 101, or any one of the numbers which follow, those among the first 100 integers which were less than 10 will remain less than 10, those which were greater will remain greater; the classification is predicative.

On the contrary let us imagine that we want to classify the points in space and that we differentiate between those which can be defined in a finite number of words and those which cannot. Among the possible sentences, there will be some which will refer to the entire collection, that is, to space or else to some portions of space. When we introduce new points in space, these sentences will change in meaning, they will no longer define the same point; or they will lose all meaning; or else they will acquire a meaning although they did not have any previously. And then points which were not definable will become capable of being defined; others which were definable will cease to be definable. They will have to change from one category into another. The classification will not be predicative.

There are well-meaning persons who believe that the only objects about which one may reason are those which can be defined in a finite number of words, and I would be all the more unwilling not to consider them well-meaning persons, since I myself shall soon defend their opinion. It is therefore possible to consider the preceding example as ill chosen but it is easy to modify it.

In order to classify the integers, or the points in space, I shall consider the sentence which defines each integer or each point. Since it can happen that the same number or the same point can be defined by many sentences, I shall arrange these sentences in alphabetical order and I shall choose the first among these. With this as a condition, this sentence shall end with a vowel or with a consonant, and the classification can be made according to this criterion. But this classification would not be predicative; by the introduction of new integers, or of new points, sentences which had no meaning could acquire one. And then to the list of sentences which define an integer or a point already introduced, it will become necessary to add new sentences, which up to this point were devoid of meaning, have just acquired a meaning, and which define precisely this same point. It can happen that these new sentences assume the first position in the alphabetical order, and that they end with a vowel, whereas the previous sentences ended in a consonant. And then the integer or the point which had been provisionally placed in one category will have to be transferred to another.

If, on the other hand, we classify the points in space according to the size of their coordinates, if we agree to classify together all those whose abscissa is less than 10, the introduction of new points will not change anything in the classification; the points already introduced which satisfied the condition will not cease to satisfy the condition after this introduction. The classification will be predicative.

What we have just said about the classifications applies directly to the definitions. Every definition is, in effect, a classification. It separates the objects which satisfy the definition from those which do not, and it arranges them in two distinct classes. If it proceeds, as Scholastic philosophy has it, *per proximum genus et differentiam specificam*, it rests evidently on the subdivision of the genus into species. A definition, like all classifications, may or may not be predicative.

But here a difficulty is encountered. Let us consider the previous example again. The integers belong to the class A or to the class B, depending on whether they are smaller or larger than 10.5. I have defined certain integers, $\alpha, \beta, \gamma, \ldots$, I have distributed them in these two classes, A and B. I define and introduce new integers. I have said that the distribution was not modified and that consequently the classification was predicative. But in order that the position of number α in the classification not be modified, it is not sufficient that the plan of the classification have not changed; it is also necessary that the number α have remained the same; that is, that its definition be predicative. Therefore, from a certain point of view, we should not say that a classification is predicative in an absolute manner, but that it is predicative in relation to a method of definition.

2. THE CARDINAL NUMBER

We must not forget the preceding considerations when defining the cardinal number. If we consider two collections, we can try to establish a law of correspondence between the objects of these two collections, in such a way that to each object of the first there corresponds one and only one object of the second, and inversely. If this is possible, we say that the two collections have the same cardinal number.

But, here again, it is necessary that this law of correspondence be predicative. If we deal with two infinite collections, it will never be possible to imagine these two collections as exhausted. If we suppose that we have taken a certain number of objects in the first collection, the law of correspondence will enable us to define the corresponding objects of the second. If we then introduce new objects, this introduction of new objects might change the sense of the law of correspondence in such a manner that the object A' of the second collection, which before this introduction corresponded to an object A of the first collection, will no longer correspond to it after this introduction. In this case the law of correspondence will not be predicative.

And that is what we shall explain by means of two opposing examples. I am considering the collection of integers and the collection of even numbers. To each integer n there can correspond the number $2n$. When I introduce new integers, it will always be the same number $2n$ which will correspond to n. The law of correspondence is predicative, and so are all those that Cantor considered in order to prove, for example, that the cardinal number of rational numbers is equal to that of the integers, or that of the points of space is equal to that of the points on a line.

Let us suppose, on the other hand, that we are comparing the set of integers to that of the points in space capable of being defined by a finite number of words and that I establish between them the following correspondence. I shall list all possible sentences. I shall arrange them according to the number of words in them, placing in alphabetical order those which have the same number of words. I shall erase all those which have no meaning or which do not define any point, or which define a point already defined by one of the preceding sentences. To each point I shall have correspond the sentence which defines it, and the *number* which represents the position of this sentence in the revised list.

When I introduce new points, it may happen that some sentences which had been devoid of meaning will acquire one; we will have to replace them in the list from which we had at first erased them; and the sequence number of all the other sentences will be changed. The correspondences will be entirely upset; our law of correspondence is not predicative.

If we did not pay attention to this condition in the comparison of cardinal numbers, we would be led to singular paradoxes. It is therefore necessary to modify the definition of cardinal numbers by specifying that the law of correspondence on which this definition is based must be predicative.

Every law of correspondence is based on a double classification. The objects from the two collections which we wish to compare must be classified; and the two classifications must be parallel. If, for example, the objects from the first collection are divided into classes, which in turn are subdivided into orders, and the latter into families, etc., the same must follow for the objects of the second. To each class of the first classification must correspond one and only one class of the second, to each order an order, and so on, down to the individual objects themselves.

And thus we see what the condition must be for a law of correspondence to be predicative. It is necessary that the two classifications on which the law rests themselves be predicative.

3. MR. RUSSELL'S PAPER

Mr. Russell has published in the *American Journal of Mathematics*, Vol. XXX, under the title *Mathematical Logic as Based on the Theory of Types*, a paper based on considerations entirely analogous to the preceding ones. After calling attention to some of the best known paradoxes among the logicians, he seeks their origin and finds it correctly in a sort of vicious circle. Antinomies have been arrived at because collections were considered which contained objects in whose definition the notion of the collection itself is inherent. Non-predicative definitions have been used; there has been confusion, says Mr. Russell, between the words *all* and *any*, which can be expressed in French by the words *tous* and *quelconque*.

He is thus led to imagine what he calls the *hierarchy of types*. Let us suppose a proposition to be true about *any* individual in a given class. By any individual, we must first understand all the individuals of this class which can be defined without making use of the notion of the proposition itself. I shall refer to them as *any individual of the first order*; when I assert that the proposition is true of all these individuals, I shall assert a *proposition of the first order*. Any individual of the second order will then be an individual whose definition could involve the notion of this proposition of the first order. If I assert the proposition about all the individuals of the second order, I shall have a proposition of the second order. The individuals of the third order will be those whose definition can involve the notion of this proposition of the second order; and so on.

Let us take the example of Epimenides. A liar of the first order will be one who always lies except when he says "I am a liar of the first order"; a liar of the second order will be one who always lies even when he says "I am a liar of the first order," but who no longer lies when he says "I am a liar of the second order." And so forth. And then when Epimenides tells us: "I am a liar," we can ask him: "Of what order?" And it is only after he replies to this legitimate question that his assertion will have meaning.

Let us proceed to a more scientific example and consider the definition of integers. A property is said to be recurrent if it is a property of zero, and if it cannot be a property of n without being a property of $n+1$; we say that all numbers which possess a recurrent property form a recurrent class. Therefore an integer is by definition a number which possesses all the recurrent properties, that is, it belongs to all the recurrent classes.

From this definition can we conclude that the sum of two integers is an integer? It seems so; for, if n is a *given* integer, the numbers x, such that $n+x$ is an integer, form a recurrent class. The number x would therefore not be an integer, if $n+x$ were not. But the definition of this recurrent class about which we have been speaking is not predicative, for in this definition (which teaches us that $n+x$ must be an *integer*) occurs the notion of integers which presupposes the notion of all recurrent classes.

Thus arises the necessity of using the following devious approach: let us consider as recurrent classes of the first order all those which can be defined without introducing the notion of integers, and as integers of the first order the numbers which belong to all the recurrent classes of the first order. Let us next consider as recurrent classes of the second order those which can be defined by introducing as the need arises the notion of integers of the first order, but without introducing the notion of integers of a higher order. Let us call integers of the second order the numbers which belong to all the recurrent classes of the second order, and so forth. And then what we can demonstrate is not that the sum of two integers is an integer, but that the sum of two integers of order K is an integer of order $K-1$.

These examples will suffice, I think, to convey what Mr. Russell calls the hierarchy of types. But then diverse questions arise about which the author did not offer an opinion.

1. In this hierarchy there occur without difficulty propositions of the first, second,... order, etc., and in general of the nth order, n being any finite integer. Is it possible to consider similarly propositions of order α, α being a transfinite ordinal number? It is thus that Mr. König thought of a theory which does not differ essentially from that of Mr. Russell. He makes use of a special system of notation in which he designates by $A(NV)$ the objects of the first order, by $A(NV)^2$ those of the second order, etc., NV being the initials of the expression *ne varietur*. As far as he is concerned, he does not hesitate to introduce $A(NV)^\alpha$, in which α is transfinite, without however explaining sufficiently what he understands by this.

2. If we answer "yes" to the first question, it will be necessary to explain what is understood by the objects of order ω, ω being ordinary infinity, that is, the first transfinite ordinal number; or by objects of order α, α being any transfinite ordinal number.

3. If, on the other hand, we reply "no" to the first question, how shall it be possible to base on the theory of types the distinction between finite or infinite numbers, since this theory is devoid of meaning if it is not assumed that this distinction has already been made?

4. More generally, whether we reply either "yes" or "no" to the first question, the theory of types is incomprehensible, if we do not suppose the theory of ordinal numbers already established. How will it then be possible to base the theory of ordinal numbers on that of types?

4. THE AXIOM OF REDUCIBILITY

Mr. Russell introduces a new axiom which he calls the *axiom of reducibility*. Since I am not sure of having understood his idea perfectly, I shall quote him directly, "We assume that every function is equivalent for all its values to some predicative function of the same argument." But, in order to understand this assertion, it is necessary to refer to the definitions given at the beginning of the paper. What is a function, and what is a predicative function? If a proposition is asserted about a given object *a*, this is a particular proposition; if it is asserted about an indeterminate object *x*, it is a propositional function of *x*. The proposition will be of a certain order in the hierarchy of types, and this order will not be the same whatever *x* may be, since it will depend on the order of *x*. The function will then be said to be predicative if it is of the order $K+1$ when *x* is of order *K*.

Even after these definitions, the meaning of the axiom is still not very clear and a few examples would not be superfluous. Mr. Russell did not give any, and I hesitate to give any of my own because I am afraid to misrepresent his idea which I am not sure of having grasped entirely. But, even without having grasped it, there is one thing which I cannot doubt, and this is that a new axiom is involved. By means of this axiom, it is hoped that the principle of mathematical induction can be proved; I should want all the less to deny the possibility of this as I suspect this axiom to be another form of the same principle.

And then I cannot help but think of all the persons who claim to be proving Euclid's postulates by relying on one of his inferences and by considering this inference as a self-evident truth. What have they gained? However self-evident this truth may be, will it be more so than the postulate itself?

We therefore gain nothing as to the number of postulates. Do we at least gain as to the quality?

In what respect does the new axiom recommend itself over the principle of induction?

First, is it capable of being stated in simpler and clearer terms? It is possible, for the one which Mr. Russell has given us can without doubt be improved; but it is not probable.

Secondly, is the axiom of reducibility more general than the principle of induction, so that it is possible to prove this axiom if one starts from this principle?

Thirdly, is the axiom, on the contrary, less general *in appearance* than the principle, so that we do not perceive immediately that the second is contained in the first, although it is so contained?

Fourthly, does the use of this axiom conform more closely to the natural tendencies of our mind; can it be justified psychologically?

I limit myself to these questions; I lack the means of resolving them since I did not succeed in understanding completely the meaning of this axiom.

But if I cannot hope, with the very limited information given by Mr. Russell, to grasp the meaning completely, I may at least make a few conjectures. Here is a proposition such as, for example, the definition of an integer; a finite integer is a number which is a member of all the recurrent classes. This proposition has no meaning by itself; it would have meaning only if the order of the recurrent classes concerned were specified. But fortunately this happens; every integer of the second order is *a fortiori* an integer of the first order, since it belongs to all the recurrent classes of the first two orders, and consequently to all those of the first order, so also every integer of the Kth order will be *a fortiori* an integer of the $(K-1)$th order. We are thus led to define a series of more and more limited classes, integers of the first, of the second,..., of the nth order, each of which will be contained in the preceding one. I shall call "integer of order ω" every number which belongs to all those classes at the same time; and this definition of the integer of order ω will have a meaning and can be regarded as equivalent to the definition first proposed for the integer and which did not have any meaning. Is that a correct application of the axiom of reducibility, as understood by Mr. Russell? I offer this example timidly.

Let us accept it nevertheless, and let us consider again the theorem to be proved concerning the sum of two integers. We have established that the sum of two integers of the Kth order is an integer of order $K-1$, and we wish to conclude that if x and n are two integers of order ω, the sum $n+x$ is also an integer of order ω. And in fact it suffices for this to establish that it is an integer of order K however large K may be. For, if n and x are integers of order ω, they will be *a fortiori* integers of order $K+1$; therefore, by virtue of the theorem already established, $n+x$ is an integer of order K...

Q.E.D.

Can Mr. Russell's axiom be used in this fashion? I rather feel that this is not exactly so and that Mr. Russell would give an altogether different form to the reasoning, but the basis would remain the same.

I do not wish to discuss here the validity of the method of proof.

I shall limit myself for the moment to the following observations. We have been led to introduce along with the notion of objects of the nth order that of the objects of order ω and we believe we have succeeded, as far as the integers are concerned, in defining this new notion. But this would not always be successful; for Epimenides, for instance, this would not work at all. The following circumstance has assured the success. The classification under study was not predicative, and the addition of the new elements necessitated the modification of the classification of the elements previously introduced and classified. However, this modification could be done only in one direction; it might have been necessary to transfer some objects from class A to class B (namely, from that of the integers to that of non-integers), but never to transfer them from class B to class A. A new convention would be necessary to define the objects of order ω in those cases in which the modification must be made now in one direction, and now in the other.

In the second place, the definition of the integers of order ω *is not the same* as that of the integers of order K, K being finite. The integers of order K are defined *by recurrence* by inferring the notion of integers of order K from the notion of the integers of order $K-1$. The integers of order ω are defined *by passing to the limit*, by making this new notion dependent on an infinite number of previous notions, those of the integers of *all* finite orders. The two definitions would then be incomprehensible to anyone who did not already know what a finite number is; they *presuppose* the distinction between finite and infinite numbers. It is therefore not on these definitions that this distinction can be based.

5. MR. ZERMELO'S PAPER

It is in an altogether different direction that Mr. Zermelo seeks the solution to the difficulties which we have indicated. He strives to posit a system of *a priori* axioms which will permit him to prove all mathematical truths without being exposed to contradictions. There are many ways to view the role of axioms; they can be regarded as arbitrary decrees which are nothing but the disguised definitions of fundamental notions. It is thus that Mr. Hilbert at the beginning of geometry introduces "*things*" which he calls points, straight lines and planes, and that, either forgetting or seeming to forget for an instant the common meaning of these words, he lays down about these *things* various relations which define them.

For this to be legitimate, it is necessary to prove that the axioms thus introduced are not contradictory, and Mr. Hilbert has succeeded perfectly as far as geometry is concerned, because he assumed the analysis to be already established and because he could make use of it in this proof. Mr. Zermelo did not prove that his axioms were exempt from contradictions, and he could not do so, for, in order to do so, he would have had to use as a basis other truths already established. But as for truths already established and a science already completed—he supposes that there are none as yet; he sweeps away everything, and he wants his axioms to be entirely self-sufficient.

The postulates can therefore owe their value only to a sort of arbitrary decree; it is necessary that they be self-evident. Rather than prove this evidence, since evidence cannot be proved, we must consequently seek to penetrate the psychological mechanism which has created this sense of evidence. And this is where the difficulty arises; Mr. Zermelo admits certain axioms and rejects others which at first sight may seem just as evident as those which he retains. If he retained them all, he would fall into contradictions; it was therefore necessary for him to make a choice. But we may wonder what the reasons are for his choice, and this obliges us to be cautious.

Thus he begins by rejecting Cantor's definition: a set is a collection of any distinct objects whatever considered as forming a whole. I therefore do not have the right to speak of the set of all the objects which satisfy this or that condition. These objects do not form a set, a *Menge*, but it is necessary to substitute something for the definition which we reject. Mr. Zermelo limits himself to the statement: let us consider a domain (*Bereich*) of any type of objects; it can happen that between two of these objects x and y, there be a relation of the form $x \varepsilon y$; we will then say that x is an element of y, and that y is a set, a *Menge*.

Evidently this is no definition. Anyone who does not know what a *Menge* is will not know it any better when he learns that it is represented by the symbol ε, since he does not know what ε is. This would be all right if the symbol ε were defined later by the axioms themselves which would be considered as arbitrary decrees. But we have just seen that this point of view was untenable. We must therefore know beforehand what a *Menge* is, we must have an intuitive idea of it. And it is this intuition which will enable us to understand what ε is; without this, ε would be merely a symbol devoid of meaning, and about which we could not assert any self-evident property. But what can this intuition be if it is not Cantor's definition which we have scornfully rejected?

Let us skip this difficulty, which we shall attempt to clarify later, and let us enumerate the axioms assumed by Mr. Zermelo; they are seven in number:

1. Two *Mengen* which have the same elements are identical.
2. There is a *Menge* which does not contain any element, this is

the *Nullmenge*; if there exists an object *a*, there exists a *Menge* (*a*) of which this object is the sole element; if there exist two objects *a* and *b*, there exists a *Menge* (*a*, *b*) of which these two objects are the sole elements.

3. The set of all the elements of a *Menge M* which satisfy a condition *x* form a subset, an *Untermenge*, of *M*.

4. To each *Menge T* there corresponds another *Menge UT*, formed by all the *Untermengen* of *T*.

5. Let us consider a *Menge T* whose elements are themselves *Mengen*; there exists a *Menge ST*, whose elements are the elements of the elements of *T*. If, for example, *T* has three elements *A*, *B*, *C*, which are themselves *Mengen*; if *A* has two elements *a* and *a′*, *B* has two elements *b* and *b′*, *C* has two elements *c* and *c′*, *ST* will have six elements *a*, *b*, *c*, *a′*, *b′*, *c′*.

6. If there is a *Menge T* whose elements are themselves *Mengen*, it is possible to choose an element in each of these elementary *Mengen*, and the set of the elements so chosen forms an *Untermenge* of *ST*.

7. There exists at least one infinite *Menge*.

Before discussing these axioms, I must answer one question: why have I, in stating them, retained the German term *Menge* instead of translating it by the French word *ensemble* [set]? It is because I am not sure that the word *Menge* in these axioms retains its intuitive meaning, without which it would be difficult to reject Cantor's definition; now, the French word *ensemble* suggests this intuitive meaning too forcefully for us to use it without inconvenience when the meaning is altered.

I shall not stress the seventh axiom very much; I must, however, say a few words about it in order to call attention to the very original way in which Mr. Zermelo states it. He does not content himself with the statement which I have given. He says: there exists a *Menge M* which cannot contain the element *a* without also containing as an element the *Menge* (*a*), that is, the one in which *a* is the only element. And so, if *M* admits the element *a*, it will admit a series of others, namely, the *Menge* in which *a* is the only element, the *Menge* in which the only element is the *Menge* in which the only element is *a*, and so forth. It is plain to see that the number of these elements must be infinite. At first sight, this detour seems very strange and artificial, and in fact it is; but Mr. Zermelo wanted to avoid having to use the word infinite, because he considers his axioms as anterior to the distinction between finite and infinite.

Let us consider the first six axioms; they can be regarded as evident, once we assign to the word *Menge* its intuitive meaning and *if only finite numbers of objects are considered.* But they are not any more so than this other axiom which the author expressly rejects:

8. *Objects of any kind form a* Menge.

And so we must ask a question; why does the self-evidence of axiom 8 cease whenever infinite collections are concerned, while that of the first six axioms remains?

If, in order to resolve this question, we go back to the statement of the axioms, we shall experience our first amazement. We shall notice that all these axioms without exception teach us only one thing, that is, that certain collections, formed according to certain laws, constitute *Mengen*; so that these axioms will seem to us merely as rules destined to extend the meaning of the word *Menge*, as pure definitions of words. And this is just as true of the eighth axiom which we reject as of the first seven which we accept.

And yet we are soon warned that this first impression is misleading; similar definitions of words would not lead us to a contradiction; we would have to form a contradiction only if we had other axioms asserting that certain collections *are not Mengen*; and we have none. And yet if we reject the eighth axiom, it is to avoid the contradiction. Mr. Zermelo says so explicitly.

It must therefore be that he did not consider his axioms as simple definitions of words, and that he attributed to the word *Menge* an intuitive meaning existing before all his statements, though differing somewhat from the usual meaning. It is not impossible to notice it when searching for the author's usage of it in his arguments. A *Menge* is something about which we can reason; it is something fixed and unalterable to a certain degree. To define a set, a *Menge*, any collection whatever, is always to make a classification, to separate the objects which belong to this set from those which are not part of it. We will then say that this set is not a *Menge*, if the corresponding classification is not predicative; and that it is a *Menge* if this classification is predicative or if it is possible to reason as if it were.

If we reject the eighth axiom, it is because any objects whatever will without doubt form a collection, but a collection which will never be closed; and whose order can be upset at any instant by the addition of unforeseen elements. It is a collection which is not predicative and, on the contrary, when we say, for example, that to each *Menge* T there corresponds another *Menge* UT or ST defined in this or that manner, we assert that this definition is predicative, or that we have the right to act as if it were.

And this is the place to speak of a distinction which plays an essential role in Mr. Zermelo's theory: "Eine Frage oder Aussage *E*, uber deren Gultigkeit oder Ungültigkeit die Grundbeziehungen des Bereiches vermoge der Axiome und der allgemeingültigen logischen Gesetze ohne Willkür unterscheiden, heisst *definit*." The word *definit* here seems reasonably synonymous with predicative. But the use made of it by Mr. Zermelo shows that the synonymy is not perfect. Thus, let us suppose, for example, that this question *E* be the following: does a certain element of the *Menge M* possess some relation with *all the other* elements of the same *Menge*, and that we agree to say that all the elements for which we must reply *yes* form a class *K*? As for me, and I believe for Mr. Russell also, such a question is not predicative; because *the other* elements of *M* are of infinite number, because it will be possible to introduce new ones ceaselessly, and because among the new elements introduced, there may be some whose definition involves the notion of class *K*; that is, of the set of elements which possess the property *E*. For Mr. Zermelo, this question would be *definit* without my krowing exactly where the exact demarcation is between the questions which are *definit* and those which are not. It would seem to him that, in order to know if an element possesses the property *E* in relation to all the other elements of *M*, it suffices to verify whether it possesses it in relation to each of them. If the question is *definit* in relation to each one of its elements, it would be so *ipso facto* in relation to *all* these elements.

And it is here that the divergence in our views appears. Mr. Zermelo does not allow himself to consider the set of all the objects which satisfy a certain condition because it seems to him that this set is never closed; that it will always be possible to introduce new objects. On the other hand, he has no scruple in speaking of the set of objects which are part of a certain *Menge M* and which also satisfy a certain condition. It seems to him that one cannot possess a *Menge* without possessing at the same time all its elements. Among these elements, he will choose those which satisfy a given condition, and will be able to make this choice very calmly, without fear of being disturbed by the introduction of new and unforeseen elements, since he *already* has all these elements in his hands. By positing beforehand this *Menge M*, he has erected an enclosing wall which keeps out the intruders who could come from without. But he does not query whether there could be intruders from within whom he enclosed inside his wall. If the *Menge M* possesses an infinite number of elements, this means not that these elements can be conceived of as existing beforehand all at once, but that it is possible for new ones to arise constantly; they will arise inside the wall instead of outside, that is all. When I speak of all the integers, I mean all the integers which have been invented and all those which could be invented some day. When I speak of all the points in space, I mean all the points whose coordinates can be expressed by rational numbers or by algebraic numbers, or by integrals, or by any other method that can be invented. And it is this "*that can*" which is the infinity. But it will be possible to invent some which will be capable of being defined in many ways, and if we refer as we did a moment ago to our question E and our class K, the question E arises again each time a new element of M is defined; for, among the elements which we shall be able to define, there will be some whose definition will depend on this class K. So that it will not have been possible to avoid the vicious circle.

This is why Mr. Zermelo's axioms could not be satisfactory to me. Not only do they not seem evident to me, but when I am asked whether they are free from contradictions, I shall not know what to answer. The author believed he was avoiding the paradox of the largest cardinal number by denying himself any speculation beyond the limits of a closed *Menge*. He believed he was avoiding Richard's paradox by asking only questions which are *definit*, and this, according to the meaning which he attaches to this expression, excludes all consideration of objects which can be defined in a finite number of words. But even though he has closed his sheepfold carefully, I am not sure that he has not set the wolf to mind the sheep. I would be at ease only if he had proved that he is safe from contradictions; I know only too well that he could not do so since it would have been necessary to invoke the principle of induction, which he did not call into question but which he proposed to prove later on. He ought to have disregarded it; this would have been at the cost of an error in logic, but at least we would be sure of it.

6. THE ROLE OF INFINITY

Is it possible to reason about objects which cannot be defined in a finite number of words? Is it possible even to speak of them and know what we are talking about and be saying other than empty words? Or, on the contrary, must they be regarded as inconceivable? As for me, I do not hesitate to reply that they are mere nothingness.

All the objects which we will ever encounter will either be defined in a finite number of words or will be only imperfectly determined and will remain indistinguishable from a heap of other objects; and we shall be able to reason appropriately about them only after we have distinguished them from the other objects with which they have been mingled; that is, when we have succeeded in defining them in a finite number of words.

If we consider a set and we wish to define the different elements in it, this definition can be broken down naturally into two parts; the first part of the definition, common to all the elements of the set, will teach us to distinguish them from the elements which are alien to this set; this will be the definition of the set; the second part will teach us to distinguish the different elements of the set from one another.

Each of these two parts will be made up of a finite number of words. If we speak of all the elements of a set whose definition is given, we wish to speak of all the objects which satisfy the first part of the definition and which we shall succeed in defining by means of a sentence made up of whatever finite number of words we may wish. Only one half of the definition is given, you can then complete it by choosing a second half which pleases you; but you must complete it. If I state a proposition concerning all the objects of a set, I mean that if an object satisfies the first part of the definition, the proposition in so far as it concerns this object will remain true, whatever the manner in which you express the second part. But if you can state it as you may wish, it is necessary that you state it; otherwise the object would be inconceivable and the proposition would have no meaning.

It is not impossible to raise a few objections to this point of view, and, indeed, it has been done. Sentences consisting of a finite number of words can always be numbered, since it is possible, for example, to classify them in alphabetical order. If all conceivable objects must be defined by such sentences, it will also be possible to assign a number to them. There would therefore be no more conceivable objects than there are integers; and if we consider space, for example, and if we exclude from it the points which cannot be defined in a finite number of words and which are absolute nothingness, there will remain no more points than there are integers. And Cantor proved the opposite.

This is merely an illusion. To represent points in space by the sentence which serves to define them, to classify these sentences and the corresponding points according to the letters which form these sentences, this is to construct a classification which is not predicative, one which entails all the inconveniences, all the paralogisms, all the antinomies which I mentioned at the beginning of this chapter. What did Cantor mean and what did he really prove? It is not possible to find, among the integers and the points in space which are definable in a finite number of words, a law of correspondence which satisfies the following conditions:

1. This law can be stated in a finite number of words.
2. Being given any integer whatever, it is possible to find the corresponding point in space, and this point will be completely defined without ambiguity; this point's definition, made up of two parts, the definition of the integer and the statement of the law of correspondence, can be reduced to a finite number of words, since this integer can be defined and the law can be stated in a finite number of words.
3. Being given a point P in space which I assume to be defined in a finite number of words (*without denying myself the use of references in this definition to the law of correspondence itself*, which is essential in Cantor's proof), there will exist an integer which will be determined without ambiguity by the statement of the law of correspondence and by the definition of the point P.
4. The law of correspondence must be predicative, that is, if it makes a point P correspond to an integer, it must not cease making this point P correspond to the same integer when new points are introduced in the space. That is what Cantor proved and this still remains true. We note the complicated meaning contained in this brief proposition: the cardinal number of points in space is greater than that of integers.

And then what are we to conclude? Every mathematical theorem must be capable of verification. When I state this theorem, I assert that all the verifications of it which I shall attempt will succeed; and even if one of these proofs requires efforts which exceed the capability of a man, I assert that, if many generations, one hundred if need be, deem it appropriate to undertake this verification, it will still succeed. The theorem has no other meaning, and this is still true if we mention infinite numbers in its statement. But since the verifications can apply only to finite numbers, it follows that every theorem concerning infinite numbers or particularly what are called infinite sets, or transfinite cardinals, or transfinite ordinals, etc., etc., can only be a concise manner of stating propositions about finite numbers. If it is otherwise, this theorem will not be verifiable, and if it is not verifiable, it will be meaningless.

And it follows that there could not be any evident axiom concerning infinite numbers; every property of infinite numbers is nothing more than a translation of a property of finite numbers. It is the latter which could be evident, while it would be necessary to prove the first by comparing it with the latter and by showing that the translation is exact.

7. RÉSUMÉ

The antinomies to which certain logicians have been led arise from the fact that they have been unable to avoid certain vicious circles. This happened when they considered finite collections, but this happened much more often when they laid claim to treating of infinite collections. In the first case, they could easily have avoided the trap into which they fell; or, more exactly, they themselves laid the trap into which they chose to fall, and they were even obliged to be very careful not to miss the trap; briefly, in this case, antinomies are merely toys. Very different are those generated by the notion of infinity; it often happens that the logicians fall into it without doing it on purpose, and even when forewarned, they are still not at ease.

The attempts which have been made to solve these difficulties are interesting for more than one good reason, but they are not entirely satisfactory. Mr. Zermelo wanted to construct an impeccable system of axioms; but these axioms can be regarded only as arbitrary decrees, since it would be necessary to prove that these decrees are not contradictory, and having made an entirely clean sweep there is nothing left on which to base such a proof. It is therefore necessary that these axioms be self-evident. Now, by what mechanicalism were they constructed? Those axioms were taken which are true for finite collections; they could not be extended to all infinite collections, this extension was made for only a certain number among them, chosen more or less arbitrarily. To my mind, moreover, as I said above, no proposition concerning infinite collections can be intuitively evident.

Mr. Russell understood better the nature of the difficulty to be overcome. He did not, however, completely overcome it, because his hierarchy of types supposes that the theory of ordinals has already been enunciated.

As for me, I would propose that we be guided by the following rules:

1. Never consider any objects but those capable of being defined in a finite number of words;
2. Never lose sight of the fact that every proposition concerning infinity must be the translation, the precise statement of propositions concerning the finite;
3. Avoid non-predicative classifications and definitions.

All the researches so far mentioned have a common characteristic. They propose to teach mathematics to a pupil who does not yet know the difference that exists between infinity and the finite; they do not hasten to teach him what this difference consists of; they begin by teaching him all that can be known about infinity without being concerned about this distinction. Then in a remote region of the field in which they made him wander, they show him a small corner where the finite numbers are hidden.

This seems to me psychologically false; the human mind does not proceed naturally in this manner, and even though we might extricate ourselves without too many antinomical mishaps, this method would be no less contrary to sound psychology.

Mr. Russell will tell me no doubt that it is not a question of psychology, but of logic and epistemology; and I shall be led to answer that there is no logic and epistemology independent of psychology; and this profession of faith will probably close the discussion because it will make evident an irremediable divergence of views.

Chapter V

MATHEMATICS AND LOGIC

A few years ago I had occasion to set forth certain ideas about the logic of infinity, on the role of infinity in mathematics, and the use made of it since Cantor's time. I explained why I did not consider as legitimate certain methods of reasoning which various eminent mathematicians had believed they could employ.[1] Naturally I drew some sharp replies. These mathematicians did not believe they had erred; they believed they had the right to do what they had done. The discussion dragged on, not because new arguments arose ceaselessly, but because we kept going around in the same circle, each one repeating what he had just said, seemingly not having heard what the opponent had said. On each occasion, I was sent a new proof of the principle under contention in order, it was said, to be safe from all objection; but this proof was always the same, hardly revised. No conclusion consequently was reached. If I should say that I was surprised, I would convey a false impression of my psychological acumen.

Under these conditions, it hardly seems advisable to repeat once more the same arguments to which I could probably give a new form but which I could not change fundamentally, since it seems to me that my opponents have not even tried to refute them. It seems preferable to seek what can be the origin of this difference of mentality which engenders such divergent views. I have just said that these irreducible divergences had not astonished me, that I had foreseen them from the very beginning. But this does not exempt us from seeking the explanation; it is possible to foresee a fact after repeated experiences, and yet be very hard pressed to explain it.

Let us attempt therefore to study the psychology of the two opposing schools from a purely objective point of view just as if we ourselves were not a member of these schools, as if we were describing a war between two ants' nests. We shall first of all observe that there are two opposite tendencies among mathematicians in their manner of considering infinity. For some, infinity is derived from

[1] See chap. IV.

the finite; infinity exists because there is an infinity of possible finite things. For others, infinity exists before the finite; the finite is obtained by cutting out a small piece from infinity.

A theorem must be capable of proof, but since we ourselves are finite, we can only deal with finite objects. Thus even though the notion of infinity plays a role in the statement of the theorem, there must be no reference to it in the proof; otherwise this proof would be impossible. I shall cite the following theorems as examples: the set of prime numbers is without bound; the series $\sum 1/n^2$ is convergent, etc. Each one of these can be translated into equalities or inequalities in which only finite numbers are involved. These theorems partake of infinity not because one of the possible proofs itself partakes of infinity but because the possible proofs are infinite in number.

In stating a theorem, I affirm that all its proofs will hold true. It is understood that not all proofs are given in full. There are some which I consider possible because they would require only a finite length of time, but which would be *practically* impossible because they would require years of work. I am satisfied if we can conceive of someone being rich and foolish enough to attempt it by hiring a sufficient number of aides. The proof of a theorem has as its very object to make this folly unnecessary.

Does a theorem which does not result in any verifiable conclusion have a meaning? Or, more generally, does any theorem whatever have a meaning apart from the proofs which it involves? This is where the mathematicians differ. Those of the first school, whom I shall call *pragmatists* (since it is necessary to assign them a name) say no; and when a theorem is brought to their attention without giving them a means of verifying it, they see in it only unintelligible verbiage. They wish to consider only objects which can be defined in a finite number of words. When in an argument an object A is mentioned as satisfying certain conditions, they understand an object which satisfies these conditions, whatever may be the words used to complete its definition, provided these words are finite in number.

Those of the other school, whom I shall call for short *Cantorians*, do not wish to allow this. A man, however talkative he may be, will never in his lifetime utter more than a billion words. Consequently, shall we exclude from science the objects whose definition contains one billion and one words? And if we do not exclude them, why should we exclude those which can be defined only by an infinite number of words, since the formulation of the first type of definitions is, like that of the second, beyond the scope of mankind?

This argument understandably leaves the pragmatists cold; however talkative a man may be, mankind will be still more talkative and, since we do not know how long mankind will last, we cannot limit beforehand the field of its investigations. We merely know that this field will always remain limited; and even though we might be able to determine the date of its disappearance, there are other celestial bodies which could take up the work left unfinished on Earth. The pragmatists, moreover, would have no qualms in imagining a mankind much more talkative than ours, but still retaining something human; they refuse to argue on the hypothesis of some infinitely talkative divinity capable of thinking of an infinite number of words in a finite length of time. And the others, on the other hand, think that objects exist in a sort of large store, independently of any mankind or of any divinity that could talk or think about them; that in this store we can choose freely; that no doubt we do not have a good enough appetite nor enough money to buy everything; but that the inventory of the store is independent of the resources of the buyers. And from this initial misunderstanding all sorts of divergences in detail result.

Let us take for example Zermelo's theorem according to which space is capable of being transformed into a well-ordered set. The Cantorians will be charmed by the rigor, real or apparent, of the proof. The pragmatists will answer:

"You say that you can transform space into a well-ordered set. Well! Transform it!"

"It would take too long."

"Then at least show us that someone with enough time and patience could execute the transformation."

"No, we cannot, because the number of operations to be performed is infinite; it is even greater than aleph zero."

"Can you indicate how the law which would permit space to be well-ordered can be expressed in a finite number of words?"

"No."

And the pragmatists conclude that the theorem is devoid of meaning or false or at least not proved.

The pragmatists adopt the point of view of extension, and the Cantorians the point of view of comprehension. When a finite collection is concerned, this distinction can be of interest only to the theorists of formal logic; but this distinction seems to us much more profound when infinite collections are concerned. If we adopt the point of view of extension, a collection is formed by the successive addition of new members; we can construct new objects by combining old objects, then with these new objects construct newer ones, and if the collection is infinite, it is because there is no reason for stopping.

From the point of view of comprehension, on the other hand, we begin with the collection in which there are pre-existing objects, which seem indistinct at first but a few of which we finally recognize because we label them and we arrange them in drawers. But the objects existed prior to being labeled and the collection would exist even though there might not be a curator to classify them.

As for the Cantorians the notion of cardinal numbers does not entail any mystery. Two collections have the same cardinal number when they can be arranged in the same drawers; nothing could be easier, since the two collections exist beforehand and it is equally possible to consider as pre-existing a collection of drawers independent of the curators charged with the task of arranging the objects. As for the pragmatists, this is not so. The collection does not exist beforehand; it increases each day; new objects become associated with it ceaselessly which one could not define without reference to the notion of the objects already classified previously and to the manner in which they are classified. At each new acquisition, the curator may be forced to upset the drawers in order to find a means of setting it in the proper order; it will never be known if two collections can be arranged in the same drawers since there is always the fear that it will be necessary to upset them.

For example, the pragmatists admit only objects which can be defined in a finite number of words; the possible definitions, which can be expressed in sentences, can always be numbered with ordinary numbers from one to infinity. According to this reckoning, there would be only a single infinite cardinal number possible, the number Aleph-zero. Why, then, do we say that the power of the continuum is not the power of the integers? Yes, being given all the points in space which we can define with a finite number of words, we can imagine a law which is itself capable of being expressed in a finite number of words and which establishes a correspondence between them and the set of integers. But let us now consider sentences in which the notion of this law of correspondence is involved. A moment ago these sentences had no meaning since this law had not yet been invented, and they could not serve to define points in space. Now they have acquired a meaning; they will permit us to define new points in space. But these new points will not find any room in the classification already adopted, and this will compel us to upset it. And this is what we mean, according to the pragmatists, when we say that the power of the continuum is not the power of the integers. We mean that it is impossible to establish between these two sets a law of correspondence which will be free from this sort of disruption; whereas it is possible to do it, for example, when a straight line and a plane are involved.

And then the pragmatists are not certain that any set whatever has, properly speaking, a cardinal number; or that, two sets being given, it is always possible to know if they have the same power or if one has a greater power than the other. They are thus led to doubt the existence of Aleph-one.

Another source of divergence arises from the manner of conceiving the definition. There are many kinds of definitions; the direct definition which can be derived either by *genus proximum et differentiam specificam* or by composition.

Let us note in passing that there are definitions which are incomplete in the sense that they do not define a particular thing but rather an entire genus. They are legitimate and they are even the ones most frequently used. But according to the pragmatists it is necessary to understand therein the set of the particular objects which satisfy the definition and which could finally be defined in a finite number of words. For the Cantorians this restriction is artificial and devoid of meaning.

If there were only direct definitions, the impotence of pure logic could not be contested. It would then be possible in any proposition whatever to substitute its definition for each of the terms. And when this substitution was completed, either the proposition could not be reduced to an identity and then would not be capable of a purely logical proof, or else it could be reduced to an identity and then would be merely a more or less cleverly disguised tautology.

But there is yet another kind of definition; definitions by postulates. Generally, we will know that the object to be defined belongs to a genus; but when it is a question of stating the specific difference, it will not be stated directly but with the aid of a "postulate" which the object being defined must satisfy. It is thus that mathematicians can define a quantity x by means of an explicit equation $x=f(y)$ or of an implicit equation $F(x, y)=0$.

A definition by postulate has value only when the existence of the object defined has been proved. In mathematical language, this means that the postulate does not imply a contradiction; we do not have the right to neglect this condition. Either it is necessary to admit the absence of contradiction as an intuitive truth, as an axiom, by a kind of act of faith—but then it is necessary to realize what we are doing and to remember that we have extended the list of undemonstrable axioms—or else it is necessary to construct a formal proof, either by means of rules or postulates or by the use of reasoning by recurrence. Not that this proof is less necessary when a direct definition is involved, but it is generally easier.

Some pragmatists will be more exacting; in order for them to consider a definition as legitimate, it is not sufficient that it does not lead to contradictions in terms; they will require that it have a meaning according to their particular point of view which I tried to define above.

However that may be, will logic remain sterile after the introduction of definitions by postulates? A proposition having been given, we can no longer substitute in it the definition for a term. All we can do is to *eliminate* this term between the proposition and the postulate which serves as its definition. If this operation, carried out according to what could be called the rules of logical elimination, does not lead to an identity, it is because the proposition cannot be proved by means of pure logic. If it leads to an identity, it is because the proposition is merely a tautology. We need not change anything in our conclusions of a few moments ago.

But there is a third type of definitions, and this is the origin of a new misunderstanding between the pragmatists and the Cantorians. These are again definitions by postulates, but the postulate here is a relation between the object to be defined and *all* the individual objects of a genus of which the object to be defined is itself supposed to be a member (or of which one supposes to be members objects which themselves can be defined only by the object to be defined). This is what happens if we posit the two following postulates:

X (object to be defined) is related in such and such a way to *all* members of the genus *G*.
X is a member of *G*.

Or else the three following postulates:

X is related in such and such a way to *all* the members of the genus *G*.
Y is related in such and such a way to *X*.
Y is a member of *G*.

To the pragmatists such a definition implies a vicious circle. It is not possible to define *X* without knowing all the members of the genus *G*, and consequently without knowing *X* which is one of these members. The Cantorians do not admit this; the genus *G* is given, consequently we know *all* its members. The definition has as purpose only to *distinguish* from among these members the one which has the expressed relation with all its fellow-members.

"No," reply their opponents, "the knowledge of the genus does not result in your knowing all its members; it merely provides you with the possibility of constructing them all, or rather of constructing as many of them as you may wish. They will exist only after they have been constructed; that is, after they have been defined; X exists only by virtue of its definition, which has meaning only if all the members of G are known beforehand, and X in particular. It would be useless to say," they add, "that it is not a vicious circle to define X by its relation to X; that this relation is, in short, a postulate which can be used to define X; for it would be necessary to establish beforehand that this postulate does not imply a contradiction. But ordinarily that is not what is done in this type of definitions. We prove first of all that whatever genus G, all of whose members are supposedly known, may be, there exists an object X which bears with this genus the relation in question; that is, that the existence of this object does not result in a contradiction. There would remain to show that there is no contradiction between the existence of this object and the hypothesis and this object is itself a member of the genus."

The debate could go on for a long time; but the point which I would like to emphasize is that if this type of definition were permitted, logic would no longer be sterile, and the proof is that a multitude of arguments have been formulated in this manner which are destined to prove propositions which were in no way tautologies since there are persons who wonder if they are not false. Therefore, we are amazed at the power which a word can possess. Here is an object from which nothing could be derived until it had been christened; all it needed was to be given a name and it worked wonders. How can this happen? It is because by giving it a name we have asserted implicitly that the object did exist (that is, was free from all contradiction) and that it was entirely determined. But we do not know this at all, according to the pragmatists. What is, indeed, the mechanicalism which makes this proof fruitful? It is very simple; we assume that the proposition to be proved is false and we show that this results in a contradiction with the fact that the object X exists. And this is legitimate only if we are certain of its existence and, moreover, if we know that the object is entirely determined. And in fact if X is deduced from the genus G by definition; if, next, the genus G is completed by including the object X and all the other members of the same genus which can be derived from it; if the genus thus completed is called G' and if we call that X' which could be derived from G' by definition in the same manner that X was derived from G, it is necessary to be sure that X' is identical to X. If this were not so and if by assuming as false the proposition to be proved we were led to two contradictory statements

$$\varphi_1(X) = 0, \qquad \varphi_2(X) = 0,$$

how would we know that it is the same X which is involved in both? If X were involved in one and X' in the other, the two propositions would be written

$$\varphi_1(X) = 0, \qquad \varphi_2(X') = 0,$$

and would no longer be contradictory in general.

Therefore, why do the pragmatists raise this objection? Because the genus G seems to them to be only a collection capable of being increased indefinitely, whenever new members are formed which possess appropriate characteristics. Thus G can never be posited *ne varietur* as the Cantorians do, and we are thus not sure that it will not become G' by means of new annexations.

I have endeavored to explain as clearly and as impartially as I could the nature of the divergences between the two schools of mathematicians. And it seems to me that we can already perceive the true cause. The scientists of the two schools have opposite mental tendencies. Those whom I have called pragmatists are idealists, and the Cantorians are realists.

There is one thing which will bear out this point of view. We see that the Cantorians (let me use this convenient term even though I do not wish to speak here of the mathematicians who follow in Cantor's steps, nor even perhaps of the philosophers who identify themselves with him, but of those who have the same tendencies in an independent fashion), that the Cantorians, as I was saying, speak constantly of epistemology; that is, the science of sciences. And it is well understood that this epistemology is completely independent of psychology; that is, that it must teach us what the sciences would be if there were no scientists; that we must study the sciences, not of course with the supposition that there are no scientists, but at least without the supposition that there are. Thus not only is Nature a reality independent of the physicist who could be tempted to study it, but physics itself is also a reality which would exist even if there were no physicists. This is realism indeed.

And why do the pragmatists refuse to permit objects which could not be defined in a finite number of words? It is because they believe that an object exists only when it is conceived by the mind and that an object could not be conceived by the mind independently of a being capable of thinking. There is indeed idealism in that. And since a rational subject is a man, or something which resembles man, and consequently is a finite being, infinity can have no other meaning than the possibility of creating as many finite objects as we wish.

And then we can make a somewhat peculiar remark. Realists ordinarily adopt the point of view of physics. They affirm the independent existence of material objects or of individual souls, or what they call substances. For them, the world existed before the creation of man, even before the creation of living beings; it would still exist even if there were no God nor any rational being. That is the point of view of common sense, and it is only through reflection that we can be led to abandon it. The partisans of physical realism are generally finitists. As to the question of Kantian antinomies, they go along with the theses; they believe that the world is finite. This is, for example, the point of view of Mr. Evellin. On the other hand, the idealists do not have the same scruples and are quite ready to subscribe to the antitheses.

But the Cantorians are realists even where mathematical entities are concerned. These entities seem to them to have an independent existence; the geometer does not create them, he discovers them. These objects therefore exist so to speak without existing, since they can be reduced to pure essences. But since, by nature, these objects are infinite in number, the partisans of mathematical realism are much more infinitist than the idealists. Infinity to them is no longer a becoming since it exists before the mind which discovers it. Whether they admit or deny it, they must therefore believe in actual infinity.

We recognize in this the theory of ideas of Plato; and it may seem strange to see Plato classified among the realists. There is nevertheless nothing more opposed to contemporary idealism than Platonism, even though this doctrine is also far removed from physical realism.

I have never known a more realistic mathematician, in the Platonic sense, than Hermite, and yet I must admit that I have never met one more opposed to Cantorism. There is in this a seeming contradiction, all the more so since he repeated readily: "I am anti-Cantorian *because* I am a realist." He blamed Cantor for creating objects instead of being content to discover them. It was no doubt due to his religious convictions that he considered it a sort of impiety to wish to penetrate without difficulty a domain which God alone can comprehend and not to wait until He reveals its mysteries to us one by one. He compared the mathematical sciences to the natural sciences. A naturalist who sought to guess God's secret instead of learning through experience would have seemed to him not only presumptuous but disrespectful of the Divine Majesty. To him the Cantorians seemed to want to act in the same manner in mathematics. And that is why he was an idealist in practice whereas he was a realist in theory. There is one reality to be known and it is exterior to us and independent of us; but all that we can know of it depends on us, and is then merely a becoming, a kind of stratification of successive conquests. The rest is real but eternally unknowable.

Hermite's is, at any rate, an isolated case and I do not wish to dwell on it further. At all times, there have been opposite tendencies in philosophy and it does not seem that these tendencies are on the verge of being reconciled. It is no doubt because there are different souls and that we cannot change anything in these souls. There is therefore no hope of seeing harmony established between the pragmatists and the Cantorians. Men do not agree because they do not speak the same language, and there are languages which cannot be learned.

And yet in mathematics men ordinarily understand one another; but it is due precisely to what I have called proofs. These proofs pass judgment without appeal and before them the entire world bows. But wherever these proofs are lacking, mathematicians are no better off than simple philosophers. When it is necessary to know if a theorem can have meaning without being capable of proof, who can judge, since by definition we forbid ourselves to prove it? There would be no other resource but to corner one's adversary with a contradiction. But the experiment has been attempted and it has not succeeded.

Many antinomies have been pointed out, and the discord has remained; no one has been convinced. It is always possible to extricate oneself from a contradiction by a change of arguments; I mean by a *distinguo.*

Chapter VI

THE QUANTUM THEORY

One may wonder if mechanics is not on the eve of a new commotion. A congress of about twenty physicists from different countries assembled recently in Brussels, and at every moment they could be heard talking of the new mechanics which they contrasted with the old mechanics. Now, what was that old mechanics? Was it that of Newton, the one which still reigned uncontested at the close of the nineteenth century? No, it was the mechanics of Lorentz, the one dealing with the principle of relativity; the one which, hardly five years ago, seemed to be the height of boldness.

Does this mean that this mechanics of Lorentz has had only an ephemeral fortune; that it has been merely a vagary, and that we are about to return to the ancient gods whom we had imprudently abandoned? Not in the least. The conquests of yesterday are not jeopardized. In all instances in which it differs from that of Newton, the mechanics of Lorentz endures. We continue to believe that no body in motion will ever be able to exceed the speed of light; that the mass of a body is not a constant, but depends on its speed and the angle formed by this speed with the force which acts upon the body; that no experiment will ever be able to determine whether a body is at rest or in absolute motion either in relation to absolute space or even in relation to the ether.

To these strokes of boldness, however, we wish to add more, and much more disconcerting ones. We now wonder not only whether the differential equations of dynamics must be modified, but whether the laws of motion can still be expressed by means of differential equations. And therein would lie the most profound revolution that natural philosophy has experienced since Newton. The brilliant genius of Newton had seen (or had believed it saw, we begin to wonder) that the state of a system in motion, or more generally that of the universe, could depend only on its immediately preceding state; that all variations in nature must take place in a continuous manner. Most certainly, he was not the one who invented this idea; it existed in the thought of the ancients and the

Scholastics, who proclaimed the adage: *Natura non facit saltus*; but it was strangled there by a multitude of weeds which prevented it from developing and which the great philosophers of the seventeenth century finally cut away.

Well, it is this fundamental idea which is today in question. It is being asked whether it is not necessary to introduce discontinuities into the natural laws, not apparent ones but essential ones; and we must explain first of all how such an extraordinary point of view could have come into being.

I. THERMODYNAMICS AND PROBABILITY

Let us refer to the kinetic theory of gases. Gases are made up of molecules which move in all directions with great velocities. Their trajectories would be rectilinear if they did not from time to time collide with one another or if they did not strike the walls of the vessel. The chances of these collisions finally establish a certain average distribution of the velocities, whether we consider their direction or whether we consider their magnitude. This average distribution tends to re-establish itself whenever it is disturbed; so that, in spite of the inextricable complication of the movements, the observer who can recognize only averages notices only very simple laws which are the effect of the play of probabilities and large numbers. He observes the *statistical equilibrium*. It is thus, for example, that the velocities will be equally distributed in every direction; for, if they ceased to be so distributed for one instant, if they tended to assume a common direction, at the end of a very short time the collisions would cause them to lose this common direction.

The calculations lead to another consequence. The average kinetic energy which each molecule will generate is proportional to its number of degrees of freedom. To explain: a body can assume a certain number of very small and different movements. For example, a material point can move along the three axes; it has three degrees of freedom. A sphere can undergo a translation parallel to each of the three axes, or, again, a rotation around these three axes. It has six degrees of freedom. But a molecule is not a simple material point; it is susceptible to deformation; it will therefore have many degrees of freedom. For example, a molecule of argon will have 3, a molecule of oxygen will have 5. Thus, according to the law which we express and which is called the *principle of equipartition of energy*, if according to the statistical equilibrium a molecule of argon possesses at a certain temperature a kinetic energy of 3, a molecule of oxygen must possess a kinetic energy of 5. In other words, the specific molecular heats of argon and of oxygen at a constant volume must be respectively as 3 is to 5.

And this law, correctly interpreted, is true not only for gases; it arises in fact from the very form which always has been attributed to the equations of dynamics and which is the form according to Hamilton. If the general laws of dynamics are applicable to liquids and to solids, these bodies must obey the principle of equipartition of energy, *mutatis mutandis*.

Carnot's principle, or the second principle of thermodynamics, teaches us that the world is tending toward a final state from which it will no longer be able to deviate. It teaches us therefore that the statistical equilibrium is possible. If it were not, it would always be possible to find some clever expedient which would permit us to achieve what has been called perpetual motion of the second type, making it possible for example to heat a steam engine with ice by taking advantage of the fact that this ice, cold as it may be, is not in fact at absolute zero and consequently contains a certain amount of heat. If the laws of statistical equilibrium were not the same when the two bodies *A* and *B*, or *B* and *C*, or finally *C* and *A* are placed opposite each other, it would be easy to change the conditions of this equilibrium ceaselessly by bringing closer together first two of these bodies, then two others. These bodies would thus never come to complete rest, and there would not be any true statistical equilibrium. Carnot's principle would be false.

By what singular coincidence are the conditions of this equilibrium always the same, whatever the bodies placed opposite each other may be? The preceding remarks make it clear. It is because the general laws of dynamics, expressed in Hamilton's differential equations, apply to all bodies.

These conceptions up to now have always been confirmed by experiment, and the proofs today are numerous enough that they cannot be attributed to chance. It will therefore be necessary, if new experiments reveal exceptions, not to abandon the theory but to modify it, to make it more comprehensive so as to permit it to include new facts.

Not that certain objections did not, from the very first day, occur at all. Molecules, atoms themselves, are not material points. If they possess dimensions, is it permissible to liken them to *absolutely* rigid bodies? Or again, however simple a molecule of argon may be, it cannot be a mathematical point; it will be a sphere. Why could not this sphere rotate? If it does rotate, this will result in 6 degrees of freedom instead of 3.[1] Unless it is supposed that the collisions, capable of modifying the translation of the molecule, are absolutely without influence on its rotation, that they cannot cause this molecule to undergo the least deformation, etc. Besides, each line of the spectrum corresponds to one degree of freedom. It is superfluous to say that the spectrum of oxygen consists of more than 5 lines. Why do certain degrees of freedom seem to play no role; why are they, so to speak, ankylosed as long as no mysterious circumstances intervene?

2. THE LAW OF RADIATION

The physicists at first were not engrossed with these difficulties, but two new facts changed the face of things. The first is the one called the law of *black radiation.* A perfectly black body is one whose coefficient of absorption is equal to 1; a similar body raised to incandescence emits light of all wave lengths, and the intensity of this light varies according to a certain law as a function of the temperature and of the wave length. Direct observation is not possible because there is no body which is perfectly black, but there is a means of overcoming this difficulty. We can place the incandescent body in a completely sealed enclosure; the light which it emits cannot escape and undergoes a series of reflections until it is completely absorbed. When the state of equilibrium is reached, the temperature of the enclosure has become uniform and the enclosure is filled with a radiation which obeys the law of black radiation.

It is clear that this is a case of statistical equilibrium, the exchanges of energy having taken place until each part of the system has gained on an average, in a short lapse of time, exactly what it has lost. But this is where the difficulty begins. The material molecules contained in the enclosure are finite in number, even though exceedingly numerous, and they have only a finite number of degrees of freedom. On the other hand, the ether has an infinite

[1] Nothing would be gained by saying that the ratio of the specific heats would not be changed by attributing 6 degrees of freedom to argon and 10 to oxygen. It is truly 3 degrees of freedom and not 6 which are required by the kinetic theory of gases based on the theorem of the virial.*

* [Translator's note: "*Virial.* .. The sum of the attractions between all the pairs of particles of a system, each multiplied by the distance between the pair.—*Theorem of the virial*, the proposition that when a system of particles is in stationary motion its mean kinetic energy is equal to its virial," p. 6765, *The Century Dictionary and Cyclopedia*, Volume X, The Century Company, New York, 1911 (revised and enlarged edition).]

number, for it can vibrate in an infinite number of ways corresponding to the different wave lengths with which the enclosure is in resonance. If the principle of equipartition of energy were applicable, the ether should therefore absorb all the energy and leave none to matter.

It would be possible to limit the freedom of the ether by imposing relationships upon it which would, for example, make it incapable of transmitting wave lengths which are too short. The contradiction just indicated would thus be avoided, but a law would still be arrived at which, in order for it not to be absurd, would again be contradicted by experiment. This is Rayleigh's law, according to which radiated energy, for a given length, would be proportional to the absolute temperature and, for a given temperature, in an inverse ratio to the fourth power of the wave length.

The true law, proved by experiment, is Planck's law. Radiation is much less for short wave lengths or for low temperatures than Rayleigh's law demands, in conformity to the principle of equipartition of energy.

The second fact arises from the measure of the specific heats of solid bodies at very low temperatures in liquid air or in liquid hydrogen. These specific heats, far from being perceptibly constant, decrease rapidly as if to cancel each other at absolute zero. Everything takes place as if the molecules lost degrees of freedom in the process of cooling, as if a few of their bonds ended by freezing.

3. THE QUANTA OF ENERGY

The explanation of these phenomena must be sought without discarding the principles of thermodynamics. It is necessary first of all to allow the possibility of statistical equilibrium without which nothing would be left of Carnot's principle. In thermodynamics no breach can be permitted without everything collapsing. Mr. Jeans has sought to reconcile everything by supposing that what we observe is not a definitive statistical equilibrium but a sort of temporary equilibrium. It is difficult to accept this point of view. His theory, anticipating nothing, is not contradicted by experiment, but it does not explain all the known laws, which it avoids contradicting and which then seem merely the effect of some happy chance.

Mr. Planck sought another explanation of the law which he had discovered. According to him, it is a matter of a true equilibrium and, if it does not conform to the principle of equipartition of energy, it is because Hamilton's equations are not exact. In order to arrive at the experimental law, it is necessary to introduce a very surprising modification into these equations. How must we imagine a radiant body? We know that a Hertz resonator sends into the ether Hertzian waves which are nothing more than light waves; an incandescent body will therefore be considered as containing a very large number of small resonators. When the body becomes heated, these resonators acquire energy, begin to vibrate and consequently to radiate heat.

Mr. Planck's hypothesis consists in supposing that each of these resonators can acquire or lose energy only by *sudden jumps* so that the amount of energy which it possesses must always be a multiple of the same constant quantity called "quantum," and that it must be made up of an integral number of *quanta*. This indivisible unit, this quantum, is not the same for all resonators; it is in inverse ratio to the wave length, so that the resonators of short periods can swallow energy only in big chunks, whereas the resonators of long periods can absorb or emit it only in little mouthfuls. What then is the result? Much effort is required to disturb a resonator with a short period since a quantity of energy is required at least equal to its quantum, which is large. There are therefore many chances that these resonators will remain at rest, especially if the temperature is low, and it is for this reason that there will be relatively little light of short wave length in the black radiation.

This hypothesis accounts well for the facts, provided that we allow that the relation between the energy of the resonator and its radiation is the same as in the older theories. And therein lies a major difficulty. Why save this, when everything else has been destroyed? But we must save something, otherwise we would have nothing on which to build.

The decrease of the specific heats can be explained in the same manner: when the temperature decreases, a very large number of vibrators fall below their quantum and, instead of vibrating slightly, no longer vibrate at all, so that the total energy diminishes faster than in the former theories. That is merely a qualitative insight but it is not necessary to effect an exaggerated number of changes in order to obtain a sufficiently quantitative concordance.

4. DISCUSSION OF THE PRECEDING HYPOTHESIS

The statistical equilibrium can be established only if there is an exchange of energy between the resonators without which each resonator would maintain indefinitely its initial energy, which is arbitrary, and the final distribution would not obey any law. This exchange could not take place by radiation if the resonators were stationary and were shut in a stationary enclosure. In fact, each resonator could only emit or absorb light of a definite wave length, and it could therefore send out energy only to the resonators of the same period.

The same is no longer true if we suppose that the enclosure can be deformed or that it contains bodies in motion. And in fact light, when being reflected in a moving mirror, changes its wave length by virtue of the well-known Döppler-Fizeau principle. And therein is a first method of exchange by radiation.

There is a second one; the resonators can react mechanically on one another, either directly or rather through the medium of atoms in motion and electrons which travel from one to the other and which collide with them. This is exchange by collision. It is the one which I have studied recently, rediscovering and confirming Mr. Planck's results.

As I have explained above, all methods of exchange of energy must lead to the same conditions of statistical equilibrium without which Carnot's principle would be lacking. This is necessary in order to account for experience, but it is still necessary that we be able to give a satisfactory explanation for this suprising concordance, that we be not forced to attribute it to some sort of providential chance. In the older mechanics this explanation was completely known; it was the universality of Hamilton's equations. Shall we find something analogous here?

I have not yet completed the study of exchange by radiation, and I do not know yet if all the conditions of equilibrium to which this type of exchange leads are known. I would not be surprised if new ones were discovered which could cause us some difficulty.

For the present, there is one which has been revealed by the work of Mr. Wien. It is what is called Wien's law, according to which the product of radiant energy by the fifth power of the wave length depends only on the temperature multiplied by the wave length.

It is seen immediately that in order for this law of Wien to be compatible with the statistical equilibrium due to exchange by collision, it is necessary that in this exchange by collision, the energy should be able to vary only by quanta which are *inversely proportional to the wave length.* That is a *mechanical* property of the resonators which is evidently completely independent of the Döppler-Fizeau principle, and it is not well understood by what mysterious, pre-established harmony these resonators have been endowed with the only mechanical property which could be suitable. If the statistical equilibrium is invariable, it is no longer for a unique and universal reason; it is by the combination of multiple and independent circumstances.

In Mr. Planck's method of exposition, this duality of the methods of exchange does not appear, but it is merely hidden; and I thought it necessary to call attention to this fact.

This is not the only difficulty. A resonator can yield energy to another only in integral multiples of its quantum; the latter can receive energy only in integral multiples of its own quantum. Since these two quanta are generally incommensurable, this is sufficient to exclude the possibility of a direct exchange. But the exchange can take place through the medium of atoms, if we suppose that the energy of these atoms can vary in a continuous manner.

This is not the most serious difficulty. The resonators must lose or gain each quantum *suddenly*, or rather they must gain their whole quantum or nothing at all. And yet they require a certain length of time either to gain or to lose it; and this is necessarily so according to the phenomenon of interferences. Two quanta emitted by the same resonator at different instants could not interfere with one another. The two emissions should in fact be considered as two independent phenomena and there would be no reason for the interval of time which separates them to be constant. This is even impossible; this interval must be greater if the light is weak than if it is intense; unless it is supposed that the interval is constant, that each emission can consist of several quanta and that the intensity depends on the number of quanta emitted simultaneously. But this cannot take place either. The interval must be small in relation to a period in order to agree with the observations of interference; the value of the quantum arises from Planck's formula itself. There would therefore be a minimum possible intensity of light, and emissions of light less than this minimum have been observed.

It is therefore each quantum which really interferes with itself; it is therefore necessary that, having once been placed under the aspect of luminous vibrations of the ether, it should subdivide itself into several parts; that certain parts should be behind the others by several wave lengths and consequently that they should not have been emitted at the same time.

There seems to be a contradiction in this; but it may not be insolvable. Let us imagine a system made up of a certain number of Hertzian exciters, all identical. Each of them is charged by an electrical source, and as soon as its charge has reached a certain value, the spark is formed, the emission begins and nothing henceforth can stop it, until the exciter is completely discharged. It must therefore lose its whole quantum or lose nothing (the quantum in this case is the quantity of energy which corresponds to the explosive potential). But this quantum is not lost suddenly; each emission lasts a certain length of time and the waves emitted are susceptible to regular interferences.

Mr. Planck has supposed that the relation between the energy of a resonator and its radiation was the same as in the electrodynamics of Maxwell. We could abandon this hypothesis and suppose that the mechanical collisions take place according to the former laws. The distribution of energy between the resonators would then take place according to the principle of equipartition of energy but the resonators of short period would radiate less at equal energy. It would then be possible to account for the law of radiation, but this would not explain the anomalies of the specific heats at low temperatures, unless we admit that the exchange by collisions is no longer possible for very cold solids, and that their molecules no longer exchange heat except by radiation at very close proximity.

It would be possible to go further and to suppose that there never is any collision, that all the so-called mechanical forces are of an electromagnetic origin; that they are due to distant forces which can themselves be explained by radiation. It would then be necessary to maintain only the method of exchange by radiation and as a result of the Doppler-Fizeau principle. Perhaps then we might thus be led to hypotheses which are very different from those of the quanta.

5. THE QUANTA OF ACTION

The new conception is fascinating in a certain aspect. For some time now, the trend has been in favor of the atomic theory. Matter seems to be made up of indivisible atoms; electricity is no longer continuous; it is no longer divisible without limit; it is made up of electrons all of the same charge, and all alike. For some time now, we have had the magneton, or the atom of magnetism. According to this reckoning, quanta seem to be *atoms of energy*. Unfortunately the comparison cannot be pursued to an ultimate conclusion. A hydrogen atom, for example, is truly invariable; it always maintains the same mass, whatever the compound of which it may be an element. The electrons likewise maintain their individuality throughout the most diverse vicissitudes. Is this equally true of the so-called atoms of energy? There are, for instance, 3 quanta of energy on a resonator whose wave length is 3; this energy passes to a second resonator whose wave length is 5. Therefore, it no longer represents 3 but 5 quanta, since the quantum of the new resonator is smaller; and since in the transformation the number of atoms and the size of each of them have changed.

This is why the theory does not yet satisfy the mind. It is necessary besides to explain *why* the quantum of a resonator is in inverse ratio to the wave length. And this is what induced Mr. Planck to modify the method of setting forth his ideas. But at this point, I am a bit puzzled. I would neither wish to betray Mr. Planck by over-extending his ideas and going further than he had intended, nor neglect to show where it seems to me that he is leading us. I shall therefore first of all translate his text as exactly as possible while at the same time abridging it somewhat. I shall first of all recall that the study of the thermodynamic equilibrium has been reduced to a question of statistics and probability. "The probability of a continuous variable is obtained by considering independent elementary domains of equal probability. . . In classical dynamics, in order to find these elementary domains, the theorem is used which affirms that two physical states, in which one is the necessary effect of the other, are equally probable. In a physical system, if one of the generalized coordinates is represented by q and the corresponding moment by p, according to Liouville's theorem the domain $\iint dp\, dq$ taken at any instant whatever is an invariant with respect to time, if q and p vary in conformity with Hamilton's equations. Moreover, p and q can, at a given instant, assume all the possible values independently of one another. From which it follows that the elementary domain of probability is infinitely small, of the size of $dp\, dq$. The new hypothesis must have as its objective to limit the variability of p and q, in such a way that these variables no longer vary except by jumps, or that they are considered as linked in part to one another. We thus succeed in reducing the number of elementary domains of probability so that the range of each of them is increased. The hypothesis of the quanta of action consists in supposing that these domains, all equal to each other, are no longer infinitely small but finite and that for each of them

$$\iint dp\, dq = h$$

h being a constant."

I consider it necessary to complete this quotation with a few explanations. I cannot explain here what the action is, the generalized coordinates and the moments, nor the various integrals which Mr. Planck brings into play. I shall limit myself by saying that the element of energy is equal to the product of the frequency by the element of action; and, if the quantum of energy is proportional to the frequency, as we have said, it is because the quantum of action is a universal constant, a true atom.

But I must attempt to clarify what is meant by the elementary domains of probability. These domains are indivisible; that is, as soon as we know that we are in one of these domains, everything is thereby determined; otherwise, if the events which are to follow were not entirely known as a result of this fact, if they were to differ according to which part of the domain we would happen to be in, then this domain would not be indivisible from the point of view of probability since the probability of certain future events would not be the same in its different parts.

This is tantamount to saying that all the states of the system which correspond to the same domain cannot be distinguished among themselves; that they constitute one and the same state, and we are thus led to the following statement, more precise than that of Mr. Planck and not, I believe, contrary to his idea.

A physical system is capable of only a finite number of distinct states; it jumps from one of these states to another without going through a continuous series of intermediate states.

To simplify this, let us suppose that the state of the system depends on only three parameters so that it is possible for us to represent it geometrically by a point in space. The set of points representative of the various possible states will then not be the entire space or a region of this space as we ordinarily suppose. It will be a very large number of isolated points strewn in space. These points, it is true, are very close, which gives us the illusion of continuity.

All these states must be considered as equally probable. In fact, if we accept the concept of determinism, each of these states must necessarily be followed by another state, exactly as probable, since it is certain that the first entails the second. We would thus see by degrees that if we start from an initial state, all the states which we shall attain one day or another are all equally probable; the others must not be considered as possible states.

But our representative isolated points must not be distributed in space in any fashion whatever. They must be so distributed that when observing them with our untrained senses, we could have believed in the ordinary laws of dynamics, for example, in those of Hamilton. A comparison, which is much closer to reality than may seem to be the case, may perhaps help me to make myself clear. We observe a liquid, and our senses lead us at first to believe that this is continuous matter. A more precise experiment teaches us that this liquid is incompressible, so that the volume of any portion whatever of matter remains constant. Various reasons then lead us to think that this liquid is made up of very small and very numerous, but discrete molecules. However, we shall no longer be able to imagine a distribution of these molecules without placing some limit to our imagination. It will be necessary, because of the incompressibility, to suppose that two small and equal volumes contain the same number of molecules. As to the distribution of possible states, Mr. Planck finds himself under an analogous restriction, and this is what he expresses by the equations which I have cited above, and which I cannot explain further at this point.

It would be possible, it is true, to imagine mixed hypotheses. Let us suppose once again that the physical system depends on only three parameters and that its state can be represented by a point in space. The set of points representative of the possible states can be neither a region of space nor a cluster of isolated points. It can be made up of a large number of small surfaces or of small curves separate from one another. Either, for example, one of the material points of the system could describe only certain trajectories; but describe them in a continuous manner except when it jumps from one trajectory to another under the influence of neighboring points—this could be so in the case of the resonators of which we spoke above; or else the state of ponderable matter could vary in a discontinuous manner, with only a finite number of possible states, whereas the state of the ether would vary in a continuous manner. None of all this would be incompatible with Mr. Planck's idea.

But no doubt the first solution will be preferred, the solution free from all these bastard hypotheses; but it is necessary to take into account the consequences which this entails. What we have said should be applicable to any isolated system whatever, even to the universe. The universe therefore would jump suddenly from one state to another; but in the interval it would remain motionless. The various instants during which it would remain in the same state could no longer be distinguished one from another. This would thus result in the discontinuous variation of time, *the atom of time.*

6. PLANCK'S NEW THEORY

Let us refer again to less general and more precise problems, for example, to the theory of radiation. Mr. Planck has thought of a modification to his first theory and I would like to say a few words about it. According to his new ideas, the emission of light would take place suddenly by quanta, but the absorption would be continuous. He wished thereby to escape the following difficulty, which, I do not know why, seemed to him more perplexing insofar as absorption is concerned. Light strikes each resonator in a continuous fashion. If it can be absorbed only one quantum at a time, the energy must accumulate in a sort of antechamber of the resonator until there is enough to enter. In the second theory, this difficulty disappears, but a waiting-room is always needed for the energy which leaves, since the ether can transmit it only in infinitely small fractions.

In the new theory, the resonators will maintain a residue of energy even at absolute zero. If we adopt Mr. Planck's new point of view, it is then necessary to modify the relation between the energy of a radiant body and the intensity of its radiation. This radiation is no longer proportional to the energy, but only to the excess of this energy to the residue which still remains at absolute zero.

Must I admit that I have not been entirely satisfied with this new hypothesis? Mr. Planck speaks only of the emission and of the absorption, and speaks of them as if the resonator were stationary; there is no mention of the exchange of energy by collisions, nor of the Döppler-Fizeau principle. Under these conditions, there can therefore be no tendency toward a final state. This is what I have said above. The proof by which an attempt is made to have us know the final state is therefore merely an illusion. The author does not say if the exchanges by collisions are continuous like the absorption, or discontinuous like the emission. And when we wish to apply the general theory of the exchanges by collisions, Mr. Planck's results are no longer to be found. It is therefore more suitable to abide by his first ideas.

7. MR. SOMMERFELD'S VIEWS

Mr. Sommerfeld has proposed a theory which he wishes to link to that of Mr. Planck, although the only link between them is that the letter *h* figures in both formulas, and that the same name "quantum of action" has been given to the two very different objects which this letter represents.

The collision of electrons would not follow at all the same laws as the collision of complex bodies which we know and which are available for experiment. When an electron encountered an obstacle, it would come to a stop more quickly the greater its velocity. (If this law were applicable to trains, the problem of braking would show itself at a new advantage.) And this applies to the production of X-rays. The cathode rays are electrons in motion; these electrons come to a stop by colliding with an anode. This sudden stop disturbs the ether whose vibrations produce the X-rays. Mr. Sommerfeld's theory explains why the X-rays are more penetrating and much "stronger," the greater the velocity of the cathode rays. The greater this velocity, in fact, the more sudden is the coming to a stop, the more intense and the shorter in duration, consequently, the perturbation of the ether.

8. CONCLUSIONS

We see what the state of the question is: the former theories, which up to now seemed to account for all known phenomena, have encountered an unexpected obstacle. It seemed that a modification was necessary. Mr. Planck first conceived of an hypothesis, but so strange did it seem that we were tempted to seek all sorts of means to break away from it. These means have been sought in vain up to now. And this does not prevent a multitude of difficulties arising from this new theory, many of which are real and are not simple illusions due to our laziness of mind which resists changing its habits.

It is impossible, for the moment, to foresee what the final outcome will be. Shall we find another, entirely different explanation? Or else, on the contrary, will the partisans of the new theory succeed in setting aside the obstacles which prevent us from adopting it without reservations? Will discontinuity reign over the physical universe? Is its triumph conclusive? Or else shall we admit that this discontinuity is merely apparent and cloaks a series of continuous processes? The first person to see a collision believed he had observed a discontinuous phenomenon, and we know today that he merely saw the effect of very great, but continuous changes of speed. To seek this very day to give an opinion on these questions would be to waste one's ink.

Chapter VII

THE RELATIONS BETWEEN MATTER AND ETHER[1]

When Mr. Abraham asked me to bring to a close the series of lectures organized by the French Society of Physics, I was at first about to refuse; it seemed to me that each subject had been fully discussed and that I could add nothing to what had already been so well said. I could only attempt to summarize the impression which seems to emanate from this collection of researches, and this impression is so distinct that each of you must have experienced it as well as I, and I could not make it any clearer by striving to express it in sentences. But Mr. Abraham insisted so graciously that I finally resigned myself to inevitable inconveniences, the greatest of which is to repeat what each of you has thought for a long time and the least of which is to cover a multitude of different subjects without having the time to dwell on them at length.

An important idea must have occurred to all listeners. The former hypotheses of mechanics and atomic theory have of late assumed sufficient consistence to cease appearing to us as hypotheses. The atoms are no longer a convenient fiction; it seems, so to speak, that we can see them since we know how to count them. An hypothesis takes form and becomes more convincing when it explains new facts. But this happens in many ways. More often, it must become broader in scope in order to account for new facts; but at times it loses in precision when it becomes more inclusive; at other times, it is necessary to graft onto it an auxiliary hypothesis which conforms plausibly with it, which does not clash too much with the stock onto which it is grafted, but which is nonetheless something alien to it, something thought up with an express view to the goal to be reached —in a word, an added touch. In this case we cannot say that experience has confirmed the original hypothesis, but at the very most that it has not contradicted it. Or then again, there is between the new facts and the older facts, for which the hypothesis had been

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originally conceived, a close connection, one of such a nature that any hypothesis which accounts for the former must, by this very fact, account for the latter, so that the facts verified are new only in appearance.

The same is not so when experience reveals a coincidence which could have been anticipated and could not be due to chance, and particularly when a numerical coincidence is involved. Now, coincidences of this type have, in recent times, confirmed the atomistic concepts.

The kinetic theory of gases has acquired, so to speak, unexpected props. New arrivals have modeled themselves upon it exactly; these are on one side the theory of solutions and on the other the electronic theory of metals. The molecules of the solute, as well as the free electrons to which metals owe their electrical conductibility, behave like gas molecules in the enclosed spaces in which they are contained. The parallelism is perfect and can be pursued even to numerical coincidences. In that way, what was doubtful becomes probable; each of these three theories, if it were isolated, would seem to be merely an ingenious hypothesis, for which it would be possible to substitute other explanations nearly as plausible. But, as in each of the three cases a different explanation would be necessary, the coincidences which have been observed could no longer be attributed to chance, which is inadmissible, whereas the three kinetic theories make these coincidences necessary. Besides, the theory of solutions leads us very naturally to that of the Brownian movement, in which it is impossible to consider the thermal disturbance as a figment of the imagination, since it can be seen directly under the microscope.

The brilliant determinations of the number of atoms computed by Mr. Perrin have completed the triumph of atomism. What makes it all the more convincing are the multiple correspondences between results obtained by entirely different processes. Not too long ago, we would have considered ourselves fortunate if the numbers thus derived had contained the same number of digits. We would not even have required that the first significant figure be the same; this first figure is now determined; and what is remarkable is that the most diverse properties of the atom have been considered. In the processes deriving from the Brownian movement or in those in which the law of radiation is invoked, not the atoms have been counted directly, but the degrees of freedom. In the one in which we use the blue of the sky, the mechanical properties of the atoms no longer come into play; they are considered as causes of optical discontinuity. Finally, when radium is used, it is the emissions of projectiles that are counted. We have arrived at such a point that, if there had been any discordances, we would not have been puzzled as to how to explain them; but fortunately there have not been any.

The atom of the chemist is now a reality; but this does not mean that we are about to arrive at the ultimate elements of matter. When Democritus invented atoms, he considered them as absolutely indivisible elements beyond which there is nothing to seek. That is what that means in Greek; and it is for this reason, after all, that he had invented them. Behind the atom, he wanted no more mystery. The atom of the chemist would therefore not have given him any satisfaction; for this atom is by no means indivisible; it is not truly an element; it is not free from mystery; this atom is a world. Democritus would have thought that having taken such pain to discover it, we are no further along than at the outset. These philosophers are never satisfied.

For, and this is the second consideration which asserts itself, each new discovery in physics reveals a new complexity of the atom. And first of all the bodies which were believed to be simple, and which, in many respects behave exactly like simple bodies, are capable of being broken down into simpler ones still. The atom breaks up into smaller atoms. What is called radioactivity is merely a perpetual breaking up of the atom. This is what has been called at times the transmutation of the elements, which is not quite exact, since an element does not in reality transform itself into another but breaks up into several others. The products of this decomposition are still chemical atoms, analogous in many respects to the complex atoms which gave rise to them in the process of breaking up, so that the phenomenon could be expressed just like the most commonplace reactions by a chemical equation, capable of being accepted without too much hesitation by the most conservative chemist.

This is not all. In the atom we find many other things: in it, first of all, we find electrons. Each atom therefore seems to be a sort of solar system in which small negative electrons playing the role of planets gravitate around a large positive electron which plays the role of central sun. It is the mutual attraction of these electrons of opposite charge which maintains the cohesion of the system and which makes it a whole. It is this attraction which regulates the planets' periods and it is these periods which determine the wave length of the light emitted by the atom. It is to the self-induction of the convection currents produced by the movements of these electrons that the atom which is made up of them owes its apparent inertia, which we call its mass. In addition to these captive electrons, there are free electrons, those which obey the same kinetic laws as gas molecules, those which make metals conductors. These are comparable to the comets which circulate from one stellar system to another and establish a free exchange of energy between these distant systems.

But we have not come to the end. After the electrons or atoms of electricity come the magnetons or atoms of magnetism which come to us today by two different paths, by the study of magnetic bodies and by the study of the spectrum of simple bodies. I need not remind you here of Mr. Weiss's excellent lecture and the astonishing relations of commensurability that these experiments have revealed in such an unexpected fashion. Therein also are numerical relations which cannot be attributed to chance and for which an explanation must be sought.

At the same time it is necessary to explain the very odd laws of the distribution of lines in the spectrum. According to the researches of Balmer, Runge, Kaiser, and Rydberg these lines are distributed in series, and in each series obey simple laws. The first thought that occurs is to compare these laws with those of the harmonics. Just as a vibrating string has an infinite number of degrees of freedom, which permits it to produce an infinite number of sounds whose frequencies are the multiples of the fundamental frequency; just as a resonant body of a complex form also produces harmonics whose laws are analogous although much simpler; just as a Hertzian resonator is capable of an infinite number of different periods, could not the atom give off, for identical reasons, an infinite number of different lights? You know that this very simple idea has failed because, according to the spectroscopic laws, it is the frequency, and not its square, whose expression is simple, because the frequency does not become infinite for the harmonics of an infinitely high range. The idea must either be modified or abandoned. Up to now it has resisted all attempts; it has refused to adapt itself. This is what led Mr. Ritz to abandon it. He therefore imagines a vibrating atom to be composed of a rotating electron and of several magnetons placed end to end. It is no longer the mutual electrostatic attraction of the electrons which regulates the wave lengths; it is the magnetic field created by these magnetons.

It is somewhat difficult to accept this conception, which contains something artificial; but we must resign ourselves to it, at least provisionally, since up to now nothing different has been discovered although we have been actively searching. Why can hydrogen atoms give off several lines? It is not because each of them could give off all the lines of the hydrogen spectrum nor because they give off one or the other according to the initial circumstances of the movement. It is because there are many kinds of hydrogen atoms differing from one another in the number of magnetons aligned in them, and because each of these types of atoms gives off a different line. We wonder if these different atoms can transform themselves one into the other and how. How can an atom lose magnetons (and that is what seems to happen when one allotropic form of iron is transformed into another)? Can the magneton leave the atom or can some of the magnetons leave the alignment to arrange themselves irregularly?

This end-to-end arrangement of the magnetons is also a singular feature of Ritz's hypothesis. Mr. Weiss's ideas must, however, make it seem less strange. It is indeed necessary for the arrangement of the magnetons to be if not end-to-end, at least parallel, since they can be added arithmetically or at least algebraically, but not geometrically.

What then is a magneton? Is it something simple? No, if we do not wish to abandon Ampère's hypothesis of currents of particles. A magneton is therefore a vortex of electrons and our atom now becomes more and more complicated.

However, what causes us to appreciate the complexity of the atom more than anything else is the thought expressed by Mr. Debierne at the close of his lecture. It consists in explaining the law of radioactive transformation; this law is very simple; it is exponential. But, if we consider its form, we see that it is a statistical law; we can recognize in it a chance factor. But the chance factor is not due here to the fortuitous encounter of other atoms and other exterior agents. It is within the very interior of the atom that the causes of the transformation are found. I mean, the determining cause as well as the material cause. Otherwise we would see external circumstances, temperature, for example, exercise an influence on the coefficient of time raised to a given power; and this coefficient is remarkably constant, and Curie proposes to make use of it in the measurement of absolute time.

The chance factor which regulates these transformations is therefore an internal chance factor; that is, the atom of a radioactive body is a world, and one subject to chance; but let us beware: he who says chance understands large numbers. A world made up of a few elements will obey more or less complicated laws, but not statistical laws. Therefore it must be that the atom is a complex world; it is true that it is a closed world (or at least almost closed). It is free from external perturbations which we can bring about. Since there are statistics about the atom, and consequently internal thermodynamics, we can speak of the internal temperature of the atom. Well! It has no tendency to assume an equilibrium with the external temperature as if the atom were enclosed in a shell perfectly impervious to radiant heat. And it is precisely because it is closed, and because its functions are clearly plotted, guarded by strict customs officers, that the atom is an individual.

At first sight, this complexity of the atom contains nothing shocking to the mind; it seems that it should not cause us any confusion. But slight reflection will soon reveal the difficulties which escaped us at first. What was counted, in counting the atoms, were the degrees of freedom. We have assumed implicitly that each atom had only three. This is what accounts for the observed specific heats. But each new complication should introduce a new degree of freedom and then we are wide of the mark. This difficulty did not escape the originators of the principle of equipartition of energy. They were already amazed at the number of lines in the spectrum but, finding no means of avoiding it, they had the daring to disregard it.

What seems the natural explanation is exactly that the atom is a complex world, but a closed world. External perturbations have no repercussions whatever on what goes on inside, and what goes on inside does not affect the outside. This could not be entirely true; otherwise, we would never know what goes on inside and the atom would seem to be a simple material point. What is true is that we can see the inside only through a very small window, and there is practically no exchange of energy between the exterior and the interior and consequently no tendency to the partition of energy between the two worlds. The internal temperature, as I was saying a while ago, does not tend toward an equilibrium with the external temperature, and that is why the specific heat is the same as if all this internal complexity did not exist. Let us imagine a complex body composed of a hollow sphere whose internal wall is absolutely impervious to heat, and inside the body a multitude of different bodies. The observed specific heat of this complex body will be that of the sphere, as if all the bodies enclosed within did not exist.

The door which shuts in the internal world of the atom opens slightly, however, from time to time. That is what happens when, by the emission of a helium particle, the atom degrades itself and descends one rank in the radioactive hierarchy. What then takes place? How does this decomposition differ from ordinary chemical decompositions? Why is the uranium atom, composed of helium and other things, more entitled to be called an atom than the semi-molecule of cyanogen, for example, which behaves in so many respects like that of a simple body and which is composed of carbon and nitrogen? It is doubtless because the atomic heat of uranium would obey (I do not know if it has been measured) the law of Dulong and Petit and that it would indeed be that of a simple atom. It should therefore double at the instant of the emission of the helium particle and when the original atom splits up into two secondary atoms. By this decomposition, the atom would acquire new degrees of freedom capable of influencing the external world, and these new degrees of freedom would reveal their presence through an increase in specific heat. What would be the consequence of this difference between the total specific heat of the components and that of the compounds? It is that the heat liberated by this decomposition should vary rapidly with the temperature; so that the formation of radioactive molecules, highly endothermic at the ordinary temperature, would become exothermic at a higher temperature. We would thus understand better how the radioactive compounds could have been formed, which did not cease to be somewhat mysterious.

However that may be, this conception of these small closed or only slightly open worlds does not suffice to solve the problem. It would be necessary that the principle of equipartition of energy should reign beyond all question outside these closed worlds, except at the instant when one of the doors were to open slightly; and that is not what happens.

The specific heat of solid bodies decreases rapidly when the temperature decreases as if some of their degrees of freedom ankylosed successively, froze, so to speak; or, if you prefer, lost all contact with the exterior and withdrew one after another behind some enclosed space in some closed world.

Moreover, the law of black radiation is not the one that would be required by the principle of equipartition of energy.

The law which would be adaptable to this theory is that of Rayleigh, and this law, which would, moreover, imply a contradiction, since it would lead to a total and infinite radiation, is absolutely contradicted by experiment. There is much less light of short wave length in the emission of black bodies than would be required by the principle of equipartition of energy.

That is why Mr. Planck has conceived his quantum theory, according to which the exchanges of energy between ordinary matter and the small resonators whose vibrations give rise to the light of incandescent bodies, could take place only by sudden jumps. One of these resonators could not gain or lose energy in a continuous manner. It could not gain a fraction of a quantum; it would gain a whole quantum or nothing at all.

Why therefore does the specific heat of a solid decrease at low temperature? Why do some of its degrees of freedom seem not to play a part? It is because the supply of energy available to them at low temperature is not sufficient to furnish each of them a quantum; some of them would be entitled only to a fraction of a quantum. But, since they want all or nothing, they have nothing and remain as if ankylosed.

So also in radiation some resonators, which cannot have a whole quantum, have nothing and remain motionless, so that there is much less light radiated at low temperature than there would be without this condition. And since the required quantum is all the greater when the wave length is shorter, it is particularly the resonators of short wave length which remain silent, so that the proportion of light of short wave length is much smaller than would be required according to Rayleigh's law.

To say that such a theory raises many difficulties would be quite naive. When such a bold idea is put forth, we may well expect to encounter difficulties. We know that we are upsetting all the accepted opinions and we are not surprised at any obstacle; on the contrary, we would be surprised not to find any before us. Therefore these difficulties do not seem to be valid objections.

I shall have the courage, however, to point out a few, and I shall not choose the biggest, the most obvious, those which come to anyone's mind; in fact, this is quite useless since everybody thinks of them immediately. I simply wish to describe to you the series of successive mental reactions I have experienced.

First of all, I wondered what was the value of the proposed proofs. I noticed that we were evaluating the probability of the various divisions of energy by simply enumerating them since, thanks to the given hypothesis, they were finite in number, but I could not very well see why they were considered as equally probable. Then, we were introducing the known relations between the temperature, the entropy, and the probability. This assumed the possibility of thermodynamic equilibrium since these relations are proved by assuming this equilibrium to be possible. I know very well that experiment teaches us that this equilibrium is realizable, since it is achieved. But this did not satisfy me; it was necessary to show that this equilibrium is compatible with the stated hypothesis and even that it is a necessary consequence of it. I did not exactly have doubts, but I felt the need to see somewhat more clearly, and for this it was necessary to delve a little into the particulars of the mechanism.

For a distribution of energy to take place between the resonators of different wave length whose oscillations are the cause of the radiation, the resonators must be capable of exchanging their energy. Otherwise, the initial distribution would go on indefinitely and, since this initial distribution is arbitrary, there could be no question of a law of radiation. But, a resonator can give off to the ether and can receive from it only light of an exactly determined wave length. If, therefore, resonators could not react on one another mechanically, that is, without the ether as a medium; if, moreover, they were fixed and shut in a fixed enclosed space, each of them could emit or absorb light of a determined color only. It could therefore exchange energy only with resonators with which it was in perfect resonance, and the initial distribution would remain unalterable. But we can conceive of two methods of exchange which do not lend themselves to this objection. For one thing, atoms and free electrons can circulate from one resonator to another, collide with a resonator, impart to it and receive from it some energy. Secondly, light, when reflecting in movable mirrors, changes its wave length by virtue of the Doppler-Fizeau principle.

Are we free to choose between these two mechanisms? No, it is certain that one and the other must come into play, and it is necessary that one and the other lead us to the same result, to the same law of radiation. What would in fact happen if the results were contradictory, if the mechanism of the collisions acting alone tended to give rise to a certain law of radiation, Planck's for example, while the mechanism of Doppler-Fizeau tended to give rise to another? Well! What would happen is that these two mechanisms both needing to come into play, but becoming preponderant in alternation under the influence of fortuitous circumstances, the world would oscillate constantly from one law to another, it would not tend toward a final stable state, toward that thermic death in which it will no longer know change. The second thermodynamic principle would not be true.

I therefore decided to examine the two processes one after the other, and I began with the mechanical action, with the collision. You know why the older theories lead us necessarily to the principle of equipartition of energy. It is because they assume that all the equations of mechanics are of the form of Hamilton's and that consequently they admit unity as the last multiplier as understood by Jacobi. It is then necessary to assume that the laws of collision between a free electron and a resonator are not of the same form and that the equations which express them admit of a last multiplier other than unity. They must indeed have a last multiplier; otherwise the second principle of thermodynamics would not be true—we would encounter again the difficulty of a few moments ago—but that multiplier must not be unity.

It is precisely this last multiplier which measures the probability of a given state of a system (or rather what could be called the density of the probability). In the quantum theory, this multiplier cannot be a continuous function, since the probability of a state must be null, every time the corresponding energy is not a multiple of the quantum. Therein lies an obvious difficulty, but it is one of those to which we have resigned ourselves beforehand. I did not dwell on it; I then pursued the calculation to the end and I met again Planck's law, fully justifying the views of the German physicist.

I then proceeded to the mechanism of Döppler-Fizeau. Let us imagine an enclosed space made up of a pump and of a piston, and whose walls are perfectly reflective. In this enclosed space is contained a certain quantity of luminous energy without any distribution whatever of wave lengths, but *no source of light.* The luminous energy is enclosed once for all.

So long as the piston does not move, this distribution cannot vary, for the light will maintain its wave length by reflection. But, when the piston is moved, the distribution will vary. If the speed of the piston is very low, the phenomenon is reversible and the entropy must remain constant. Thus we encounter again Wien's analysis and Wien's law, but we are no better off since this law is common to the old and to the new theories. If the speed of the piston is not too low, the phenomenon becomes irreversible; so that the thermodynamic analysis no longer leads us to equalities but to simple inequalities from which conclusions cannot be drawn.

Yet it seems that it should be possible to reason as follows: let us assume that the initial distribution of energy be that of black radiation; it is evidently that which corresponds to the maximum entropy. With a few strokes of the piston, the final distribution must therefore remain the same, otherwise the entropy would have decreased. In fact, whatever the initial distribution may be, after a great many piston strokes, the final distribution ought to be that which makes the entropy maximum, that of black radiation. This reasoning would be valueless.

The distribution has a tendency to approximate that of black radiation; it can no more escape this than heat can proceed from a cold body to a warm body; that is, it cannot do this *without an alternative.* But there is here an alternative; with each piston stroke, work is done, which can be noted in an increase in the luminous energy enclosed in the pump; that is, it is transformed into heat.

The same difficulty would no longer be encountered if the bodies in motion on which the light is reflected were infinitely small and infinitely numerous, because then their kinetic energy would not stem from mechanical work but from the heat. It would therefore be possible to compensate the decrease of entropy which corresponds to a change in the distribution of the wave lengths by the transformation of this work into heat. And then we would have the right to conclude that, if the initial distribution is that of black radiation, this distribution must persist indefinitely.

Let us imagine an enclosure with *fixed* and reflective walls. We shall enclose in it not only luminous energy but also a gas; the molecules of this gas will play the role of moving mirrors. If the distribution of the wave lengths is that of black radiation corresponding to the temperature of the gas, this state must be stable, that is:

First, the action of the light on the molecules must not cause a variation in temperature.

Secondly, the action of the molecules on the light must not disturb the distribution.

Mr. Einstein has studied the action of light on molecules. These molecules undergo, in fact, something similar to the pressure of radiation. Mr. Einstein, however, did not altogether adopt such a simple point of view. He compared the molecules to small movable resonators capable of possessing at the same time kinetic energy due to translation and energy due to electrical oscillations. The result would have been the same in any case; he would have recognized Rayleigh's law.

As for me, I shall do the reverse; that is, I shall study the action of the molecules on light. The molecules are too small to give off a steady reflection; they produce only a diffusion. When we do not take into account the movements of the molecules, we know what this diffusion is, both by theory and by experiment; it is this diffusion, in fact, which produces the blue of the sky.

This diffusion does not affect the wave length, but is the more intense the shorter the wave length is.

It is now necessary to proceed from the action of a molecule at rest to the action of a molecule in motion in order to account for the thermal agitation. This is easy; we have only to apply Lorentz' principle of relativity. The result is that various beams of the same real wave length, striking the molecule from different directions, will not possess the same apparent wave length for an observer who believes the molecule to be at rest. The *apparent* wave length is not affected by the diffraction, but the same is not true for the real wave length.

We thus arrive at an interesting law; luminous energy, either reflected or diffused, is not equal to incident luminous energy; it is not the energy but the product of the energy by the wave length which remains unaffected. I was at first very pleased. The result of this was, in fact, that an incident quantum was equal to a diffused quantum, since the quantum is in inverse ratio to the wave length. Unfortunately, this was of no value.

I was led by this analysis to Rayleigh's law; this, I already knew. But I hoped that when I saw *how* I would be led to Rayleigh's law, I would perceive more clearly to what modifications the hypotheses must be subjected in order to recognize Planck's law. It is this hope which was denied.

My first thought was to seek something which would resemble the quantum theory. It would indeed be surprising if two entirely different explanations should account for the same exception to the principle of equipartition of energy depending on which mechanism produced this exception. Now, how could the discontinuous structure of energy arise? It could be assumed that this discontinuity belongs to the luminous energy itself, when it circulates in the free ether, that consequently light does not strike the molecules in a compact mass, but in separate small battalions. It is easy to see that this would change nothing in the result.

Or else it could be assumed that the discontinuity is produced at the moment of the diffusion itself, that the diffusing molecule does not transform light in a continuous fashion, but in successive quanta. This will not do either because, if the light to be transformed must remain in a waiting room, as if we were dealing with a bus which waits till it is full before leaving, a delay would necessarily result. But Lord Rayleigh's law teaches us that the diffusion by the molecules, when it takes place without deviation in the direction of the incident ray, produces ordinary refraction, quite simply; that is, the diffused light interferes regularly with the incident light, which would not be possible if there were a loss of phase.

If we ask, with an open mind, which of our premises it is best to abandon, we shall be no less puzzled. We cannot see how we can give up the principle of relativity. Is it then the law of diffusion by molecules at rest which must be modified? This is also very difficult; we can hardly stretch our imagination to the point of believing that the sky is not blue.

I shall leave this quandary, and I shall conclude with the following reflection. As science progresses, it becomes more and more difficult to make room for a new fact which does not fit in naturally. The older theories rest on a large number of numerical coincidences which cannot be attributed to chance. We cannot therefore put asunder that which they have joined together; we can no longer destroy the framework, we must try to "bend" it. And it does not always lend itself to this. The principle of equipartition of energy explained so many facts that it must contain some truth; on the other hand, it is not entirely true since it does not explain all the facts. We can neither discard it nor retain it without modification, and the modifications which seem imperative are so strange that we hesitate to accept them. In the present state of science, we can only admit these difficulties without resolving them.

Chapter VIII

ETHICS AND SCIENCE

During the latter half of the nineteenth century, people often dreamed of formulating a scientific ethics. We were not content to sing the praises of the educational virtue of science, the advantages that the human soul derives for its own improvement from looking truth in the eye. We relied on science to place moral truths beyond all contestation as it has done for the theorems of mathematics and the laws stated by the physicists.

Religions can have a great power over believers; but not all persons are believers. Faith can be imposed on only a few; reason would impress itself upon all. We must address ourselves to reason; and I do not mean to that of the metaphysicist whose constructs are brilliant but ephemeral like soap bubbles which amuse us for an instant, then burst. Science alone builds firmly; it has constructed astronomy and physics; today it is constructing biology; by the same processes tomorrow it shall construct ethics. Its ordinances shall reign uncontested; no one shall be able to oppose them, and we shall no more think of rebelling against the moral law than we think today of rebelling against the theorem of the three perpendiculars or the law of gravitation.

On the other hand, there were people who associated with science every possible evil; who considered it as a school of immorality. It is not only that it assigns too much importance to matter, and that it deprives us of a sense of respect because we only respect that which we dare not look at. But shall not its conclusions be the negation of morality? As some famous author has said, it shall extinguish the lights of heaven, or at least, deprive them of all their mystery and reduce them to the state of common gas jets. It shall expose the stage effects of the Creator, who will thereby lose some of his prestige. It is not good to let children look into the wings; this could arouse in them doubts of the existence of the bogeyman. If we permit scientists to have their way, there shall soon be no morality.

What are we to think of the hopes of the one group and the fears of the other? I do not hesitate to reply: they are equally idle. There cannot be a scientific morality; but neither can there be immoral science. And the reason for this is simple; it is a—purely grammatical reason.

If the premises of a syllogism are both in the indicative, the conclusion will also be in the indicative. For the conclusion to have been stated in the imperative, at least one of the premises must itself have been in the imperative. But scientific principles and geometric postulates are and can be only in the indicative. Experimental truths are again in that same mood, and at the basis of the sciences, there is and there can be nothing else. That being given, the most subtle dialectician can juggle these principles as he may wish, combine them, and pile them up on one another. All that he will derive from this will be in the indicative. He will never obtain a proposition which will state: do this, or, do not do that; that is, a proposition which affirms or which contradicts morality.

And therein is a difficulty which the moralists have encountered for a long time. They strive to prove the moral law; we must forgive them, since this is their trade. They wish to base ethics on something, as if it could be based on anything but itself. Science shows us that man can only debase himself by living in such or such a manner. And what if I care little about debasing myself and what if, what you call degradation, I call progress? Metaphysics obliges us to conform to the general law of being which it claims to have discovered. It is possible to reply: I prefer to obey my own particular law. I do not know what metaphysics will reply, but I can assure you that it will not have the last word.

Will religious ethics be more fortunate than science or metaphysics? Obey because God commands it, and because He is a master who can overcome all resistance. Is this a proof, and can we not hold that it is a fine thing to rise up against omnipotence and that, in the dual between Jupiter and Prometheus, it is Prometheus in torture who is the true victor? And also, to give in to force is not to obey; submission of the heart cannot be dictated.

Nor can we base ethics on the interests of society, on the notion of the fatherland, on altruism, since it would still need to be proved that one must of necessity sacrifice oneself to the city of which one is a part, or else to the happiness of others. And no logic, no science can provide us with this proof. What is more, the very morality of pure self-interest, that of egoism, would be powerless since, after all, we cannot be sure that it is best to be egoistic, and since there are people who are not.

All dogmatic ethics, all demonstrative ethics are therefore doomed in advance to certain failure; it is like a machine with only transmission of motion, but without motor energy. The moral motor, the one which can set in motion all the apparatus of rods and gears, can only be something felt. It cannot be proved that we must have pity for the unfortunate; but let us be confronted with undeserved misery, a spectacle which is, alas! only too frequent, and we shall find ourselves aroused by a feeling of revolt; some energy will arise in us which will not listen to reason and which will draw us on irresistibly and as if against our will.

It cannot be shown that we must obey a God, even though it were proved that He is all-powerful and that He could crush us; even though it could be proved that He is good and that we owe Him gratitude. There are persons who consider the right to be ungrateful as the most precious of all liberties. But if we love this God, all proof will become needless, and obedience will seem perfectly natural; and that is why religions are powerful, while metaphysical systems are not.

When we are asked to justify through rational arguments our love for our country, we may be very much at a loss. But let us imagine our armies as conquered, France as invaded; and our heart will rise up, our eyes will fill with tears, and we shall no longer hear anything. And if some people gather so many sophisms today, it is no doubt due to their lack of imagination. They cannot imagine all these ills, and if misfortune or some punishment from heaven wanted them to see these with their own eyes, their soul would revolt the same as ours.

Science cannot therefore of itself create morality; nor can it of itself and directly weaken or destroy traditional morality. But can it not exercise an indirect influence? What I have just said indicates by what mechanism it could play a part. It can give birth to new feelings, not that feelings can be subject to proof; but because every form of human activity reacts on man himself and renews his soul. There is a professional psychology for each trade. The feelings of the plowman are not those of the financier; the scientist therefore also has his particular psychology, I mean his emotional psychology, and something of this affects the man who is only occasionally in contact with science.

On the other hand, science can bring into operation the feelings which naturally exist in man. To resume the metaphor of a few moments ago, we can build as many complicated assemblies of rods and cranks as we want; the machine will not work if there is no steam in the boiler. But if there is steam, the work done will not always be equal to it; it will depend on the mechanism to which it is applied. In the same way, we can say that feeling provides us only with a general motive power for action. It will provide us with the major premise of our syllogism which, suitably to the occasion, will be in the imperative. Science, for its part, will provide us with the minor premise, which will be in the indicative, and will draw from it the conclusion, which can be in the imperative. We shall consider these two points of view one after the other.

First of all, can science become the creator or the inspirer of feelings? What science cannot do, will the love of science be able to do?

Science keeps us in constant relation with something which is greater than ourselves; it offers us a spectacle which is constantly renewing itself and growing always more vast. Behind the great vision it affords us, it leads us to guess at something greater still, this spectacle is a joy to us, but it is a joy in which we forget ourselves and thus it is morally sound.

He who has tasted of this, who has seen, if only from afar, the spendid harmony of the natural laws will be better disposed than another to pay little attention to his petty, egoistic interests. He will have an ideal which he will value more than himself, and that is the only ground on which we can build an ethics. He will work for this ideal without sparing himself and without expecting any of those vulgar rewards which are everything to some persons; and when he has assumed the habit of disinterestedness, this habit will follow him everywhere; his entire life will remain as if flavored with it.

It is the love of truth even more than passion which inspires him. And is not such a love an entire code of morality? Is there anything which is more important than to combat lies because they are one of the most common vices in primitive man and one of the most degrading? Well! When we have acquired the habit of scientific methods, of their scrupulous exactitude, of the horror of all attempts to deflect the course of experiment; when we have become accustomed to dread as the height of ignominy the censure of having, even innocently, slightly tampered with our results; when this has become with us an indelible professional habit, a second nature: shall we not then reveal in all our actions this concern for absolute sincerity to the extent of no longer understanding what makes other men lie? And is this not the best means of acquiring the rarest, the most difficult of all sincerities, the one which consists in not deceiving oneself?

In our shortcomings, the loftiness of our ideal will sustain us. We may prefer another, but, after all, is not the God of the scientist the greater the farther he withdraws from us? It is true that He is inflexible, and many souls shall be sorry for it; but at least He does not share our pettiness and mean rancor as does too often the God of the theologians. This notion of a rule stronger than ourselves, to which we must submit and to which we must become accustomed at any cost, can also have a salutary effect. We can at the very least uphold it. Would it not be better for our peasants to believe that the law can never yield instead of believing that the government shall make it relent in their favor, if ever they invoke the intercession of a sufficiently powerful legislator?

Science, as Aristotle said, has the universal as object. In the presence of a particular fact, it will want to know the universal law; it will aspire to a more and more extensive generalization. It would seem at first that this is merely a mental habit; but mental habits also have their moral repercussions. If you have become accustomed to pay little attention to the particular, the accidental, because your mind is not interested in it, you will naturally be led to attach little significance to it, not to see it as a desirable object, and to sacrifice it without concern. As a result of always looking afar, we become farsighted, so to speak; we no longer see what is small, and, not seeing it any more, we are not in danger of making it our life goal. Thus we shall naturally find ourselves inclined to subordinate our particular interests to universal interests, and this is indeed a code of ethics.

And then science renders us another service. It is a collective task; and it cannot be otherwise. It is like a monument whose construction requires centuries and for which each one must carry a stone, and this stone in some cases requires a lifetime. It therefore imparts to us the feeling of necessary cooperation, of the solidarity of our efforts and those of our contemporaries, and even those of our forefathers and of our successors. We understand that each man is only a soldier, only a small fragment of the whole. It is this same feeling of discipline which fashions military minds and which metamorphoses the rough soul of the peasant and the unscrupulous soul of the adventurer to such an extent that it makes them capable of all sorts of heroism and of devotion. In very different conditions, it can bring about in a similar fashion a charitable act. We feel that we are working in behalf of humanity, and humanity becomes dearer to us as a result.

Here is the pro and the con. If science no longer seems to us to be powerless over the hearts of men and indifferent in matters of morality, can it not have a harmful as well as a useful influence? First of all, every passion is exclusive. Will it not cause us to lose sight of everything which is not the passion itself? Love of truth is a great thing, no doubt; but what a state of affairs if, in order to pursue it, we sacrifice other things which are infinitely more precious, such as goodness, piety, and love of our neighbor. Upon learning of any catastrophe whatever, an earthquake, we shall forget the sufferings of the victims and think only of the direction and amplitude of the tremors; we shall almost recognize in it a piece of good fortune if it reveals some unknown law of seismology.

Here is an example which comes immediately to mind. Physiologists practice vivisection without scruples, and this is a crime which, in the eyes of many old ladies, no benefit of science, either past or future, will ever be able to justify. If we were to believe the old ladies, the biologists, who reveal themselves as pitiless toward animals, must become ferocious to men. They are mistaken without a doubt; I have known many who were very gentle.

The matter of vivisection deserves our dwelling on it for a moment, even though it leads me somewhat from the subject. There is in this matter one of those conflicts of duties which practical life reveals to us at every instant. Man cannot renounce knowledge without belittling himself; and that is why the interests of science are sacred. It is also because of the incalculable number of ills which it can cure or prevent. On the other hand, to cause suffering is impious (I do not say death, I say suffering). Even though the lower animals are no doubt less sensitive than man, they deserve to be pitied. It will only be by rough compromises that we shall be able to extricate ourselves. The biologist must undertake, even *in anima vili*, only experiments which are really useful; and very often there are means to minimize the pain; he must make use of them. But, in this regard, we must rely on our conscience; any legal intervention would be inopportune and somewhat ridiculous. In England it is said that Parliament can do everything, except to change a man into a woman. It can do everything, I should say, except to deliver a competent judgment in scientific matters. There is no authority which can enact rules to decide whether an experiment is useful.

But I must come back to my subject. There are people who say that science hardens the heart, that it binds us to material things, that it kills poetry, the sole source of all noble sentiments. The soul that it touches withers and becomes rebellious to all noble impulses, to all emotions, to all enthusiasm. I do not believe this; a moment ago I stated the contrary. But this is a very widespread opinion, which must have some foundation. It proves that the same food does not agree with everyone.

What are we to infer? Widely understood, and taught by teachers who understand and love it, science can play a very useful and very important role in moral education. But it would be a mistake to want to give it an exclusive role. It can evoke benevolent feelings which can serve as a moral force; but other disciplines can be so equally well. It would be foolish to deprive ourselves of any assistance; all their combined strengths are not too much for us. There are people who have no understanding of science; it is a fact of common observation that there are in all classrooms students who are "good" in literature but not "good" in science. What an illusion to believe that if science does not speak to their mind, it will be able to speak to their heart!

I come to the second point. Not only can science, like all types of activity, evoke new feelings, but it can erect a new edifice on the old feelings, on those which rise spontaneously in man's heart. It is not possible to conceive of a syllogism in which both premises are in the indicative and the conclusion in the imperative. But we can conceive of some constructed according to the following type: Do this; now, when we do not do that, we cannot do this; therefore do that. And such reasonings are not beyond the scope of science.

The feelings on which ethics can be based are of a very different nature; they are not found to the same degree in all souls. In some souls, some feelings predominate while in other souls other strings are always ready to vibrate. Some will be especially moved to pity; they will be moved by their neighbor's sufferings. Others will subordinate everything to social harmony, to the general welfare; or then again they will wish for the greatness of their country. Others will perhaps have an ideal of beauty, or else they will believe that our first duty is to become more perfect ourselves, to strive to become stronger, to become superior to things and indifferent to wealth, and not to lower ourselves in our own esteem.

All these tendencies are praiseworthy; but they are different. Perhaps a conflict may arise from this. If science proves to us that this conflict need not be feared, if it proves that one of these goals cannot be attained without aiming at the other (and this is within the scope of science), it will have performed useful work; it will have rendered valuable assistance to the moralists. Those troops who up to now were fighting in disarray, in which each soldier marched toward his own particular objective, will now close ranks because they will have been shown that each one's victory is everyone's victory. Their efforts will be coordinated, and the disorderly mob will become a disciplined army.

Is this the true direction of the march of science? It is permissible to hope so; it is tending more and more to show us the mutual dependence of the different parts of the universe; to unveil its harmony to us. Is it because this harmony is real or because it is a need of our mind and consequently a postulate of science? This is a question which I shall not attempt to resolve. The fact remains that science tends toward unity and leads us toward unity. Just as it coordinates the particular laws and links them to a more general law, will it not also reduce to unity the intimate aspirations of our hearts which are seemingly so divergent, so capricious, and so alien to one another?

But if it fails in this task, what danger and what a disillusion! Can it not do as much harm as it could have done good? Will these affections, these feelings, so frail, so delicate, withstand analysis? Will not the least amount of light reveal their vanity, and shall we not go on endlessly to no avail? Of what use is pity if the more we do for men, the more demanding they become, and the more dissatisfied, consequently, they are with their lot?—if pity can not only produce only ingrates—this would not be so important—but can produce only embittered souls? Of what use is love of country if its greatness is most often merely a brilliant distress? What is the use of striving to become more perfect if we live only for a day? Woe be the day when science places the weight of its authority in favor of these sophisms!

Also, our souls are a complex fabric in which the threads formed by the associations of our ideas criss-cross and become entangled in all directions. To cut one of these threads is to risk incurring a vast number of rips, the extent of which no one could foresee. We are not the ones who made this fabric; it is a legacy of the past. Often our noblest aspirations are, without our knowing it, thus linked to the most superannuated and ridiculous prejudices. Science will destroy these prejudices; it is its natural task, it is its duty. Will not the noble tendencies which old habits had linked to the prejudices suffer as a consequence? No, not in strong souls, no doubt; but there are not only strong souls and not only clear-sighted spirits; there are also simple souls who risk not resisting the trial.

Therefore it is claimed that science will be destructive. People are frightened at the ruins it will bring about and fear that, wherever it has passed, society will no longer be able to survive. Is there not a kind of self-contradiction in these fears? If it is scientifically demonstrated that such or such a custom, which was considered as indispensable to the very existence of human societies, did not in reality possess the importance which was attributed to it and deceived us only by its venerable antiquity, if this is proved, allowing that this proof is possible, will the moral life of mankind be weakened? Either of two things: either this custom is useful and then a true science will not be able to prove that it is not useful; or it is useless and it should not be wept over. The moment we use as a basis of our syllogisms one of these noble sentiments which engender morality, it is this sentiment and, consequently, morality which we shall encounter at the end of our whole chain of reasoning, if it has been carried out in conformity with the rules of logic. What runs the risk of failing is that which is not essential, that which was in our moral life merely an accident. The only thing that matters cannot help but be found in the conclusions since it is in the premises.

We must fear only that science which is incomplete, the one which is in error, the one which lures us with vain appearance and thus incites us to destroy that which we would want to reconstruct later, when we are better informed and when it is too late. There are people who become infatuated with an idea, not because it is sound but because it is new, because it is fashionable. These people are terrible spoilers, but they are not . . I was about to say that they are not scientists, but I notice that many of them have rendered great services to science; they are, therefore, scientists, but they are scientists not because of, but in spite of that.

True science fears hasty generalizations, theoretical deductions. If the physicist distrusts them, even though those with which he deals are sound and coherent, what are the moralist and the sociologist to do when the so-called theories which they find before them boil down to gross comparisons like the comparison of society with organisms! Science, on the contrary, is not and cannot but be experimental, and an experiment in sociology is the history of the past; it is the tradition which, no doubt, we must criticize but which we must not discard completely.

Morality has nothing to fear from a science which is motivated by a true experimental spirit; such a science is respectful of the past; it is opposed to that scientific snobbery which is so easily duped by novelties. It goes forward merely one step at a time but always in the same direction and always in the right direction. The best remedy against pseudo-science is more science.

There is still another way to conceive of the relations of science with morality. There is no phenomenon which cannot be the object of science since there is none which cannot be observed. Moral phenomena do not escape this any more than the others. The naturalist studies ant and bee colonies and studies them with serenity. So also the scientist strives to judge men as if he were not a man, by assuming the position of some distant inhabitant of Sirius for whom cities would merely be anthills. That is his right; that is his job as a scientist.

The science of ethics will at first be purely descriptive; it will teach us about men's morals and it will tell us what they are without saying what they ought to be. Next, it will be comparative; it will carry us across space to have us compare the morals of different peoples, those of the savage and of civilized man; and it will also take us through time to have us compare those of yesterday and those of today. It will finally strive to become explanatory; and that is the natural evolution of every science.

The Darwinians will strive to explain why all known peoples submit to a moral law by telling us that the principle of the survival of the fittest has for a long time caused the disappearance of those who had been foolish enough to try to escape it. The psychologists will explain why the rules of morality are not always in agreement with the general interest. They will tell us that man, caught in the whirlwind of life, does not have time to consider all the consequences of his actions; that he can obey only general precepts; that these shall be the less challenged the simpler they are; and that, if their role is to be useful and, consequently, if selection is to be able to create them, it is sufficient that they agree *most of the time* with the general interest. The historians will explain how, of the two systems of morality—the one which subordinates the individual to society and the one which takes pity on the individual and proposes as our goal the welfare of our neighbor—it is the second which progresses constantly as societies become more vast, more complex, and, after all is said and done, less exposed to catastrophes.

This science of ethics is not a system of morals; it will never be one; it can no more be a substitute for morality than a treatise on the physiology of digestion can be a substitute for a good dinner. What I have said up to now exempts me from having to say more.

But this is not what is involved. This science of ethics is not a system of morals, but can it be useful, can it be dangerous for morality? Some will say that to explain is always to justify to a certain extent; and this can easily be upheld. Others will say, on the other hand, that it is not without danger to teach us the diversity of morals of the different races and regions; that this can teach us to question what should be accepted blindly, and accustom us to notice the contingency where we would do better to see only the necessity. And perhaps they are not altogether wrong, either. But, frankly, is this not exaggerating the influence on men of theories which are merely skin-deep, of abstractions which will always remain foreign to them? When passions—some noble, some ignominious—quarrel over the possession of our conscience, what weight can the metaphysical distinction between the contingent and the necessary carry before such powerful adversaries?

And yet I can hardly remain silent on an important point, in spite of the fact that I have little time in which to discuss it. Science is deterministic; it is so *a priori;* it postulates determinism because, without it, it could not exist. It is deterministic *a posteriori* also; if it began by postulating it, as an indispensable condition for its existence, it proves it later by the very fact of existing, and each of its conquests is a victory of determinism. Perhaps a conciliation is possible. Can we admit that this forward march of determinism will continue without halting and without backward movement, without encountering insurmountable obstacles, and that nevertheless we do not have the right to pass to the limit, as we mathematicians say, and to infer an absolute determinism because at the limit determinism would vanish in a tautology or in a contradiction? This is a question which has been studied for centuries without hope of resolving it and I cannot even touch lightly on it in the few minutes left at my disposal.

But we are in the presence of a fact; science, rightly or wrongly, is deterministic. Wherever it penetrates, it introduces determinism. As long as only physics or even biology is concerned, this matters little; the domain of conscience remains inviolate. What will happen on the day that ethics becomes in its turn an object of science? It will necessarily become imbued with determinism, and this without a doubt will be its ruin.

Is everything hopeless; or, if some day morality should become reconciled with determinism, could it adjust to it without perishing as a result? Such a profound metaphysical revolution would without doubt have much less influence on morals than we may think. It is well understood that penal repression is not involved. What was formerly called crime or punishment would be called illness or prophylaxis; but society would keep intact its right, which is not the right to punish but very simply the right to defend itself. What is more serious is that the idea of merit or demerit would disappear or become transformed. But we would go on loving the good man just as we love all that is beautiful. We would no longer have the right to hate the vicious man, who would only fill us with disgust. But is this truly necessary? It is enough that we do not stop hating vice.

Other than that, everything would be as in the past. Instinct is stronger than all metaphysical systems; and even though this should be proved, even though the secret of its force should become known, its power would not be weakened as a result. Is gravitation less irresistible since Newton? The moral forces which lead us would continue to lead us.

And if the concept of liberty is itself a force, as Fouillée says, this force would hardly be diminished if the scientists should ever prove that it is based only on an illusion. This illusion is too tenacious to be dissipated by a few arguments. The most intransigent determinist will still continue for a long time, in everyday conversation, to say "I want" and even "I must," and even to think it with the most powerful part of his soul, the part which is not conscience and which does not reason. It is just as impossible not to act as a free man when we act than it is not to reason as a determinist when doing scientific work.

The ghost, therefore, is not so formidable as people say, and there may perhaps be other reasons for not fearing it. We can hope that in the absolute everything could be reconciled and that, to an infinite intelligence, the two attitudes, that of the man who acts as if he were free and that of the man who thinks as if freedom were nowhere, would seem equally legitimate.

We have assumed in turn the different points of view with which it is possible to consider the relations of science and ethics. We must now reach conclusions. There is not now and there will never be scientific ethics in the strict sense of the word; but science can be an aid to ethics in an indirect manner. Science widely understood cannot help but serve it; pseudo-science alone is to be feared. On the other hand, science cannot suffice, because it can see only one part of man, or, if you prefer, it sees everything but it sees everything from the same angle; and secondly, because it is necessary to consider the minds that are not scientific. On the other hand, the fears, like the hopes which are too lofty, seem to me equally chimerical; ethics and science, as they both progress, will surely adapt themselves to each other.

Chapter IX

THE MORAL ALLIANCE[1]

Today's conference joins together men whose ideas are very different and who are brought closer only by a common good-will and an equal desire for good. Yet I do not doubt that they will easily agree; for if they do not have the same opinion as to the means, they are in accord as to the goal to be attained. And this is the only thing that matters.

It was possible to read recently, it is still possible to read on the walls of Paris posters which announce a contradictory conference on "The Conflict of Morals." Does this conflict exist; need it exist? No. Morality can be based on a multitude of reasons; some of these are transcendental; they are probably the best and surely the noblest; but they are the ones which are challenged. There is at least one, perhaps a little more commonplace, with which we must surely be in accord.

Man's life, in fact, is a continual struggle; forces rise up against him which are blind, no doubt, but formidable, and which would promptly overwhelm him, which would cause him to perish, which would weigh him down with countless hardships if he did not stand up constantly to resist them.

If occasionally we enjoy relative tranquility, it is because our forefathers fought hard. Let our energy, let our vigilance flag for an instant, and we shall lose all the fruits of their struggles, all that they won for us. Mankind is therefore like an army at war; now, every army needs discipline, and it is not sufficient that it submit to it on the day of combat. It must adapt to it during times of peace; without this, its defeat is certain, no amount of bravery can save it.

What I have just said is just as applicable to the struggle that mankind must wage for life. The discipline which mankind must accept is called morality. The day that mankind forgot it, it would be doomed to defeat and be plunged into an abyss of sufferings. On that

[1] This address was delivered by Henri Poincaré at the inaugural session of the French League of Moral Education, June 26, 1912, three weeks before his death. It was the last time that he spoke in public.

day, moreover, mankind would experience a moral decay; it would consider itself less beautiful and, so to speak, smaller. We should grieve at this not only because of the sufferings which would follow, but because it would be the darkening of something beautiful.

On all these counts, we all think the same; we all know where we must go. Why do we become divided when it is a matter of deciding which way to go? If reasonings were of any avail, it would be easy to reach an accord. Mathematicians never argue when it is a matter of knowing how to prove a theorem. But something very different is involved here. To work in the field of morals by means of reasonings is labor lost; in such matters, there is no reasoning to which we cannot retort.

Explain to the soldier what sufferings defeat will entail, and that it will even jeopardize his personal safety. He can always reply that his personal safety will be more assured if others do the fighting. If the soldier does not answer thus, it is because he is moved by some force which silences all reasonings. What we need are forces like that one. Now, the human soul is an inexhaustible reservoir of strength, a fecund source, a rich source of motive energy. Our feelings are this motive energy. The moralists must, so to speak, channel these forces and direct them in the proper direction, just as engineers tame natural energies and bend them to the needs of industry.

But—and this is where the difference arises—in order to make the same machine work, the engineers can just as well make use of either steam or water power. So also the professors of morality will at their pleasure be able to put into motion one or the other of the psychological forces. Each of them will naturally choose the force which he feels in himself; as to those forces which might come to him from without, or which he might borrow from his neighbor, he will manipulate them awkwardly. In his hands, they would be lifeless and ineffective, he will give them up, and he will be right. It is because their weapons are different that their methods must also be different. Why should they have a grudge against each other?

In the meantime, it is always the same morality which is taught. Whether you pursue the general welfare or whether you appeal to pity or to the sense of human dignity, you will always end up with the same precepts, those which cannot be forgotten without the nations perishing, without sufferings multiplying at the same time and man beginning to decline.

Why is it, therefore, that all these men who fight the same enemy, though with different weapons, so rarely remember that they are allies? Why do some occasionally rejoice in the defeats of the others? Do they forget that each of these defeats is a triumph for the eternal adversary, a diminution of the common patrimony? Oh, no, we are too much in need of all our forces to be in a position to neglect any; therefore, we reject no one, we ban only hate.

Certainly, hate is also a force, a very powerful force; but we may not make use of it because it makes everything appear smaller, because it is like opera glasses in which only the large ends can be used. Even from nation to nation hate is nefarious; and it is not hate that creates true heroes. I do not know if, beyond certain frontiers, it is considered advantageous to be patriotic by means of hate, but this is contrary to the instincts of our race and its traditions. The French armies have always fought either for someone or for something and not against anyone. And they have fought no less well for all that.

If, in internal affairs, the various parties forget the great ideas which were once their honor and their justification for existence, and recall only their hate; if one says: "I am anti-this," and the other replies: "I am anti-that," the horizon immediately narrows as if clouds had descended and obscured the summits. The vilest means are used; they shrink neither from the use of calumny nor from the use of informers, and those who are surprised at this become suspects. We see people rise who seem to possess only enough intelligence to lie, only enough heart to hate. And souls who are in no way vulgar, however slightly they take cover under the same banner, set aside for them treasures of indulgence, occasionally of admiration. And in view of such opposing hatreds, we hesitate to wish the defeat of one, which would be the triumph of the others.

Hatred is capable of all of this; and that is exactly what we do not want. Let us become reconciled therefore; let us learn to know each other and, in that way, to respect each other, in order to pursue the common ideal. Let us guard against imposing uniform methods on all; that is unrealizable and, moreover, it is not desirable. Uniformity is death because it is a door closed to all progress; and also, all constraint is sterile and odious.

Men are different; some are rebellious; they can be moved by a single word and remain indifferent to everything else. I have no way of knowing if this decisive word is not the one which you are about to say, and I would forbid you to say it! . . . But then, you see the danger: these men, who have not received the same education, are called upon to collide in life; as a result of these repeated collisions their souls will be disturbed and changed; perhaps they will change faith. What will happen if the new ideas which they adopt are those which their former teachers conveyed to them as the very negation of morality? Can this mental habit be lost in one day? At the same time, their new friends will not only teach them to reject what they have adored but even to despise it. They will not retain for the noble ideas which molded their souls that tender memory which outlives faith. Their moral ideals risk being carried away in this general ruin. Too old to acquire a new education, they shall lose the fruits of the old!

This danger would be averted or at least attenuated if we learned to speak only with respect of all the sincere efforts of those who work alongside us; this respect would be easy if we knew each other better.

And that is precisely the object of the League of Moral Education. Today's session, the lectures which you have just heard, prove adequately that it is possible to possess an ardent faith and to render justice to the faith of our neighbor, and that, when all is said and done, though our uniforms are different, we are so to speak only the different corps of the same army, fighting side by side.

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