SCIENCE AND METHOD

Henri Poincaré

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INTRODUCTION.

IN this work I have collected various studies which are more or less directly concerned with scientific methodology. The scientific method consists in observation and experiment. If the scientist had an infinity of time at his disposal, it would be sufficient to say to him, “ Look, and look carefully.” But, since he has not time to look at everything, and above all to look carefully, and since it is better not to look at all than to look carelessly, he is forced to make a selection. The first question, then, is to know how to make this selection. This question confronts the physicist as well as the historian ; it also confronts the mathematician, and the principles which should guide them all are not very dissimilar. The scientist conforms to them instinctively, and by reflecting on these principles one can foresee the possible future of mathematics.

We shall understand this still better if we observe the scientist at work ; and, to begin with, we must have some acquaintance with the psychological mechanism of discovery, more especially that of mathematical discovery. Observation of the mathematician’s method of working is specially instructive for the psychologist.

In all sciences depending on observation, we must reckon with errors due to imperfections of our senses and of our instruments. Happily we may admit that, under certain conditions, there is a partial compensation of these errors, so that they disappear in averages. This compensation is due to chance. But what is chance? It is a notion which is difficult of justification, and even of definition ; and yet what I have just said with regard to errors of observation, shows that the scientist cannot get on without it. It is necessary, therefore, to give as accurate a definition as possible of this notion, at once so indispensable and so elusive.

These are generalities which apply in the main to all sciences. For instance, there is no appreciable difference between the mechanism of mathematical discovery and the mechanism of discovery in general. Further on I approach questions more particularly concerned with certain special sciences, beginning with pure mathematics.

In the chapters devoted to them, I am obliged to treat of somewhat more abstract subjects, and, to begin with, I have to speak of the notion of space. Every one knows that space is relative, or rather every one says so, but how many people think still as if they considered it absolute. Nevertheless, a little reflection will show to what contradictions they are exposed.

Questions concerning methods of instruction are of importance, firstly, on their own account, and secondly, because one cannot reflect on the best method of imbuing virgin brains with new notions without, at the same time, reflecting on the manner in which these notions have been acquired by our ancestors, and consequently on their true origin—that is, in reality, on their true nature. Why is it that, in most cases, the definitions which satisfy scientists mean nothing at all to children? Why is it necessary to give them other definitions? This is the question I have set myself in the chapter which follows, and its solution might, I think, suggest useful reflections to philosophers interested in the logic of sciences.

On the other hand, there are many geometricians who believe that mathematics can be reduced to the rules of formal logic. Untold efforts have been made in this direction. To attain their object they have not hesitated, for instance, to reverse the historical order of the genesis of our conceptions, and have endeavoured to explain the finite by the infinite. I think I have succeeded in showing, for all who approach the problem with an open mind, that there is in this a deceptive illusion. I trust the reader will understand the importance of the question, and will pardon the aridity of the pages I have been constrained to devote to it.

The last chapters, relating to mechanics and astronomy, will be found easier reading.

Mechanics seem to be on the point of undergoing a complete revolution. The ideas which seemed most firmly established are being shattered by daring innovators. It would certainly be premature to decide in their favour from the start, solely because they are innovators ; but it is interesting to state their views, and this is what I have tried to do. As far as possible I have followed the historical order, for the new ideas would appear too surprising if we did not see the manner in which they had come into existence.

Astronomy offers us magnificent spectacles, and raises tremendous problems. We cannot dream of applying the experimental method to them directly ; our laboratories are too small. But analogy with the phenomena which these laboratories enable us to reach may nevertheless serve as a guide to the astronomer. The Milky Way, for instance, is an assemblage of suns whose motions appear at first sight capricious. But may not this assemblage be compared with that of the molecules of a gas whose properties we have learnt from the kinetic theory of gases? Thus the method of the physicist may come to the aid of the astronomer by a side-track.

Lastly, I have attempted to sketch in a few lines the history of the development of French geodesy. I have shown at what cost, and by what persevering efforts and often dangers, geodesists have secured for us the few notions we possess about the shape of the earth. Is this really a question of method ? Yes, for this history certainly teaches us what precautions must surround any serious scientific operation, and what time and trouble are involved in the conquest of a single new decimal.

BOOK I.

THE SCIENTIST AND SCIENCE.

I.

THE SELECTION OF FACTS.

TOLSTOI explains somewhere in his writings why, in his opinion, " Science for Science's sake " is an absurd conception. We cannot know all the facts, since they are practically infinite in number. We must make a selection ; and that being so, can this selection be governed by the mere caprice of our curiosity? Is it not better to be guided by utility, by our practical, and more especially our moral, necessities ? Have we not some better occupation than counting the number of lady-birds in existence on this planet ?

It is clear that for him the word utility has not the meaning assigned to it by business men, and, after them, by the greater number of our contemporaries. He cares but little for the industrial applications of science, for the marvels of electricity or of automobilism, which he regards rather as hindrances to moral progress. For him the useful is exclusively what is capable of making men better.

It is hardly necessary for me to state that, for my part, I could not be satisfied with either of these ideals. I have no liking either for a greedy and narrow plutocracy, or for a virtuous unaspiring democracy, solely occupied in turning the other cheek, in which we should find good people devoid of curiosity, who, avoiding all excesses, would not die of any disease—save boredom. But it is all a matter of taste, and that is not the point I wish to discuss.

None the less the question remains, and it claims our attention. if our selection is only determined by caprice or by immediate necessity, there can be no science for science's sake, and consequently no science. Is this true? There is no disputing the fact that a selection must be made: however great our activity, facts outstrip us, and we can never overtake them; while the scientist is discovering one fact, millions and millions are produced in every cubic inch of his body. Trying to make science contain nature is like trying to make the part contain the whole.

But scientists believe that there is a hierarchy of facts, and that a judicious selection can be made. They are right, for otherwise there would be no science, and science does exist. One has only to open one's eyes to see that the triumphs of industry, which have enriched so many practical men, would never have seen the light if only these practical men had existed, and if they had not been preceded by disinterested fools who died poor, who never thought of the useful, and yet had a guide that was not their own caprice.

What these fools did, as Mach has said, was to save their successors the trouble of thinking. If they had worked solely in view of an immediate application, they would have left nothing behind them, and in face of a new requirement, all would have had to be done again. Now the majority of men do not like thinking, and this is perhaps a good thing, since instinct guides them, and very often better than reason would guide a pure intelligence, at least whenever they are pursuing an end that is immediate and always the same. But instinct is routine, and if it were not fertilized by thought, it would advance no further with man than with the bee or the ant. It is necessary, therefore, to think for those who do not like thinking, and as they are many, each one of our thoughts must be useful in as many circumstances as possible. For this reason, the more general a law is, the greater is its value.

This shows us how our selection should be made. The most interesting facts are those which can be used several times, those which have a chance of recurring. We have been fortunate enough to be born in a world where there are such facts. Suppose that instead of eighty chemical elements we had eighty millions, and that they were not some common and others rare, but uniformly distributed. Then each time we picked up a new pebble there would be a strong probability that it was composed of some unknown substance. Nothing that we knew of other pebbles would tell us anything about it. Before each new object we should be like a new-born child ; like him we could but obey our caprices or our necessities. In such a world there would be no science, perhaps thought and even life would be impossible, since evolution could not have developed the instincts of self-preservation. Providentially it is not so ; but this blessing, like all those to which we are accustomed, is not appreciated at its true value. The biologist would be equally embarrassed if there were only individuals and no species, and if heredity did not make children resemble their parents.

Which, then, are the facts that have a chance of recurring? In the first place, simple facts. It is evident that in a complex fact many circumstances are united by chance, and that only a still more improbable chance could ever so unite them again. But are there such things as simple facts ? and if there are, how are we to recognize them? Who can tell that what we believe to be simple does not conceal an alarming complexity ? All that we can say is that we must prefer facts which appear simple, to those in which our rude vision detects dissimilar elements. Then only two alternatives are possible ; either this simplicity is real, or else the elements are so intimately mingled that they do not admit of being distinguished. In the first case we have a chance of meeting the same simple fact again, either in all its purity, or itself entering as an element into some complex whole. In the second case the intimate mixture has similarly a greater chance of being reproduced than a heterogeneous assemblage. Chance can mingle, but it cannot unmingle, and a combination of various elements in a well-ordered edifice in which something can be distinguished, can only be made deliberately. There is, therefore, but little chance that an assemblage in which different things can be distinguished should ever be reproduced. On the other hand, there is great probability that a mixture which appears homogeneous at first sight will be reproduced several times. Accordingly facts which appear simple, even if they are not so in reality, will be more easily brought about again by chance.

It is this that justifies the method instinctively adopted by scientists, and what perhaps justifies it still better is that facts which occur frequently appear to us simple just because we are accustomed to them.

But where is the simple fact ? Scientists have tried to find it in the two extremes, in the infinitely great and in the infinitely small. The astronomer has found it because the distances of the stars are immense, so great that each of them appears only as a point and qualitative differences disappear, and because a point is simpler than a body which has shape and qualities. The physicist, on the other hand, has sought the elementary phenomenon in an imaginary division of bodies into infinitely small atoms, because the conditions of the problem, which undergo slow and continuous variations as we pass from one point of the body to another, may be regarded as constant within each of these little atoms. Similarly the biologist has been led instinctively to regard the cell as more interesting than the whole animal, and the event has proved him right, since cells belonging to the most diverse organisms have greater resemblances, for those who can recognize them, than the organisms themselves. The sociologist is in a more embarrassing position. The elements, which for him are men, are too dissimilar, too variable, too capricious, in a word, too complex themselves. Furthermore, history does not repeat itself; how, then, is he to select the interesting fact, the fact which is repeated ? Method is precisely the selection of facts, and accordingly our first care must be to devise a method. Many have been devised because none holds the field undisputed. Nearly every sociological thesis proposes a new method, which, however, its author is very careful not to apply, so that sociology is the science with the greatest number of methods and the least results.

It is with regular facts, therefore, that we ought to begin ; but as soon as the rule is well established, as soon as it is no longer in doubt, the facts which are in complete conformity with it lose their interest, since they can teach us nothing new. Then it is the exception which becomes important. We cease to look for resemblances, and apply ourselves before all else to differences, and of these differences we select first those that are most accentuated, not only because they are the most striking, but because they will be the most instructive. This will be best explained by a simple example. Suppose we are seeking to determine a curve by observing some of the points on it. The practical man who looked only to immediate utility would merely observe the points he required for some special object ; these points would be badly distributed on the curve, they would be crowded together in certain parts and scarce in others, so that it would be impossible to connect them by a continuous line, and they would be useless for any other application. The scientist would proceed in a different manner. Since he wishes to study the curve for itself, he will distribute the points to be observed regularly, and as soon as he knows some of them, he will join them by a regular line, and he will then have the complete curve. But how is he to accomplish this ? If he has determined one extreme point on the curve, he will not remain close to this extremity, but will move to the other end. After the two extremities, the central point is the most instructive, and so on.

Thus when a rule has been established, we have first to look for the cases in which the rule stands the best chance of being found in fault. This is one of many reasons for the interest of astronomical facts and of geological ages. By making long excursions in space or in time, we may find our ordinary rules completely upset, and these great upsettings will give us a clearer view and better comprehension of such small changes as may occur nearer us, in the small corner of the world in which we are called to live and move. We shall know this corner better for the journey we have taken into distant lands where we had no concern.

But what we must aim at is not so much to ascertain resemblances and differences, as to discover similarities hidden under apparent discrepancies. The individual rules appear at first discordant, but on looking closer we can generally detect a resemblance ; though differing in matter, they approximate in form and in the order of their parts. When we examine them from this point of view, we shall see them widen and tend to embrace everything. This is what gives a value to certain facts that come to complete a whole, and show that it is the faithful image of other known wholes.

I cannot dwell further on this point, but these few words will suffice to show that the scientist does not make a random selection of the facts to be observed. He does not count lady-birds, as Tolstoi says, because the number of these insects, interesting as they are, is subject to capricious variations. He tries to condense a great deal of experience and a great deal of thought into a small volume, and that is why a little book on physics contains so many past experiments, and a thousand times as many possible ones, whose results are known in advance.

But so far we have only considered one side of the question. The scientist does not study nature because it is useful to do so. He studies it because he takes pleasure in it, and he takes pleasure in it because it is beautiful. If nature were not beautiful it would not be worth knowing, and life would not be worth living. I am not speaking, of course, of that beauty which strikes the senses, of the beauty of qualities and appearances. I am far from despising this, but it has nothing to do with science. What I mean is that more intimate beauty which comes from the harmonious order of its parts, and which a pure intelligence can grasp. It is this that gives a body a skeleton, so to speak, to the shimmering visions that flatter our senses, and without this support the beauty of these fleeting dreams would be imperfect, because it would be indefinite and ever elusive. Intellectual beauty, on the contrary, is self-sufficing, and it is for it, more perhaps than for the future good of humanity, that the scientist condemns himself to long and painful labours.

It is, then, the search for this special beauty, the sense of the harmony of the world, that makes us select the facts best suited to contribute to this harmony ; just as the artist selects those features of his sitter which complete the portrait and give it character and life. And there is no fear that this instinctive and unacknowledged preoccupation will divert the scientist from the search for truth. We may dream of a harmonious world, but how far it will fall short of the real world ! The Greeks, the greatest artists that ever were, constructed a heaven for themselves ; how poor a thing it is beside the heaven as we know it !

It is because simplicity and vastness are both beautiful that we seek by preference simple facts and vast facts ; that we take delight, now in following the giant courses of the stars, now in scrutinizing with a microscope that prodigious smallness which is also a vastness, and now in seeking in geological ages the traces of a past that attracts us because of its remoteness.

Thus we see that care for the beautiful leads us to the same selection as care for the useful. Similarly economy of thought, that economy of effort which, according to Mach, is the constant tendency of science, is a source of beauty as well as a practical advantage. The buildings we admire are those in which the architect has succeeded in proportioning the means to the end, in which the columns seem to carry the burdens imposed on them lightly and without effort, like the graceful caryatids of the Erechtheum.

Whence comes this concordance? Is it merely that things which seem to us beautiful are those which are best adapted to our intelligence, and that consequently they are at the same time the tools that intelligence knows best how to handle ? Or is it due rather to evolution and natural selection ? Have the peoples whose ideal conformed best to their own interests, properly understood, exterminated the others and taken their place? One and all pursued their ideal without considering the consequences, but while this pursuit led some to their destruction, it gave empire to others. We are tempted to believe this, for if the Greeks triumphed over the barbarians, and if Europe, heir of the thought of the Greeks, dominates the world, it is due to the fact that the savages loved garish colours and the blatant noise of the drum, which appealed to their senses, while the Greeks loved the intellectual beauty hidden behind sensible beauty, and that it is this beauty which gives certainty and strength to the intelligence.

No doubt Tolstoi would be horrified at such a triumph, and he would refuse to admit that it could be truly useful. But this disinterested pursuit of truth for its own beauty is also wholesome, and can make men better. I know very well there are disappointments, that the thinker does not always find the serenity he should, and even that some scientists have thoroughly bad tempers.

Must we therefore say that science should be abandoned, and morality alone be studied ? Does any one suppose that moralists themselves are entirely above reproach when they have come down from the pulpit ?

II.

THE FUTURE OF MATHEMATICS.

IF we wish to foresee the future of mathematics, our proper course is to study the history and present condition of the science.

For us mathematicians, is not this procedure to some extent professional ? We are accustomed to *extrapolation*, which is a method of deducing the future from the past and the present; and since we are well aware of its limitations, we run no risk of deluding ourselves as to the scope of the results it gives us.

In the past there have been prophets of ill. They took pleasure in repeating that all problems susceptible of being solved had already been solved, and that after them there would be nothing left but gleanings. Happily we are reassured by the example of the past. Many times already men have thought that they had solved all the problems, or at least that they had made an inventory of all that admit of solution. And then the meaning of the word solution has been extended; the insoluble problems have become the most interesting of all, and other problems hitherto undreamed of have presented themselves. For the Greeks a good solution was one that employed only rule and compass ; later it became one obtained by the extraction of radicals, then one in which algebraical functions and radicals alone figured. Thus the pessimists found themselves continually passed over, continually forced to retreat, so that at present I verily believe there are none left.

My intention, therefore, is not to refute them, since they are dead. We know very well that mathematics will continue to develop, but we have to find out in what direction. I shall be told " in all directions," and that is partly true ; but if it were altogether true, it would become somewhat alarming. Our riches would soon become embarrassing, and their accumulation would soon produce a mass just as impenetrable as the unknown truth was to the ignorant.

The historian and the physicist himself must make a selection of facts.

The scientist’s brain, which is only a corner of the universe, will never be able to contain the whole universe ; whence it follows that, of the innumerable facts offered by nature, we shall leave some aside and retain others. The same is true, *a fortiori*, in mathematics. The mathematician similarly cannot retain pell-mell all the facts that are presented to him, the more so that it is himself—I was almost going to say his own caprice—that creates these facts. It is he who assembles the elements and constructs a new combination from top to bottom ; it is generally not brought to him ready-made by nature.

No doubt it is sometimes the case that a mathematician attacks a problem to satisfy some requirement of physics, that the physicist or the engineer asks him to make a calculation in view of some particular application. Will it be said that we geometricians are to confine ourselves to waiting for orders, and, instead of cultivating our science for our own pleasure, to have no other care but that of accommodating ourselves to our clients’ tastes? If the only object of mathematics is to come to the help of those who make a study of nature, it is to them we must look for the word of command. Is this the correct view of the matter? Certainly not ; for if we had not cultivated the exact sciences for themselves, we should never have created the mathematical instrument, and when the word of command came from the physicist we should have been found without arms.

Similarly, physicists do not wait to study a phenomenon until some pressing need of material life makes it an absolute necessity, and they are quite right. If the scientists of the eighteenth century had disregarded electricity, because it appeared to them merely a curiosity having no practical interest, we should not have, in the twentieth century, either telegraphy or electro-chemistry or electro-traction. Physicists forced to select are not guided in their selection solely by utility. What method, then, do they pursue in making a selection between the different natural facts? I have explained this in the preceding chapter. The facts that interest them are those that may lead to the discovery of a law, those that have an analogy with many other facts and do not appear to us as isolated, but as closely grouped with others. The isolated fact attracts the attention of all, of the layman as well as the scientist. But what the true scientist alone can see is the link that unites several facts which have a deep but hidden analogy. The anecdote of Newton’s apple is probably not true, but it is symbolical, so we will treat it as if it were true. Well, we must suppose that before Newton’s day many men had seen apples fall, but none had been able to draw any conclusion. Facts would be barren if there were not minds capable of selecting between them and distinguishing those which have something hidden behind them and recognizing what is hidden—minds which, behind the bare fact, can detect the soul of the fact.

In mathematics we do exactly the same thing. Of the various elements at our disposal we can form millions of different combinations, but any one of these combinations, so long as it is isolated, is absolutely without value ; often we have taken great trouble to construct it, but it is of absolutely no use, unless it be, perhaps, to supply a subject for an exercise in secondary schools. It will be quite different as soon as this combination takes its place in a class of analogous combinations whose analogy we have recognized ; we shall then be no longer in presence of a fact, but of a law. And then the true discoverer will not be the workman who has patiently built up some of these combinations, but the man who has brought out their relation. The former has only seen the bare fact, the latter alone has detected the soul of the fact. The invention of a new word will often be sufficient to bring out the relation, and the word will be creative. The history of science furnishes us with a host of examples that are familiar to all.

The celebrated Viennese philosopher Mach has said that the part of science is to effect economy of thought, just as a machine effects economy of effort, and this is very true. The savage calculates on his fingers, or by putting together pebbles. By teaching children the multiplication table we save them later on countless operations with pebbles. Some one once recognized, whether by pebbles or otherwise, that 6 times 7 are 42, and had the idea of recording the result, and that is the reason why we do not need to repeat the operation. His time was not wasted even if he was only calculating for his own amusement. His operation only took him two minutes, but it would have taken two million, if a million people had had to repeat it after him.

Thus the importance of a fact is measured by the return it gives—that is, by the amount of thought it enables us to economize.

In physics, the facts which give a large return are those which take their place in a very general law, because they enable us to foresee a very large number of others, and it is exactly the same in mathematics. Suppose I apply myself to a complicated calculation and with much difficulty arrive at a result, I shall have gained nothing by my trouble if it has not enabled me to foresee the results of other analogous calculations, and to direct them with certainty, avoiding the blind groping with which I had to be contented the first time. On the contrary, my time will not have been lost if this very groping has succeeded in revealing to me the profound analogy between the problem just dealt with and a much more extensive class of other problems; if it has shown me at once their resemblances and their differences ; if, in a word, it has enabled me to perceive the possibility of a generalization. Then it will not be merely a new result that I have acquired, but a new force.

An algebraical formula which gives us the solution of a type of numerical problems, if we finally replace the letters by numbers, is the simple example which occurs to one's mind at once. Thanks to the formula, a single algebraical calculation saves us the trouble of a constant repetition of numerical calculations. But this is only a rough example: every one feels that there are analogies which cannot be expressed by a formula, and that they are the most valuable.

If a new result is to have any value, it must unite elements long since known, but till then scattered and seemingly foreign to each other, and suddenly introduce order where the appearance of disorder reigned. Then it enables us to see at a glance each of these elements in the place it occupies in the whole. Not only is the new fact valuable on its own account, but it alone gives a value to the old facts it unites. Our mind is frail as our senses are; it would lose itself in the complexity of the world if that complexity were not harmonious ; like the short-sighted, it would only see the details, and would be obliged to forget each of these details before examining the next, because it would be incapable of taking in the whole. The only facts worthy of our attention are those which introduce order into this complexity and so make it accessible to us.

Mathematicians attach a great importance to the elegance of their methods and of their results, and this is not mere dilettantism. What is it that gives us the feeling of elegance in a solution or a demonstration ? It is the harmony of the different parts, their symmetry, and their happy adjustment; it is, in a word, all that introduces order, all that gives them unity, that enables us to obtain a clear comprehension of the whole as well as of the parts. But that is also precisely what causes it to give a large return ; and in fact the more we see this whole clearly and at a single glance, the better we shall perceive the analogies with other neighbouring objects, and consequently the better chance we shall have of guessing the possible generalizations. Elegance may result from the feeling of surprise caused by the unlooked-for occurrence together of objects not habitually associated. In this, again, it is fruitful, since it thus discloses relations till then unrecognized. It is also fruitful even when it only results from the contrast between the simplicity of the means and the complexity of the problem presented, for it then causes us to reflect on the reason for this contrast, and generally shows us that this reason is not chance, but is to be found in some unsuspected law. Briefly stated, the sentiment of mathematical elegance is nothing but the satisfaction due to some conformity between the solution we wish to discover and the necessities of our mind, and it is on account of this very conformity that the solution can be an instrument for us. This æsthetic satisfaction is consequently connected with the economy of thought. Again the comparison with the Erechtheum occurs to me, but I do not wish to serve it up too often.

It is for the same reason that, when a somewhat lengthy calculation has conducted us to some simple and striking result, we are not satisfied until we have shown that we might have foreseen, if not the whole result, at least its most characteristic features. Why is this ? What is it that prevents our being contented with a calculation which has taught us apparently all that we wished to know? The reason is that, in analogous cases, the lengthy calculation might not be able to be used again, while this is not true of the reasoning, often semi-intuitive, which might have enabled us to foresee the result. This reasoning being short, we can see all the parts at a single glance, so that we perceive immediately what must be changed to adapt it to all the problems of a similar nature that may be presented. And since it enables us to foresee whether the solution of these problems will be simple, it shows us at least whether the calculation is worth undertaking.

What I have just said is sufficient to show how vain it would be to attempt to replace the mathematician’s free initiative by a mechanical process of any kind. In order to obtain a result having any real value, it is not enough to grind out calculations, or to have a machine for putting things in order: it is not order only, but unexpected order, that has a value. A machine can take hold of the bare fact, but the soul of the fact will always escape it.

Since the middle of last century, mathematicians have become more and more anxious to attain to absolute exactness. They are quite right, and this tendency will become more and more marked. In mathematics, exactness is not everything, but without it there is nothing: a demonstration which lacks exactness is nothing at all. This is a truth that I think no one will dispute, but if it is taken too literally it leads us to the conclusion that before 1820, for instance, there was no such thing as mathematics, and this is clearly an exaggeration. The geometricians of that day were willing to assume what we explain by prolix dissertations. This does not mean that they did not see it at all, but they passed it over too hastily, and, in order to see it clearly, they would have had to take the trouble to state it.

Only, is it always necessary to state it so many times? Those who were the first to pay special attention to exactness have given us reasonings that we may attempt to imitate ; but if the demonstrations of the future are to be constructed on this model, mathematical works will become exceedingly long, and if I dread length, it is not only because I am afraid of the congestion of our libraries, but because I fear that as they grow in length our demonstrations will lose that appearance of harmony which plays such a useful part, as I have just explained.

It is economy of thought that we should aim at, and therefore it is not sufficient to give models to be copied. We must enable those that come after us to do without the models, and not to repeat a previous reasoning, but summarize it in a few lines. And this has already been done successfully in certain cases. For instance, there was a whole class of reasonings that resembled each other, and were found everywhere ; they were perfectly exact, but they were long. One day some one thought of the term " uniformity of convergence," and this term alone made them useless ; it was no longer necessary to repeat them, since they could now be assumed. Thus the hair-splitters can render us a double service, first by teaching us to do as they do if necessary, but more especially by enabling us as often as possible not to do as they do, and yet make no sacrifice of exactness.

One example has just shown us the importance of terms in mathematics; but I could quote many others. It is hardly possible to believe what economy of thought, as Mach used to say, can be effected by a well-chosen term. I think I have already said somewhere that mathematics is the art of giving the same name to different things. It is enough that these things, though differing in matter, should be similar in form, to permit of their being, so to speak, run in the same mould. When language has been well chosen, one is astonished to find that all demonstrations made for a known object apply immediately to many new objects : nothing requires to be changed, not even the terms, since the names have become the same.

A well-chosen term is very often sufficient to remove the exceptions permitted by the rules as stated in the old phraseology. This accounts for the invention of negative quantities, imaginary quantities, decimals to infinity, and I know not what else. And we must never forget that exceptions are pernicious, because they conceal laws.

This is one of the characteristics by which we recognize facts which give a great return : they are the facts which permit of these happy innovations of language. The bare fact, then, has sometimes no great interest: it may have been noted many times without rendering any great service to science ; it only acquires a value when some more careful thinker perceives the connexion it brings out, and symbolizes it by a term.

The physicists also proceed in exactly the same way. They have invented the term “energy,” and the term has been enormously fruitful, because it also creates a law by eliminating exceptions ; because it gives the same name to things which differ in matter, but are similar in form.

Among the terms which have exercised the most happy influence I would note “group” and “invariable.” They have enabled us to perceive the essence of many mathematical reasonings, and have shown us in how many cases the old mathematicians were dealing with groups without knowing it, and how, believing themselves far removed from each other, they suddenly found themselves close together without understanding why.

To-day we should say that they had been examining isomorphic groups. We now know that, in a group, the matter is of little interest, that the form only is of importance, and that when we are well acquainted with one group, we know by that very fact all the isomorphic groups. Thanks to the terms “group” and “isomorphism,” which sum up this subtle rule in a few syllables, and make it readily familiar to all minds, the passage is immediate, and can be made without expending any effort of thinking. The idea of group is, moreover, connected with that of transformation. Why do we attach so much value to the discovery of a new transformation? It is because, from a single theorem, it enables us to draw ten or twenty others. It has the same value as a zero added to the right of a whole number.

This is what has determined the direction of the movement of mathematical science up to the present, and it is also most certainly what will determine it in the future. But the nature of the problems which present themselves contributes to it in an equal degree. We cannot forget what our aim should be, and in my opinion this aim is a double one. Our science borders on both philosophy and physics, and it is for these two neighbours that we must work. And so we have always seen, and we shall still see, mathematicians advancing in two opposite directions.

On the one side, mathematical science must reflect upon itself, and this is useful because reflecting upon itself is reflecting upon the human mind which has created it ; the more so because, of all its creations, mathematics is the one for which it has borrowed least from outside. This is the reason for the utility of certain mathematical speculations, such as those which have in view the study of postulates, of unusual geometries, of functions with strange behaviour. The more these speculations depart from the most ordinary conceptions, and, consequently, from nature and applications to natural problems, the better will they show us what the human mind can do when it is more and more withdrawn from the tyranny of the exterior world ; the better, consequently, will they make us know this mind itself.

But it is to the opposite side, to the side of nature, that we must direct our main forces.

There we meet the physicist or the engineer, who says, “Will you integrate this differential equation for me ; I shall need it within a week for a piece of construction work that has to be completed by a certain date ?” “This equation,” we answer, “is not included in one of the types that can be integrated, of which you know there are not very many.” “Yes, I know ; but, then, what good are you ? ” More often than not a mutual understanding is sufficient. The engineer does not really require the integral in finite terms, he only requires to know the general behaviour of the integral function, or he merely wants a certain figure which would be easily deduced from this integral if we knew it. Ordinarily we do not know it, but we could calculate the figure without it, if we knew just what figure and what degree of exactness the engineer required.

Formerly an equation was not considered to have been solved until the solution had been expressed by means of a finite number of known functions. But this is impossible in about ninety-nine cases out of a hundred. What we can always do, or rather what we should always try to do, is to solve the problem *qualitatively*, so to speak—that is, to try to know approximately the general form of the curve which represents the unknown function.

It then remains to find the *exact* solution of the problem. But if the unknown cannot be determined by a finite calculation, we can always represent it by an infinite converging series which enables us to calculate it. Can this be regarded as a true solution ? The story goes that Newton once communicated to Leibnitz an anagram somewhat like the following: *aaaaabbbeeeeii*, etc. Naturally, Leibnitz did not understand it at all, but we who have the key know that the anagram, translated into modern phraseology, means, "I know how to integrate all differential equations," and we are tempted to make the comment that Newton was either exceedingly fortunate or that he had very singular illusions. What he meant to say was simply that he could form (by means of indeterminate coefficients) a series of powers formally satisfying the equation presented.

To-day a similar solution would no longer satisfy us, for two reasons—because the convergence is too slow, and because the terms succeed one another without obeying any law. On the other hand the series θ appears to us to leave nothing to be desired, first, because it converges very rapidly (this is for the practical man who wants his number as quickly as possible), and secondly, because we perceive at a glance the law of the terms, which satisfies the æsthetic requirements of the theorist.

There are, therefore, no longer some problems solved and others unsolved, there are only problems *more or less* solved, according as this is accomplished by a series of more or less rapid convergence or regulated by a more or less harmonious law. Nevertheless an imperfect solution may happen to lead us towards a better one.

Sometimes the series is of such slow convergence that the calculation is impracticable, and we have only succeeded in demonstrating the possibility of the problem. The engineer considers this absurd, and he is right, since it will not help him to complete his construction within the time allowed. He doesn't trouble himself with the question whether it will be of use to the engineers of the twenty-second century. We think differently, and we are sometimes more pleased at having economized a day's work for our grandchildren than an hour for our contemporaries.

Sometimes by groping, so to speak, empirically, we arrive at a formula that is sufficiently convergent. What more would you have ? says the engineer; and yet, in spite of everything, we are not satisfied, for we should have liked to be able to *predict* the convergence. And why? Because if we had known how to predict it in the one case, we should know how to predict it in another. We have been successful, it is true, but that is little in our eyes if we have no real hope of repeating our success.

In proportion as the science develops, it becomes more difficult to take it in in its entirety. Then an attempt is made to cut it in pieces and to be satisfied with one of these pieces—in a word, to specialize. Too great a movement in this direction would constitute a serious obstacle to the progress of the science. As I have said, it is by unexpected concurrences between its different parts that it can make progress. Too much specializing would prohibit these concurrences. Let us hope that congresses, such as those of Heidelberg and Rome, by putting us in touch with each other, will open up a view of our neighbours' territory, and force us to compare it with our own, and so escape in a measure from our own little village. In this way they will be the best remedy against the danger I have just noted.

But I have delayed too long over generalities ; it is time to enter into details.

Let us review the different particular sciences which go to make up mathematics ; Jet us see what each of them has done, in what direction it is tending, and what we may expect of it. If the preceding views are correct, we should see that the great progress of the past has been made when two of these sciences have been brought into conjunction, when men have become aware of the similarity of their form in spite of the dissimilarity of their matter, when they have modelled themselves upon each other in such a way that each could profit by the triumphs of the other. At the same time we should look to concurrences of a similar nature for progress in the future.

ARITHMETIC.

The progress of arithmetic has been much slower than that of algebra and analysis, and it is easy to understand the reason. The feeling of continuity is a precious guide which fails the arithmetician. Every whole number is separated from the rest, and has, so to speak, its own individuality ; each of them is a sort of exception, and that is the reason why general theorems will always be less common in the theory of numbers, and also why those that do exist will be more hidden and will longer escape detection.

If arithmetic is backward as compared with algebra and analysis, the best thing for it to do is to try to model itself on these sciences, in order to profit by their advance. The arithmetician then should be guided by the analogies with algebra. These analogies are numerous, and if in many cases they have not yet been studied sufficiently closely to become serviceable, they have at least been long foreshadowed, and the very language of the two sciences shows that they have been perceived. Thus we speak of transcendental numbers, and so become aware of the fact that the future classification of these numbers has already a model in the classification of transcendental functions. However, it is not yet very clear how we are to pass from one classification to the other; but if it were clear it would be already done, and would no longer be the work of the future.

The first example that comes to my mind is the theory of congruents, in which we find a perfect parallelism with that of algebraic equations. We shall certainly succeed in completing this parallelism, which must exist, for instance, between the theory of algebraic curves and that of congruents with two variables. When the problems relating to congruents with several variables have been solved, we shall have made the first step towards the solution of many questions of indeterminate analysis.

ALGEBRA.

The theory of algebraic equations will long continue to attract the attention of geometricians, the sides by which it may be approached being so numerous and so different.

It must not be supposed that algebra is finished because it furnishes rules for forming all possible combinations ; it still remains to find interesting combinations, those that satisfy such and such conditions. Thus there will be built up a kind of indeterminate analysis, in which the unknown quantities will no longer be whole numbers but polynomials. So this time it is algebra that will model itself on arithmetic, being guided by the analogy of the whole number, either with the whole polynomial with indefinite coefficients, or with the whole polynomial with whole coefficients.

GEOMETRY.

It would seem that geometry can contain nothing that is not already contained in algebra or analysis, and that geometric facts are nothing but the facts of algebra or analysis expressed in another language. It might be supposed, then, that after the review that has just been made, there would be nothing left to say having any special bearing on geometry. But this would imply a failure to recognize the great importance of a well-formed language, or to understand what is added to things themselves by the method of expressing, and consequently of grouping, those things.

To begin with, geometric considerations lead us to set ourselves new problems. These are certainly, if you will, analytical problems, but they are problems we should never have set ourselves on the score of analysis. Analysis, however, profits by them, as it profits by those it is obliged to solve in order to satisfy the requirements of physics.

One great advantage of geometry lies precisely in the fact that the senses can come to the assistance of the intellect, and help to determine the road to be followed, and many minds prefer to reduce the problems of analysis to geometric form. Unfortunately our senses cannot carry us very far, and they leave us in the lurch as soon as we wish to pass outside the three classical dimensions. Does this mean that when we have left this restricted domain in which they would seem to wish to imprison us, we must no longer count on anything but pure analysis, and that all geometry of more than three dimensions is vain and without object? In the generation which preceded ours, the greatest masters would have answered "Yes." To-day we are so familiar with this notion that we can speak of it, even in a university course, without exciting too much astonishment.

But of what use can it be? This is easy to see. In the first place it gives us a very convenient language, which expresses in very concise terms what the ordinary language of analysis would state in long-winded phrases. More than that, this language causes us to give the same name to things which resemble one another, and states analogies which it does not allow us to forget. It thus enables us still to find our way in that space which is too great for us, by calling to our mind continually the visible space, which is only an imperfect image of it, no doubt, but still an image. Here again, as in all the preceding examples, it is the analogy with what is simple that enables us to understand what is complex.

This geometry of more than three dimensions is not a simple analytical geometry, it is not purely quantitative, but also qualitative, and it is principally on this ground that it becomes interesting. There is a science called *Geometry of Position*, which has for its object the study of the relations of position of the different elements of a figure, after eliminating their magnitudes. This geometry is purely qualitative ; its theorems would remain true if the figures, instead of being exact, were rudely imitated by a child. We can also construct a *Geometry of Position* ot more than three dimensions. The importance of *Geometry of Position* is immense, and I cannot insist upon it too much ; what Riemann, one of its principal creators, has gained from it would be sufficient to demonstrate this. We must succeed in constructing it completely in the higher spaces, and we shall then have an instrument which will enable us really to see into hyperspace and to supplement our senses.

The problems of *Geometry of Position* would perhaps not have presented themselves if only the language of analysis had been used. Or rather I am wrong, for they would certainly have presented themselves, since their solution is necessary for a host of questions of analysis, but they would have presented themselves isolated, one after the other, and without our being able to perceive their common link.

CANTORISM.

I have spoken above of the need we have of returning continually to the first principles of our science, and of the advantage of this process to the study of the human mind. It is this need which has inspired two attempts which have held a very great place in the most recent history of mathematics. The first is Cantorism, and the services it has rendered to the science are well known. Cantor introduced into the science a new method of considering mathematical infinity, and I shall have occasion to speak of it again in Book II., chapter iii. One of the characteristic features of Cantorism is that, instead of rising to the general by erecting more and more complicated constructions, and defining by construction, it starts with the *genus supremum* and only defines, as the scholastics would have said, *per genus proximum et differential specificam.* Hence the horror he has sometimes inspired in certain minds, such as Hermitte's, whose favourite idea was to compare the mathematical with the natural sciences. For the greater number of us these prejudices had been dissipated, but it has come about that we have run against certain paradoxes and apparent contradictions, which would have rejoiced the heart of Zeno of Elea and the school of Megara. Then began the business of searching for a remedy, each man his own way. For my part I think, and I am not alone in so thinking, that the important thing is never to introduce any entities but such as can be completely defined in a finite number of words. Whatever be the remedy adopted, we can promise ourselves the joy of the doctor called in to follow a fine pathological case.

THE SEARCH FOR POSTULATES.

Attempts have been made, from another point of view, to enumerate the axioms and postulates more or less concealed which form the foundation of the different mathematical theories, and in this direction Mr. Hilbert has obtained the most brilliant results. It seems at first that this domain must be strictly limited, and that there will be nothing more to do when the inventory has been completed, which cannot be long. But when everything has been enumerated, there will be many ways of classifying it all. A good librarian always finds work to do, and each new classification will be instructive for the philosopher.

I here close this review, which I cannot dream of making complete. I think that these examples will have been sufficient to show the mechanism by which the mathematical sciences have progressed in the past, and the direction in which they must advance in the future.

III.

MATHEMATICAL DISCOVERY.

THE genesis of mathematical discovery is a problem which must inspire the psychologist with the keenest interest. For this is the process in which the human mind seems to borrow least from the exterior world, in which it acts, or appears to act, only by itself and on itself, so that by studying the process of geometric thought we may hope to arrive at what is most essential in the human mind.

This has long been understood, and a few months ago a review called *l'Enseignement Mathématique*, edited by MM. Laisant and Fehr, instituted an enquiry into the habits of mind and methods of work of different mathematicians. I had outlined the principal features of this article when the results of the enquiry were published, so that I have hardly been able to make any use of them, and I will content myself with saying that the majority of the evidence confirms my conclusions. I do not say there is unanimity, for on an appeal to universal suffrage we cannot hope to obtain unanimity.

One first fact must astonish us, or rather would astonish us if we were not too much accustomed to it. How does it happen that there are people who do not understand mathematics ? If the science invokes only the rules of logic, those accepted by all well-formed minds, if its evidence is founded on principles that are common to all men, and that none but a madman would attempt to deny, how does it happen that there are so many people who are entirely impervious to it ?

There is nothing mysterious in the fact that every one is not capable of discovery. That every one should not be able to retain a demonstration he has once learnt is still comprehensible. But what does seem most surprising, when we consider it, is that any one should be unable to understand a mathematical argument at the very moment it is stated to him. And yet those who can only follow the argument with difficulty are in a majority ; this is incontestable, and the experience of teachers of secondary education will certainly not contradict me.

And still further, how is error possible in mathematics ? A healthy intellect should not be guilty of any error in logic, and yet there are very keen minds which will not make a false step in a short argument such as those we have to make in the ordinary actions of life, which yet are incapable of following or repeating without error the demonstrations of mathematics which are longer, but which are, after all, only accumulations of short arguments exactly analogous to those they make so easily. Is it necessary to add that mathematicians themselves are not infallible?

The answer appears to me obvious. Imagine a long series of syllogisms in which the conclusions of those that precede form the premises of those that follow. We shall be capable of grasping each of the syllogisms, and it is not in the passage from premises to conclusion that we are in danger of going astray. But between the moment when we meet a proposition for the first time as the conclusion of one syllogism, and the moment when we find it once more as the premise of another syllogism, much time will sometimes have elapsed, and we shall have unfolded many links of the chain; accordingly it may well happen that we shall have forgotten it, or, what is more serious, forgotten its meaning. So we may chance to replace it by a somewhat different proposition, or to preserve the same statement but give it a slightly different meaning, and thus we are in danger of falling into error.

A mathematician must often use a rule, and, naturally, he begins by demonstrating the rule. At the moment the demonstration is quite fresh in his memory he understands perfectly its meaning and significance, and he is in no danger of changing it. But later on he commits it to memory, and only applies it in a mechanical way, and then, if his memory fails him, he may apply it wrongly. It is thus, to take a simple and almost vulgar example, that we sometimes make mistakes in calculation, because we have forgotten our multiplication table.

On this view special aptitude for mathematics would be due to nothing but a very certain memory or a tremendous power of attention. It would be a quality analogous to that of the whist player who can remember the cards played, or, to rise a step higher, to that of the chess player who can picture a very great number of combinations and retain them in his memory. Every good mathematician should also be a good chess player and *vice versâ*, and similarly he should be a good numerical calculator.

Certainly this sometimes happens, and thus Gauss was at once a geometrician of genius and a very precocious and very certain calculator.

But there are exceptions, or rather I am wrong, for I cannot call them exceptions, otherwise the exceptions would be more numerous than the cases of conformity with the rule. On the contrary, it was Gauss who was an exception. As for myself, I must confess I am absolutely incapable of doing an addition sum without a mistake. Similarly I should be a very bad chess player. I could easily calculate that by playing in a certain way I should be exposed to such and such a danger; I should then review many other moves, which I should reject for other reasons, and I should end by making the move I first examined, having forgotten in the interval the danger I had foreseen.

In a word, my memory is not bad, but it would be insufficient to make me a good chess player. Why, then, does it not fail me in a difficult mathematical argument in which the majority of chess players would be lost ? Clearly because it is guided by the general trend of the argument. A mathematical demonstration is not a simple juxtaposition of syllogisms ; it consists of syllogisms *placed in a certain order*, and the order in which these elements are placed is much more important than the elements themselves. If I have the feeling, so to speak the intuition, of this order, so that I can perceive the whole of the argument at a glance, I need no longer be afraid of forgetting one of the elements; each of them will place itself naturally in the position prepared for it, without my having to make any effort of memory.

It seems to me, then, as I repeat an argument I have learnt, that I could have discovered it. This is often only an illusion ; but even then, even if I am not clever enough to create for myself, I rediscover it myself as I repeat it.

We can understand that this feeling, this intuition of mathematical order, which enables us to guess hidden harmonies and relations, cannot belong to every one. Some have neither this delicate feeling that is difficult to define, nor a power of memory and attention above the common, and so they are absolutely incapable of understanding even the first steps of higher mathematics. This applies to the majority of people. Others have the feeling only in a slight degree, but they are gifted with an uncommon memory and a great capacity for attention. They learn the details one after the other by heart, they can understand mathemathics and sometimes apply them, but they are not in a condition to create.

Lastly, others possess the special intuition I have spoken of more or less highly developed, and they can not only understand mathematics, even though their memory is in no way extraordinary, but they can become creators, and seek to make discovery with more or less chance of success, according as their intuition is more or less developed.

What, in fact, is mathematical discovery ? It does not consist in making new combinations with mathematical entities that are already known. That can be done by any one, and the combinations that could be so formed would be infinite in number, and the greater part of them would be absolutely devoid of interest. Discovery consists precisely in not constructing useless combinations, but in constructing those that are useful, which are an infinitely small minority. Discovery is discernment, selection.

How this selection is to be made I have explained above. Mathematical facts worthy of being studied are those which, by their analogy with other facts, are capable of conducting us to the knowledge of a mathematical law, in the same way that experimental facts conduct us to the knowledge of a physical law. They are those which reveal unsuspected relations between other facts, long since known, but wrongly believed to be unrelated to each other.

Among the combinations we choose, the most fruitful are often those which are formed of elements borrowed from widely separated domains. I do not mean to say that for discovery it is sufficient to bring together objects that are as incongruous as possible. The greater part of the combinations so formed would be entirely fruitless, but some among them, though very rare, are the most fruitful of all.

Discovery, as I have said, is selection. But this is perhaps not quite the right word. It suggests a purchaser who has been shown a large number of samples, and examines them one after the other in order to make his selection. In our case the samples would be so numerous that a whole life would not give sufficient time to examine them. Things do not happen in this way. Unfruitful combinations do not so much as present themselves to the mind of the discoverer. In the field of his consciousness there never appear any but really useful combinations, and some that he rejects, which, however, partake to some extent of the character of useful combinations. Everything happens as if the discoverer were a secondary examiner who had only to interrogate candidates declared eligible after passing a preliminary test.

But what I have said up to now is only what can be observed or inferred by reading the works of geometricians, provided they are read with some reflection.

It is time to penetrate further, and to see what happens in the very soul of the mathematician. For this purpose I think I cannot do better than recount my personal recollections. Only I am going to confine myself to relating how I wrote my first treatise on Fuchsian functions. I must apologize, for I am going to introduce some technical expressions, but they need not alarm the reader, for he has no need to understand them. I shall say, for instance, that I found the demonstration of such and such a theorem under such and such circumstances ; the theorem will have a barbarous name that many will not know, but that is of no importance. What is interesting for the psychologist is not the theorem but the circumstances.

For a fortnight I had been attempting to prove that there could not be any function analogous to what I have since called Fuchsian functions. I was at that time very ignorant. Every day I sat down at my table and spent an hour or two trying a great number of combinations, and I arrived at no result. One night I took some black coffee, contrary to my custom, and was unable to sleep. A host of ideas kept surging in my head; I could almost feel then jostling one another, until two of them coalesced, so to speak, to form a stable combination. When morning came, I had established the existence of one class of Fuchsian functions, those that are derived from the hypergeometric series. I had only to verify the results, which only took a few hours.

Then I wished to represent these functions by the quotient of two series. This idea was perfectly conscious and deliberate ; I was guided by the analogy with elliptical functions. I asked myself what must be the properties of these series, if they existed, and I succeeded without difficulty in forming the series that I have called Theta-Fuchsian.

At this moment I left Caen, where I was then living, to take part in a geological conference arranged by the School of Mines. The incidents of the journey made me forget my mathematical work. When we arrived at Coutances, we got into a break to go for a drive, and, just as I put my foot on the step, the idea came to me, though nothing in my former thoughts seemed to have prepared me for it, that the transformations I had used to define Fuchsian functions were identical with those of non-Euclidian geometry. I made no verification, and had no time to do so, since I took up the conversation again as soon as I had sat down in the break, but I felt absolute certainty at once. When I got back to Caen I verified the result at my leisure to satisfy my conscience.

I then began to study arithmetical questions without any great apparent result, and without suspecting that they could have the least connexion with my previous researches. Disgusted at my want of success, I went away to spend a few days at the seaside, and thought of entirely different things. One day, as I was walking on the cliff, the idea came to me, again with the same characteristics of conciseness, suddenness, and immediate certainty, that arithmetical transformations of indefinite ternary quadratic forms are identical with those of non-Euclidian geometry.

Returning to Caen, I reflected on this result and deduced its consequences. The example of quadratic forms showed me that there are Fuchsian groups other than those which correspond with the hypergeometric series ; I saw that I could apply to them the theory of the Theta-Fuchsian series, and that, consequently, there are Fuchsian functions other than those which are derived from the hypergeometric series, the only ones I knew up to that time. Naturally, I proposed to form all these functions. I laid siege to them systematically and captured all the outworks one after the other. There was one, however, which still held out, whose fall would carry with it that of the central fortress. But all my efforts were of no avail at first, except to make me better understand the difficulty, which was already something. All this work was perfectly conscious.

Thereupon I left for Mont-Valérien, where I had to serve my time in the army, and so my mind was preoccupied with very different matters. One day, as I was crossing the street, the solution of the difficulty which had brought me to a standstill came to me all at once. I did not try to fathom it immediately, and it was only after my service was finished that I returned to the question. I had all the elements, and had only to assemble and arrange them. Accordingly I composed my definitive treatise at a sitting and without any difficulty.

It is useless to multiply examples, and I will content myself with this one alone. As regards my other researches, the accounts I should give would be exactly similar, and the observations related by other mathematicians in the enquiry of *l'Enseignement Mathématique* would only confirm them.

One is at once struck by these appearances of sudden illumination, obvious indications of. a long course of previous unconscious work. The part played by this unconscious work in mathematical discovery seems to me indisputable, and we shall find traces of it in other cases where it is less evident. Often when a man is working at a difficult question, he accomplishes nothing the first time he sets to work. Then he takes more or less of a rest, and sits down again at his table. During the first half-hour he still finds nothing, and then all at once the decisive idea presents itself to his mind. We might say that the conscious work proved more fruitful because it was interrupted and the rest restored force and freshness to the mind. But it is more probable that the rest was occupied with unconscious work, and that the result of this work was afterwards revealed to the geometrician exactly as in the cases I have quoted, except that the revelation, instead of coming to light during a walk or a journey, came during a period of conscious work, but independently of that work, which at most only performs the unlocking process, as if it were the spur that excited into conscious form the results already acquired during the rest, which till then remained unconscious.

There is another remark to be made regarding the conditions of this unconscious work, which is, that it is not possible, or in any case not fruitful, unless it is first preceded and then followed by a period of conscious work. These sudden inspirations are never produced (and this is sufficiently proved already by the examples I have quoted) except after some days of voluntary efforts which appeared absolutely fruitless, in which one thought one had accomplished nothing, and seemed to be on a totally wrong track. These efforts, however, were not as barren as one thought ; they set the unconscious machine in motion, and without them it would not have worked at all, and would not have produced anything.

The necessity for the second period of conscious work can be even more readily understood. It is necessary to work out the results of the inspiration, to deduce the immediate consequences and put them in order and to set out the demonstrations ; but, above all, it is necessary to verify them. I have spoken of the feeling of absolute certainty which accompanies the inspiration ; in the cases quoted this feeling was not deceptive, and more often than not this will be the case. But we must beware of thinking that this is a rule without exceptions. Often the feeling deceives us without being any less distinct on that account, and we only detect it when we attempt to establish the demonstration. I have observed this fact most notably with regard to ideas that have come to me in the morning or at night when I have been in bed in a semisomnolent condition.

Such are the facts of the case, and they suggest the following reflections. The result of all that precedes is to show that the unconscious ego, or, as it is called, the subliminal ego, plays a most important part in mathematical discovery. But the subliminal ego is generally thought of as purely automatic. Now we have seen that mathematical work is not a simple mechanical work, and that it could not be entrusted to any machine, whatever the degree of perfection we suppose it to have been brought to. It is not merely a question of applying certain rules, of manufacturing as many combinations as possible according to certain fixed laws. The combinations so obtained would be extremely numerous, useless, and encumbering. The real work of the discoverer consists in choosing between these combinations with a view to eliminating those that are useless, or rather not giving himself the trouble of making them at all. The rules which must guide this choice are extremely subtle and delicate, and it is practically impossible to state them in precise language ; they must be felt rather than formulated. Under these conditions, how can we imagine a sieve capable of applying them mechanically ?

The following, then, presents itself as a first hypothesis. The subliminal ego is in no way inferior to the conscious ego ; it is not purely automatic ; it is capable of discernment; it has tact and lightness of touch ; it can select, and it can divine. More than that, it can divine better than the conscious ego, since it succeeds where the latter fails. In a word, is not the subliminal ego superior to the conscious ego? The importance of this question will be readily understood. In a recent lecture, M. Boutroux showed how it had arisen on entirely different occasions, and what consequences would be involved by an answer in the affirmative. (See also the same author's *Science et Religion*, pp. 313 *et seq*.)

Are we forced to give this affirmative answer by the facts I have just stated ? I confess that, for my part, I should be loth to accept it. Let us, then, return to the facts, and see if they do not admit of some other explanation.

It is certain that the combinations which present themselves to the mind in a kind of sudden illumination after a somewhat prolonged period of unconscious work are generally useful and fruitful combinations, which appear to be the result of a preliminary sifting. Does it follow from this that the subliminal ego, having divined by a delicate intuition that these combinations could be useful, has formed none but these, or has it formed a great many others which were devoid of interest, and remained unconscious ?

Under this second aspect, all the combinations are formed as a result of the automatic action of the subliminal ego, but those only which are interesting find their way into the field of consciousness. This, too, is most mysterious. How can we explain the fact that, of the thousand products of our unconscious activity, some are invited to cross the threshold, while others remain outside? Is it mere chance that gives them this privilege ? Evidently not. For instance, of all the excitements of our senses, it is only the most intense that retain our attention, unless it has been directed upon them by other causes. More commonly the privileged unconscious phenomena, those that are capable of becoming conscious, are those which, directly or indirectly, most deeply affect our sensibility.

It may appear surprising that sensibility should be introduced in connexion with mathematical demonstrations, which, it would seem, can only interest the intellect. But not if we bear in mind the feeling of mathematical beauty, of the harmony of numbers and forms and of geometric elegance. It is a real aesthetic feeling that all true mathematicians recognize, and this is truly sensibility.

Now, what are the mathematical entities to which we attribute this character of beauty and elegance, which are capable of developing in us a kind of æsthetic emotion ? Those whose elements are harmoniously arranged so that the mind can, without effort, take in the whole without neglecting the details. This harmony is at once a satisfaction to our æsthetic requirements, and an assistance to the mind which it supports and guides. At the same time, by setting before our eyes a well-ordered whole, it gives us a presentiment of a mathematical law. Now, as I have said above, the only mathematical facts worthy of retaining our attention and capable of being useful are those which can make us acquainted with a mathematical law. Accordingly we arrive at the following conclusion. The useful combinations are precisely the most beautiful, I mean those that can most charm that special sensibility that all mathematicians know, but of which laymen are so ignorant that they are often tempted to smile at it.

What follows, then ? Of the very large number of combinations which the subliminal ego blindly forms, almost all are without interest and without utility. But, for that very reason, they are without action on the æsthetic sensibility; the consciousness will never know them. A few only are harmonious, and consequently at once useful and beautiful, and they will be capable of affecting the geometrician's special sensibility I have been speaking of; which, once aroused, will direct our attention upon them, and will thus give them the opportunity of becoming conscious.

This is only a hypothesis, and yet there is an observation which tends to confirm it. When a sudden illumination invades the mathematician's mind, it most frequently happens that it does not mislead him. But it also happens sometimes, as I have said, that it will not stand the test of verification. Well, it is to be observed almost always that this false idea, if it had been correct, would have flattered our natural instinct for mathematical elegance.

Thus it is this special æsthetic sensibility that plays the part of the delicate sieve of which I spoke above, and this makes it sufficiently clear why the man who has it not will never be a real discoverer.

All the difficulties, however, have not disappeared. The conscious ego is strictly limited, but as regards the subliminal ego, we do not know its limitations, and that is why we are not too loth to suppose that in a brief space of time it can form more different combinations than could be comprised in the whole life of a conscient being. These limitations do exist, however. Is it conceivable that it can form all the possible combinations, whose number staggers the imagination ? Nevertheless this would seem to be necessary, for if it produces only a small portion of the combinations, and that by chance, there will be very small likelihood of the *right* one, the one that must be selected, being found among them.

Perhaps we must look for the explanation in that period of preliminary conscious work which always precedes all fruitful unconscious work. If I may be permitted a crude comparison, let us represent the future elements of our combinations as something resembling Epicurus's hooked atoms. When the mind is in complete repose these atoms are immovable; they are, so to speak, attached to the wall. This complete repose may continue indefinitely without the atoms meeting, and, consequently, without the possibility of the formation of any combination.

On the other hand, during a period of apparent repose, but of unconscious work, some of them are detached from the wall and set in motion. They plough through space in all directions, like a swarm of gnats, for instance, or, if we prefer a more learned comparison, like the gaseous molecules in the kinetic theory of gases. Their mutual collisions may then produce new combinations.

What is the part to be played by the preliminary conscious work ? Clearly it is to liberate some of these atoms, to detach them from the wall and set them in motion. We think we have accomplished nothing, when we have stirred up the elements in a thousand different ways to try to arrange them, and have not succeeded in finding a satisfactory arrangement. But after this agitation imparted to them by our will, they do not return to their original repose, but continue to circulate freely.

Now our will did not select them at random, but in pursuit of a perfectly definite aim. Those it has liberated are not, therefore, chance atoms ; they are those from which we may reasonably expect the desired solution. The liberated atoms will then experience collisions, either with each other, or with the atoms that have remained stationary, which they will run against in their course. I apologize once more. My comparison is very crude, but I cannot well see how I could explain my thought in any other way.

However it be, the only combinations that have any chance of being formed are those in which one at least of the elements is one of the atoms deliberately selected by our will. Now it is evidently among these that what I called just now the *right* combination is to be found. Perhaps there is here a means of modifying what was paradoxical in the original hypothesis.

Yet another observation. It never happens that unconscious work supplies *ready-made* the result of a lengthy calculation in which we have only to apply fixed rules. It might be supposed that the subliminal ego, purely automatic as it is, was peculiarly fitted for this kind of work, which is, in a sense, exclusively mechanical. It would seem that, by thinking overnight of the factors of a multiplication sum, we might hope to find the product ready-made for us on waking; or, again, that an algebraical calculation, for instance, or a verification could be made unconsciously. Observation proves that such is by no means the case. All that we can hope from these inspirations, which are the fruits of unconscious work, is to obtain points of departure for such calculations. As for the calculations themselves, they must be made in the second period of conscious work which follows the inspiration, and in which the results of the inspiration are verified and the consequences deduced. The rules of these calculations are strict and complicated ; they demand discipline, attention, will, and consequently consciousness. In the subliminal ego, on the contrary, there reigns what I would call liberty, if one could give this name to the mere absence of discipline and to disorder born of chance. Only, this very disorder permits of unexpected couplings.

I will make one last remark. When I related above some personal observations, I spoke of a night of excitement, on which I worked as though in spite of myself. The cases of this are frequent, and it is not necessary that the abnormal cerebral activity should be caused by a physical stimulant, as in the case quoted. Well, it appears that, in these cases, we are ourselves assisting at our own unconscious work, which becomes partly perceptible to the overexcited consciousness, but does not on that account change its nature. We then become vaguely aware of what distinguishes the two mechanisms, or, if you will, of the methods of working of the two egos. The psychological observations I have thus succeeded in making appear to me, in their general characteristics, to confirm the views I have been enunciating.

Truly there is great need of this, for in spite of everything they are and remain largely hypothetical. The interest of the question is so great that I do not regret having submitted them to the reader.