# I. GENERAL VIEWS AND INQUIRIES

THE SUBJECT we are dealing with is far from unexplored and though, of course, it still holds many mysteries for us, we seem to possess fairly copious data, more copious and more coherent than might have been expected, considering the difficulty of the problem.

That difficulty is not only an intrinsic one, but one which, in an increasing number of instances, hampers the progress of our knowledge: I mean the fact that the subject involves two disciplines, psychology and mathematics, and would require, in order to be treated adequately, that one be both a psychologist and a mathematician. Owing to the lack of this composite equipment, the subject has been investigated by mathematicians on one side, by psychologists on the other and even, as we shall see, by a neurologist.

As always in psychology, two kinds of methods are available: the "subjective" and the "objective" methods.[1] Sub-

[1] I speak of objective or introspective methods. I see that the modern behaviorist distinguishes between objective *psychology* and introspective *psychology* (the latter being said to belong to the past since the death of William James and Titchener), as though these were two different sciences, differing as to their object, while it seems to me that both kinds of methods of observation could be applied and even help each other for the study of the same psychological processes. I understand, however, that for the behaviorist, the object of introspection, i.e., thought and consciousness, is to be ignored.

Already, in older times, the prominent biologist Le Dantec eliminated consciousness by qualifying it as an "epiphenomenon." I have always considered that an unscientific attitude, because if consciousness were an epiphenomenon, it would be the only epiphenomenon in nature, where everything reacts on everything else. But, epiphenomenon or not, it exists and can be observed. We are not unjustified in presenting such observajective (or "introspective") methods are those which could be called "observing from the inside," that is, those where information about the ways of thought is directly obtained by the thinker himself who, looking inwards, reports on his own mental process. The obvious disadvantage of such a procedure is that the observer may disturb the very phenomenon which he is investigating. Indeed, as both operations—to think and to observe one's thought—are to take place at the same time, it may be supposed a priori that they are likely to hamper each other. We shall see, however, that this is less to be feared in the inventive process (at least, in some of its stages) than in other mental phenomena. In the present study, I shall use the results of introspection, the only ones I feel qualified to speak of. In our case, these results are clear enough to deserve, it would seem, a certain degree of confidence. In doing so, I face an objection for which I apologize in advance: that is, the writer is obliged to speak too much about himself.

Objective methods—observing from the outside—are those in which the experimenter is other than the thinker. Observation and thought do not interfere with each other;

tions, made by ourselves or by others, as I shall do in the course of this study.

It must be noticed that most instances considered by behaviorists (I found them in J. B. Watson's *Behaviorism*) are very different from those which may concern us, being generally taken from thoughts having a direct relation with our bodily sensations and which are more easily interpreted in terms of the doctrine than others. In such cases, correspondences between bodily phenomena and states of consciousness are easily seen and are more or less known things. They are more hidden for cases of abstract thought, such as those we are going to study; but there is no reason why they should not be discovered at some future date. This may happen, for instance, with the help of the electric waves which accompany cerebral processes (a suggestion which I take from an article of Henri Laugier in the *Revue Moderne*, reproduced in his book *Service de France au Canada*).

but on the other hand, only indirect information is thus obtained, the significance of which is not easily seen. One chief reason why they chance to be difficult to employ in our case is because they require the comparison of numerous instances. In agreement with the general principle of experimental science, this would be an essential condition for arriving at the "fact of great yield," as Poincaré says, that is, the fact which penetrates deeply into the nature of the question; but, precisely, these instances cannot be found for such an exceptional phenomenon as invention.

*The Mathematics "Bump."* Objective methods have generally been applied to invention of any kind, no special investigation being devoted to mathematics. One exception, which we shall very briefly mention, is a curious attempt which has been initiated by the celebrated Gall. It depends on his principle of the so-called "phrenology," that is, on the connection of every mental aptitude with a greater development not only of some part of the brain, but also of the corresponding part of the skull; a rather unhappy idea, as recent neurologists think, of that man who had other very fruitful ones (he was a forerunner of the notion of cerebral localization). According to that principle, mathematical ability ought to be characterized by a special "bump" on the head, the localization of which he actually indicates.

Gall's ideas were taken up (1900)[2] by the neurologist Möbius, who happened to be the grandson of a mathematician, though he himself had no special knowledge of mathematics.

Möbius' book is a rather extensive and thorough study of mathematical ability from the naturalist's standpoint.

2 *Die Anlage zur Mathematik* (Leipzig).

It contains a series of data which, eventually, are likely to be of interest for that study. They bear, for instance, on heredity (families of mathematicians),[8] longevity, abilities of other kinds, etc. Though such an important collection of data may prove useful at a later date, it seems so far not to have given rise to any general rule except as concerns the artistic inclinations of mathematicians. (Möbius confirms the somewhat classic opinion that most mathematicians are fond of music, and asserts that they are also interested in other arts.)

Now, Möbius agrees with Gall's conclusions in general, considering, however, in the first place, that the mathematical sign, though always present, may assume a greater variety of forms than would be understood from Gall's description.

However, that "bump" hypothesis of Gall-Möbius has not met with agreement. Anatomists and neurologists strongly assailed the "Gall redivivus," as they called him, because Gall's phrenological principle, i.e., conformity of skull to brain form, is now considered inaccurate.

Let us not insist any longer on this phase of the ques-

[8] There had been, some years earlier (1869), an important work of Francis Galton on *Hereditary Genius* (London and New York). An extensive chapter is devoted to men of science.

In connection with the methods to which Möbius' book was generally devoted, interesting data are contained in Leonard George Guthrie's *Contributions to the Study of Precocity in Children*, concerning early inclinations of prominent men. To speak only of mathematicians, Galilei's first calling was toward painting, after which, when seventeen, he began to study medicine, and only later mathematics. William Herschell's first education was as a musician. Besides, it is known that Gauss hesitated between mathematics and philology.

Similar instances exist as concerns contemporary men. I heard from Paul Painlevé himself that he hesitated greatly between devoting himself to mathematics or to political life. He at first adopted the former activity, but, as is well known, finally engaged in both of them.

tion, which is to be left to specialists. But it is not useless to speak of it from the mathematical standpoint. From that point of view also, some objections can be raised, at least at a first glance, against the very principle of such research. It is more than doubtful that there exists one definite "mathematical aptitude." Mathematical creation and mathematical intelligence are not without connection with creation in general and with general intelligence. It rarely happens, in high schools, that the pupil who is first in mathematics is the last in other branches of learning; and, to consider a higher level, a great proportion of prominent mathematicians have been creators in other fields. One of the greatest, Gauss, carried out important and classical experiments on magnetism; and Newton's fundamental discoveries in optics are well known. Was the shape of the head of Descartes or Leibniz influenced by their mathematical abilities or by their philosophical ones?

Also there is a counterpart. We shall see that there is not just one single category of mathematical minds, but several kinds, the differences being important enough to make it doubtful that all such minds correspond to one and the same characteristic of the brain.

All this would not be contradictory to the principle of Gall interpreted in a general way, i.e., to interdependence of the mathematical functioning of the mind with the physiology and anatomy of the brain; but the first application of it which Gall and Möbius proposed does not seem to be justified.

Generally speaking, we must admit that mental faculties which seem at first to be simple are composite in an unexpected way. It has been recognized by objective methods (observation of the effects of wounds or other injuries of the head) that such is the case with the best known faculty of all, the language faculty, which consists of several different ones. There are cerebral localizations, as Gall had announced, but without such simple and precise correspondences as he supposed.

There is every reason to think that the mathematical faculty must be at least as composite as has been found for the faculty of language. Though, of course, decisive documents are not and will probably never be available in the former case as they are in the latter, observations on the one phenomenon may help us to understand the other.

*Psychologists' Views on the Subject.* Many psychologists have also meditated not especially on mathematical invention, but on invention in general. Among them, I shall mention only two names, Souriau and Paulhan. These two psychologists show a contrast in their opinions. Souriau (1881) was, it seems, the first to have maintained that invention occurs by pure chance, while Paulhan (1901)[4] remains faithful to the more classic theory of logic and systematic reasoning. There is also a difference in method, which can hardly be accounted for by the small difference in the dates, for while Paulhan has taken much information from scientists and other inventors, there is hardly any to be found in Souriau's work. It is curious that, operating in such a way, he is led to some very shrewd and accurate remarks; but, on the other hand, he has not avoided one or two serious errors which we shall have to mention.

Later on, a most important study in that line was

[4] Souriau, *Théorie de l'Invention* (Paris, 1881). Paulhan, *Psychologie de l'Invention.*

conducted (1937) at the Centre de Synthèse in Paris, as mentioned in the introduction.

*Mathematical Inquiries.* Let us come to mathematicians. One of them, Maillet, started a first inquiry as to their methods of work. One famous question, in particular, was already raised by him: that of the "mathematical dream," it having been suggested often that the solution of problems that have defied investigation may appear in dreams.

Though not asserting the absolute non-existence of "mathematical dreams," Maillet's inquiry shows that they cannot be considered as having a serious significance. Only one remarkable observation is reported by the prominent American mathematician, Leonard Eugene Dickson, who can positively assert its accuracy. His mother and her sister, who, at school, were rivals in geometry, had spent a long and futile evening over a certain problem. During the night, his mother dreamed of it and began developing the solution in a loud and clear voice; her sister, hearing that, arose and took notes. On the following morning in class, she happened to have the right solution which Dickson's mother failed to know.

This observation, an important one on account of the personality of the relator and the certitude with which it is reported, is a most extraordinary one. Except for that very curious case, most of the 69 correspondents who answered Maillet on that question never experienced any mathematical dream (I never did) or, in that line, dreamed of wholly absurd things, or were unable to state precisely the question they happened to dream of. Five dreamed of quite naive arguments. There is one more positive answer; but it is difficult to take account of it, as its author remains anonymous.

Besides, in that matter, there is a confusion which raises grave doubts. One phenomenon is certain and I can vouch for its absolute certainty: the sudden and immediate appearance of a solution at the very moment of sudden awakening. On being very abruptly awakened by an external noise, a solution[5] long searched for appeared to me at once without the slightest instant of reflection on my part—the fact was remarkable enough to have struck me unforgettably—and in a quite different direction from any of those which I had previously tried to follow. Of course, such a phenomenon, which is fully certain in my own case, could be easily confused with a "mathematical dream," from which it differs.

I shall not dwell any longer on Maillet's inquiry because a more important one was started, a few years later, by some mathematicians with the help of Claparède and another prominent Genevese psychologist, Flournoy, and published in the periodical *L'Enseignement Mathématique*. An extensive questionnaire was sent out, consisting of a few more than 30 questions (See Appendix I). These questions (including "mathematical dream") belonged to both classes of investigation methods which we have already differentiated, some of them being "objective" (as much as a questionnaire can be). For instance, mathematicians were asked whether they were influenced by noises and to what extent, or by meteorological circumstances, whether literary or artistic courses of thought were considered useful or harmful.

Other questions were of a more introspective character

[5] For technicians, the beginning of No. 27 (pp. 199-200) in *Journal de Mathématiques pures et appliquées*, Series 4, Vol. IX, 1898 (valuation of a determinant).

and penetrated more directly and deeply into the nature of the subject. Authors were asked whether they were deeply interested in reading the works of their predecessors or, on the contrary, preferred to study problems directly by themselves; whether they were in the habit of abandoning a problem for a while to resume it again only later on (which I, personally, do in many cases and which I always recommend to beginners who consult me). Above all, they were asked what they could say on the genesis of their chief discoveries.

*Some Criticisms.* Reading that questionnaire, one may notice the lack of some questions, even when analogous to some which have actually been asked. For instance, when asking mathematicians whether they indulged in music or poetry, the questionnaire did not mention possible interest in sciences other than mathematics. Especially, biology, as Hermite used to observe, may be a most useful study even for mathematicians, as hidden and eventually fruitful analogies may appear between processes in both kinds of study.

Similarly, when inquiring about the influence of meteorological circumstances or the existence of periods of exaltation or depression, no more precise question was asked concerning the influence of the psychical state of the worker and especially the emotions which he may be experiencing. This question is all the more interesting because it has been taken up by Paul Valéry in a lecture at the French Society of Philosophy, in which he suggested that emotions are evidently likely to influence poetical production. Now, however likely it may seem at first glance that some kind of emotions may favor poetry because they more or less directly find their expression in poetry, it is not certain that the cause is the right one or at least the only one. Indeed, I know by personal experience that powerful emotions may favor entirely different kinds of mental creation (e.g., the mathematical one[1]); and in this connection, I should agree with this curious statement of Daunou: "In Sciences, even the most rigid ones, no truth is born of the genius of an Archimedes or a Newton without a poetical emotion and some quivering of intelligent nature."

Moreover, the most essential question—I mean the one which concerns the genesis of discovery—suggests another one, which is not mentioned in the questionnaire though its interest is obvious. Mathematicians are asked how they have succeeded. Now, there are not only successes but also failures, and the reasons for failures would be at least as important to know.

This is in relation to the most important criticism which can be formulated against such inquiries as Maillet's or Claparède and Flournoy's: indeed, such inquiries are subject to a cause of error which they can hardly avoid. Who can be considered a mathematician, especially a mathematician whose creative processes are worthy of interest? Most of the answers which reached the inquirers come from alleged mathematicians whose names are now completely unknown. This explains why they could not be asked for the reasons of their failures, which only first-rate men would dare to speak of. In the above mentioned inquiries, I could hardly find one or two significant names, such as the physico-mathematician Boltzmann. Such

[1] The above mentioned finding of a solution on a sudden awakening occurred during such a period of emotion.

masters as Appell, Darboux, Picard, Painlevé sent no answers, which was perhaps a mistake on their part.

Since most answers to the inquiries of Maillet and of the *Enseignement Mathématique* were of slight interest for that reason, it occurred to me to submit some of the questions to a man whose mathematical creation is one of the most audacious and penetrating, Jules Drach. Some of his answers were especially suggestive, in the first place, as concerns biology in which, like Hermite, he takes much interest and, chiefly, on the study of previous discoverers. This is a question where it appears that even among men who are born mathematicians, important mental differences may exist. The historians of the amazing life of Evariste Galois have revealed to us that, according to the testimony of one of his schoolfellows, even from his high school time, he hated reading treatises on algebra, because he failed to find in them the characteristic traits of inventors. Now, Mr. Drach, whose work, besides, is closely related to Galois', has the same way of approach. He always wishes to refer to the very form in which discoveries have appeared to their authors. On the contrary, most mathematicians who have answered Claparède and Flournoy's inquiry prefer, when studying any previous work, to think it out and rediscover it by themselves. This is my approach, so that finally I know, in any case, of only one inventor, who is myself.

*Poincaré's Statements.* Again, we shall put aside the inquiry of the *Enseignement Mathématique.* While it failed, as we have said, to distinguish adequately between those who replied, it did, on the other hand, provoke, somewhat later, a testimony which was the most authoritative one could wish to obtain. Conditions of invention have been investigated by the greatest genius which our science has known during the last half century, by the man whose impulse is felt throughout contemporary mathematical science. I allude to the celebrated lecture of Henri Poincaré at the Société de Psychologie in Paris.[6] Poincaré's observations throw a resplendent light on relations between the conscious and the unconscious, between the logical and the fortuitous which lie at the base of the problem. Notwithstanding possible objections which will be discussed in due time, the conclusions which he reaches in that lecture seem to me fully justified and, at least in the first five sections, I shall follow him[7] throughout.

Poincaré's example is taken from one of his greatest discoveries, the first which has consecrated his glory, the theory of fuchsian groups and fuchsian functions. In the first place, I must take Poincaré's own precaution and state that we shall have to use technical terms without the reader's needing to understand them. "I shall say, for example," he says, "that I have found the demonstration of such a theorem under such circumstances. This theorem will have a barbarous name, unfamiliar to many, but that is unimportant; what is of interest for the psychologist is not the theorem, but the circumstances."[8]

So, we are going to speak of fuchsian functions. At first, Poincaré attacked the subject vainly for a fortnight, at-

6 "Mathematical Creation," in *The Foundations of Science*. Translated by G. Bruce Halsted (New York: The Science Press, 1913), p. 387.

7 Quotations without an author's name which will be found in the following pages are taken from Poincaré's lecture.

8 Poincaré deals with the case of mathematics. As Dr. de Saussure, to whom I am indebted for various interesting remarks, suggested to me, independence between the process of invention and the invented thing may be less in more concrete subjects (see below, Section IX, p. 131).

tempting to prove that there could not be any such functions: an idea which was going to prove to be a false one.

Indeed, during a night of sleeplessness and under conditions to which we shall come back, he builds up one first class of those functions. Then he wishes to find an expression for them.

"I wanted to represent these functions by the quotient of two series; this idea was perfectly conscious and deliberate; the analogy with elliptic functions guided me. I asked myself what properties these series must have if they existed, and succeeded without difficulty in forming the series I have called thetafuchsian.

"Just at this time, I left Caen, where I was living, to go on a geologic excursion under the auspices of the School of Mines. The incidents of the travel made me forget my mathematical work. Having reached Coutances, we entered an omnibus to go some place or other. At the moment when I put my foot on the step, the idea came to me, without anything in my former thoughts seeming to have paved the way for it, that the transformations I had used to define the Fuchsian functions were identical with those of non-Euclidian geometry. I did not verify the idea; I should not have had time, as, upon taking my seat in the omnibus, I went on with a conversation already commenced, but I felt a perfect certainty. On my return to Caen, for conscience' sake, I verified the result at my leisure.

"Then I turned my attention to the study of some arithmetical questions apparently without much success and without a suspicion of any connection with my preceding researches. Disgusted with my failure, I went to spend a few days at the seaside and thought of something else. One morning, walking on the bluff, the idea came to me, with just the same characteristics of brevity, suddenness and immediate certainty, that the arithmetic transformations of indefinite ternary quadratic forms were identical with those of non-Euclidian geometry."

These two results showed to Poincaré that there existed other fuchsian groups and, consequently, other fuchsian functions than those which he had found during his sleeplessness. The latter constituted only a special case: the question was to investigate the most general ones. In this he was stopped by most serious difficulties, which a persistent conscious effort allowed him to define more adequately, but not, at first, to overcome. Then, again, the solution appeared to him as unexpectedly, as unpreparedly as in the other two instances, while he was serving his time in the army.

And he adds: "Most striking at first is this appearance of sudden illumination, a manifest sign of long, unconscious prior work. The role of this unconscious work in mathematical invention appears to me incontestable."

*Looking at One's Own Unconsciousness.* Before examining the latter conclusion, let us resume the history of that sleepless night which initiated all that memorable work, and which we set aside in the beginning because it offered very special characteristics.

"One evening," Poincaré says, "contrary to my custom, I drank black coffee and could not sleep. Ideas rose in crowds; I felt them collide until pairs interlocked, so to speak, making a stable combination."

That strange phenomenon is perhaps the more interesting for the psychologist because it is more exceptional. Poincaré lets us know that it is rather frequent as concerns himself: "It seems, in such cases, that one is present at his own unconscious work, made partially perceptible to the over-excited consciousness, yet without having changed its nature. Then we vaguely comprehend what distinguishes the two mechanisms or, if you wish, the working methods of the two egos."

But that extraordinary fact of watching passively, as if from the outside, the evolution of subconscious ideas seems to be quite special to him. I have never experienced that marvelous sensation, nor have I ever heard of its happening to others.

*Instances in Other Fields.* What he reports in the remainder of his lecture is, on the contrary, absolutely general and common to every student of research. Thus Gauss, referring to an arithmetical theorem which he had unsuccessfully tried to prove for years, writes: "Finally, two days ago, I succeeded, not on account of my painful efforts, but by the grace of God. Like a sudden flash of lightning, the riddle happened to be solved. I myself cannot say what was the conducting thread which connected what I previously knew with what made my success possible."

It is unnecessary to observe that what happened to me on my awakening is perfectly similar and typical, as the solution which appeared to me: (1) was without any relation to my attempts of former days, so that it could not have been elaborated by my previous conscious work; (2) appeared without any time for thought, however brief.

The same character of suddenness and spontaneousness had been pointed out, some years earlier, by another great scholar of contemporary science. Helmholtz reported it in an important speech delivered in 1896. Since Helmholtz and Poincaré, it has been recognized by psychologists as being very general in every kind of invention. Graham Wallas, in his *Art of Thought*, suggested calling it "illumination," this illumination being generally preceded by an "incubation" stage wherein the study seems to be completely interrupted and the subject dropped. Such an illumination is even mentioned in several replies on the inquiry of *L'Enseignement Mathématique*. Other physicists like Langevin, chemists like Ostwald, tell us of having experienced it. In quite different fields, let us cite a couple of instances. One has attracted the attention of the psychologist Paulhan. It is a celebrated letter of Mozart:

"When I feel well and in a good humor, or when I am taking a drive or walking after a good meal, or in the night when I cannot sleep, thoughts crowd into my mind as easily as you could wish. Whence and how do they come? I do not know and I have nothing to do with it. Those which please me, I keep in my head and hum them; at least others have told me that I do so. Once I have my theme, another melody comes, linking itself to the first one, in accordance with the needs of the composition as a whole: the counterpoint, the part of each instrument, and all these melodic fragments at last produce the entire work. Then my soul is on fire with inspiration, if however nothing occurs to distract my attention. The work grows; I keep expanding it, conceiving it more and more clearly until I have the entire composition finished in my head though it may be long. Then my mind seizes it as a glance of my eye a beautiful picture or a handsome youth. It does not come to me successively, with its various parts worked out in detail, as they will be later on, but it is in its entirety that my imagination lets me hear it.

"Now, how does it happen, that, while I am at work, my compositions assume the form or the style which characterize Mozart and are not like anybody else's? Just as it happens that my nose is big and hooked, Mozart's nose and not another man's. I do not aim at originality and I should be much at a loss to describe my style. It is quite natural that people who really have something particular about them should be different from each other on the outside as well as on the inside."

Poetical inspiration is reported to have been as spontaneous with Lamartine who happened to compose verses instantly, without one moment of reflection; and we have the following most suggestive statement made at the French Philosophical Society by our great poet Paul Valéry: "In this process, there are two stages.

"There is that one where the man whose business is writing experiences a kind of flash—for this intellectual life, anything but passive, is really made of fragments; it is in a way composed of elements very brief, yet felt to be very rich in possibilities, which do not illuminate the whole mind, which indicate to the mind, rather, that there are forms completely new which it is sure to be able to possess after a certain amount of work. Sometimes I have observed this moment when a sensation arrives at the mind; it is as a gleam of light, not so much illuminating as dazzling. This arrival calls attention, points, rather than illuminates, and in fine, is itself an enigma which carries with it the assurance that it can be postponed. You say, 'I see, and then tomorrow I shall see more.' There is an activity, a special sensitization; soon you will go into the dark-room and the picture will be seen to emerge.

"I do not affirm that this is well described, for it is extremely hard to describe. . . ."

Similarly, as Catherine Patrick has noticed in the article cited below, footnote 10 to Section III, the English poet A. E. Housman in a lecture delivered at Cambridge, England (see his valuable booklet *The Name and Nature of Poetry*) also describes that spontaneous and almost involuntary creation, eventually alternating with conscious effort.

Likewise observations occur even in ordinary life. Does it not frequently happen that the name of a person or of a place which you have vainly tried to remember, recurs to you when you are no longer thinking of it?

That this fact is more analogous to the process of invention than would be believed at first is shown by a remark of Remy de Gourmont: he notices that the right word to express an idea is also very often, after long and fruitless search, found in the same way, viz., when one is thinking of something else. This case is interesting as it presents an intermediate character, being obviously analogous to the preceding one and nevertheless already belonging to the field of invention.

No less similar to the above observation is the well-known proverb: "Sleep on it." This again may be considered as belonging to the realm of invention if we follow modern philosophers and take the word in a broad sense, as we said in the Introduction.

*The Chance Hypothesis.* The biologist Nicolle[9] also mentions creative inspirations and even strongly insists on them. But it is necessary to discuss the way in which he interprets them.

For Poincaré, as we saw, they are evident manifestations

[9] *Biologie de l'Invention*, pp. 5-7.

of a previous unconscious work, and here I must say that I do not see how this view can be seriously disputed.

However, Nicolle does not seem to agree with it; or, more exactly, he does not speak of the unconscious. "The inventor," he writes, "does not know prudence nor its junior sister, slowness. He does not sound the ground nor quibble. He at once jumps into the unexplored domain and by this sole act, he conquers it. By a streak of lightning, the hitherto obscure problem, which no ordinary feeble lamp would have revealed, is at once flooded with light. It is like a creation. Contrary to progressive acquirements, such an act owes nothing to logic or to reason. The act of discovery is an accident."

This is, in its most extreme form, the theory of chance which psychologists like Souriau also set forth.

Not only can I not accept it, but I can hardly understand how a scientist like Nicolle could have conceived of such an idea. Whatever respect we must have for the great personality of Charles Nicolle, explanation by *pure* chance is equivalent to no explanation at all and to asserting that there are effects without causes. Would Nicolle have been contented to say that diphtheria—or typhus, which he so admirably investigated—are the result of pure chance? Even if we do not, at this juncture, enter into the analysis which we shall try to give in the next section, chance is chance: that is, it is the same for Nicolle or Poincaré or for the man in the street. Chance cannot explain that the discovery of the cause of typhus was made by Nicolle (that is, by a man having pondered scientific subjects and the conditions of experiments for years and also having shown his marvellous ability) rather than by any of his nurses. And as to Poincaré, if chance could explain one of the genial intuitions which he describes in his lecture—which I cannot even believe—how would that explanation account for all those which he successively mentions, not to speak of all those which have occurred throughout the various theories which constitute his immense work and have transformed almost every branch of mathematical science? You could as well imagine, according to well-known comparison, a monkey striking a typewriter and fortuitously printing the American Constitution.

This does not mean that chance has no role in the inventing process.[10] Chance does act. We shall see in Section III how it acts inside unconsciousness.

10 Dealing with the mathematical domain, we speak only of *psychological* chance, i.e., fortuitous mental processes. As Claparède points out (Meetings of 1937 at the Centre de Synthèse), it must be distinguished from *external* hazards, such as occur in the well-known case of Galvani's frogs and which, of course, are likely to play the initial role in experimental discovery.