IV. THE PREPARATION STAGE. LOGIC AND CHANCE

Throughout Conscious Work. Reviewing Poincaré's lecture, the literary critic Emile Faguet wrote: "A problem . . . reveals itself suddenly when it is no longer investigated, probably because it is no longer investigated and when one only expects, for a short time, to rest and relax: a fact which would prove—lazy people, it is to be feared, might make ill use of it—that rest is the condition of work."

It would, of course, be very easy if, being told of the terms of a problem, we could simply think that it would be very nice to find it, then go to bed and find the solution ready on awakening the next morning. Indeed, we could think it to be perhaps too easy from the moral point of view.

As a matter of fact, things by no means behave that way. In the first place, it often happens that, in some of its parts, the work is perfectly conscious.[1] This has been the case for some parts of Poincaré's work itself, as has been shown in the beginning: for instance, in the step just after the initial one.

Very typical, from that point of view, is Newton's discovery of universal attraction. He is reported to have been asked how he had obtained it and to have answered, "By constantly thinking it over"; but we do not need that anecdote, which may not be authentic, to understand that his

[1] However, the word "conscious" ought not, perhaps, to be understood in too strict a sense. A more attentive analysis will show us (see Section VI) a cooperation between perfect consciousness and that superficial subconscious or "fringe-consciousness" we spoke of in Section II.

discovery was a work of high and inflexible logic, the main and essential idea, i.e., that the moon must really fall toward the earth, being a necessary and unavoidable consequence of the fact that any material body (be it an apple or not) does so. A tenacious continuity of attention, "a consented, a voluntary faithfulness to an idea"[2] was necessary for that.

Must we, then, agree with Buffon's thesis that genius may often be nothing else than a long patience? This idea is obviously contrary to all that we have noticed so far. I confess that I cannot share the admiration for it, nor even approve of it. In Newton's case, one can certainly see, from the beginning, a continuous course of thought constantly directed toward its goal. But this process was started by the initial recognition that the subject was worthy of this continuity of attention, of the consented and voluntary faithfulness we have just spoken of. This is again an inspiration, a choice; only, this takes place in the conscious will.

Conscious Work as Preparatory. Let us now consider the opposite case, the unexpected inspirations which repeatedly illuminated Poincaré's mind. We have acquired the notion that they are the consequence of a more or less intense and lengthy unconscious work. But is that unconscious work itself an effect without a cause? We should be utterly mistaken in thinking so; we have only to come back to Poincaré's report to be led to the contrary conclusion. His first inspiration on getting into the car at Coutances follows a preliminary period of deliberate labor; and after that, we see him studying arithmetical questions "apparently without much success" and finally, "disgusted with his failure";

[2] Delacroix, *L'Invention et le Génie.*

upon which new fruitful steps reveal themselves to him. Then he makes a systematic attack upon the chief remaining question, "carrying all the outworks, one after the other. There was one, however, that still held out, whose fall would involve that of the whole place. But all my efforts only served at first the better to show me the difficulty, which indeed was something." And he again notices that all this work was perfectly conscious.

Only then, and after having been compelled even to set it aside for a while, the solution of the difficulty suddenly appeared.

In all these successive steps, as we see, "sudden inspirations (and the examples already cited sufficiently prove this), never happen except after some days of voluntary effort which has appeared absolutely fruitless and whence nothing good seems to have come, where the way taken seems totally astray. These efforts then have not been as sterile as one thinks. They have set going the unconscious machine and without them it would not have moved and would have produced nothing."

Helmholtz had similarly observed that what we have called incubation and illumination must be preceded by this stage of *preparation*. Its existence has been, after Helmholtz and Poincaré, recognized by psychologists as a general fact, and probably it exists even when it is not apparent, as in the case of Mozart (who does not mention incubation either).

It is not useless to notice that independently of the reasons we have already given, this, by itself, is sufficient to settle the question whether discovery is a matter of pure chance. Discovery cannot be produced only by chance, although chance is to some extent involved therein, any more

than does the inevitable role of chance in artillery dispense with the necessity for the gunner to take aim, and to aim very precisely. Discovery necessarily depends on preliminary and more or less intense action of the conscious.

Not only does this answer the question of the chance-hypothesis, but at the same time it prevents us from admitting the other hypotheses which we have examined in the preceding section. It ought, indeed, to be noticed that the rest and forgetting hypotheses have one feature in common; whether it be in one or in the other of them, the preparatory work, if not bringing directly the solution by itself, is assumed to be completely useless and even harmful. Then, discovery would happen just as if there had been no preparation work at all; that is, we should be again compelled to go back to the inadmissible hypothesis of pure chance.

Poincaré's View on the Mode of Action of Preparatory Work. Having recognized this, we cannot any longer think of the conscious as being subordinated to the unconscious. On the contrary, it starts its action and defines, to a greater or lesser extent, the general direction in which that unconscious has to work.

To illustrate that directing action, Poincaré uses a striking and remarkably fruitful comparison. He imagines that the ideas which are the future elements of our combinations are "something like the hooked atoms of Epicurus. During the complete repose of the mind, these atoms are motionless; they are, so to speak, hooked to the wall; so this complete rest may be indefinitely prolonged without the atoms meeting, and consequently without any combination between them." The act of studying a question consists of mobilizing ideas, not just any ones, but those from which we might

reasonably expect the desired solution. It may happen that that work has no immediate result. "We think we have done no good, because we have moved these elements a thousand different ways in seeking to assemble them and have found no satisfactory aggregate." But, as a matter of fact, it seems as though these atoms are thus launched, so to speak, like so many projectiles and flash in various directions through space. "After this shaking-up imposed upon them by our will, these atoms do not return to their primitive rest. They freely continue their dance."

Consequences can now be foreseen. "The mobilized atoms undergo impacts which make them enter into combinations among themselves or with other atoms at rest, which they struck against in their course." In those new combinations, in those indirect results of the original conscious work, lie the possibilities of apparently spontaneous inspiration.

Logic and Chance. Though Poincaré presents that comparison as a very rough one and it could hardly avoid being such, it proves, as a matter of fact, to be highly instructive.

We shall now see that, by pursuing it, other points can be elucidated. Let us consider, from that point of view, the question of logic and chance in discovery, on which authors are most divided. Several of them, though not as extreme as we have seen to be the case with Nicolle, insist on the importance of chance, while others emphasize the preeminence of logic. Among the two psychologists whom we mentioned in the beginning, Paulhan belongs to the latter school, while Souriau represents the former. It seems to me, in accordance with my personal introspection, that we can get a good understanding of that question by using Poincaré's comparison of projected atoms: a comparison which I shall complete by assimilating that projection with that

which is produced by a hunting cartridge. It is well known that good hunting cartridges are those which have a proper scattering. If this scattering is too wide, it is useless to aim; but if it is too narrow, you have too many chances to miss your game by a line. I see quite similar circumstances in our subject. Again comparing ideas to Poincaré's atoms, it may happen that the mind projects them, exactly or almost exactly, in certain determinate directions. Doing so has this advantage that the proportion of useful meetings between them happens to be relatively great compared to the sterile ones; but we may fear lest these meetings be insufficiently different from each other. On the contrary, it may happen that the atoms are launched in a rather disorderly manner. If so, most of the meetings will be uninteresting ones; but on the other hand, as in a kind of lottery, that disorder can be highly valuable, because the few meetings which are useful, being of an exceptional nature and between seemingly very remote ideas, will probably be the most important ones.

This is what Souriau expresses by the quite striking phrase: "In order to invent, one must think aside";[3] and, even in mathematics—though, in that realm, its meaning is rather different from what it is in experimental sciences—we can remember Claude Bernard's statement, "Those who have an excessive faith in their ideas are not well fitted to make discoveries."

Errors and Failures. The reason for the difference between the meanings of Claude Bernard's sentence in mathematics and in experimental sciences is that, in the latter case, too stubbornly following an idea once conceived may lead to errors: that is, to inaccurate conclusions.

[3] "Pour inventer, il faut penser à côté."

On the contrary, in our domain, we do not need to insist on errors. Good mathematicians, when they make them, which is not infrequent, soon perceive and correct them. As for me (and mine is the case of many mathematicians), I make many more of them than my students do; only I always correct them so that no trace of them remains in the final result. The reason for that is that whenever an error has been made, insight—that same scientific sensibility we have spoken of—warns me that my calculations do not look as they ought to.

There are, however, celebrated exceptions concerning some delicate points of reasoning; those may sometimes prove more fruitful than accurate results, as has been an insufficient proof of Riemann for "Dirichlet's principle."

But, in both domains, the mathematical and the experimental, the fact of not sufficiently "thinking aside" is a most ordinary cause of failure—i.e., the lack of success in finding a solution which may appear to better inspired thinkers—a circumstance which is at least as interesting as discovery for psychology.

This, especially, often explains the failures which may be called "paradoxical," viz., the failure of a research scholar to perceive an important immediate consequence of his own conclusions.

Of course, we must insist on speaking of *immediate* consequences. When the discoverer of a certain fact hears that another scholar has found a notable consequence of it, if this improvement has required some effort, the former will consider it not a failure but a success: he has the right to claim his part in the new discovery.

Such paradoxical failures are reported by Claparède in the above mentioned session and they are, in my opinion, to

be explained as we have just said. The most striking instance which he gives, concerns the invention of the ophthalmoscope. The physiologist Brücke had investigated the means of illuminating the back part of the eye and succeeded in doing so; but it was Helmholtz who, while preparing a lecture on that result of Brücke, conceived the idea that optical images could be generated by the rays thus reflected by the retina: an almost obvious idea, which as it seems, Brücke could hardly have overlooked. In that case, most evidently—at least to me—Brücke's mind was too narrowly directed toward his problem.

Similarly, as Claparède also reports, de la Rive failed to invent the galvanoplastic method; Freud missed finding the application of cocaine to the surgery of the eye.

Personal Instances. Every scientist can probably record similar failures. In my own case, I have several times happened to overlook results which ought to have struck me blind, as being immediate consequences of other ones which I had obtained. Most of these failures proceed from the cause which we have just mentioned, viz., from attention too narrowly directed.

The first instance I remember in my life had to do with a formula which I obtained at the very beginning of my research work. I decided not to publish it and to wait till I could deduce some significant consequences from it. At that time, all my thoughts, like many other analysts', were concentrated on one question, the proof of the celebrated "Picard's theorem." Now, that formula most obviously gave one of the chief results which I found four years later by a much more complicated way: a thing which I was never aware of until years after, when Jensen published that formula and noted, as an evident consequence, the re-

sults which, happily for my self-esteem, I had obtained in the meanwhile. It is clear that, in 1888, I had thought too exclusively of Picard's theorem.

My next work was my thesis. Two theorems, important to the subject,[4] were such obvious and immediate consequences of the ideas contained therein that, years later, other authors imputed them to me, and I was obliged to confess that, evident as they were, I had not perceived them.

Some years later, I was interested in generalizing to hyperspaces the classic notion of curvature of surfaces. I had to deal with Riemann's notion of curvature in hyperspaces, which is the generalization of the more elementary notion of the curvature of a surface in ordinary space. What interested me was to obtain that Riemann curvature as the curvature of a certain surface S, drawn in the considered hyperspace, the shape of S being chosen in order to reduce the curvature to a minimum. I succeeded in showing that the minimum thus obtained was precisely Riemann's expression; only, thinking of that question, I neglected to take into consideration the circumstances under which the minimum is reached, i.e., the proper way of constructing S in order to reach the minimum. Now, investigating that would have led me to the principle of the so-called "Absolute Differential Calculus," the discovery of which belongs to Ricci and Levi Civita.

Absolute differential calculus is closely connected with the theory of relativity; and in this connection, I must con-

[4] For technicians: "If the coefficients of a Maclaurin series are real positive numbers, the radius of convergence being R, $x = R$ must be a singular point"; "A Maclaurin series with a finite radius of convergence generally admits its whole circle of convergence as an essentially singular line."

fess that, having observed that the equation of propagation of light is invariant under a set of transformations (what is now known as Lorentz's group) by which space and time are combined together, I added that "such transformations are obviously devoid of physical meaning." Now, these transformations, supposedly without any physical meaning, are the base of Einstein's theory.

To continue about my failures, I shall mention one which I particularly regret. It concerns the celebrated Dirichlet problem which I, for years, tried to solve in the same initial direction as Fredholm did, i.e., by reconducting it to a system of an infinite number of equations of the first degree in an infinite number of unknowns. But physical interpretation, which is in general a very sure guide and had been most often such for me, misled me in that case. It suggested an attempt to solve the problem by a "potential of simple layer"—in that question, a blind alley—while the solution was to be looked for in the introduction of a "potential of a double layer." This shows how justified Claude Bernard is in the above-mentioned sentence, and that one ought not to follow too stubbornly a determinate principle, however justifiable and fruitful in general.

In all these examples, as we see, the reason for the failures was basicly the same. But the opposite case occurred when I overlooked the fact that a problem in "inversive geometry" could be indeterminate—a fact which leads to the beautiful properties discovered by André Bloch. It is not, this time, a consequence of having too strictly followed my original direction, which would precisely have led me to discuss more thoroughly the problem which I had solved, and therefore, to notice the possibility of indetermination.

That case is exactly contrary to the preceding ones. I was unsufficiently faithful to my main idea.

I must close the enumeration of these failures with one which I can hardly explain: having found, for constructing conditions of possibility for a problem in partial differential equations,[5] a method which gives the result in a very complicated and intricate form, how did I fail to notice, in my own calculations, a feature which enlightens the whole problem, and leave that discovery to happier and better inspired successors? That is what is difficult for me to conceive.

The Case of Pascal. It is probable that many scholars, if not all of them, can remember similar experiences. It is a comforting thing to think that the same may happen to some of the greatest ones.

In his *Art de Persuader*, Pascal has stated a principle which is fundamental for method not only in mathematics, but in any deductive subject or any matter of reasoning, viz.:

"One must substitute definitions instead of defined."

On the other hand, in another place, he points out the obvious fact that, for the same reason that it is not possible to prove everything, it is also impossible to define everything. There are primitive ideas which it is not possible to define.

If he had only thought of juxtaposing these two statements, he would have found himself before a great problem of logic which not only enables us to understand the true

[5] For technicians: see pp. 257-260 of my Yale *Lectures on Cauchy's Problem*; pp. 351-355 of the French edition. The improved answer is given in Hilbert-Courant's *Methoden der Mathematischen Physik* (pp. 425-430), following works of John and Asgueiersson.

meaning of the celebrated Euclid postulate, but, more generally, has produced a profound revolution which, as we see it, might have taken place three centuries earlier.

However, he did not connect both ideas. Whether the reason for this was that his thoughts were too intensely directed toward theological consequences, as a friend of mine suggested to me, is a question which it would be difficult to elucidate.

Attempts to Govern our Unconscious. Such instances show us that, in research, it may be detrimental to scatter our attention too much, while overstraining it too strongly in one particular direction may also be harmful to discovery.

What should we do in order to avoid these opposite objections?

Of course, there is the obvious influence of the way in which we direct our preparation work which gives the impulse to the unconscious work: and in fact, especially with reference to Poincaré's conception, this can be considered as a way to educate our unconscious. The formula of Souriau, "To invent, one must think aside," is to be understood in that sense.

But this is not yet completely satisfactory: in this way we shall think of expected directions for "aside" thoughts, but not of those unexpected and all the more interesting for this very reason. We must notice, in that direction, that it is important for him who wants to discover not to confine himself to one chapter of science, but to keep in touch with various others.

Could we find other means of influencing our unconscious? That would be of great importance, in fact not only for invention but also for the whole conduct of life and es-

pecially for education. The study of that question, which deserves to be pursued, has been undertaken at least in one periodical, *La Psychologie et la Vie*; a whole fascicle was devoted to it by that review in 1932, with contributions of several authors. Particularly, Dwelshauvers suggests an analysis of the conditions of the phenomenon, such as the time of the day at which it takes place, how much time elapses between voluntary preparation and solution; whether such incubation lasts for hours or for days, whether its duration is in proportion to the difficulty of the question, etc.

Pending the results of such studies, one rule proves evidently useful: that is, after working on a subject and seeing no further advance seems possible, to drop it and try something else, but to do so provisionally, intending to resume it after an interval of some months. This is useful advice for every student who is beginning research work.

There is another direction in which that education of the unconscious could be pursued, though I cannot undertake to speak of it. Indeed, as Dr. de Saussure suggested to me, very powerful means for that purpose may be supplied by the methods of psychoanalysis.