VII. DIFFERENT KINDS OF MATHEMATICAL MINDS

THE PHENOMENA considered in Sections I-V seem to happen similarly in many mathematical research scholars. On the contrary, concrete representations studied in the preceding section were far from the same for everybody. This section will also be devoted to differences among various ways of mathematical thinking. It will have to our former considerations the same relation which the distinction between various zoological genera and species has to general physiology.

The Case of Common Sense. Let us start from the beginning, that is from the case of people simply reasoning with their common sense. In that case, we can say that much is afforded by the unconscious and little is asked from further conscious elaboration.

It often happens, besides, that that unconscious is a very superficial one and its data are not essentially different from regular reasoning. Thus, Spencer, alluding to the classic syllogism "every man is mortal;—now, Peter is a man;—therefore, Peter is mortal," supposes that you hear of a ninety-year-old man who undertakes to build a new house for himself. Spencer has no difficulty in proving that the syllogism is really present in your fringe-consciousness and that, between it and the stream of thought—an instantaneous one, as is general in unconsciousness—which leads you to speak of the man as being unreasonable, there is but a difference of form. Things can happen similarly in the case of many simple mathematical deductions.

In other instances, however, ways followed by common sense may be very different from those which we can formulate by explicit reasoning. It happens especially in questions of a concrete nature—say, geometrical or mechanical ones. Our ideas on such subjects, acquired in early childhood, seem to be relegated to a remote unconsciousness; we cannot know them exactly and it is probable that they often imply empirical reasons, taken not from true reasoning but from the experience of our senses. Let us take one or two examples.

Let us imagine that I throw what is called a "material point"—that is, a very small body, such as a very small marble—which will go on moving on account of its initial velocity and its weight. Common sense tells us that the motion must take place in a vertical plane, the vertical plane P drawn through the initial line of projection. In that case, it is hardly doubtful that the subconscious reasoning uses the "principle of sufficient reason," there being no reason why the movable point should go to the right rather than to the left side of the aforesaid plane P.

The mathematical proof, such as classically given in courses of rational mechanics, proceeds in an utterly different way, with the use of several theorems of the differential and integral calculus. It is to be noticed, however, that the proof which occurs to common sense could be transformed into a perfectly rigorous one, by applying a general theorem (also belonging to the integral calculus) which says that under the aforesaid conditions (the direction and magnitude of the initial velocity being given) the motion is uniquely determined. That theorem, in its turn, can be rigorously proved; but the latter proof takes place only in higher courses of calculus, so that, in regular teaching, the way suggested by common sense to reach our conclusions appears actually less elementary than the other one.

Let us now consider two examples in geometry. If I think of drawing a curve in a plane, by the continuous motion of a point, it is a fact of common sense that at all of its points (some exceptional ones being perhaps excepted) that curve will admit of a tangent (in other words, that, at every instant, the motion must take place in some determined direction). We do not know how our common sense, i.e., our unconscious, reaches such a conclusion: perhaps by empirism, i.e., by the memory of the lines we are accustomed to see or, as F. Klein supposes, by a confusion of geometrical curves, which have no thickness whatever, with the lines which we can actually draw and which always have some thickness. As a matter of fact, the conclusion is false; mathematicians can construct continuous curves which have no tangent at any point.

In the second place, let us consider a plane closed curve which has no "double point," that is, which nowhere intersects itself. It is evident to common sense that such a curve, whatever its shape may be, divides the plane (a) into two different regions; (b) into not more than two.

How common sense elaborates that conclusion, is not positively known, an intervention of empirism being again probable. This time, the conclusion (Jordan's theorem) is correct; but, evident as it is for our common sense, its proof is of great difficulty.

By such examples as these two, it has been realized that, at least in a certain class of questions relating to principles,[1] we cannot surely rely on our ordinary space-intui-

[1] The questions we are alluding to depend on arithmetization rather than on Hilbert's ideas such as mentioned in Section VI.

tion: as geometrical properties can always be reconducted to numerical ones, thanks to the invention of analytical geometry, arguments should always be fully arithmetized, or, at least, it must be ascertained that this arithmetization, if not given at length for brevity's sake, is possible. Pascal's word "Tout ce qui passe la Géométrie nous passe" is replaced, for the modern mathematician, by "Tout ce qui passe l'Arithmétique nous passe."

For instance, a proof of Jordan's theorem, which we have just enunciated, is not satisfactory if not fully arithmetizable.[2]

Second Step: the Student in Mathematics. After that common-sense state of human thought, there has come the scientific state. We have seen that it is characterized by the intervention of the threefold operation of verifying the result; "precising" it; and, above all, making it utilizable, which, as we have seen, requires the enunciation of relay-results. We have seen that that too is essential, first for the certitude of the knowledge thus acquired; then, for its fruitfulness and the possibility of extending it.

These characters can help us to understand what takes place, psychologically speaking, in the passage from the

[2] Same remark on this subject as on Hilbert's *Principles*. I have given a simplified proof of part (a) of Jordan's theorem. Of course, my proof is completely arithmetizable (otherwise it would be considered non-existent); but, investigating it, I never ceased thinking of the diagram (only thinking of a very twisted curve), and so do I still when remembering it. I cannot even say that I explicitly verified or verify every link of the argument as to its being arithmetizable (in other words, the arithmetized argument *does not* generally appear in my full consciousness). However, that each link can be arithmetized is doubtless as well for me as for any mathematician who will read the proof: I can give it instantly in its arithmetized form, which proves that that arithmetized form is present in my fringe-consciousness.

former state to the latter: in other words, what concerns the case of the student of mathematics.

How commonly total misunderstandings and failures occur in that case, is well known. I shall, besides, be very brief on that subject, because it has been profoundly treated by Poincaré (*Les Définitions dans l'Enseignement* in *Science et Méthode*). Even before quoting him, it is not useless to observe that that case of the mathematical student already belongs to our subject of invention. Between the work of the student who tries to solve a problem in geometry or algebra and a work of invention, one can say that there is only a difference or degree, a difference of level, both works being of a similar nature.

Now, how does it happen that so many are incapable of that work, incapable of understanding mathematics? That is what Poincaré examines and of which he in a striking way shows the true reason which lies in the meaning that ought to be given to the word "to understand."

"To understand the demonstration of a theorem, is that to examine successively each syllogism composing it and ascertain its correctness, its conformity to the rules of the game? . . . For some, yes; when they have done this, they will say, I understand.

"For the majority, no. Almost all are much more exacting; they wish to know not merely whether all the syllogisms of a demonstration are correct, but why they link together in this order rather than another. In so far as to them they seem engendered by caprice and not by an intelligence always conscious of the end to be attained, they do not believe they understand.

"Doubtless they are not themselves just conscious of what they crave, and they could not formulate their desire, but if they do not get satisfaction, they vaguely feel that something is lacking."

The connection of this with our former considerations is easy to understand. For the purpose of teaching—be it oral or written—every part of the argument is brought into its entirely conscious form, corresponding to the simultaneous verifying and "precising" stages which we have described above. Even, in view of further consequences, there is a tendency to increase the number of relay-results. In this way of working, which seems to be the best one of getting a rigorous and clear presentation for the beginner, nothing remains, however, of the synthesis, the importance of which we have underlined in the preceding section. But that synthesis gives the leading thread, without which one would be like the blind man who can walk but would never know in what direction to go.

Those to whom such a synthesis appears "understand mathematics." In the contrary case, there are the two attitudes mentioned by Poincaré. The rather general one is the second one: the student feels that something is lacking, but cannot realize what is wrong; if he does not overcome that difficulty, he will get lost.

In the first case mentioned by Poincaré, the student, not finding any synthetic process, will do without it. Although this allows him to pursue his studies, often for long years, his case is, from a certain point of view, worse than the other one in which at least the existence of some difficulty was understood. On account of the mathematical knowledge more and more required for entrance to several careers, one frequently meets with such a student. I have seen a case in which a candidate, guided by his common sense, knew the right answer to my question, but did not think he was allowed to give it and did not realize that the suggestion of his subconscious could be very easily translated into a correct and rigorous proof.

Curious instances of this sort are not uncommon among students in differential and integral calculus. Most often the question is whether such and such a theorem or formula is appropriately invoked, whether the conditions for its application are satisfied or not. Students sometimes industriously investigate that question when common sense indicates the answer to be a practically evident one—and on the other hand neglect to study it in the case where it is a delicate one and does deserve a careful examination. Such remark or analogous ones could be eventually useful in pedagogy.

Logic and Intuitive Minds. A Political Aspect of the Question. After having spoken of students, let us now deal with mathematicians themselves, able not only to understand mathematical theories, but also to investigate new ones. Not only do these differ from ordinary students, but they also profoundly differ from each other. A capital distinction has been emphasized: some mathematicians are "intuitive" and others "logical." Poincaré has dealt with that distinction and so has the German mathematician Klein. Poincaré's lecture on the subject begins as follows:

"The one sort are above all preoccupied by logic; to read their works, one is tempted to believe they have advanced only step by step, after the manner of a Vauban who pushes on his trenches against the place besieged, leaving nothing to chance. The other sort are guided by intuition and at the first stroke, make quick but sometimes precarious conquests, like bold cavalrymen of the advance guard."

With Klein, even politics has been introduced into the question: he asserts[3] that "It would seem as if a strong naive space intuition were an attribute of the Teutonic race, while the critical, purely logical sense is more developed in the Latin and Hebrew races." That such an assertion is not in agreement with facts will appear clearly when we come to examples. It is hardly doubtful that, in stating it, Klein implicitly considers intuition, with its mysterious character, as being superior to the prosaic way of logic (we have already met with such a tendency in Section III) and is evidently happy to claim that superiority for his countrymen. We have heard recently of that special kind of ethnography with Nazism: we see that there was already something of this kind in 1893.

One will find such tendentious interpretations of facts whenever nationalistic passions enter into play. At the beginning of the First World War, one of our greatest scientists and historians of sciences, the physicist Duhem, was misled by them just as Klein had been, only in the opposite sense. In a rather detailed article,[4] he depicts German scientists, especially mathematicians, as lacking intuition or even deliberately setting it aside. It is especially hard to understand how he can characterize in that way Bernhard Riemann who is undoubtedly one of the most typical examples of an intuitive mind. Duhem's assertion of 1915 seems to me as unreasonable as Klein's in 1893. If one or the other were right, the reader will realize by all that precedes that either Frenchmen or Germans would never have made any significant discovery. The only thing for which I should think of reproaching the German mathematical school in that line is a systematic, though hardly

[3] *The Evanston Colloquium*, p. 46.

[4] *Revue des Deux Mondes* (January-February, 1915), p. 657.

defendable and somewhat pedantic claim, chiefly under Klein's influence, that, for certain proofs in analysis and its arithmetical applications, "series" must be used in preference to "integrals." Precisely in those questions, the use of series looks more logical and the use of integrals more intuitive. Perhaps there is again some nationalism in such a tendency, because series are used by the celebrated Weierstrass—a most evident logician—whose reputation and influence have been enormous among German scholars, while, into similar subjects, Cauchy or Hermite introduced integrals[5] (though this was also the case of Riemann).

Poincaré's View of the Distinction. Poincaré, more wisely I think, does not connect the matter with politics. On the contrary, he implicitly shows how doubtful such a connection is: for, in order to illustrate the opposition between the two kinds of mind, he opposes to each other in the first place two Frenchmen and, then, two Germans.

However, after having fully accepted and faithfully followed Poincaré's ideas in Sections I to V, I shall, this time, disagree with him. We have cited the first paragraph of his lecture; let us reproduce the second. It reads as follows:

"The method is not imposed by the matter treated. Though one often says of the first that they are *analysts* and calls the others *geometers*, that does not prevent the one sort from remaining analysts even when they work at geometry, while the others are still geometers even when

[5] According to that doctrine, Klein thinks it necessary to modify the proof of a celebrated theorem of Hermite; and even, on reaching a certain point, says "the proof is not yet perfectly simple: something still remains of the ideas of Hermite," this inducing him to a further modification. As a matter of fact, these "simplifications" are superficial ones, and after them just as before them, everything—absolutely everything essential—rests on Hermite's fundamental idea.

they occupy themselves with pure Analysis. It is the very nature of their mind which makes them logicians or intuitionalists, and they cannot lay it aside when they approach a new subject."

What must we think of the comparison between those two paragraphs? Both times, a distinction is made between intuition and logic, but on bases quite different, though somewhat related to each other.[6]

This appears even more clearly in the examples set forth by Poincaré. To Joseph Bertrand, who visibly had a concrete, spatial view of every question, he opposes Hermite whose eyes "seem to shun contact with the world" and who seeks "within, not without, the vision of truth."

That Hermite was not used to thinking in the concrete is certain. He had a kind of positive hatred for geometry and once curiously reproached me with having made a geometrical memoir. As natural, his own memoirs on concrete subjects are very few and not among his most remarkable ones. So, from the second point of view of Poincaré, Hermite ought to be considered as a logical mathematician.

But to call Hermite a logician! Nothing can appear to me as more directly contrary to the truth. Methods always seemed to be born in his mind in some mysterious way. In his lectures at the Sorbonne, which we attended with unfailing enthusiasm, he liked to begin his argument by: "Let us start from the identity . . ." and here he was writing a formula the accuracy of which was certain, but whose origin in his brain and way of discovery he did not explain and we could not guess. This quality of his mind is also most evidently illustrated by his celebrated discovery in the theory of quadratic forms. In that question, two cases

[6] See the remarks at the beginning of this section (p. 101).

are possible in which, as is obvious, things happen quite differently. In the first one, "reduction" has been known since Gauss. Nobody, as it seemed, would have thought of the idea of merely carrying out, in the second case, the very calculations which suited the first one and which apparently, had nothing to do with that second case; it seemed quite absurd that they would, that time, lead to the solution; and yet, by a kind of witchcraft, they do. The mechanism of that extraordinary phenomenon was, some years later, partly explained by a geometrical interpretation (of course, given not by Hermite, but by Klein); but it did not become entirely clear to me before reading Poincaré's conception of it, in one of his early notes.[7] I can hardly imagine a more perfect type of an intuitive mind than Hermite's, if not taking account of the extreme cases which

[7] Poincaré himself, in spite of the inspiration phenomena which we have mentioned, does not make that same impression on me. Reading one of his great discoveries, I should fancy (evidently a delusion) that, however magnificent, one ought to have found it long before, while such memoirs of Hermite as the one referred to in the text arouse in me the idea: "What magnificent results! How could he dream of such a thing?"

There is obviously something subjective in such a judgment. A deduction which will seem to be a logical one for me—that is, congenial to my mind, one which I should be naturally inclined to think of—may appear as intuitive to some other man. Perhaps, almost every mathematician would be a logician according to his own judgment. For instance, I have been asked by what kind of guessing I thought of the device of the "finite part of infinite integral," which I have used for the integration of partial differential equations. Certainly, considering it in itself, it looks typically like "thinking aside." But, in fact, for a long while my mind refused to conceive that idea until positively compelled to. I was led to it step by step as the mathematical reader will easily verify if he takes the trouble to consult my researches on the subject, especially my *Recherches sur les Solutions Fondamentales et l'Intégration des Équations Linéaires aux Dérivées Partielles*, 2nd Memoir, especially pp. 121 ff. (Annales scientifiques de l'Ecole Normale Supérieure, Vol. $XXII_3$, 1905). I could not avoid it any more than the prisoner in Poe's tale *The Pit and the Pendulum* could avoid the hole at the center of his cell.

will be mentioned in the next section. Hermite's example undoubtedly shows that the two definitions of intuition and logic given by Poincaré do not agree or do not necessarily agree, which Poincaré finally admits to a certain extent on account of that case.

The two German mathematicians whom Poincaré compares are Weierstrass and Riemann. That, as he concludes, Riemann is typically intuitive and Weierstrass typically logical is beyond contestation. But as to the latter, Poincaré says "You may leaf through all his books without finding a figure." It strikes me that there happens to be there an error of fact.[8] It is true that *almost* no memoir of Weierstrass implied any figure: there is only one exception; but there is one, and this exception occurs in one of his most masterly and clear-cut works, one giving the most complete impression of perfection: I mean his fundamental method in the calculus of variations. Weierstrass draws a simple diagram[8a] and, after that initial step is taken, everything goes on in the profoundly logical way which is undoubtedly his characteristic, so that, by merely looking at that diagram, anyone sufficiently acquainted with mathematical methods could have rebuilt the whole argument. But of course there was an initial intuition, that of constructing the diagram. This was the more difficult and the more evidently an act of genius because it meant breaking from the general methods which had continued to become more and more successful after the invention of infinitesi-

[8] An error for which, however, Poincaré is not to be reproached (see next footnote).

[8a] Whether Weierstrass himself actually drew the diagram (or simply described it in words) cannot be said because he did not develop his method elsewhere than in his oral lectures. That method remained unknown for years, except to his former students.

mal calculus, which had been beautifully successful in the hands of Lagrange for obtaining the first stage of the solution, though not enabling anybody to complete it correctly. Weierstrass showed that abandoning these methods and operating directly was the right way for that.

In reality, as we see, this is an undeniable case of the general fact of logic following an initial intuition.

Application of Our Previous Data. We are thus compelled to admit that there is not a single definition of intuition *vs.* logic, but there are at least two different ones. Now, for elucidating this, why should we not make use of what we have found in our former analysis of the phenomena?

Summing up the results of that analysis, let us remember that every mental work and especially the work of discovery implies the cooperation of the unconscious, be it the superficial or (fairly often) the more or less remote one; that, inside of that unconscious (resulting from a preliminary conscious work), there is that starting of ideas which Poincaré has compared to a projection of atoms and which can be more or less scattered; that concrete representations are generally used by the mind for the maintenance and synthesis of combinations.

This carries, in the first place, the consequence that, strictly speaking, there is hardly any completely logical discovery. Some intervention of intuition issuing from the unconscious is necessary at least to initiate the logical work.

With this reservation, we immediately see that the processes such as those described above can behave differently in different minds.

(A) More or Less Depth in the Unconscious. As we know

that there must be several layers in the unconscious, some quite near consciousness while some may lie more and more remote, it is clear that the levels at which ideas meet and combine may be deeper or, on the contrary, more superficial; and it is not unreasonable to admit that there is a usual behavior of every single mind from that point of view.

It is quite natural to speak of a more intuitive mind if the zone where ideas are combined is deeper, and of a logical one if that zone is rather superficial. This manner of facing the distinction is the one I should believe to be the most important.

If that zone is deeper, there will be more difficulty in bringing the result to the knowledge of consciousness and it is likely to happen that the mind will have a tendency to do so only for what is strictly necessary. I should think this to be the case with Hermite, who certainly did not omit anything strictly essential in the results of his reflections, so that his methods were quite correct and rigorous, but without letting any trace remain of the way in which he had been led to them.

The contrary may happen: some minds may be such that ideas elaborated in the depth of the unconscious are nevertheless integrally brought to the light of consciousness. I should fancy that as happening with Poincaré, whose ideas, inspired as they may have been by farsighted intuitions, generally seemed to follow a quite natural way. One sees that there can be apparent logicians, who are logical in the enunciation of their ideas, after having been intuitive in their discovery.[9]

9 Generally speaking, as some authors observe (see Meyerson *Du Cheminement de la Pensée*, Vol. 1, cited in Delacroix *L'Invention et le*

(B) More or Less Narrowly Directed Thought. In the second place, we have seen that the projection of Poincaré's atoms—the starting of ideas, to use a less metaphorical language—can be more or less scattered. This is another reason why we can have the sensation of an intuitive mind (which will happen if there is much scattering) or (in the contrary case) of a logical mind; and the second reason can, at least a priori, be without any connection with the first one: the direction of thought may be narrower or wider, be it at one level of unconsciousness or at another. A priori, we do not know whether there is not a connection between these two kinds of "intuitive tendencies"; but in fact, an example (see below the case of Galois) will show us independence.

(C) Different Auxiliary Representations. We have seen how differently scientists behave as to the way in which their thought is helped by mental pictures or other concrete representations: differences can bear either on the nature of representations or on the way they influence the work of the mind. It is evident that some of these kinds of representations may give the thought a rather logical course, some others a rather intuitive one. But this side of the question is much less accessible to study, precisely because phenomena are not always comparable in different minds.

Most generally images are used, very often of a geometrical nature. But it would have been interesting to get, on such questions, the auto-observations of Hermite, who seemed to be so completely distant from concrete considerations. (In my own case, the role of geometrical images when

Genie, p. 480), there is often a great difference between the discovery of an idea and its enunciation.

thinking of analytical questions is very different from the way they intervene in geometrical research.)

Other Differences in Mathematical Minds. The above question is the only one which has been examined so far, concerning different kinds of mathematical minds; but, of course, there is no doubt that mathematicians can differ from each other from various other points of view.

For instance, there exists a theory, the theory of groups, the importance of which, in our science, grew increasingly for more than one century, especially since the work of Sophus Lie at the end of the nineteenth century. Some mathematicians, especially contemporary ones, have improved it by most beautiful discoveries. Some others—I confess that I belong to the latter category—though being eventually able to use it for simple applications, feel insuperable difficulty in mastering more than a rather elementary and superficial knowledge of it. Psychological reasons for that difference, which seems to me incontestable, would be interesting to find.