The Role of Intuition

Intuition plays a basic and indispensable role in mathematical research and in modern teaching methods.

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I can recall that when I was a doctoral student, I was admonished again and again by my advisers, "Don't let your intuition fool you." I cannot, however, remember just what I took this to mean; I probably thought it meant, "Don't let your imagination lead you astray; what you think is true may very possibly turn out to be false."

One of my favorite articles in this connection is a transcription of a lecture by Hans Hahn, entitled "The crisis in intuition," in the anthology edited by J. R. Newman entitled *The World of Mathematics* (*1*). This article echoes the warnings of my early teachers, and especially the admonition that "what you think is true may very possibly turn out to be false." In fact, one can easily get the impression from Hahn's article that "intuition" is a thoroughly unreliable guide and that one should regard it with suspicion even when its every suggestion has been rigorously checked.

Now insofar as checking carefully the suggestions of one's intuition is concerned, no one would quarrel with this, I believe. But as for intuition being thoroughly unreliable, I am of the opinion that this mental quality, whatever it is, has been too much maligned. Indeed, I would go so far as to say that without it, mathematical creation would well-nigh cease, and modern methods of teaching would be difficult to justify.

Nature of Mathematical Intuition

In order to support these contentions, it must be made clear just what is meant, in mathematics, by "intuition."

Not so long ago, those who were trying to test intelligence got into a predicament because they ignored the problem of defining exactly what they meant by "intelligence." Subsequently, these testers came up with a number, the "intelligence quotient" or I.Q., and it was demonstrated that the student with the higher I.Q. would, generally speaking, do better in his studies than the one with lower I.Q. But the concept of the I.Q. as a measure of something called "native intelligence," that is, the intelligence bequeathed to the individual by his heredity, had to be abandoned. After numerous experiments, especially with inductees during World War I, it became clear that the cultural environment so modifies this native intelligence as to render the I.Q., at most, a measure of the combined effect of heredity and environment on the individual's capacity for learning, his perception, and his degree of conformity to cultural directives. And an I.Q. could, over a period of time, be lowered or raised by the environmental factors active during the period (*2*).

Coming back to mathematical intuition, we might expect to find an analogous situation. I believe that the intuition about which some philosophers speak is—if not wholly, at least partially—a "native intuition." Thus Descartes stated (*3*): "By *intuition* I understand, not the fluctuating testimony of the senses, nor the misleading judgment that proceeds from the blundering constructions of imagination, but the conception which an unclouded and attentive mind gives us so readily and distinctly that we are wholly freed from doubt about that which we understand." And Kant, as I interpret him, conceived of the concepts of both time and space as deriving from an a priori intuition which is independent of experience. Among the more modern philosophers, especially those of a mystical bent, knowledge imparted by this native intuition may be considered more valid than that gained from observation and experience. The "intuitionism" of Brouwer and Poincaré, insofar as it conceived of the natural numbers as "intutitively given," seems to proceed from this native intuition (*4*).

I do not believe that my teachers had in mind anything like this native intuition. Moreover, I have always doubted whether they ever tried to analyze just exactly what they meant by "intuition." But I believe that they associated it, in some way, with experience—mathematical experience, to be more precise—and that the more experienced the mathematician became, the more reliable did his "intuition" become. That is, mathematical intuition, like intelligence, is a psychological quality which stems possibly from a hereditarily derived faculty, but which is, at any given time, principally an accumulation of attitudes derived from one's mathematical experience. This should not be taken to mean that mathematical intuition is something which already contains one's attitude toward a mathematical situation which one has never faced before. Indeed, in this day of widely diversified branches of mathematics, a mathematician may be expected to have little or no intuition regarding a branch of mathematics in which he has never worked; his intuition is of use chiefly in those areas with which he has had some experience. There is some agreement between this assertion, I think, and the one with which Hahn concluded his article (*1*), namely, that intuition "is force of habit rooted in psychological inertia." Like intelligence, and I refer here to the kind that I.Q. testers have in mind, intuition is greatly influenced, possibly wholly formed, by the cultural environment—probably even more so than is intelligence. For I believe, in particular, that the average nonmathematician has no mathematical intuition at all, except that nebulous quality of the mind which, if nourished by experience with mathematics, would develop into what we call mathematical intuition.

Individual versus Collective Intuition

I have used the word "psychological" with reference to intuition (from here on, "intuition" will mean "mathematical intuition"). I wish to emphasize that my ultimate concern is with the intuition of the individual mathematician. I am not unaware of the fact

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that concerning certain questions there is essentially what might be called a collective or cultural intuition. For instance, before Weierstrass gave his example of a real continuous function having no derivative at any point of its interval of definition, probably almost every mathematician felt intuitively that such a function could not exist; this intuition had become a cultural attitude, a common belief. But consider the four-color map problem (5): I doubt if the average mathematician today has any intuitive feeling regarding whether there exists or does not exist a map that cannot be colored with at most four colors—simply because he has never worked on the problem. And an analogous statement can be made about the so-called "last theorem of Fermat," as well as a host of other problems. Before one can have a really intuitive feeling about such problems, one must have worked on them. But everyone who has gone very far in mathematics will have worked with functions of a real variable and can be expected to have developed an intuition for them. A similar remark holds for the structure of the real-number continuum. So far as those mathematical concepts that form part of the equipment of every mathematician are concerned, there can be expected to exist a kind of intuition that is common to most members of the mathematical community. But as soon as one goes beyond these concepts to mathematical specialties—particularly to their frontiers—then the intuition becomes a quite individual affair; and it is this intuition that is of immediate importance in creative work.

But this is entirely in accord with the concept of mathematical intuition as an accumulation of attitudes derived from one's experience. Regarding matters of common knowledge, such as function theory, the attitudes we acquire are determined by our teachers, and the relation to the general mathematical culture of the time is apparent. But when one cultivates a special area of interest, and especially as he becomes involved in research on its frontiers, then one develops his own attitudes in the light of his own personal experiences. Only he can make the educated guess, since he has developed his own intuition. And although the connection with the current cultural atmosphere is still traceable, it is much less direct.

Coming to my main topic—the role of intuition—it is advisable, I believe, to look at some specific examples first. And since the manner in which intuition exerts its influence varies according to whether it is collective (cultural) or individual, and whether it is true or false, I shall separate my examples along those lines.

Examples

Let us first consider the intuition apparently possessed by the Greeks, and certainly by their medieval successors, that the parallel axiom was true. I use the word "true" in the absolute sense in which *they* seem to have used it. This was an intuitive belief possessed by all mathematicians, since during the period involved everyone who professed to be expert in mathematics was expected to be familiar with Euclid's *Elements*. It was an instance in which the collective intuition was a false guide—a case typical of those which Hahn cited in his article. It is interesting, however, to try to assess the overall influence which this intuition had on mathematics. That an intuition was false is not sufficient reason to conclude that it was bad. And in this case, I believe that the influence was highly beneficial. For if it had not been for the conviction that the parallel axiom could be proved from the other axioms of Euclid—and this conviction was a direct result of the common intuition concerning its truth—then possibly the appearance of the non-Euclidean geometries together with their effect on all mathematics and philosophy might have been delayed. Of course the non-Euclidean geometry would most certainly have been discovered sooner or later, if for no other reason than that the axiomatic method, as Hilbert, for example, conceived it in his *Foundations of Geometry*, was already beginning to emerge in the work of such mathematicians as Boole, Hamilton, and others. And someone would no doubt ultimately have experimented with alternatives for the parallel axiom, just as Hamilton and Grassmann experimented with denials of the commutative laws of algebra. But because of the special position held by Euclidean geometry, not only in philosophy, but as part of the general mathematical curriculum, the impact of the eventual realization of the independence of the parallel axiom on the mathematical and philosophical community started a chain of research, the effects of which caused a virtual revolution in philosophical and mathematical thought.

I have already mentioned the intuition, also false, which underlay the conviction that every continuous function must have a derivative at some point of its interval of definition. I am confident that, if a study were made of the historical background preceding publication of Weierstrass's example (*6*), it would be found that the influence of this false intuition had had its beneficial aspects. I can immediately recall Lagrange's proposed method for calculating derivatives by expanding functions in Taylor's series; thereby making a start on the theory of analytic functions. If he had known, as we now know, that most continuous function ("most" in the sense of the Baire category) have no derivatives anywhere in the interval of definition, might he not have been deterred from proposing a method which he considered applicable to all continuous functions?

To take a more recent case, consider the general "closed curve"; more specifically, a curve which is a common boundary of two domains in the plane. There was more general interest in this topological configuration 65 years ago than now, since both the Jordan curve theorem and Peano's space-filling curve had stimulated interest in plane curves. Although we now know quite simple examples of closed curves which have complementary domains other than the two of which they are the common boundary (*7*), apparently around the turn of the century the common intuition was that there could be only two such domains, an "inside" and an "outside." Just how much influence the proving of the Jordan curve theorem had on this intuition we can only surmise. At any rate, Schoenflies, who had recently given such a proof and who could be considered an expert on the topology of the plane, as well as one of the principal founders of the topology of Euclidean spaces, published a number of results in which he took it for granted, as intuitively clear, that a closed curve could have only two complementary domains. Now this was bad, of course; but was its influence on the development of mathematics bad? I think not. For example, it evidently came to the attention of L. E. J. Brouwer, the "father" of modern Intuitionism, and inspired him to look into the validity of the assumption.

I surmise that this helped arouse Brouwer's interest in topology (although that interest possibly had other stimuli too), and that his classical work in this regard (giving counterexamples which included closed curves which are the common boundaries of an arbitrary countable number of domains) influenced his continuing interest in topology. In particular, it led to his interest in the topological invariance of closed curves. In the proof of this, which he was the first to give, he started a chain of ideas which led to the extension of homology theory to general spaces. For several years thereafter he was quite active in this branch of mathematics, finding a number of results which have become classical (such as his fixed point theorems and work on mappings of locally Euclidean manifolds), and which were not fully appreciated in the mainstream of topology until over a decade later.

Common Features of the Examples

All three of the examples of collective intuition that I have mentioned were false, yet it is difficult to believe that their influence was entirely bad. It is curious how much good mathematics can be done even when the collective intuition concerning basic matters is false. This is most striking during the period preceding Weierstrass's example. For during that period the collective intuition concerning continuity, existence of derivatives, infinite series, the real-number system, and a host of other fundamental concepts was at best faulty and usually full of error. Yet on such a basis much of classical analysis was built up. It would be quite as apt to speak of the "modern miracle" as we do of the "Greek miracle."

Speaking of the "Greek miracle" recalls the classical crisis regarding commensurability. Here again, the collective intuition regarding number and magnitude, according to which all magnitudes were commensurable, though false, was able to support the creation of much good mathematics. Moreover, the ultimate discovery of their true character led, in all these cases, to very fruitful periods of mathematical activity. It is my individual opinion that they all represent natural phenomena in the evolution of mathematics. In each case, the evidence is strong that the discovery of the error in the basic intuition was about to burst forth through the medium of several mathematical leaders, all working independently. In the case of the discovery of incommensurability, some have attributed it to Pythagoras himself, others to Hippasus (a student of Pythagoras); but the truth is that nobody really knows. However, since the Pythagorean theorem had become known quite generally at that time, the incommensurability between the diagonal and side of a square could not have been long concealed, no matter who first detected it. In the case of the parallel axiom, Gauss, Bolyai, and Lobachewski all discovered the facts at about the same time. We now know that Bolzano had an example similar to Weierstrass's. And about the same time that Brouwer found his example of a "pathological" closed curve, a Japanese mathematician, Wada, also apparently produced one. And no one knows how many other individual mathematicians were either working on, or had produced examples, proving the faulty character of each of these collective intuitions. The discovery of the space-filling curve, which I mentioned above only incidentally, was evidently another typical case. Parametric representation of plane curves had proved extremely useful, although evidently its introduction (possibly by Cauchy) was made under the influence of the intuitive belief that such curves would always be curves of the intuitively accepted kind —that is, having no "breadth" or "thickness." The usual pattern of events followed. After much good research based on the concept, almost simultaneously Peano, E. H. Moore, and Hilbert came up with examples showing that the intuition underlying the concept of parametric representation was false (*8*). There followed a period of 40 years or so of research in plane topology and problems related to it. The pattern is quite typical.

Role of Intuition in Evolution of Concepts

What do these case histories indicate concerning the manner in which mathematical concepts evolve on the cultural level? I am asking this question with a twofold purpose in mind. Thus far I have recounted only cases in which the collective intuition was false—I have not given any cases where it was true—and mathematical intuition is not always wrong, fortunately. Consequently I would like to cite some cases where the intuition was correct; but at the same time I would like to consider how these cases dovetail, so to speak, with the former cases in forcing the formulation of new concepts.

Let me begin with the most basic concept of all, namely number; more specifically, the "counting" or "natural" numbers, 1, 2, 3, ... The Intuitionist philosophy regards the origin of these to be in man's intuition of "fundamental series" of mental acts, consisting of a first act, a second act, a third act, and so on. I presume this must have been an intuition which was derived from the physical and cultural environment. More specifically—and this can be inferred from a study of the forms of primitive number-words, as well as of the practice of tallying —the use of one-to-one correspondence to compare collections of physical objects, along with the repetitive character of the actual determination of such correspondences, built up a set of attitudes which formed, ultimately, the intuition of fundamental series. And I presume this was an intuition on the cultural level, shared by virtually all who found it necessary to engage in the primitive forms of counting. Probably an analogous kind of intuition was involved in the genesis of geometry, where it became necessary to compare lengths and areas. All of this is very conjectural, of course, but it seems fairly representative of what occurred prior to those periods for which the historical records are more complete. And it is our earliest example of how correct intuition on the collective level serves to build the mathematical edifice.

However, it was an intuition which finally led to concepts that produced the "Greek crisis"; and it was necessary for Eudoxus and his contemporaries to create a new conceptual framework which, while containing the major part of the old, rejected the parts that had been found false. There followed that flowering of activity that we call the "Greek miracle," based on a new intuition of the number concept—the so-called geometric "magnitudes"—which permitted a further construction of mathematical theory atop the old of the Pythagoreans. Although couched in the language of geometry, this intuition comprised virtually a complete theory of the real-number system. Unfortunately, the course taken by Western culture precluded further development of the

Greek intuition. And it was not until early times that activity in mathematical analysis, based on the foundation laid by the Greeks and their successors (who added new symbolic representations for number), brought to light the inadequacy of the intuition created by the work of Eudoxus. By the latter half of the 19th century, real analysis had reached a more precise formulation of the real-number continuum with the notion of set. The so-called "arithmetization of analysis" by Weierstrass and others provided a new conception of the real continuum and made possible the theory of measure and the brilliant researches of the first half of the present century in both analysis and topology.

But this new conception of the real continuum gave birth to a new intuition—that of the theory of sets. The work of Cantor was the classical formulation of this new intuition. Some of its faults were discovered early, in the guise of the set-theoretic contradictions. By now, the mathematical world had developed new standards of rigor, and it was realized that the remedy must be sought in a more precise formulation of the theory of sets. The axiomatic method, used by the Greeks to avoid the Zeno paradoxes and the commensurability assumption, was approaching a new maturity and again offered a method for attaining the desired precision. For most ordinary purposes, axiomatic systems for set theory provided quite a satisfactory basis. But so far as a unique formulation of general set theory is concerned, we are today in little better position than were the Pythagoreans with respect to geometry, or the early analysts with respect to the real continuum. Our knowledge of the axiom of choice, for instance, is purely intuitive. We have an accumulation of good mathematics based upon its use, but we feel uneasy about its paradoxical consequences, such as the Banach-Tarski theorem (*9*). The same holds for the continuum hypothesis, although this is perhaps not so serious for most of us. It does serve as a reminder, however, that our intuition of the real continuum was not thoroughly clarified by the work of Weierstrass and his contemporaries. They, of necessity, brought into being a new intuition—the theory of sets—and so long as this theory has only an intuitive base, so must all the mathematics dependent upon it.

I think that there is only one conclusion that we can draw from all this, namely, that so far as mathematics being ultimately based on intuition is concerned, the Intuitionists are correct. But the mathematical intuition, as I have used the notion, is not precisely that of Intuitionism; and, moreover, the methods which the majority of mathematicians use are not those of the Intuitionistic doctrine.

But to summarize the role of the mathematical intuition in the evolution of mathematical concepts—our collective intuition of basic concepts has grown by a series of discoveries of faulty features in the current concepts, with ultimate replacement by new concepts which not only clear up the faults, but lead to feverish activity on the new foundation with consequent creation of much good mathematics. Ultimately, the new concepts begin to reveal faults; in particular, we discover that they have brought in with them new intuitions which have to be made more conceptually precise. And the cycle goes on.

Role of Intuition in Research

I come now to the role of intuition in research. The biographical comments in Poincaré's writings, and the more complete work of Hadamard (*10*), embody a good account of how intuition works on the individual level in creative work. As I remarked before, this is intuition which is of a highly specialized nature. It relates to the particular problem on which only the individual, or a few individuals, are working. It is true, of course, that in their background is collective intuition, and they are certainly influenced by it. In particular, their choice of the problems on which they work is guided by what the collective intuition deems the most fruitful direction for research. But once having selected the particular problem, the individual begins to build new concepts and their resultant intuitions. In a way, he repeats the experience of the general mathematical culture, but on a different level and at greater rates of change. His false intuitions are usually recognized to be such in a relatively short time ("relatively short" can be as much as several years, of course), and they are patched up by correct conceptual material.

These remarks apply, too, in the case of problems that remain unsolved for many years and become "classical." The experienced individuals may have stopped working on them, having found their efforts at solution frustrated, and therefore have gone on to problems promising quicker results. I believe that what happens here is that the collective intuition in the field of a particular problem continues to grow, being passed on by the older workers to the younger. Ultimately, due to a combination of a more mature collective intuition (which has been growing unnoticed), new methods, and individual genius, someone (usually a younger mathematician, relatively new in the field, and possessing a fresh individual intuition) is able to solve the problem. That feeling of awe, which I am sure many older creative mathematicians must get regarding the powers of the younger generation of creative workers, has a firm basis. The younger man has not only come into the particular field without having to clutter up his brain with concepts and methods which served their purpose and are now discarded, but using new concepts and methods he has built up an individual intuition which forms a platform from which he can regard his field of research with an eye undimmed by the recollection of earlier and faulty intuitions. The director of his first research has no stronger responsibility than that of guiding and steering this young intuition into the most up-to-date conceptual channels. It is almost a truism that without intuition, there is no creativity in mathematics.

Role of Intuition in Teaching

Like collective intuition, individual mathematical intuition is not a static but a growing thing. It starts developing when we are children, during the time when we learn to distinguish shapes and sizes (geometric intuition) and to count (arithmetic intuition). We are not born with it, for without a cultural basis for its development, there can apparently be no mathematical intuition. By the time the child starts school in our culture, however, he usually has some basis to build on—his parents have probably taught him to count, for example—and the continuing development of this basis undoubtedly forms one of the central responsibilities of primary teachers.

By the time the student reaches high

school he should have a fairly substantial intuitive base from which to work. Presumably his teachers have used his arithmetic intuition to develop both higher arithmetic and algebra, and—at least under new curricular ideas—his geometric intuition, not only to develop elementary geometric facts but to aid in solving arithmetic and algebraic problems. And in this process, the teacher should have added to the intuitive base. In short, as the student comes to the high school teachers, his mathematical equipment should have two main components—the intuitive component and the knowledge component. These are difficult to separate, particularly since the intuitive component is dependent for its growth on the knowledge component.

Perhaps I can make this clearer by stating my conception of what the new curricula being developed today should accomplish in contrast to the old, standard, mathematical curriculum. The old curriculum was designed chiefly for the knowledge component; the student was taught how to perform arithmetic and algebraic operations and how to prove theorems. But little conscious development of mathematical intuition took place; what there was of this seemed to find expression chiefly in the problems that were given to be solved. But insofar as these were mechanical repetition of the operations or modes of proof that had been taught, they added little or nothing to the intuitive component. In contrast to this, the new curricula should try to turn teaching of the knowledge component into a process whereby the student's intuition is actually used and developed further in acquiring the new knowledge.

For example, while under the old system the student was *told* the formula for carrying out a process, under the new he should be invited to do a little guessing as to what form the process should take. This guessing and the accompanying experimentation, resulting in a decision as to the final result, develops and strengthens his mathematical intuition. In an embryonic way, this procedure is precisely the same as that pursued by the research mathematician, and in my opinion the teacher who cultivates it is doing creative teaching. And I believe that all concepts should be introduced in this way. To explain a concept to a student adds to his knowledge component, perhaps, but does not strengthen his intuition. Probably the worst example of this kind of thing is the writing of a definition on the board, then explaining what it means and how it is used.

For example, consider the mathematical induction principle. One can proceed by first writing it on the blackboard in the form in which it is usually stated, as an axiom; secondly, by explaining what it means; and thirdly, by showing how to use it in proof of simple arithmetic formulas. This is followed by homework in which the student applies the process much as a proof algorithm, imitating what the teacher has done. The brighter students will not have any trouble with this, perhaps, but the average ones will be beset by minor questions such as: "How do I find the $(n + 1)$st term?" —questions which are largely due to the algorithmic character of what they have been taught.

Now this kind of teaching is certainly not going to help the student recognize, when he later comes to a problem in which mathematical induction is a natural mode of proof or definition, that the mathematical induction principle may be called upon. For while he may "know" mathematical induction he has not acquired any intuitive feeling for it. If, on the other hand, his teacher had given him credit for knowing how to count and having an intuition of "fundamental series," and if the teacher had proceeded from there to guide him to the *discovery* of the mathematical induction principle, then the student would have acquired not only a knowledge of the principle, but also an intuitive base for later recognizing instances when the principle could be applied. In this way, the intuition would be permitted to play its proper role in creative teaching.

Perhaps most experienced teachers already use such creative teaching methods, and they would not think of presenting a definition without first calling upon the student's intuitive powers to help formulate the definition. However, two matters worry me: First, that they may find, under the pressure of crowding a certain amount of material into a given amount of time, that it is necessary to resort to the old mode of teaching which consists of (i) statement of the definition, (ii) explanation of it, and (iii) application of the concept to a particular problem. In discussing the so-called "Moore method" of teaching—which exemplifies much of what I have been saying—Moise commented that "sheer knowledge does not play the crucial role in mathematical development that most people suppose" (*11*). And a propos of the time lost in using the Moore method, he stated: "The resulting ignorance ought to be a hopeless handicap, but in fact it isn't; and the only way that I can see to resolve this paradox is to conclude that mathematics is capable of being learned as an *activity*, and that knowledge which is acquired in this way has a power which is out of all proportion to its quantity." And in the second volume of Polya's recent book *Mathematical Discovery* (*12*), there is the quotation from the 18th-century German physicist Lichtenberg: "What you have been obliged to discover by yourself leaves a path in your mind which you can use again when the need arises." This is an expressive way of saying that you have added to the accumulation of your mathematical intuition.

The second matter that worries me is related to the use of the axiomatic method in secondary school teaching, particularly where the function is that of definition. What I said before regarding mathematical induction applies here. The student should not be introduced to a theory by means of axioms. Consider the arithmetic of integers. Here is a theory with which the student is already familiar, a circumstance which makes it an excellent subject for a proper introduction to axiomatics. But before stating a single axiom, the teacher ought to respect the student's imagination enough to tell him something about the purpose of axiomatics. In particular, he should be told that one wishes to seek out certain specific aspects of the arithmetic of integers from which the other aspects can be derived; for having done so, then not only can one test the accuracy of an operation against the axiomatic base, but one can also try out one's imagination by finding models for all or some of the axioms other than that of the arithmetic of integers —as, for example, the arithmetic of rationals, elementary algebra, and the like.

After having decided to try to list axioms, the student should be encouraged to discover suitable axioms himself—under the guiding hand of the teacher, of course. It should go without saying, however, that if these matters are too advanced to be comprehended by the student, then the axiomatic method should not be introduced at all. Nor should axioms be sneaked in under the guise of so-called "laws" presumably handed down by some obscure mathematical Moses.

Most of this applies, I believe, to college teaching—certainly up to the end of the first two years of college. As the student goes on to more advanced work, the intuitive component of his training begins to assume more importance. At this stage of his career it may be assumed that he is possibly going on to do some kind of creative work, if not in mathematics, then in some other science. And it is desirable that his teachers have had some experience with creative work. This does not mean that the teacher must have a Ph.D. degree; this is a fetish I wish we could get rid of. I would much prefer a teacher without a Ph.D. who is excited about mathematics and can teach creatively, than a teacher with a Ph.D. who is neither enthusiastic about mathematics nor capable of inspiring his students. Naturally, as the student progresses into graduate work, most of his teachers will, as a matter of course, have Ph.D.'s, since they should themselves either be doing creative work, or at least have done sufficient work to realize the role of intuition in such work and the importance of using methods that will develop it. The student in the graduate stage should be capable of adding to his knowledge component on his own; his mentor's responsibility is chiefly to nourish his mathematical intuition, for it is this that is going to be of greater importance in his career as a mathematician.

Summary

"Intuition," as used by the modern mathematician, means an accumulation of attitudes (including beliefs and opinions) derived from experience, both individual and cultural. It is closely associated with mathematical knowledge, which forms the basis of intuition. This knowledge contributes to the growth of intuition and is in turn increased by new conceptual materials suggested by intuition.

The major role of intuition is to provide a conceptual foundation that suggests the directions which new research should take. The opinion of the individual mathematician regarding existence of mathematical concepts (number, geometric notions, and the like) are provided by this intuition; these opinions are frequently so firmly held as to merit the appellation "Platonic." The role of intuition in research is to provide the "educated guess," which may prove to be true or false; but in either case, progress cannot be made without it and even a false guess may lead to progress. Thus intuition also plays a major role in the evolution of mathematical concepts. The advance of mathematical knowledge periodically reveals flaws in cultural intuition; these result in "crises," the solution of which result in a more mature intuition.

The ultimate basis of modern mathematics is thus mathematical intuition. and it is in this sense that the Intuitionistic doctrine of Brouwer and his followers is correct. Modern instructional methods recognize this role of intuition by replacing the "do this, do that" mode of teaching by a "what should be done next?" attitude which appeals to the intuitive background already developed. It is in this way that understanding and appreciation of new mathematical knowledge may be properly instilled in the student.

References and Notes

1. J. R. Newman, *The World of Mathematics* (Simon and Schuster, New York, 1956), pp. 1956–1976.
2. C. S. Yoakum, personal communication.
3. R. Descartes, in *The Philosophical Works of Descartes*, E. S. Haldane and G. R. T. Ross, Trans. (Cambridge Univ. Press, Cambridge, 1911).
4. R. L. Wilder, *Introduction to the Foundations of Mathematics* (Wiley, New York, ed. 2, 1965), pp. 211–212 and chap. 10. The Intuitionist philosophy holds that all mathematics should be founded on the "intuitively given" notion of sequence (such as a sequence of acts) having a first element, a second element, and so on. The sequence of natural numbers, 1, 2, 3, ..., is the mathematical model for such a sequence, and only concepts that can be built on this sequence by *constructive* methods can be accepted as mathematically valid, according to Intuitionistic tenets. Acceptance of these tenets would severely limit modern mathematical conceptualization, as well as rule out certain methods of proof; proof of existence by contradiction (contradictio ad absurdum) would not be acceptable, for example. In an even more restricted form, the tenets of Intuitionism are to be found in the doctrines of the famous 19th-century mathematician, L. Kronecker.
5. See, for instance, P. Franklin, in *Galois Lectures* (Scripta Mathematica Library, New York, 1941).
6. A simple example may be found in T. H. Hildebrandt, *Amer. Math. Mon.* **40**, 547 (1933).
7. Examples are described in R. L. Wilder, *Mathematics Teacher* **55**, 462 (1947).
8. See J. W. Young, *Fundamental Concepts of Algebra and Geometry* (Macmillan, New York, 1916), pp. 167–170. Young's statement that this is a one-to-one correspondence between the points of the line interval and the square is false, however.
9. "A solid unit sphere can be decomposed into a finite number of pieces which can be reassembled to form *two* solid unit spheres." A brief discussion may be found in E. Kasner and J. Newman, *Mathematics and the Imagination* (Simon and Schuster, New York, 1940), pp. 206–207; for technical details see S. Banach and A. Tarski, *Fund. Math.* **6**, 244 (1924); R. M. Robinson, [*ibid.* **34**, 246 (1947)] shows that the minimal number may be 5.
10. J. Hadamard, *The Psychology of Invention in the Mathematical Field*, (Princeton Univ. Press, Princeton, N.J., 1949).
11. E. E. Moise, *Amer. Math. Mon.* **72**, 407 (1965).
12. G. Polya, *Mathematical Discovery* (Wiley, New York, 1965), vol. 2, p. 103.