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Mathematical Intuition (Poincaré, Polya, Dewey)

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Summary: Practical calculation of the limit of a sequence often violates the definition of convergence to a limit as taught in calculus. Together with examples from Euler, Polya and Poincare, this fact shows that in mathematics, as in science and in everyday life, we are often obligated to use knowledge that is derived, not rigorously or deductively, but simply by making the best use of available information—plausible reasoning. The "philosophy of mathematical practice" fits into the general framework of "warranted assertibility," the pragmatist view of the logic of inquiry developed by John Dewey.

Keywords: intuition, induction, pragmatism, approximation, convergence, limits, knowledge.

In Rio de Janeiro in May 2010, I spoke at a meeting of numerical analysts honoring the 80th anniversary of the famous paper by Courant, Friedrichs and Lewy. In order to give a philosophical talk appropriate for hard-core computer-oriented mathematicians, I focused on a certain striking paradox that is situated right at the heart of analysis, both pure and applied. (That paradox was presented, with considerable mathematical elaboration, in Phil Davis's excellent article, "The Paradox of the Irrelevant Beginning.") In order to make this paradox cut as sharply as possible, I performed a little dialogue, with help from Carlos Motta. With the help of Jody Azzouni, I used that dialogue again, to introduce this talk in Rome.

To set the stage, recall the notion of a convergent sequence, which is at the heart of both pure analysis and applied mathematics. In every calculus course, the student learns that whether a sequence converges to a limit, and what that limit is, depend only on the "end" of the sequence--that is, the part that is "very far out"—in the tail, so to speak, or in the infinite part. Yet, in a specific instance when the limit is actually needed, usually all that is considered is the beginning of the sequence—the first few terms--the finite part, so to speak. (Even if the calculation is carried out to a hundred or a thousand iterations, this is still only the first few, compared to the remaining, neglected, infinite tail.)

In this little drama of mine, the hero is a sincere, well-meaning student, who has not yet learned to accept life as it really is. A second character is the Successful Mathematician--the Ideal Mathematician's son-in-law. His mathematics is ecumenical: a little pure, a little applied, and a little in-between. He has grants from federal agencies, a corporation here and there, and a private foundation or two. His conversation with the Stubborn Student is somewhat reminiscent of a famous conversation between his Dad, the Ideal Mathematician, and a philosophy grad student, who long ago asked, "What is a mathematical proof, really?"

The Successful Mathematician (SM) is accosted by the Stubborn Student (SS) from his Applied Analysis course.

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SS: Sir, do you mind if I ask a stupid question?
SM: Of course not. There is no such thing as a stupid question.
SS: Right. I remember, you said that. So here's my question. What is the real definition of "convergence"? Like, convergence of an infinite sequence, for instance?

SM: Well, I'm sure you already know the answer. The sequence converges to a limit, L, if it gets within a distance epsilon of L, and stays there, for any positive epsilon, no matter how small.

SS: Sure, that's in the book, I know that. But then, what do people mean when they say, keep iterating till the iteration converges? How does that work?

SM: Well, it's obvious, isn't it? If after a hundred terms your sequence stays at 3, correct to four decimal places, then the limit is 3.

SS: Right. But how long is it supposed to stay there? For a hundred terms, for two hundred, for a hundred million terms?

SM: Of course you wouldn't go on for a hundred million. That really *would* be stupid. Why would you waste time and money like that?

SS: Yes, I see what you mean. But what then? A hundred and ten? Two hundred? A thousand?

SM: It all depends on how much you care. And how much it is costing, and how much time it is taking.

SS: All right, that's what I would *do*. But when does it *converge*?

SM: I told you. It converges if it gets within epsilon—

SS: Never mind about that. I am supposed to go on computing "until it converges," so how am I supposed to recognize that "it has converged"?

SM: When it gets within four decimal points of some particular number and stays there.

SS: Stays there how long? Till when?

SM: Whatever is reasonable. Use your judgment! It's just plain common sense, for Pete's sake!

SS: But what if it keeps bouncing around within four decimal points and never gets any closer? You said *any* epsilon, no matter how small, not just point 0001. Or if I keep on long enough, it might finally get bigger than 3, even bigger than 4, way, way out, past the thousandth term.

SM: Maybe this, maybe that. We haven't got time for all these maybes. Somebody else is waiting to get on that machine. And your bill from the computing center is getting pretty big.

SS: (mournfully) I guess you're not going to tell me the answer.

SM: You just don't get it, do you? Why don't you go bother that Reuben Hersh over there, he looks like he has nothing better to do.

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SS: Excuse me, Professor Hersh. My name is---

RH: That's OK. I overheard your conversation with Professor Successful over there. Have a seat.

SS: Thank you. So, you already know what my question is.

RH: Yes, I do.

SS: So, what is the answer?

RH: He told you the truth. The real definition of convergence is exactly what he said, with the epsilon in it, the epsilon that is arbitrarily small but positive.

SS: So then, what does it mean, "go on until the sequence converges, then stop"?

RH: It's meaningless. It's not a precise mathematical statement. As a precise mathematical statement, it's meaningless.

SS: So, if it's meaningless, what does it mean?

RH: He told you what it means. Quit when you can see, when you can be pretty sure, what the limit must be. That's what it means.

SS: But that has nothing to do with convergence!

RH: Right.

SS: Convergence only depends on the last part, the end, the infinite part of the sequence. It has nothing to do with the front part. You can change the first hundred million terms of the sequence, and that won't affect whether it converges, or what the limit is.

RH: Right! Right! Right! You really are an A student.

SS: I know…. So it all just doesn't make any sense. You teach us some fancy definition of convergence, but when you want to compute a number, you just forget about it and say it converges when common sense, or whatever you call it, says something must be the answer. Even though it might not be the answer at all!

RH: Excellent. I am impressed.

SS: Stop patronizing me. I'm not a child.

RH: Right. I will stop patronizing me, because you are not a child.

SS: You're still doing it.

RH: It's a habit. I can't help it.

SS: Time to break a bad habit.

RH: OK. But seriously, you are absolutely right. I agree with every word you say.

SS: Yes, and you also agree with every word Professor Successful says.

RH: He was telling the truth, but he couldn't make you understand.

SS: All right. *You* make me understand.

RH: It's like theory and practice. Or the ideal and the actual. Or Heaven and Earth.

SS: How is that?

RH: The definition of convergence lives in a theoretical world. An ideal world. Where things can happen as long as we can clearly imagine them. As long as we can understand and agree on them. Like really being positive and arbitrarily small. No number we can write down is positive and arbitrarily small. It has to have some definite size if it is actually a number. But we can imagine it getting smaller and smaller and smaller while staying positive, and we can even express that idea in a formal sentence, so we accept it and work with it. It seems to convey what we want to mean by converging to a limit. But it's only an ideal, something we can imagine, not something we can ever really do.

SS: So you're saying mathematics is all a big fairy tale, a fiction, it doesn't actually exist?

RH: NO! I never said fairy tale or fiction. I said imaginary. Maybe I should have said consensual. Something we can all agree on and work with, because we all understand it the same way.

SS: That's cool. We all. All of you. Does that include me?

RH: Sure. Stay in school a few more years. Learn some more. You'll get into the club. You've got what it takes.

SS: I'm not so sure. I have trouble believing two opposite things at once.

RH: Then how do you get along in daily life? How do you even get out of bed in the morning?

SS: What are you talking about?

RH: How do you know someone hasn't left a bear trap by your bedside that will chop off your foot as soon as you step down?

SS: That's ridiculous.

RH: It is. But how do you know it is?

SS: Never mind how I know. I just know it's ridiculous. And so do you.

RH: Exactly. We know stuff, but we don't always know how we know it. Still, we do know it.

SS: So you're saying, we know that what looks like a limit really is a limit, even though we can't prove it, or explain it, still we know it.

RH: We know it the same way you know nobody has left a bear trap by your bedside. You just know it.

SS: Right.

RH: But it's still possible that you're wrong. It is possible that something ridiculous actually happens. Not likely, not worth worrying about. But not impossible.
SS: Then math is really just like everything else. What a bummer! I like math because it's not like everything else. In math, we know for sure. We prove things. One and one is two. Pi is irrational. A circle is round, not square. For sure.
RH: Then why are you upset? Everything is just fine, isn't it?
SS: Why don't you admit it? If you don't have a proof, you just don't know if L is the limit or not.
RH: That's a fair question. So what is the answer?
SS: Because you really want to *think you know* L is the limit, even if it's not true.
RH: Not that it's not true, just that it *might* not be true.

End of dialogue

Thanks for your kind attention. What is supposed to be the meaning of this performance? What am I getting at? In this talk I am NOT attempting to make a contribution to the "problem of induction." Therefore I may be allowed to omit a review of its 2,500-year literature. I am reporting and discussing what people really do, in practical convergence calculations, and in the process of mathematical discovery. I am going into a discussion of practical knowledge in mathematics, as a kind of real knowledge, even though it is not demonstrative or deductive knowledge. I try to explain why people must do what they do, in order to accomplish what they are trying to accomplish. I will conclude by arguing that the right broader context for the philosophy of mathematical practice is actually the philosophy of pragmatism, as expounded by John Dewey.

But first of all, just this remarkable fact. What we do when we want actual numbers may be totally unjustified, according to our theory and our definition. And even more remarkable—nobody seems to notice, or to worry about it!

Why is that? Well, the definition of convergence taught in calculus classes, as developed by those great men Augustin Cauchy and Karl Weierstrass, seems to actually convey what we want to mean by limit and convergence. It is a great success. Just look at the glorious edifice of mathematical analysis! On the other hand, in specific cases, it often is beyond our powers to give a rigorous error estimate, even when we have an approximation scheme that seems perfectly sound. As in the major problems of three-dimensional continuum mechanics with realistic nonlinearities, such as oceanography, weather prediction, stability of large complex structures like big bridges and airplanes….And even when we could possibly give a rigorous error estimate, it often would require great expenditure of time and labor. Surely it's OK to just use the result of a calculation when it makes itself evident and there's no particular reason to expect any hidden difficulty.

In brief, we are virtually compelled by the practicalities to accept the number that computation seems to give us, even though, by the standards of rigorous logic, there is still an admitted possibility that we may be mistaken. This computational result is a kind of mathematical knowledge! It is practical knowledge, knowledge sound enough to be the basis of practical decisions about things like designing bridges and airplanes—matters of life and death.

In short, I am proclaiming that in mathematics, apart from and distinct from so-called deductive or demonstrative knowledge, there is also ordinary, fallible knowledge, of the same sort as our daily knowledge of our physical environment and our own bodies. "Anything new that we learn about the world involves plausible reasoning, which is the only kind of reasoning for which we care in everyday affairs." (Polya, 1954). This sentence of his makes an implicit separation between mathematics and everyday affairs. But nowadays, in many different ways, for many different kinds of people, mathematics blends into every-day affairs. In these situations, the dominance of plausible over demonstrative reasoning applies even to mathematics itself, as in the daily labors of numerical analysts, applied mathematicians, design engineers… Controlling a rocket trip to the moon is not an exercise in mathematical rigor. It relies on a lack of malice on the part of that Being referred to by Albert Einstein as *der lieber Gott*.

(For fear of misunderstanding, I explain—this is ***not*** a confession of belief in a Supreme Being. It's just Einstein's poetic or metaphoric way of saying, Nature is not an opponent consciously trying to trick us.)

But it's not only that we have no choice in the matter. It's also that, truth to tell, it seems perfectly reasonable! Believing what the computation tells us is just what people have been doing all along, and (nearly always) it does seem to be OK. What's wrong with that?

This kind of reasoning is sometimes called "plausible," and sometimes called "intuitive." I will say a little more about those two words pretty soon. But I want to draw your attention very clearly to two glaring facts about this kind of plausible or intuitive reasoning. First of all, it is pretty much the kind of reasoning that we are accustomed to in ordinary empirical science, and in technology, and in fact in everyday thinking, dealing with any kind of practical or realistic problem of human life. Secondly, it makes no claim to be demonstrative, or deductive, or conclusive, as is often said to be the essential characteristic of mathematical thinking. We are face to face with mathematical knowledge that is not different in kind from ordinary everyday commonplace human knowledge. Fallible! But knowledge, nonetheless!

Never mind the pretend doubt of philosophical skepticism. We are adults, not infants. Human adults know a lot! How to find their way from bed to breakfast—and people's names and faces--and so forth and so on. This is real knowledge. It is not infallible, not eternal, not heavenly, not Platonic, it is just what daily life depends on, that's all. That's what I mean by ordinary, practical, everyday knowledge. Based not mainly on rigorous demonstration or deduction, but mainly on experience properly interpreted. And here we see mathematical knowledge that is of the same ordinary, everyday kind, based not on infallible deduction, but on fallible, plausible, intuitive thinking.

Then what justifies it in a logical sense? That is, what fundamental presupposition about the world, about reality, lies behind our willingness to commit this logical offense, of believing what isn't proved?

I have already quoted the famous saying of Albert Einstein that supplies the key to unlocking this paradox.

My friend Peter Lax supplied the original German, I only remembered the English translation.

Raffinniert ist der lieber Gott, aber boshaft ist Er nicht.
The Lord God is subtle, but He is not malicious.

Of course, Einstein was speaking as a physicist struggling to unravel the secrets of Nature. The laws of Nature are not always obvious or simple, they are often subtle. But we can believe, we *must* believe, that Nature is not set up to trick us, by a malicious opponent. God, or Nature, must be playing fair. How do we know that? We really *don't* know it, as a matter of certainty! But we must believe it, if we seek to understand Nature with any hope of success. And since *we do have some success* in that search, our belief that Nature is subtle but not malicious is justified.

This problem of inferring generalizations from specific instances is known in logic as "the problem of induction." My purpose is to point out that such generalizations in fact are made, and must be made, not only in daily life and in empirical science, but also in mathematics.

That is, in the practice of mathematics also we must believe that we are not dealing with a malicious opponent who is seeking to trick us. We experiment, we calculate, we draw diagrams. And eventually, using caution and the experience of the ages, we see the light. Gauss famously said, "I have my theorems. Now I have to find my proofs."

But is it not naïve, for people who have lived through the hideous twentieth century, to still hope that God is not malicious? Consider, for example, a people who for thousands of years have lived safely on some atoll in the South Pacific. Today an unforeseen tsunami drowns them all. Might they not curse God in their last breath?

Here is an extensive quote from Leonhard Euler, by way of George Polya. Euler is speaking of a certain beautiful and surprising regularity in the sum of the divisors of the integers.

> This law, which I shall explain in a moment, is in my opinion, so much more remarkable as it is of such a nature that we can be assured of its truth without giving it a perfect demonstration. Nevertheless, I shall present such evidence for it as might be regarded as almost equivalent to a rigorous demonstration….anybody can satisfy himself of its truth by as many examples

> as he may wish to develop. And since I must admit that I am not in a position to give it a rigorous demonstration, I will justify it by a sufficiently large number of examples…I think these examples are sufficient to discourage anyone from imagining that it is by mere chance that my rule is in agreement with the truth…The examples that I have just developed will undoubtedly dispel any qualms which we might have had about the truth of my formula….it seems impossible that the law which has been discovered to hold for 20 terms, for example, would not be observed in the terms that follow. (Polya, 1954).

Observe two things about this quote from Euler. First of all, for him the plausible reasoning in this example is so irresistible that it leaves no room for doubt. He is certain that anyone who looks at his examples is bound to agree. Yet secondly, he strongly regrets his inability to provide a demonstration of the fact, and still hopes to find one.

But since he is already certain of the truth of his finding, why ask for a demonstrative proof? The answer is easy, for anyone familiar with mathematical work. The demonstration would not just affirm the truth of the formula, it would show why the formula MUST be true. That is the main importance of proof in mathematics! A plausible argument, relying on examples, analogy and induction, can be very strong, can carry total conviction. But if it is not demonstrative, it fails to show why the result MUST be true. That is to say, it fails to show that it is rigidly connected to established mathematics.

At the head of Chapter V, Polya (1954) placed the following apocryphal quotation, attributed to "the traditional mathematics professor": "When you have satisfied yourself that the theorem is true, you start proving it." (Polya 1954)

This faith---that experience is not a trap laid to mislead us--is the unstated axiom. It lets us believe the numbers that come out of our calculations, including the canned programs that engineers use every day as black boxes. We know that it can sometimes be false. But even as we keep possible tsunamis in mind, we have no alternative but to act as if the world makes sense. We must continue to act on the basis of our experience. (Including, of course, experiences of unexpected disasters.)

Consider this recollection of infantile mathematical research by the famous physicist Freeman Dyson, who wrote in 2004:

> One episode I remember vividly, I don't know how old I was; I only know that I was young enough to be put down for an afternoon nap in my crib…I didn't feel like sleeping, so I spent the time calculating. I added one plus a half plus a quarter plus an eighth plus a sixteenth and so on, and I discovered that if you go on adding like this forever you end up with two. Then I tried adding one plus a third plus a ninth and so on, and discovered that if you go on adding like this forever you end up with one and a half. Then I tried one plus a quarter and so on, and ended up with one and a third. So I had discovered infinite series. I don't remember talking about this to anybody at the time. It was just a game I enjoyed playing. (Dyson 2004)

Yes, he knew the limit! How did he know it? Not the way we teach it in high school (by getting an exact formula for the sum of n terms of a geometric sequence, and then proving that as n goes to infinity, the difference from the proposed limit becomes and remains arbitrarily small.) No, just as when we first show this to tenth-graders, he *saw* that the sums follow a simple pattern that *clearly* is "converging" to 2. The formal, rigorous proof gives insight into the *reason* for a fact we have already seen plainly.

Can we go wrong this way? Certainly we can. Another quote from Euler.

> There are even many properties of the numbers with which we are well acquainted, but which we are not yet able to prove; only observations have led us to their knowledge…the kind of knowledge which is supported only by observations and is not yet proved must be carefully distinguished from the truth; it is gained by induction, as we usually say. Yet we have seen cases in which mere induction led to error. Therefore, we should take great care not to accept as true such properties of the numbers which we have discovered by observation and which are supported by induction alone. Indeed, we should use such a discovery as an opportunity to investigate more exactly the properties discovered and to prove or disprove them. (Polya 1954, p. 3)

Notice how Euler distinguishes between "knowledge" and "truth"! He does say "knowledge," not mere "conjecture."

There is a famous theorem of Littlewood concerning a pair of number-theoretic functions PI(x) and Li(x). All calculation shows that Li(x) is greater than PI(x), for x as large as we can calculate. Yet Littlewood proved that *eventually* PI(x) becomes greater than Li(x), and not just once, but infinitely often! Yes, mathematical truth can be very subtle. While trusting it not to be malicious, we must not underestimate its subtlety. (PI(x) is the prime counting function and Li(x) is the logarithmic integral function.)

Mathematical Intuition

We are concerned with "the philosophy of mathematical practice." Mathematical practice includes studying, teaching and applying mathematics. But I suppose we have in mind first of all the discovery and creation of mathematics—mathematical research. We start with Jacques Hadamard, go on to Henri Poincare, move on to George Polya, and then to John Dewey.

Hadamard had a very long life and a very productive career. His most noted achievement (shared independently by de la Vallee Poussin) was proving the logarithmic distribution of the prime numbers. I want to recall a famous remark of Hadamard's. "The object of mathematical rigor is to sanction and legitimize the conquests of intuition, and there never was any other object for it." (Polya 1980)

From the viewpoint of standard "philosophy of mathematics," this is a very surprising, strange remark. Isn't mathematical rigor—that is, strict deductive reasoning—the most essential feature of mathematics? And indeed, what can Hadamard even mean by this word, "intuition"? A word that means one thing to Descartes, another thing to Kant. I think the philosophers of mathematics have pretty unanimously chosen to ignore this remark of Hadamard. Yet Hadamard did know a lot of mathematics, both rigorous and intuitive. And this remark was quoted approvingly by both Borel and Polya. It seems to me that this bewildering remark deserves to be taken seriously.

Let's pursue the question a step further, by recalling the famous essay "Mathematical Discovery," written by Hadamard's teacher, Henri Poincare. (Poincare 1952) Poincare was one of the supreme mathematicians of the turn of the 19th and 20th century. We've been hearing his name recently, in connection with his conjecture on the 3-sphere, just recently proved by Grisha Perelman of St. Petersburg. Poincare was not only a great mathematician, he was a brilliant essayist. And in the essay "Mathematical Discovery," Poincare makes a serious effort to explain mathematical intuition. He tells the famous story of how he discovered the Fuchsian and Theta-Fuchsian functions. He had been struggling with the problem unsuccessfully when he was distracted by being called up for military service:

> At this moment I left Caen, where I was then living, to take part in a geological conference arranged by the School of Mines. The incidents of the journey made me forget my mathematical work. When we arrived at Coutances, we got into a bus to go for a drive, and, just as I put my foot on the step the idea came to me, though nothing in my former thoughts seemed to have prepared me for it, that the transformations I had used to define Fuchsian functions were identical with those of non-Euclidean geometry. I made no verification, and had no time to do so, since I took up the conversation again as soon as I had sat down in the bus, but I felt absolute certainty at once. When I got back to Caen, I verified the result at my leisure to satisfy my conscience. (Poincare 1952)

What a perfect example of rigor "merely legitimizing the conquests of intuition"! How does Poincare explain it? First of all, he points out that some sort of subconscious thinking must be going on. But if it is subconscious, he presumes it must be running on somehow at random. How unlikely, then, for it to find one of the very few good combinations, among the huge number of useless ones! To explain further, he writes:

> If I may be permitted a crude comparison, let us represent the future elements of our combinations as something resembling Epicurus's hooked atoms. When the mind is in complete repose these atoms are immovable; they are, so to speak, attached to the wall…On the other hand, during a period of apparent repose, but of unconscious work, some of them are detached from the wall and set in motion. They plough through space in all directions, like a swarm of gnats, for instance, or, if we prefer a more learned comparison, like the gaseous molecules in the kinetic theory of gases. Their mutual impacts may then produce new combinations. (Poincare 1952)

The preliminary conscious work "detached them from the wall." The mobilized atoms, he speculated, would therefore be "those from which we might reasonably expect the desired solution….My comparison is very crude, but I cannot well see how I could explain my thought in any other way." (Poincare 1952)

What can we make of this picture of "Epicurean hooked atoms," flying about somewhere—in the mind? A striking, suggestive image, but one not subject even in principle to either verification or disproof. Our traditional philosopher remains little interested. This is fantasy or poetry, not science or philosophy. But this is Poincare! He knows what he's talking about. He has something important to tell us. It's not easy to understand, but let's take him seriously, too.

To be fair, Poincare proposed his image of gnats or gas molecules only after mentioning the possibility that the subconscious is actually more intelligent than the conscious mind. But this, he said, he was not willing to contemplate. However, other writers have proposed that the subconscious is less inhibited, more imaginative, more *creative* than the conscious. (Poincare's essay title is sometimes translated as "Mathematical Creation" rather than "Mathematical Discovery.") David Hilbert supposedly once said of a student who had given up mathematics for poetry, "Good! He didn't have enough imagination for mathematics." Hadamard (1949) carefully analyzes the role of the subconscious in mathematical discovery and its connection with intuition. It is time for contemporary cognitive psychology to pay attention to Hadamard's insights. See the reference below about current scientific work on the creative power of the subconscious

Before going on, I want to mention the work of Carlo Cellucci, Emily Grosholz and Andrei Rodin. Cellucci strongly favors plausible reasoning, but he rejects intuition. However, the intuition he rejects isn't what I'm talking about. He's rejecting the old myth, of an infallible insight straight into the Transcendental. Of course I'm not advocating that outdated myth. Emily Grosholz, on the other hand, takes intuition very seriously. Her impressive historical study of what she calls "internal intuition" is in the same direction as my own thinking being presented here. Andrei Rodin has recently written a remarkable historical study of intuition (Rodin 2010). He shows that intuition played a central role in Lobachevski's non-Euclidean geometry, in Zermelo's axiomatic set theory, and even in up-to-date category theory. (By the way, in category theory he could also have cited the standard practice of proof by "diagram chasing" as a blatant example of intuitive, visual proof.) His exposition makes the indispensable role of intuition clear and convincing. But his use of the term "intuition" remains, one might say, "intuitive," for he offers no definition of the term, nor even a general description, beyond his specific examples.

Polya

My most helpful authority is George Polya. I actually induced Polya to come give talks in New Mexico, for previously, as a young instructor, I had met him at Stanford where he was an honored and famous professor. Polya was not of the stature of Poincare or Hilbert, but he was still one of the most original, creative, versatile and influential mathematicians of his generation. His book with Gabor Szego (Polya-Szego 1970) made them both famous. It expounds large areas of advanced analytic function theory by means of a carefully arranged, graded sequence of problems with hints and solutions. Not only does it teach advanced function theory, it also teaches problem-solving. And by example, it shows how to teach mathematics by teaching problem-solving. Moreover, it implies a certain view of the nature of mathematics, so it is a philosophical work in disguise.

Later, when Polya wrote his very well-known, influential books on mathematical heuristic, he admitted that what he was doing could be regarded as having philosophical content. He writes, "I do not know whether the contents of these four chapters deserve to be called philosophy. If this is philosophy, it is certainly a pretty low-brow kind of philosophy, more concerned with understanding concrete examples and the concrete behavior of people than with expounding generalities." (Polya 1954 page viii) Unpretentious as Polya was, he was still aware of his true stature in mathematics. I suspect he was also aware of the philosophical depth of his heuristic. He played it down because, like most mathematicians (I can only think of one or two exceptions), he disliked controversy and arguing, or competing for the goal of becoming top dog in some cubbyhole of academia. The Prince of Mathematicians, Carl Friedrich Gauss, kept his monumental discovery of non-Euclidean geometry hidden in a desk drawer to avoid stirring up the Boeotions, as he called them,—meaning the post-Kantian German philosophy professors of his day. (In ancient Athens, "Boeotian" was slang for "ignorant country hick.") Raymond Wilder was a leading topologist who wrote extensively on mathematics as a culture. He admitted to me that his writings implicitly challenged both formalism and Platonism. "Why not say so?" I asked. Because he didn't relish getting involved in philosophical argument.

Well, how does Polya's work on heuristic clarify mathematical intuition? Polya's heuristic is presented as pedagogy. Polya is showing the novice how to solve problems. But what is "solving a problem"? In the very first sentence of his preface Polya (1980) writes, "Solving a problem means finding a way out of a difficulty, a way around an obstacle, attaining an aim which was not immediately attainable. Solving problems is the specific achievement of intelligence, and intelligence is the specific gift of mankind: solving problems can be regarded as the most characteristically human activity." "Problem" is simply another word for any project or enterprise which one cannot immediately take care of with the tools at hand. In mathematics, something more than a mere calculation. Showing how to solve problems amounts to showing how to do research!

Polya's exposition is never general and abstract, he always uses a specific mathematical problem for the heuristic he wants to teach. His mathematical examples are always fresh and attractive. And his heuristic methods? First of all, there is what he calls "induction." That is, looking at examples, as many as necessary, and using them to *guess* a pattern, a generalization. But be careful! Never just believe your guess! He insists that you must "Guess and test, guess and test." Along with induction, there is analogy, and there is making diagrams, graphs and every other kind of picture, and then reasoning or guessing from the picture. And finally, there's the "default hypothesis of chance"—that an observed pattern is mere coincidence.

(Mark Steiner has the distinction among philosophers of paying serious attention to Polya. After quoting at length from Polya's presentation of Euler's heuristic derivation of the sum of a certain infinite series, Steiner comes to an important conclusion: in mathematics we can have knowledge without proof! Based on the testimony of mathematicians, he even urges philosophers to pay attention to the question of mathematical intuition.)

I have two comments about Polya's heuristic that I think he would have accepted. First of all, the methods he is presenting, by means of elementary examples, are methods he used himself in research. "In fact, my main source was my own research, and my treatment of many an elementary problem mirrors my experience with advanced problems." (Polya, 1980, page xi). In teaching us how to solve problems, he's teaching us about mathematical practice: How it works. What is done. To find out "What is mathematics?" we must simply reinterpret Polya's examples as descriptive rather than pedagogical.

Secondly, with hardly any stretching or adjustment, the heuristic devices that he's teaching can be applied for any other kind of problem-solving, far beyond mathematics. He actually says that he is bringing to mathematics the kind of thinking ordinarily associated with empirical science. But we can go further. These ways of thinking are associated with every kind of problem-solving, in every area of human life! Someone needed to get across a river or lake and had the brilliant idea of "a boat"—whether it was a dugout log or a birch bark canoe. Someone else, needing shelter from the burning sun in the California Mojave, thought of digging a hole in the ground. And someone else, under the piercing wind of northern Canada, thought of making a shelter from blocks of ice.

How does anyone think of such a thing, solve such a concrete problem? By some kind of analogy with something else he has seen, or perhaps been told about. By plausible thinking. And often by a sudden insight that arises "from below."

Intuitively, you might say.

Mental Models

It often happens that a concrete problem, whether in science or in ordinary daily life, is pressing on the mind, even when the particular materials or objects in question are not physically present. You keep on thinking about it, while you're walking, and when you're waking from sleep. Productive thought commonly takes place, in the absence of the concrete objects or materials being thought about. This thinking about something not present to sight or touch can be called "abstract thinking." Abstract thinking about a concrete object. How does that work? How can our mind/brain think productively about something that's not there in front of the eyes? Evidently, it operates on something mental, what we may call a mental image or representation. In the current literature of cognitive psychology, one talks about "a mental model." In this article, I use the term "mental model" to mean a mental structure built from recollected facts (some expressed in words), along with an ensemble of sensory memories, perhaps connected, as if by walking around the object in question, or by imagining the object from underneath or above, even if never actually seen in these views. A rich complex of connected knowledge and conjecture based on verbal, visual, kinesthetic, even auditory or olfactory information, but simplified, to exclude irrelevant details. Everything that's helpful for thinking about the object of interest when the object isn't here. Under the pressure of a strong desire or need to solve a specific problem, we assemble a *mental model* which the mind-brain can manipulate or analyze.

Subconscious thinking is not a special peculiarity of mathematical thinking, but a common, taken-for-granted, part of every-day problem-solving. When we consider this commonplace fact, we aren't tempted to compare it to a swarm of gnats hooking together at random. No, we assume, as a matter of course, that this subconscious thinking follows rules, methods, habits or pathways, that somehow, to some extent, correspond to the familiar plausible thinking we do when we're wide awake. Such as thinking by analogy or by induction. After all, if it is to be productive, what else can it do? If it had any better methods, then those better methods would also be what we would follow in conscious thinking! And subconscious thinking in mathematics must be much like subconscious thinking in any other domain, carrying on plausible reasoning as enunciated by various writers, above all by George Polya. This description of subconscious thinking is not far from Michael Polanyi's "tacit dimension."

When applied to everyday problem solving, all this is rather obvious, perhaps even banal. My goal is to clarify mathematical intuition, in the sense of Hadamard and Poincare. "Intuition" in the sense of Hadamard and Poincare is a fallible psychological experience that has to be accounted for in any realistic philosophy of mathematics. It simply means guesses or insights attained by plausible reasoning, either fully conscious or partly subconscious. In this sense it is a specific phenomenon of common experience. It has nothing to do with the ancient mystical myth of an intuition that surpasses logic by making a direct connection to the Transcendental.

The term "abstract thinking" is commonplace in talk about mathematics. The triangle, the main subject of Euclidean geometry, is an abstraction, even though it's idealized from visible triangles on the blackboard. Thinking of a physical object in its absence, like a stream to be crossed or a boat to be imagined and then built, is already "abstract" thinking, and the word "abstract" connects us to the abstract objects of mathematics.

Let me be as clear and simple as I can be about the connection. After we have some practice drawing triangles, we can think about triangles, we discover properties of triangles. We do this by reasoning about mental images, as well as images on paper. This is already abstract thinking. When we go on to regular polygons of arbitrarily many sides, we have made another departure. Eventually we think of the triangle as a 2-simplex, and abstract from the triangle to the n-simplex. For $n = 3$ this is just the tetrahedron, but for $n = 4$ or 5 or 6, it is something never yet seen by human eye. Yet these higher simplexes also can become familiar, and, as it were, concrete-seeming. If we devote our waking lives to thinking about them, then we have some kind of "mental model" of them. Having this mental model, we can access it, and thereby we can reason intuitively—have intuitive insights—by which I mean simply insights not based on consciously known reasoning. An "intuition" is then simply a belief (possibly mistaken!) arising from internal inspection of a mental image or representation—a "model." It may be assisted by subconscious plausible reasoning, based on the availability of that mental image. We do this in practical life. We do it in empirical science, and in mathematics. In empirical science and ordinary life, the image may stand for either an actual object, a physical entity, or a potential one that could be realized physically. In mathematics, our mental model is sometimes idealized from a physical object—for example, from a collection of identical coins or buttons when we're thinking about arithmetic. But in mathematics we also may possess a mental model with no physical counterpart. For example, it is generally believed that Bill Thurston's famous conjectures on the classification of four-manifolds were achieved by an exceptional ability, on the part of Thurston, to think intuitively in the fourth dimension. Perhaps Grisha Perelman was also guided by some four-dimensional intuition, in his arduous arguments and calculations to prove the Thurston program.

To summarize, mathematical intuition is an application of conscious or subconscious heuristic thinking of the same kind that is used every day in ordinary life by ordinary people, as well as in empirical science by scientists. This has been said before, by both Hadamard and Polya. In fact, this position is similar to Kurt Godel's, who famously wrote, "I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception." Why, indeed? After all, both are fallible, but both are plausible, and must be based on plausible reasoning.

For Godel, however, as for every writer in the dominant philosophy of mathematics, intuition is called in only to justify the axioms. Once the axioms are written down, the role of mathematical intuition is strictly limited to "heuristic"—to formulating conjectures. These await legitimation by deductive proof, for only deductive proof can establish "certainty." Indeed, this was stated as firmly by Polya as by any analytic philosopher. But what is meant by "mathematical certainty"? If it simply means deductive proof, this statement is a mere circular truism. However, as I meant to suggest by the little dialog at the beginning of this paper, there is also *practical* certainty, even within mathematics! We are certain of many things in ordinary daily life, without deductive proof, and this is also the case in mathematics itself. Practical certainty is a belief strong enough to lead to serious practical decisions and actions. For example, we stake our lives on the numerical values that went into the engineering design of an Airbus or the Golden Gate Bridge. Mainstream philosophy of mathematics does not recognize such practical certainty. Nevertheless, it is an undeniable fact of life.

It is a fact of life not only in applied mathematics but also in pure mathematics. For example, the familiar picture of the Mandelbrot set, a very famous bit of recent pure mathematics, is generated by a machine computation. By definition, any particular point in the complex plane is inside the Mandelbrot set if a certain associated iteration stays bounded. If that iteration at some stage produces a number with absolute value greater than two, then, from a known theorem, we can conclude that the iteration goes to infinity, and the parameter point in question is *outside* the Mandelbrot set. What if the point is *inside* the Mandelbrot set? No finite number of iterations in itself can guarantee that the iteration will never go beyond absolute value 2. If we do eventually decide that it looks like it will stay bounded, we may be right, but we are still cheating. This decision is opportunistic and unavoidable, just as in an ordinary calculation about turbulent flow.

Computation (numerics) is accepted by purists only as a source of conjectures awaiting rigorous proof. However, from the pragmatic, non-purist viewpoint, if numerics is our guide to action, then it is in effect a source of knowledge. Dewey called it "warranted assertibility." (Possibly even a "truth." A "truth" that remains open to possible reconsideration.)

Another example from pure mathematics appeared on John Baez's blog (Baez 2010) where it is credited to Sam Derbyshire. His pictures plot the location in the complex plane of the roots of all polynomials of degree 24 with coefficients plus one or minus one. The qualitative features of these pictures are absolutely convincing— i.e., impossible to disbelieve. Baez wrote, "That's 2^{24} polynomials, and about 24×2^{24} roots — or about 400 million roots! It took Mathematica 4 days to generate the coordinates of the roots, producing about 5 gigabytes of data." (Figure 1 shows the part of the plot in the first quadrant, for complex roots with non-negative real and imaginary parts.)

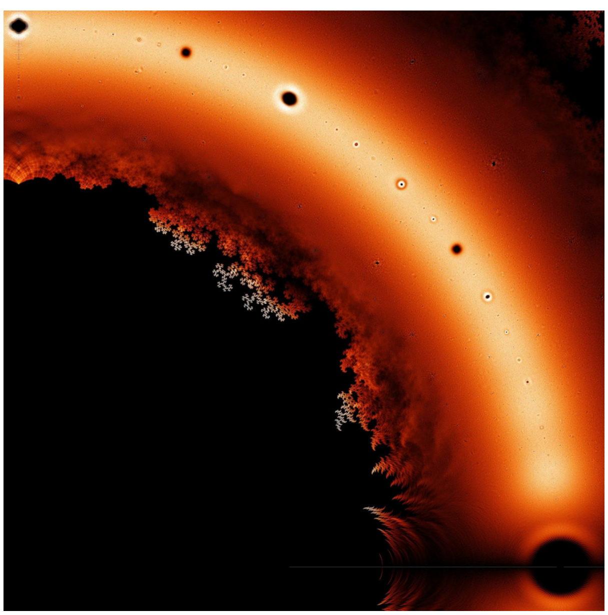

Figure 1.

There is more information in this picture than can even be formulated as conjectures, let alone seriously attacked with rigor. Since indeed we cannot help believing them (perhaps only believing with 99.999%

credence) then (pragmatically) we give them "warranted assertibility," just like my belief that I can walk out my door without encountering sudden death in one form or another. The distinction between rigorous math and plausible math, pure math and applied math, etc, becomes blurred. It is still visible, certainly, but not so sharp. It's a little fuzzy. Purely computational results in pure mathematics, when backed up by sophisticated checking against a relevant theory, have a factual status similar to that of accepted facts from empirical science. The distinction between what is taken to be "known," and what is set aside as merely guessed or "conjectured", is not so cut and dried as the usual discussions claim to believe.

Mental Models Subject to Social Control

"Plausible" or "heuristic" thinking is applied, either consciously or subconsciously, to mental models. These mental models may correspond to tangible or visible physical objects in ordinary life and empirical science. Or they may not correspond to any such things, but may be pure mental representations, as in much of contemporary analysis, algebra, and even geometry. By pure mental models I mean models not obtained directly by idealization of visual or other sensual experience.

But what controls these mental models? If they have no physical counterpart, what keeps them from being wildly idiosyncratic and incommunicable? What we have omitted up to this point, and what is the crux of the matter: mathematical images are not private, individual entities. From the origin of mathematics in bartering, buying and selling, or in building the Parthenon and the Pyramids, this subject has always been a social, an "inter-subjective" activity. Its advances and conquests have always been validated, corrected and absorbed in a social context—first of all, in the classroom. Mathematicians can and must talk to each other about their ideas. One way or the other, they do communicate, share and compare their conceptions of mathematical entities, which means precisely these models, these images and representations I have been describing. Discrepancies are recognized and worked out, either by correcting errors, reconciling differences, or splitting apart into different, independent pathways. Appropriate terminology and symbols are created as needed.

Mathematics depends on a mutually acknowledging group of competent practitioners, whose consensus decides at any time what is regarded as correct or incorrect, complete or incomplete. That is how it always worked, and that is how it works today. This was made very clear by the elaborate process in which Perelman's proposed proof of the Thurston program (including the Poincare conjecture) was vetted, examined, discussed, criticized and finally accepted by the "Ricci flow community," and then by its friends in the wider communities of differential geometry and low-dimensional topology, and then by the prize committees of the Fields Medal and the Clay Foundation.

Thus, when we speak of a mathematical concept, we speak not of a single isolated mental image, but rather of a family of mutually correcting mental images. They are privately owned, but publicly checked, examined, corrected, and accepted or rejected. This is the role of the mathematical research community, how it indoctrinates and certifies new members, how it reviews, accepts or rejects proposed publication, how it chooses directions of research to follow and develop, or to ignore and allow to die. All these social activities are based on a necessary condition: that the individual members have mental models that fit together, that yield the same answers to test questions. A new branch of mathematics is established when consensus is reached about the possible test questions and their answers. That collection of possible questions and answers (not necessarily explicit) becomes the means of accepting or rejecting proposed new members.

If two or three mathematicians do more than merely communicate about some mathematical topic, but actually collaborate to dig up new information and understanding about it, then the matching of their mental models must be even closer. They may need to establish a congruence between their subconscious thinking about it as well as their conscious thinking. This can be manifested when they are working together, and one speaks the very thought that the partner was about to speak.

And to the question "What is mathematics?" the answer is "It is socially validated reasoning about these mutually congruent mental models."

What makes mathematics possible? It is our ability to create mental models which are "precise," meaning simply that they are part of a shared family of mutually congruent models. In particular, such an image as a line segment, or two intersecting line segments, and so on. Or the image of a collection of mutually interchangeable identical objects (ideal coins or buttons). And so on. To understand better how that ability exists, both psychologically and neurophysiologically, is a worthy goal for empirical science. The current interactive flowering of developmental psychology, language acquisition, and cognitive neuroscience shows that this hope is not without substance. (See, e.g., Carey, Dehaene, Johnson-Laird, Lakoff/Nunez, Zwaan.)

The existence of mathematics shows that the human mind is capable of creating, refining, and sharing such precise concepts, which admit of reasoning that can be shared, mutually checked, and confirmed or rejected. There are great variations in the vividness, completeness, and connectivity of different mental images of the "same" mathematical entity as held by different mathematicians. And, also great variations in their ability to concentrate on that image and squeeze out all of its hidden information. Recall that well-known mathematician, Sir Isaac Newton. When asked how he made his discoveries in mathematics and physics, he answered simply, "By keeping the problem constantly before my mind, until the light gradually dawns." Indeed, neither meals nor sleep were allowed to interrupt Newton's concentration on the problem. Mathematicians are notoriously absent-minded. Their concentration, which outsiders call "absent-mindedness," is just the open secret of mathematical success.

Their reasoning is qualitatively the same as the reasoning carried out by a hunter tracking a deer in the Appalachian woodland a thousand years ago. "If the deer went to the right, I would see a hoof print here. But I don't see it. There's only one other way he could have gone. So he must have gone to the left." Concrete deductive reasoning, which is the basis for abstract deductive reasoning.

To sum up! I have drawn a picture of mathematical reasoning which claims to make sense of intuition according to Hadamard and Poincare, and which interprets Polya's heuristic as a description of ordinary practical reason, applied to the abstract situations and problems of mathematics, working on mental models in the same way that ordinary practical reasoning *in absentia* works on a mental model. (We may assist our mental images by creating images on paper—drawing pictures—that to some extent capture crucial features of the mental images.)

Dewey and Pragmatism

Before bringing in John Dewey, the third name promised at the beginning, I must first mention Dewey's precursor in American pragmatism, Charles Saunders Peirce, for Peirce was also a precursor to Polya. To deduction and induction, Peirce added a third logical operation, "abduction," something rather close to Polya's "intelligent guessing."

The philosophy of mathematics as practiced in many articles and books is a thing unto itself, hardly connected either to living mathematics or to general philosophy. But how can it be claimed that the nature of mathematics is unrelated to the general question of human knowledge? There has to be a fit between your beliefs about mathematics and your beliefs about science and about the mind. I claim that Dewey's pragmatism offers the right philosophical context for the philosophy of mathematical practice to fit into. I am thinking especially of *Logic—the Theory of Inquiry*. For Dewey, "inquiry" is conceived very broadly and inclusively. It is "the controlled or directed transformation of an indeterminate situation into one that is so determinate in its constituent distinctions and relations as to convert the elements of the original situation into a unified whole." So broadly understood, inquiry is one of the primary attributes of our species. Only because of that trait have we survived, after we climbed down from the trees. I cannot help comparing Dewey's definition of inquiry with Polya's definition of problem solving. It seems to me they are very much pointing in the same direction, taking us down the same track. With the conspicuous difference that, unlike Dewey, Polya is concise and memorable.

Dewey makes a radical departure from standard traditional philosophy (following on from his predecessors Peirce and William James, and his contemporary George Herbert Mead). He does not throw away the concept of truth, but he gives up the criterion of truthfulness, as the judge of useful or productive thinking. Immanuel Kant made clear once and for all that while we may know the truth, we cannot know for certain that we do know it. We must perforce make the best of both demonstrative and plausible reasoning. This seems rather close to "warranted assertibility," as Dewey chooses to call it. But Polya or Poincare are merely talking about mathematical thinking, Dewey is talking about human life itself.

What about deductive thinking? From Dewey's perspective of "warranted assertibility," deductive proof is not a unique, isolated mode of knowledge. A hunter tracking a deer in the North American woodland a thousand years ago concluded, "So it must have gone to the left." Concrete deductive reasoning, the necessary basis of theoretical deductive reasoning. And it never brings certainty, simply because any particular deductive proof is a proof in practice, not in principle. Proof in practice is a human artifact, and so it can't help leaving some room for possible question, even possible error. (And that remains true of machine proof, whether by analog, digital, or quantum computer. What changes is the magnitude of the remaining possible error and doubt, which can never vanish finally.) In this way, we take our leave, once and for all, of the Platonic ideal of knowledge—indubitable and unchanging—in favor, one might say, of an Aristotelian view, a scientific and empirical one. And while deductive proof becomes human and not divine or infallible, non-deductive plausible reasoning and intuition receive their due as a source of knowledge in mathematics, just as in every other part of human life. Dewey's breadth of vision--seeing philosophy always in the context of experience, that is to say of humanity at large--brings a pleasant breath of fresh air into this stuffy room.

Nicholas Rescher (2001) writes,

> The need for understanding, for 'knowing one's way about,' is one of the most fundamental demands of the human condition….Once the ball is set rolling, it keeps on going under its own momentum—far beyond the limits of strictly practical necessity….The discomfort of unknowing is a natural aspect of human sensibility. To be ignorant of what goes on about us is almost physically painful for us…The requirement for information, for cognitive orientation within our environment, is as pressing a human need as for food itself. (Rescher 2001)

The need for understanding is often met by a story of some kind. In our scientific age, we expect a story built on a sophisticated experimental-theoretical methodology. In earlier times, no such methodology was available, and a story might be invented in terms of gods or spirits or ancestors. In inventing such explanations, whether in what we now call mythology or what we now call science, people have always been guided by a second fundamental drive or need. Rescher does not mention it, but Dewey does not leave it out. That is the need to impart form, beauty, appealing shape or symmetry to our creations, whether they be straw baskets, clay pots, wooden spears and shields, or geometrical figures and algebraic calculations. In *Art as Experience* Dewey shows that the esthetic, the concern for pleasing form, for symmetry and balance, is also an inherent universal aspect of humanity. In mathematics this is no less a universal factor than the problem-solving drive. In "Mathematical Discovery" Poincare takes great pains to emphasize the key role of esthetic preference in the development of mathematics. We prefer the attractive looking problems to work on, we strive for diagrams and graphs that are graceful and pleasing. Every mathematician who has talked about the nature of mathematics has portrayed it as above all an art form. So this is a second aspect of pragmatism that sheds light on mathematical practice.

Rescher's careful development omits mathematical knowledge and activity. And Dewey himself doesn't seem to have been deeply interested in the philosophy of mathematics, although there are interesting pages about mathematics in *Logic*, as well as in his earlier books *The Quest for Certainty* and *The Psychology of Number*. He may have been somewhat influenced by the prevalent view of philosophy of mathematics as an enclave of specialists, fenced off both from the rest of philosophy and from mathematics itself.

But if we take these pragmatist remarks of Rescher's seriously and compare them to what mathematicians do, we find a remarkably good fit. Just as people living in the woodland just naturally want to know and find out about all the stuff they see growing—what makes it grow, what makes it die, what you can do with it to make a canoe or a tent—so people who get into the world of numbers, or the world of triangles and circles, just naturally want to know how it all fits together, and how it can be stretched and pulled this way or that. "Guess and test," is the way George Polya put it. "Proofs and refutations" was the phrase used by another mathematically trained Hungarian philosopher, following up an investigation started by Polya. Whichever way you want to put it, it is nothing more or less than the exploration of the mathematical environment, which we create and expand as we explore it. We are manifesting in the conceptual realm one of the characteristic behaviors of *homo sapiens*.

Even though we lack claws or teeth to match beasts of prey, or fleetness to overtake the deer, or .swimming, paddling and sailing, cooking and brewing and baking and preserving, and we expanded our social groups from families to clans to tribes to kingdoms to empires. All this by "inquiry," or by problem-solving. Dewey shows that this inquiry is an innate specific drive or need of our species. It was manifested when, motivated by practical concerns, we invented counting and the drawing of triangles. That same drive, to find projects, puzzles, and directions for growth, to make distinctions and connections, and then again make new distinctions and new connections, has resulted in the Empire of mathematics we inhabit today.

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