What are the limits of mathematical explanation?

Interview with **Charles McCarty** by **Piotr Urbańczyk**

Piotr Urbańczyk: Our guest is Professor Charles McCarty. He does research in the areas of philosophy of mathematics and philosophy of logic, especially intuitionism, as well as foundations of mathematics and early analytical philosophy. He has written about the history of mathematics and logic, especially of the late 19th and early 20th centuries. Professor McCarty, I would like to ask you, "What is an explanation in mathematics?"

Charles McCarty: I worry that this question – which is raised more frequently of late – may be prompted by an obsession with those results of mathematics that enter into scientific explanations of physical phenomena, even though those applied results may not be characteristic of mathematics overall. For all I know, persistent questions about the explanatory character of proofs are critical to legitimizing those philosophies of mathematics that ape modish philosophies of the physical sciences. Also, I worry that the question betrays a lazy desire to make all mathematical facts as simple as possible in presentation, as if we were spoiled children who must have things spelled out in the most painfully elementary terms. That said, there is a plain difference, a felt difference at least, between mathematical demonstrations that we find revelatory, that give us a sensation of "Aha! Now I know what's going on!" and mathematical demonstrations that may not engender this sensation, although both kinds of demonstration can be perfectly cogent and serve other, more important ends in mathematics. Of course, an annoying problem is to lay authoritatively definitory, and if not definitory, at least explicative hands on that strongly felt but elusive difference. The 'Aha' sensation in mathematics and its various philosophies may prove as resistant to theory as the 'Ho ho' sensations in the aesthetics of comedy.

A close associate of questions about mathematical explanation is the blithe assumption that the goal of mathematical proof is exhausted in producing conviction of some kind, attaining acquiescence to the conclusion(s) of the proof. While a great deal of mathematical proof does serve the purpose of gaining acquiescence, an equally great deal does not serve that purpose at all. A proof – in the language of *Principia Mathematica* (Russell, Whitehead, 1910–1913), for instance – that 1 plus 1 equals 2 certainly has not got conviction in its conclusion as a principal aim, getting readers to agree that 1 plus 1 equals 2 on the basis of a prior grasp of axioms governing higher-order propositional functions specified predicatively. Rather, the provision of such a proof is confirmation (in part) that the system of *Principia Mathematica* and the logicism it implements are sufficient unto some ordinary claims of gradeschool mathematics. The proof forges connections between recognizably mathematical ideas and (at times wildly) philosophical ones, and helps to justify, in its application to the latter, the title ‘foundational.’ (Here and throughout, I use the word ‘proof’ to cover epistemic, intensional constructs from a spectrum running from strictly discursive argumentation through demonstrations, constructions, calculations, mathematical thought-experiments, to glossed diagrams.)

Consider a (seemingly nonexplanatory) proof from strictly Dedekindian principles that, when you multiply the integer -1 by itself, you get 1 back. In this, you imagine an integer represented by (or identified with?) a pair of natural numbers under a primitive recursive equivalence relation. Furthermore, you imagine multiplication defined primitive recursively in terms of summed cross-products of the (natural number) components of those pairs. Then, if you run through the calculation that -1 times -1 is 1, following these definitional lines, you see, “Yes – so represented – -1 times -1 equals 1.” However, this proof, ingenious as it may be, hardly prompts the “Aha!” sensation. After having read such a proof, few would exclaim, “Wow! Now I see why -1 times -1 equals 1.” Rather, Dedekind’s calculation serves the narrowly proximal purpose of showing that the pairwise definition of integers and the primitive recursive definition of multiplication for those integers suffice for deriving a certain elementary fact about the integers from similarly elementary facts governing natural numbers. Ultimately, and much more importantly, it helps anoint and crown the Dedekindian vision of natural numbers in terms of calculation by recursion – shocking as it may have seemed originally – as foundational and deeply so. (Do we want to say that, thanks to proofs like this, the Dedekindian, calculative natural numbers won out over the old-time natural numbers, those tied to numeration and mensuration?)

A more explanatory proof of this simple result of integer arithmetic may be a manner of kinesthetic (I would say) demonstration in which you exploit the fact that multiplication by -1 takes hold of the ordinary number line, and swings it through 180 degrees, so bringing it back onto itself – but with positive and negative half-rays reversed. Then, it is patently obvious that multiplying by -1 twice gives you 1 in return. In multiplying by -1 two times, all you do is spin the number line around once, and then one more time to undo the reversal. This kind of proof might well produce the "Aha!" sensation – "Oh, yes! Okay, that's why it works." This reminds us that the word 'demonstration' shares 90% of its etymological DNA with both 'monstrance' and 'monster,' so joining it to 'display' and 'outlandish.'

I would not wish the contrastive characters of the two proofs just mentioned, the calculative and the kinesthetic, to suggest to readers that I am conceiving a contemporary felt distinction between explanatory and non-explanatory proofs either as just another battle in the ancient war between algebra and geometry or as a further guerrilla skirmish fought between the competing mathematical tribes Felix Klein once labeled 'intuitionists' and 'formalists.' Such historical antecedents that today's questions regarding mathematical explanations may boast are a matter for detailed, future investigation (Klein, 1911).

Do repeated questions about proofs as explanations mask issues more important to the foundations of mathematics, once that subject is freed from subjugation to philosophy of mathematics? The foundational issue I have in mind treats of 'proof-cores,' their existence and explication. What are proof-cores? From time to time, they have been called 'proof-constructions' or 'proof-ideas,' even 'calculi.' (Wittgenstein used 'Kalkül' in notebooks that he kept after returning to Cambridge in 1929. Then, he started – we would now say – to extract from discursive, inductive proofs recursive procedures as proof-cores. At that time, he had been poring over [Skolem, 1923]). Speaking metaphorically but with a hope for accuracy, I say that a proof-core is the dynamical engine or principle of a proof such that, once you have a proof-core fully to hand, you truly understand the mathematics of the proof, which includes, but is hardly exhausted by, any corollaries derivable from it. The dynamical, engine analogy is apposite: the word *dynamis* (featuring at least 120 times in the Greek New Testament) denotes a great power, verging on the miraculous, for performing marvelous feats of demonstration.

I hasten to remark that I do not assimilate matters relating to proof-cores, their existence and isolation, automatically to the intellectually distinct matters surrounding explanations. *A fortiori* I do not identify – in advance of much-needed investigation – a proof-core with that feature, if anything, that makes a proof explanatory. In principle, the potential reach of a proof-core is global (as the following example suggests) passing far beyond the formally deductive bounds on any single theory. A proof-core need not unify – to employ current jargon – but it does reveal and showcase a widespread mathematical phenomenon such as deductive incompleteness.

For example, take Gödel's published proof (1931) of his First Incompleteness Theorem. What lies at its core? A linked pair of great breakthroughs: the Arithmetic Fixed-Point Theorem and the Numeralwise Definability of Computable Functions. Both these 'Theorem' titles grant mathematical credentials to interlaced methods, not to isolated statements merely. Once you grasp them as dual proofcores, you can – for one thing – start generating incompleteness results for yourself, you can apply these results to other systems or theories, you can see how to extend them to arithmetized predicates other than 'is not provable.' The little gem by Tarski, Mostowski, and Robinson, *Undecidable theories* (1953) exemplify these processes of extracting, generating, applying, and extending the original Incompleteness Theorem.

There are many more examples in logic. What is at the proof-core of a pedestrian completeness theorem? It may be the processes lodged at the intellectual heart of the Prime Ideal Theorem or the Ultrafilter Extension Theorem: every nontrivial filter in a boolean algebra extends to an ultrafilter. (In intuitionism, this is the validity of the Law of Testability.) As you recognize this idea to be the dynamics behind many completeness theorems, you can start milling out completeness theorems more or less easily for domains other than – for example – conventional first-order logic. Also, you see and can assess those fascinating circumstances in which you cannot make completeness theorems go through, such as a fairly standard completeness theorem for intuitionistic formal logic within a strictly intuitionistic metatheory.

Now, how to isolate and characterize proof-cores? Here, we are talking about constructions in an extended sense of the term (perhaps 'nonconstructive constructions'?) or algorithms in a similarly extended sense. They are not algorithms à la Turing-Church-Gödel, that is, not general recursive procedures. There are times when the application of a proof-core requires great ingenuity and creativity – even good luck. In general, the business is in no way automatic or programmable and may take decades of hard mathematical work, by a squad of mathematicians, to work out plainly. So, grasping a proof-core is having to hand a procedure akin to an algorithm for reproducing a proof (not copying its syntax down blindly), adapting it to new circumstances, applying it more widely, thereby seeing hitherto invisible links among disparate topics.

There is a proof-core to the usual proofs of the Bolzano-Weierstrass Theorem – that among real numbers in a closed, bounded interval, if an infinite number have been selected, then there is an accumulation point. The operative proof-core might be represented by a standard proof, via 'divide and choose,' of König's Lemma. By such means, if I am looking up through an unbounded binary tree, then I can 'trace out' an infinite path running through it. As Kleene proved in *Recursive functions and in-*

tuitionistic mathematics (1952), this manner of tracing out may not be general recursive, even were the original tree primitive recursive.

What kinds of generalized algorithms are proof-cores? Could we have a fixed notation for representing proof-cores? Can we classify proof-cores? In reply, I put two suggestions forward: interested scholars should move away from the philosophy of mathematics to engage with the foundations of mathematics – an area in which we employ all the mathematical tools at our disposal to solve the problems of mathematics and its underpinnings delivered to us by philosophy – rather than pursuing approaches to those questions via strictly dialectical or discursive means. The questions about proof-cores are questions in the foundations of mathematics. Second, I recommend the extraction and regimentation of proof-cores. Close attention to the reverse mathematics of Simpson (1999) may yield more concrete and detailed ideas on how to achieve this.

PU: You have mentioned Gödel's Incompleteness Theorems and, in this context, I would like to ask you, "Are there any limits to mathematical explanation? Can we find places in mathematical phenomena that are so complicated in their natures that we would not be able to explain them?"

CM: That is an interesting question, unless that interest bespeaks an unhealthy fascination in predicting the future. If the area that I sketched out above constitutes a genuine and fruitful approach to understanding what mathematics is via the extraction of proof-cores, if the generalized notion of algorithm needed for that is bounded above and permanently in true complexity, then the answer to your question should be "Yes." Of course, when I say 'bounded above in true complexity,' I mean bounded in a complexity measure akin to but not identical with familiar arithmetical complexity, analytical complexity, or levels of set-theoretic definability. The relevant complexity measure cannot be the same as any of these since, for example, arithmetical complexity does not coincide at all with intelligibility or ease of understanding. The truth predicate for arithmetic, which is non-arithmetic, is likely to be graspable far more readily than any arithmetic set that is complete $\Pi^0_{3,946}$.

If the generalized algorithms that are proof-cores are bounded in true complexity in a meaningful fashion, there will be mathematical phenomena that will never be open to exploration by proof; we will never exert that measure of epistemic control over what goes on in those complex mathematical areas. Gödel seemed to believe (Gödel, 1961, p. 385) that there is no limit of this sort at all. To Gödel's thinking, set theorists are going to explore ever further reaches of the cumulative hierarchy and, as the explorations proceed, their mathematical capacities, which are potentially infinite, will continue to expand without bound. Hence, Gödel's answer to your question about limits would presumably be, "No." However, I do not wish to overlook two alternative possibilities that are yet to be mentioned: first, that some proofs might be 'one-offs,' having no discernible cores in my sense. In that case, the reach of those proofs may extend beyond that of proof-cores. Second, there may be methods leading to mathematical knowledge that are not proofs as currently conceived. As an intuitionist, I cannot, of course, insist right now that the correct answer to your question about limits is, "Either 'Yes' or 'No'."

PU: Why?

CM: The Law of Excluded Third is invalid.

PU: But Hilbert said that we will eventually know everything in mathematics – that there is no "Ignorabimus" in mathematics.

CM: He did indeed say that. In fact, he did more than say it. He wanted to shout it from the rooftops. He did so, in effect, in his 1900 Problems Lecture (Hilbert, 1902) as well as in his last lecture at Königsberg (1930). The latter he closed by referring to the ridiculous or foolish ['töricht' in his German] "Ignorabimus;" he denied flatly that there is "Ignorabimus" in mathematics: "Es gibt kein 'Ignorabimus.'" It seems to me that Hilbert's utterance was among the last manifestations of an optimistic epistemic attitude to pure mathematics that he championed, an attitude more common in the 19th Century. (Study of the writings of 19th-Century mathematician/philosopher Paul du Bois-Reymond, such as his (1882), reveals that it was hardly universal.) On Hilbert's view, there are no permitted limits to our pure mathematical cognition; the reach of such mathematical knowledge will go on extending and extending so that any statable mathematical problem will receive a clearly accessible mathematical solution (which can, under certain circumstances, take the form of an impossibility result), but maybe only after a long time. Hilbert would be more than surprised were he to come back today and discover that we now live in an age thoroughly skeptical about logic and mathematics, with much more talk, both informed and otherwise, about barriers to mathematical knowledge, limits on mathematical cognition, bounds on our intellectual abilities. A lot of that talk is just silly, to be cast adrift in the same boat as such solecisms as 'Human minds are finite.' This skepticism and its succubus pessimism are not called up among us entirely by popular scribbling about Gödel's Incompleteness Theorems and the Unsolvability of the *Entscheidungsproblem*. We live in a long-term bull market for superstition, anti-intellectualism, lapsed confidence in rational powers, crippling self-doubt.

A more thorough examination of the gulf between potential and actual infinity will allow us to grasp more plainly and assess more honestly the true extent of our mathematical gifts. Human cognition in mathematics expands (and contracts) along paths that are potentially rather than either recursively or abstractly infinite. The extensional course of mathematical knowledge traced by its change cannot be a general recursive path. When it comes to human goings-on or thoughts or theories, the relevant infinity often exhibits a potential or modal character. More specifically, given any theorem in an ordinary logical calculus, it is possible that I can produce yet a further, new, and more complicated theorem in the same calculus. It hardly follows from this that I might produce a strictly infinite number of theorems or that it is ‘in my competence’ to do so. The ‘possibly,’ ‘may,’ and ‘might’ at work at this point are too often cast out of discussion by scholars when they pontificate about intellectual capacities. Famously, in his *Language and mind* (1968), Chomsky offers one version of a fallacious modal argument for the conclusion that human linguistic capacity is strictly infinite. He reasons invalidly from such a premise as, “For every sentence we grasp, it is possible for us to recognize as grammatical another longer sentence whose grammatical structure we can also grasp,” to the conclusion, “There is a strictly infinite number of distinct sentences all of which we can recognize as grammatical and assess for grammaticality.” Somehow, the ‘it is possible’ in the premise disappears into the gap between premise and conclusion. You find a kindred error in Dedekind’s argument for the existence of actually infinite systems in his (1888). In effect, Dedekind argues, “For every thought I entertain, I can produce and entertain yet another more complicated and different thought. Also, I have what you might call a null thought that is not an output of this facility for producing yet more thoughts. Hence, I have an infinity of thoughts at my disposal.” Again, Dedekind seems to have forgotten about the ‘can’ within his main premise. I insist that the ‘can’ remain. We must study sets that are defined either modally or intuitionistically: modally – talking about possibilities – or intuitionistically – by exploiting double negations. As you know, double negation is not cancellable intuitionistically. One can pursue a detailed mathematical investigation of either kind of sets. There are clear relations between them given by the Gödel theorem, and its extensions, governing translations between modal logics like S4 and corresponding intuitionistic calculi (Gödel, 1933).

PU: Could you tell us what is the difference between proofs in intuitionistic and classical mathematics?

CM: I am not sure that – in and of themselves – there need be much difference apart from the obvious and superficial. First, intuitionists do not count as valid various inferences mistakenly deemed correct by classical mathematicians. Second, intuitionists who follow Brouwer employ mathematical principles that conventional mathematicians take to be false. If you look at standard articulations of intuitionistic proofs, if you were to write down – in first-order logic, say – the steps in an intuitionistic proof, that is, just the steps in the usual, formalized way, you see that the passages from one statement to the next will all be acceptable to the classical mathematician. What may prove unacceptable to that mathematician is the initial assumptions on which many intuitionistic proofs depend. In part, intuitionism is characterized by such assumptions. These the intuitionist finds intuitive and in future may be able to prove, but are rejected roundly by classical mathematicians. They are principles such as the intuitionistic form of Church's Thesis – every total natural number function into the natural numbers is a general recursive function. They are principles such as the Principle of Uniformity, that is, in any power set, if we label its elements using natural numbers, there must be some number that labels all the elements. They are principles such as Brouwer's Principle for numbers: that every total function from Baire space into itself is continuous.

I want to emphasize that there are axioms of set theory and number theory, analysis and algebra that the classical mathematician and the intuitionistic mathematician will agree about completely: agree about their meanings, agree about their truth, agree about many of their consequences. They include the statements that any set of sets has a union, any set of sets has an intersection, that, given any class function restricted to a set, there will be a set containing all its outputs (the Axiom of Collection). There is an infinite collection, there is an empty set, the Russell class is not a set. All these are points of firm agreement between an intuitionistic set theorist and her classical colleagues, and form the bread-and-butter of the Bishopstyle constructivist (Bishop, 1967).

However, this agreement does not mean that – and these are important, too – our most treasured self-descriptions are going to be the same or even so completely understandable to one another that we can sympathize about mathematics. One uses the word 'understand' in many ways. You can say, "Ah, I finally understand what Joe's doing" to mean "Ah, now I can see what's motivating him. I can show some sympathy for his efforts." That is the sort of understanding that I mean here. Classical mathematicians who are platonists often describe their mathematics or the doing of their mathematics in ways with which I cannot sympathize. They exclaim, "Oh, mathematics is the recording of the details of a rock-hard, crystalline, clear, beautiful domain that is far away somewhere. I'm scanning it through my noetic telescope, and recording what I see of the flora and fauna," as in Hardy (1940). Gosh, that does not sound to me like a fun mathematics, I have to tell you; I guess I am not much of an astronomer or botanist. To me, this astronomical vision of the mathematical enterprise does not seem worth the candle. Doing intuitionistic mathematics is not well compared with looking; it is far more like full, conscious, bodily activity. Robin Collingwood, in his *Principles of art* (1938), described the creative work of the artist as "an imaginary experience of total activity." This powerful thought about arts like painting applies equally well to creative mathematics.

When you describe intuitionistic mathematics in ways by which I can recognize it as my own art, ways in which I might sympathize, it seems more akin to sculpting than to astronomy. In mathematics, I work through a field with my mental fingers, as if I were a blind man playing with clay. I touch a mathematical substance much closer, more tangible, and plastic than some sort of faraway crystal. Its stuff is malleable and bendable, responding more readily to my will. The strong impression of malleability may be due to the fact, insufficiently emphasized, that intuitionistic mathematics is the mathematics of a far greater range of mathematical circumstances than is classical mathematics. The intuitionist deals happily with circumstances that simply do not exist classically. One can prove classically (or better, think to prove) that they do not exist. For instance, in classical mathematics, except for singleton sets, there are no sets that stand in one-to-one correspondence with their full function spaces. In other words, the only standard, classical models of Church's untyped lambda calculus or, if you will, of the von Neumann notion of computing, are singleton sets. Intuitionistically, there is no problem in producing standard models of the type-free calculus of varied sizes. So, the classical mathematician is obliged to made do here with a strictly second-best, with a relatively ponderous mock-up of these situations, where you add topological values or you restrict yourself by considering, not arbitrary functions, but Scott-continuous functions only. That is is perfectly fine in that it is consistent with classical set theory, but it is a register of the sad fact that true models are, in this important case, unavailable even to the extra-terrestrial telescopes of the conventional mathematicians.

PU: But there are also some classical axioms and theorems that are unacceptable for intuitionists, for example, the full Axiom of Choice.

CM: Yes, they are unacceptable because they are demonstrably false. The full Axiom of Choice implies the Law of Excluded Third, as Scott and Diaconescu proved. (See Beeson, 1985, p. 163). Therefore, the full Axiom of Choice is an antitheorem in Brouwer's intuitionistic mathematics.

This does not mean that an intuitionist cannot apply certain restricted axioms of choice. You might have an axiom of choice over the natural numbers, for example. If I have a collection of inhabited sets indexed by natural numbers – let us imagine them as sacks filled with elements, one hanging above each natural number – then such a restricted axiom tells us that there is a function on the natural numbers that selects an element out of each one of those sacks. There are other possible restrictions. For instance, intuitionists can consistently adopt the Presentation Axiom of Choice. The full Axiom of Choice says, "Every inhabited set of inhabited sets has a choice function." The Presentation Axiom of Choice – due to Peter Aczel (1978) – says, "Give me any set X, I can find a set Y of which X is a quotient such that an Axiom of Choice holds when restricted to Y." Another way of putting it is to assert that every set can be covered with a type, if you think of types as sets over which a suitably restricted axiom of choice holds. This axiom holds in intuitionistic circumstances, such as the Kleene realizability universe, where the full Axiom of Choice patently does not.

PU: Thank you very much.

CM: You are most welcome.

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