A Mathematician's View on Mathematical Creation

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In this paper we present a few personal views on aspects of mathematical creation, illustrated with some examples.

1. Discovery and Invention

There is a long debate on whether mathematics is discovered by humanity, as something existing beyond the concrete human mathematicians, in a Platonic view, or if it is something invented by these mathematicians, even if inspired by the physical world around them or the state of mathematical research at the time. The generally accepted view is the Platonic one, as mathematical results seem to endure throughout time, with the same statements and the same proofs, even though the mathematical work is often described as a creative one. We now take a look at both these elements in mathematics: statements of theorems and proofs.

Once a theorem is proved, it is clear that the result is established once and for all, and cannot be disproved, provided the proof is carefully read and scrutinized. The statement itself, however, can be more contingent than it looks like at first glance. Take, as an example, the famous Pythagorean theorem. One can say that the result is unavoidable, or necessary, but it does depend on the definition of a right triangle, which might leave more room for imagination that it might look at first sight. At the time of Euclid, of course, this definition was clear; however, after the establishment of the geometry on the sphere and of hyperbolic geometry, this concept became broader, as to include right triangles for which the Pythagorean theorem is no longer true. Einstein himself refers to this fact in very clear words, in a conversation with Rabindranath Tagore [1]:

> I believe, for instance, that the Pythagorean theorem in geometry states something that is approximately true, independent of the existence of man.

A similar situation arises with another very basic mathematical result: the existence and uniqueness of prime factorization for integer numbers. Again, if we consider the usual integers, the result is true. It remains true even for Gaussian integers (the set of complex numbers with integer real and imaginary parts); however, it is not true for some other sets of "integers" inside the complex numbers: for instance, the complex numbers of the form $a + b\sqrt{5}$, with a and b integers.

Even though these are not as familiar to most people as the usual integers, there are very good reasons for these numbers to be considered "integers", and to expect them to have a behavior similar to that of ordinary integers. There is even a rather dramatic episode related to this issue, a mistake made by Lamé in an attempt to prove Fermat's last theorem. Lamé presented a proof of Fermat's famous conjecture, which depended on uniqueness of factorization in sets of numbers such as the one we mentioned above, which is not true. This fact was brought to light by Liouville just after Lamé's talk, and the proof stood just a partial one.

One may object that the first definitions of "triangle" and "prime number" are more natural that the others presented here. This may be true at first glance, but, eventually, these non-intuitive objects end up having a more important role than the other ones. This was the case with hyperbolic geometry, which gained a more important role with the development of Riemannian geometry and relativity. Another interesting example of this phenomenon is the definition of a real valued function. At first, only continuous functions were considered as an object of study. Gradually, the concept became broader, as to include functions that were previously considered abhorrent, such as the Dirichlet function, which has value 1 on the rational numbers and value 0 on the irrationals. There is no hope of drawing a graph of this function, given the density of both these sets inside the real numbers. We'll come back to this concept in a little while.

So, we conclude that even though the results are necessary, the mathematical objects about which we speak can vary greatly, and this will of course influence the theorems one can establish about them. This choice of objects is thus an element of mathematical creativity, guided in part by the applications of the mathematics in question.

Another place for creativity in mathematics is, of course, proofs. It well known that one can find many proofs for the same result, and they can be quite different from one another. As an example, consider again the Pythagorean theorem. The proof given by Euclid in the *Elements* is based on Figure 1, sometimes called “the bride’s chair”, “the peacock tail” or “the windmill”.

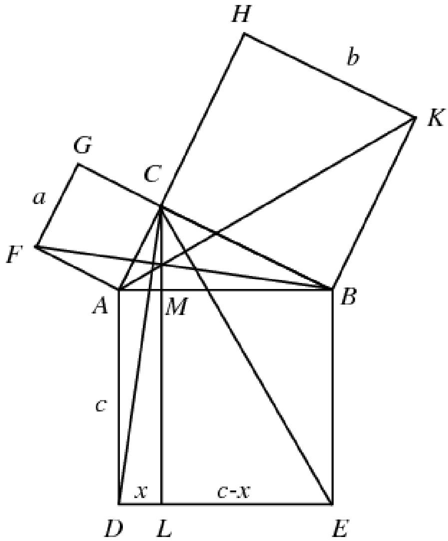

Figure 1. Illustration for Euclid’s proof of the Pythagorean theorem

It relies on comparing the areas of the three squares, by decomposing them into triangles. These triangles start as halves of the small squares, then they slide along some straight lines, preserving area, and finally end up as halves of the rectangles that comprise the large square. It is not a very simple proof — in fact, it is said that Schopenhauer called it “a brilliant piece of perversity”. Two other proofs, given by the Indian mathematician Bhaskara, are illustrated in Figure 2.

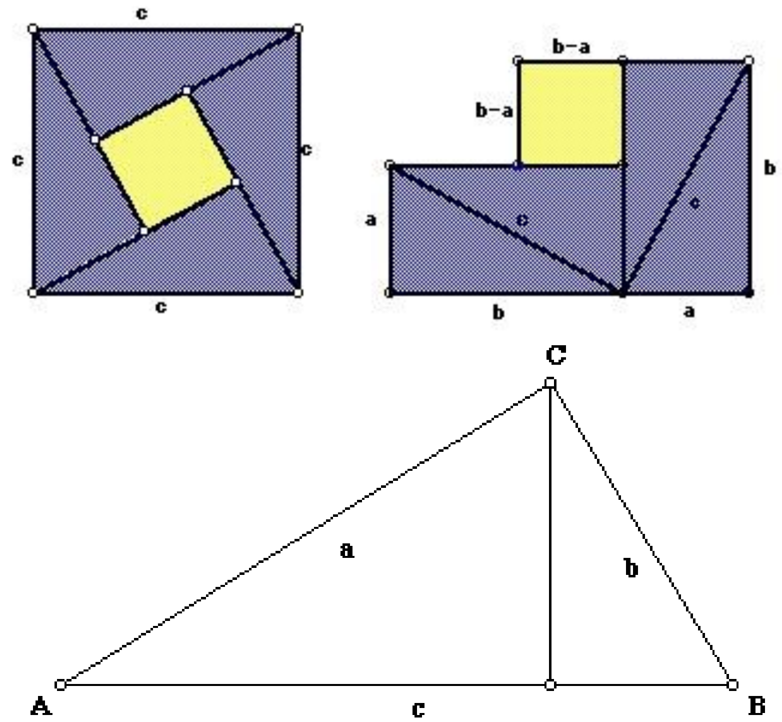

Figure 2. Illustrations for Bhaskara's proofs of the Pythagorean theorem

The first two images illustrate a proof, which is arguably the simplest one, also based on area decompositions. It is quite straightforward to deduce the argument just by looking at them. The second proof, illustrated by the large triangle decomposed into two smaller ones, depends on properties of similar triangles, and it no longer involves considerations about areas. The three triangles shown are similar and by comparing the lengths of corresponding sides, we end up proving the theorem.

These are only three out of hundreds of proofs of this result. This abundance of possibilities points to the element of creativity that exists in finding and producing a proof, probably influenced by both the individual that produces the proof and the culture this person is immersed in. The fact that this result has many possibilities of application also helps explaining this abundance.

To conclude, we could say that, even though there are rules to be followed when proving a given result, these cannot account for all the diversity we find.

2. Rigor and Intuition

Speaking of proofs, the usual understanding about mathematics is that a certain result can only be considered as established once a proof is given. This is an accurate view: no one considered Fermat's last theorem as a theorem before Wiles' work, and no one considers that the Riemann conjecture about the zeta function is true at the time this text is written, even though there is a significant list of mathematical results dependent on this conjecture being true. However, there's more to mathematical certainty than rigorous proof and there's more to proof than mathematical certainty.

To illustrate my first statement, consider Fourier analysis. The statement that a function will coincide with its Fourier series depended, at first, on the functions considered. This was proved to be true for periodic functions with a known formula (d'Alembert and Euler, 18th century), and then Fourier (beginning of the 19th century) ventured to state that the result would hold for a larger class of functions, giving a formula for calculating the coefficients of the series. The theory lacked rigor even for the standards of the time, but nevertheless Fourier's theory won the Grand Prix de l'Académie des Sciences, with a jury that included Legendre, Laplace and Lagrange. The very statement issued by the jury confirms this situation:

> [T]he manner in which the author arrives at these equations is not exempt of difficulties and... his analysis to integrate them still leaves something to be desired on the score of generality and even rigor.

Only with the definition of the Lebesgue integral, at the beginning of the 20th century, did the theory become completely clear. It was finally proved by Carlson, in 1966, that if a function is Lebesgue square-integrable then its Fourier series converges almost everywhere. Nevertheless, the absence of this final result didn't keep engineers, physicists, and even mathematicians from using the Fourier series as a tool.

Another concept that was used way before a reasonable rigorous definition was given was that of an infinitesimal. Newton and Leibniz used this concept when developing the infinitesimal calculus, again facing criticism in their own time. One of the most famous critics of this lack of rigor was George Berkley who wrote the famous sentence:

May we not call them the Ghosts of departed Quantities?

Calculus was of course used since it was established, but it was only in the 19th century, with Cauchy, Bolzano and Weierstrass, that the notion of limit was rigorously defined. Interestingly enough, this definition did away with the notion of infinitesimal as a quantity, defining it in terms of sequences or neighborhoods. However, in the 20th century, the notion of infinitesimal as a number was given a rigorous definition, first by Robinson and then by Nelson, who actually managed to define them as real numbers.

Even nowadays, mathematicians and physicists will use concepts that are still not completely established, such as the Feynman integral. To this day it was impossible to find a measure affording this integral.

On the other hand, even when there is a rigorous definition of a concept or a proof of a result, mathematicians will still look for alternative ways to establish the result. It is very frequent that the first proof of a hard result is very long and elaborate, and new proofs are welcome. There's more than a need for certainty involved in a proof: mathematicians look also for understanding. As Gian-Carlo Rota puts it in [2]:

> This gradual bringing out of the significance of a new discovery takes the appearance of a succession of proofs, each one simpler than the preceding. New and simpler versions of a theorem will stop appearing when the facts are finally understood.

Bill Thurston also states this very clearly in [6]:

> What we are doing is finding ways for people to understand and think about mathematics. The rapid advance of computers has helped dramatize this point, because computers and people are very different. For instance, when Appel and Haken completed a proof of the 4-color map theorem using a massive automatic computation, it evoked much controversy. I interpret the controversy as having little to do with doubt people had as to the veracity of the theorem or the correctness of the proof. Rather, it reflected a continuing desire for human understanding of a proof, in addition to knowledge that the theorem is true.

Thus, for a mathematician, a proof encompasses not just the logical certainty of a result, but also, and maybe more significantly, the deeper understanding of *why* the result is true, even though the question of what this "why" means cannot be formulated in a clear mathematical way.

3. The individual and the collective

The usual view on the development of mathematical tends to underline the effort of individual people, who made significant progress in the advancement of mathematical knowledge and understanding. Mark Kac, in [4], offers an interesting quote on brilliant scientists:

> In science, as well as in other fields of human endeavor, there are two kinds of geniuses: the “ordinary” and the “magicians.” An ordinary genius is a fellow that you and I would be just as good as, if we were only many times better. There is no mystery as to how his mind works. Once we understand what he has done, we feel certain that we, too, could have done it. It is different with the magicians. They are, to use mathematical jargon, in the orthogonal complement of where we are and the working of their minds is for all intents and purposes incomprehensible. Even after we understand what they have done, the process by which they have done it is completely dark. They seldom, if ever, have students because they cannot be emulated and it must be terribly frustrating for a brilliant young mind to cope with the mysterious ways in which the magician’s mind works. Richard Feynman is a magician of the highest caliber. Hans Bethe, whom [Freeman] Dyson considers to be his teacher, is an “ordinary genius.”

One could easily carry these definitions to the field of mathematics — Terence Tao would be a good candidate for a magician. However, in spite of the colorfulness of the description, it is undeniable that the body of existing mathematical results, and the applications of these results, influence the discovery of new ones, and even the proofs of these new results. It is the case, quite frequently, that more than one mathematician arrives at a given result independently and simultaneously.

In the Introduction of [3], John Gribbin makes this point very clearly — he does so in describing scientific discovery, but we believe it can be also applied to mathematical developments.

> It is natural to describe key events in terms of the work of individuals who made a mark in science […]. But this does not mean that science has progressed as a result of the work of a string of irreplaceable geniuses possessed of a special insight into how the world works. Geniuses maybe (though not always); but irreplaceable certainly not. Scientific progress builds step by step […], when the time is ripe, two or more individuals may make the next step independently of one another. It is the luck of the draw, or historical accident, whose name gets remembered as the discoverer of a new phenomenon.

The case of the establishment of infinitesimal calculus by both Newton and Leibniz is a very known example. The fact that both were very gifted mathematicians is certainly important, but the fact that both created the theory at the same time is a sign that the body of mathematical knowledge was ready to welcome the new theory. Newton’s famous quote attests to this:

If I have seen further it is by standing on ye sholders of Giants.

Another famous example of this phenomenon is the discovery of hyperbolic geometry. Farkas Bolyai, in spite of much effort, was unable to find a model proving the existence of such geometry. However, his son Janos

Bolyai (much against his father's advice) managed to succeed, at the same time as Lobachevsky, who worked on the subject independently. It was maybe Gauss's towering influence that made it possible for both mathematicians to succeed (Gauss himself had thought about the subject, even though he hadn't published anything).

So, even though the individual effort of brilliant minds cannot be erased, it is also important to notice that the state of mathematical knowledge at a given moment in a sense engenders the new results and developments.

With the latest possibilities in communication, afforded by the internet, a new type of mathematical collaboration became possible.

One of the most famous instances of this is the Polymath project, started by Tim Gowers. This is a site [8], where problems are stated and contributions are welcome. Gowers himself describes the project as follows:

> It seems to me that, at least in theory, a different model could work: different, that is, from the usual model of people working in isolation or collaborating with one or two others. Suppose one had a forum for the online discussion of a particular problem. The idea would be that anybody who had anything whatsoever to say about the problem could chip in. And the ethos of the forum — in whatever form it took — would be that comments would mostly be kept short. In other words, what you would not tend to do, at least if you wanted to keep within the spirit of things, is spend a month thinking hard about the problem and then come back and write ten pages about it. Rather, you would contribute ideas even if they were undeveloped and/or likely to be wrong.

The project stemmed from Gowers' blog [9], where he suggested that his readers contribute ideas towards finding a new proof of the Hales-Jewett theorem; and explicitly asking the question "is massively collaborative mathematics possible?". The problem became known as Polymath 1. Terence Tao also got involved, with people contributing suggestions his own blog [10], and finally, in 2009, the new proof was found and two papers were published under the pseudonym D. H. J. Polymath, one of them in the very respected Annals of Mathematics.

In the same spirit, there is another site called MathOverflow [7]. In this site, anyone can post a question on a mathematical research topic, and answers are given by other users. The site was started by Berkeley graduate students and postdocs A. Geraschenko, D. Zureick-Brown, and S. Morrison on 28 September 2009 (Terence Tao pointed out that the newsgroup *sci.math* was similar, even though MathOverflow has newer web features). According to Wikipedia, questions are answered an average of 3.9 hours after they are posted, and "Acceptable" answers take an average of 5.01 hours.

Again, the speed and breadth of this interchanging of information only became possible in the late 20th century with the Internet, and may add a distinctive new feature to the way mathematics is created. The coming decades will tell if this way of creating new mathematics will prove relevant or not.

Conclusions

Having in mind that it is quite difficult (and probably even dangerous) to expect final conclusions in subjects such as this one, I could summarize the ideas in this essay as follows:

— Even though mathematical statements seem to have an intrinsic immutable quality, a good deal of creativity is necessary in developing new mathematics;

— Even though mathematical rigor is necessary in stating mathematical results, it is equally important to pay attention to partially established results and to the understanding of mathematics that a proof of a theorem brings

— Even though most mathematical results can be attributed to the work of brilliant individuals, it is also important to pay attention to the collective state of mathematics and to modest contributions when analyzing mathematical progress.

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