Non-Formal Properties of Real Mathematical Proofs

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1. Introduction

Suppose you attend a seminar where a mathematician presents a proof to some of his colleagues. Suppose further that what he is proving is an important mathematical statement. Now the following happens: as the mathematician proceeds, his audience is amazed at first, then becomes angry and finally ends up disturbing the lecture (some walk out, some laugh, ...). If in addition, you see that the proof he is presenting is formally speaking (nearly) correct, would you say you are witnessing an extraordinary event in urgent need of explanation? Surely, your answer would be yes. But do events of this type actually occur? The matter of the fact is, yes, they do. This paper presents the details of such an event, suggests a possible explanation and examines its implications for our understanding of mathematical practice.

2. Some Mathematical Details

The Riemann Zeta Function, **Z(s)** where s is a complex number, is an important function in mathematics. Its formal definition is

$$Z(s) = \sum 1/n^s$$

where the summation sign is over **n** from **1** to infinity. Its importance in many mathematical areas may be illustrated by some examples:

(1) **Z(s)** has a close connection with the distribution of prime numbers. If the real part of s, Res is larger than **1**, then the following identity holds:

$$\sum 1/n^s = \prod 1/(1 - 1/p^s)$$

The product on the right-hand-side is taken over all prime numbers larger than or equal to **2**. The special case s = **1** leads to a straightforward proof of the infinity of the set of prime numbers, as the harmonic series $\sum 1/n$ is divergent.

(2) One of the major unsolved problems in present-day mathematics is a conjecture concerning **Z(s)**, *viz.* the statement that all non-trivial zeros of **Z(s)** have real part **1/2**.

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Although this has not been proved, an enormous amount of evidence has been accumulated. A positive answer to the conjecture would solve an important set of problems in the core of number theory.

(3) Z(s) is an important function in the study of the rational/irrational character of real numbers; *e.g.*, Euler proved that

$$Z(2) = \pi^2/6$$

Thus if one could prove that either Z(2) or π^2 is an irrational number then the irrationality of the other number is thereby implied. Actually for even integers, the value of Z(2k) is well-known:

$$Z(2k) = (-1)^{k-1}(2\pi)^{2k}B_{2k}/(2\cdot(2k)!)$$

All of these values are in fact irrational. B_{2k} is the 2k-th Bernoulli number. These numbers correspond to the coefficients in the equation:

$$x/(e^x-1) = \sum B_k x^k/k!$$

Incidentally, these numbers also occur and play a vital role in another equally famous unsolved mathematical problem, *viz.* Fermat's Last Theorem.

Much less is known about the odd values. Are Z(3), Z(5),..., Z(2n+1), rational or irrational? The problem was known to Euler but neither Euler nor mathematicians after him managed to handle the problem. In June 1978, Roger Apéry, a French mathematician, presented a proof of the irrationality of Z(3) at the "Journées Arithmétiques de Marseille-Leminy." What happened during this presentation are the events reported in 1.

3. Explaining the Events: First Element

The comments on this remarkable event (Van der Poorten [1979], Mendès France [1979], Stewart [1987] focus on two important aspects of Apéry's proof. The first feature is that the proof methods used by Apéry are "old-fashioned." The title of van der Poorten's article is "A Proof that Euler Missed ..." and he states that :

> It [the title] arose after Cohen's report at Helsinki, with someone sourly commenting "A victory for the French peasant ..." [*i.e.* Apéry!]; to this Nick Katz retorted: "No ...! No! This is marvellous! It is something Euler could have done ..." (Van der Poorten 1979, p. 203)

and Michel Mendès France makes the even bolder claim:

> ... the methods of R. Apéry are those of Euler. (Mendès France 1979, p. 170)

Actually, he also offers an explanation. One does not try to solve a problem with the same methods the "old masters" have used. If you do, then surely you are claiming that you are at least as good, if not better than the great. If on top of that the mathematician who makes the claim is not so young (Apéry was then about sixty years old), one does not expect any interesting new results at all. Needless to say that many studies in the sociology of science and mathematics confirm this idea. Therefore, the general opinion was that the proof must contain mistakes. Add to this, a rather informal presentation leaving room for doubt and disbelief and the stage is set. No doubt, this is a very reasonable explanation. After all, no mathematician today would believe that an elementary proof of Fermat's Last Theorem is likely to be found.

In Lakatosian style, I want to draw attention to a hidden assumption in the above explanation. If the "old masters" have not found the proof, it must be quite simply because there is no proof using these methods. Otherwise, surely, they would have found it. In other words, what is assumed here, is that at a certain stage in the history of a mathematical problem, methods that have been selected to solve the problem, can actually be *used up*. If this notion makes any sense, one would at least expect the following: a "hard" problem will during its history, wander through different mathematical domains. This is certainly the case for Riemann's Zeta Function. Originally a problem in number theory, it became a problem in analytical number theory (*i.e.* that part of number theory where analysis is an essential tool) into complex function theory and its related areas. For Fermat's Last Theorem and many other problems, a similar story can be told.[1] From this point of view, Apéry's proof showed that sometimes this strategy may fail. Furthermore, if the problem is an important one, the failure is intensified. Hence the outrage at the presentation.

Of course, the idea that mathematicians use strategies and heuristics to guide their research, is hardly new, hardly surprising. However, this particular strategy has the intriguing property that it is largely *problem-independent*, that is, as to the *content* of the problem. A possible formulation of the strategy could be this:

(S1) If many highly qualified mathematicians have unsuccessfully tried to solve a problem with the methods of a particular mathematical domain, then it is very unlikely that the problem has a solution in that domain.

Direct positive evidence as to the qualification of the mathematicians involved, includes the presence of first-rank, "famous" mathematicians. As one can see, the strategy does not mention anything about the nature of the problem, apart from the fact that it resists being solved. This strategy also suggests why it is important that mathematicians should know something about the quality of the work of their colleagues. An efficient implementation of (S1) implies one should have a rather good picture of the quality levels of the mathematical community. In this sense too it is very different form known strategies and heuristics used in mathematics as it involves a *social* element. Now from this point of view Apéry's proof is also a challenge to this accepted social image of the mathematical community. If this line of reasoning is correct, what the Apéry case clearly shows is that in some cases a strategy or heuristic is interpreted as or is taken to be a virtual certainty.

4. Explaining the Events: Second element

The second feature that emerges from the reports and the comments mentioned, concerns the proof itself. Van der Poorten states:

> Apéry's incredible proof appears to be a mixture of miracles and mysteries. The dominating question is how to generalise all this. (Van der Poorten 1979, p. 201)

and

> ... much of what has been presented constitutes a mystification rather than an explanation. (idem)

Mendès France uses the very same term: "*miracles mathématiques,*" and also mentions the lack of generalisation. Ian Stewart joins them by using such terms as "weird formulas" and "bizarre formulae."

As was said before, what is not in doubt is the formal correctness of the proof. Although at the presentation itself Apéry did not fill in all the gaps, this was done afterwards and van der Poorten's comments refer to this completed proof. Therefore such properties as being weird, being mysterious cannot have anything to do with the formal properties.

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How can a formula be called weird? An example of such a formula is the following. Consider the recursive relation defined by:

$$n^3 u_n = (34n^3 - 51n^2 + 27n - 5)u_{n-1} - (n-1)^3 u_{n-2}$$

Apéry claims the following: if one starts with $u_0 = 1$ and $u_1 = 5$, then all u_n are integers! This is indeed very surprising as each u_n is of the form A/n^3. Therefore the right-hand-side must be divisible by n^3. And that is an unexpected result. But what do mathematicians mean by "an unexpected result." This seems to suggest that somehow mathematicians have an idea of what the proof should look like. A result is unexpected if there is no indication as to how one could go about trying to prove it. I have now a rather strange notion: what is *an idea of a proof*? Surely it cannot be a proof in the formal sense of the word, for then the mathematician already has the answer and the distinction vanishes. It has to be something that bears a close resemblance to a proof and for such an entity I propose to use the term *proof-outline*. A proof-outline is best understood as a summary of a proof: it lists the essential steps without filling in the details. It is perfectly comparable to the high-level structure of a computer program.

The claim I want to make is this. If a proof has a simple proof-outline, then the quality of that proof is considered to be high by mathematicians. Conversely, if the proof has a highly complicated proof-outline, then the quality of the resulting proof is low.

The strategy corresponding to this claim, is easily formulated:

(S2) Prefer high quality proofs over low quality proofs.

There are at least two (rather) obvious reasons to prefer high quality proofs:

(1) They will enable mathematicians to find generalisations. Rather than presenting a formal description, let me give an example to illustrate this point. Consider one of the two famous proofs of the irrationality of square root of two:

Suppose that $a/b = \sqrt{2}$. Then $a^2 = 2 \cdot b^2$ must contain an even number of primes (every prime factor of a occurs twice). $2 \cdot b^2$ on the other hand contains an odd number of primes, because of the presence of the additional factor 2. But if uniqueness of prime decomposition holds, a product of an even number of primes can never equal a product of an odd number of primes. QED. This proof is so simple, that it is hard to make a distinction between the outline and the proof itself. From the proof/proof-outline above it is straightforward to see how it generalises to all cases where **p** is a prime number. The proof/proof-outline is easily adapted to show that **p**, where **p** is prime number, is irrational.

If a proof contains a set of curious relationships as the one mentioned in Apéry's proof, then obviously generalisation is quite impossible, a complaint expressed by van der Poorten and Stewart.

(2) The proof-outline often plays the part of an explanation.[2] Consider a famous example: the original proof of the four-color theorem. Part of the proof consists of the methodical-mechanical checking of a (large) finite set of highly complicated maps. What is required is for the computer to succeed in coloring them all. The two last sentences are a proof-outline for that part of the full proof. And really there is not that much more one could say about it. The details as to how the computer actually does the coloring, are hardly (within this context) interesting. That is all the explaining there is to do. Consider on the other hand, an equally famous example. I will present only the proof-outline and I dare make the following claim: anyone familiar with the concepts mentioned in the outline, will

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be able to fill in the details such as to obtain a full acceptable proof. The example is the well-known formula

$$e^{i\Pi} + 1 = 0$$

Proof-outline: Write down the Taylor-expansion for cos(x) and for sin(x). Multiply the last expansion with **i** (the imaginary unit). Add them together. What you find is the Taylor-expansion for e^{ix}. Now take the special case $x = \Pi$.

This outline is precisely what one encounters in mathematical texts when an attempt is made to explain the formula

$$e^{i\Pi} + 1 = 0.$$

A direct consequence of (S2) is that, even if you have found a proof of a statement, it makes sense to look for another proof, especially if the former one is of low quality. As it is, this phenomenon is clearly visible in mathematical practice. For many mathematicians the four-color theorem is not proved yet. What the existing "proof" tells us, is that very probably it is provable. The search for a "good" proof continues. No mathematician is satisfied with the "proof" of the classification of the finite groups, as it is many thousands pages long. Note that in this case however, it is generally accepted that a complete classification has been found. Thus many mathematicians are involved in the task of rewriting the proofs and the task is not considered trivial.[3] In Apéry's case too, the now "official" proof of the irrationality of **Z(3)** is quite different from the original one and one can clearly show that it is of higher quality.

5. A Lakatosian Note.

Strategies such as (S1) and (S2) are very different from the strategies and heuristics one finds *e.g.* in Lakatos's *Proofs and Refutations*. However they are not in contradiction. Euler's conjecture - the subject of Lakatos's book - has little to do with (S1) as there was a proof right from the start and has little to do with (S2) as the proof had a very simple proof-outline thus had a high quality. This does imply that a full picture of what mathematical practice is all about, will have to include at least[4] both sets of strategies.

Notes

[1]Fermat's Last Theorem started as a problem in number theory, then moved rapidly into complex number theory, and is now firmly embedded in algebraic topology, complex function theory and associated areas. Apparently mathematicians have great confidence that a solution is near. See my (1987) for more details.

[2]Perhaps the use of the term "explanation" is not particularly suited here. Let me emphasize I do not mean "explanation" in the sense it is used in the context of science and philosophy of science. But mathematicians themselves do use the word and frequently so. It appears often in connection with "providing insight," "clear," "convincing," etc. It is within that cluster of concepts, I want to defend the thesis that it makes sense to define the explanation of a proof as its proof-outline. For a recent discussion on this topic, see Resnik & Kushner (1987).

[3]See Gorenstein (1986). He speaks of "the *fundamental* task of constructing a shorter and more readily accessible "second generation" classification proof." (p. 2, my emphasis).

[4]See *e.g.* Franklin (1987) for some heuristics not mentioned here.

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