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Opinion piece

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Reshaping the metaphor of proof

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The simplistic view of Mathematics as a logical system of formal truths deduced from a limited set of axioms by a limited set of inference rules immediately shatters when confronted with the history of Mathematics, or current mathematical practice. To become useful, mathematical Philosophy should contemplate what Mathematics actually was, over centuries, and what it is now, rather than speculate what it should be according to the philosophical orthodoxy. The first dogma that must be completely revised is the idea of proof as a text, rather than what it is for mathematicians themselves: a process, a plethora of interwoven arguments, a multi-dimensional structure.

This article is part of the theme issue 'The notion of 'simple proof' - Hilbert's 24th problem'.

1. Introduction

In this article, I try to argue that a mathematical proof *considered as a text* proves nothing except the fact of the existence of proofs. Traditional proofs are not proofs in the logical sense, but rather road maps, consisting of a conceptual description of the main ideas and some key arguments. It is a fact that traditional proofs are—and always were!—full of errors and gaps of all sorts. At present, very few serious mathematical results can be completely formalized and, outside of very few cases, such a complete formalization would be simply an insupportable and unjustified misuse of resources.

At the current state of affairs, before the advent of an artificial intelligence that could fully match our natural stupidity, complete formalization of the full body of mathematical knowledge is neither possible, nor desirable. A formalized proof presented as a sequence of elementary steps can be a useful instrument. But it is a pendant to a traditional proof, rather

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than its substitute. In no way does it supercede or transcend a traditional proof. Quite the contrary, it is much longer, less convincing, less comprehensive, less surveyable, less educative, less robust and contains *substantially* less information, than a good traditional proof.

The objections raised against *long* traditional proofs are in many cases completely irrelevant and unfair. Long proofs can be more transparent and more reliable than short ones, and in many cases they are! It is not a matter of length, but of culture, structure and organization. Thus, the proof of Classification of Finite Simple Groups has a much higher degree of reliability than proofs of the majority of generally acknowledged classical results in the area of Topology, Analysis or Dynamical systems.

What should indeed be changed is the view of a traditional proof as a text. It is only organized as a text for historical reasons. Actually, for most professionals, a mathematical proof is not a sequence of steps, measured by length, but a multidimensional object. It always was in the moral sense, whereas Hilbert 24 and Voevodsky's program reveal at least two such new dimensions in the technical sense.

2. Qui dit Mathématiques, dit démonstration

The most striking hallmark of Mathematics is the presence of **proofs**. A mathematician, as such, is someone who can discover, invent or construct proofs. The treatise of Nicolas Bourbaki even *starts* with the following exalted declaration:

'Dépuis les Grécs, qui dit Mathématiques, dit démonstration; certains doutent même qu'il se trouve, en dehors des mathématiques, des démonstrations au sens précis et rigoureux que ce mot a reçu des Grecs et qu'on entend lui donner ici. On a le droit de dire que ce sens n'a pas varié, car ce qui était une démonstration pour Euclide en est toujours une à nos yeux.'

It is indeed a fact that for many centuries nothing outside of Mathematics and exact (=mathematical) Sciences could even remotely compete with the stringency of a mathematical proof, in terms of general credibility and power of persuasion. However, over the last decades, quite convincing arguments of a mathematical nature are encountered in Theoretical Computer Science and even in many areas of Theoretical Physics (such as Quantum Field Theory, Gravitation, Statistical Physics, String Theory, etc.), and also in some Engineering Sciences (such as Control Theory, Information Transmission or Cryptography). Of course, Bourbaki could justly raise an objection that all these fields are *Mathematisierung* = mathématization, activités mathématiques, rather than Science and Engineering in the traditional sense.

In the opposite direction, in Mathematics itself, with alarming frequency, one discusses the issue of the changing status of mathematical proof and our fading certainty in the reliability of mathematical results. Such censure and scepticism are most stridently, repeatedly and aggressively articulated in the following directions:

- Doubts as to the reliability of computer-aided proofs. The first such instance that resulted in a huge hype, and subsequent vehement discussion, was the initial solution of the four colour problem, that was later found to be fallacious. Later such similar doubts, sometimes in even more exaggerated forms, were expressed in connection with all other prominent results, whose proofs relied on extensive computer calculations and/or case analysis, including the remarkable Hales' solution of Kepler's problem.
- Doubts as to the reliability of particularly long and complicated proofs. The most notorious and disputed example of such a highly complex proof is of course the Classification of Finite Simple Groups, whose published proof amounts, according to the most conservative estimate, to some 15 000 pages. It is essentially the proof of the theorem itself, that further relies on scores of subsidiary facts on finite groups and their representations, algebraic groups, finite geometries, etc. However, the same question naturally arises in connection with any sufficiently advanced result, whose formidable proof takes a few hundred pages of tough, tightly knit and highly specialized arguments, that in turn rely on oodles of previous difficult results.

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In these circumstances, ignorant popularisers and journalists started to speak about 'loss of certainty' and even 'death of proof'. However, for the reasons I'm going to sketch in this article, I am inclined to believe that the status of all results mentioned above—and of their proofs!—is not significantly different from the status of deep mathematical results of previous centuries. In fact, I am ready to contend that mathematical proofs *never*—from the times of Greeks!—satisfied the declared standards. The claim of Bourbaki is utterly wrong in all of its parts, and, in particular, *none* of the arguments in the first (geometric) books of Euclid would stand as a proof 'à nos yeux'—the proof is in the eyes of the beholder! The *real* wonder of Greek Mathematics is that *some* of the proofs in the subsequent (arithmetic) books of Euclid still are proofs in our eyes, to such an extent that we are still telling them to our students!

On the other hand, unlike individual proofs, mathematical knowledge as such has *exceptionally* high reliability. Not just occasional mistakes, but even systematic delusions and fallacies never seriously endanger Mathematics as a whole, or any of its significant parts. This admirable reliability of Mathematics, like reliability of scientific and engineering knowledge, is ensured not by proofs of individual results, but by general coherence of the mathematical and scientific view of the world, its explaining and predictive power, and its ultimate relevance for *any* articulate thought and *any* viable application. On this particular point, I adhere to the traditional view, that the crux and fulcrum of Mathematics is the individual and collective *understanding*, cultivated by the prolonged exposure to the world of ideas = mathematical reality. Such infravision inevitably evolves in every mature and perceptive mathematician in the process of their work.

In particular, for many reasons, I am convinced that both the Classification of Finite Simple Groups and its proof have much higher credibility, as compared with the majority of accepted classical results of Topology, Analysis or the theory of Differential equations. This is connected exactly with the much higher degree of criticism and self-criticism in the theory of finite groups. These higher standards for proofs evolved in Algebra precisely because of the need to control exceptionally long and complicated proofs. And also because, by the very nature of the subject, before you put in the last dot, in many cases you cannot be certain whether you are proving a theorem, or constructing a counterexample. Last but not least, because these proofs used extensive computer computations which I trust more than any mathematical proofs, except the simplest ones. But, above all, the proof of the Classification *is completely written down*, which is not the case for the vast majority of other results of similar complexity.

3. All questions answered

All philosophers who are not mathematicians (i.e. virtually all philosophers, except very few, most notably Descartes and von Leibniz) show a complete misunderstanding of the essence of Mathematics[1]. Here is what philosophers and popularisers declare:

- *Proof is a formal text* written according to rigorously defined rules. Essentially, a sequence of elementary steps each of them consisting of applying inference rules to axioms and previous steps.
- It is sometimes difficult to find a proof; this process may require intuition and inventiveness, but **checking a proof is an entirely mechanical process** that can be delegated to low-skilled personnel, and, ultimately, to a computer.
- *Mathematics can be completely formalized*, that is reduced to deriving consequences of explicitly given axioms according to explicitly listed inference rules.

[1]One of the anonymous referees, Reinhard Kahle and Andrei Rodin all raised the objection that this sweeping generalization is as simplistic as the sweeping generalizations I chastise. Firstly, before the twentieth century, most philosophers never declared any such views in this form. Secondly, these dogmas themselves served as objectives of deep and substantial *technical* research, that contributed a lot to our understanding of logic and language. I fully agree with both points. The reason I do not even mention such things as *Unvollständigkeitssatz* or *Widerspruchsfreiheitsbeweis* and all subsequent development is not that I disregard or underrate them. It is simply that discussion of the foundational work at a technical level was not my intention here.

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- All of Mathematics, as we know it today, can be derived from a very small number of axioms, say from the 7–8–9 axioms of ZFC. What mathematicians do, is a game consisting of deriving the consequences of these axioms. A game not much different in spirit from chess.
- There exist universally accepted criteria of mathematical rigour common to all areas of Mathematics. After an error is discovered in a proof, either it can be easily corrected or a proof can be rejected as an incorrect or incomplete one.
- To understand and consciously use any mathematical result is possible only after its proof is fully understood. All results in all educational courses at any sufficiently advanced level should be accompanied by complete and detailed proofs.

I believe that such a simplistic propagandist picture is *infinitely* remote from reality. I would be extremely surprised if any practicing mathematician working outside mathematical logic and foundations, who has given some genuine thought to these matters, would seriously support *any* of these views traditional in Philosophy. And yet, it is exactly this grossly distorted picture of Mathematics that largely determines many practical issues, *including* its teaching at school, college and university levels!

4. All answers questioned

And here is what all professional mathematicians know about proofs but are afraid or unwilling to acknowledge.

- A proof *considered as a text* proves **nothing** except the fact that proofs exist—and, *hopefully* that a proof of the result that is claimed might exist. The texts (of any degree of detail!) as presented in the available mathematical literature are mere road maps. An actual proof is not a text at all, but a *process* consisting of reproducing all arguments and computations.
- A proof is a proof only up to the extent that it reflects a result of *understanding* and helps to reproduce and transmit this understanding. **A proof is only a proof for a person who can understand it** [1], and even for such persons only while they are able to keep in their memory both the main ideas and fragments of the general context, of the techniques used and of enough key details to control the remaining details.
- It was known to the ancient Chinese, some twenty-five centuries ago, that it is easier to write an incorrect proof than to understand a correct one. The only way to check a proof is to reproduce it, i.e., to completely repeat the underlying computation. **Checking the correctness of an existing proof is a creative process**, whose complexity is comparable to the complexity of the search for the proof. Actually, in many serious cases, it might be far easier for an expert (familiar with the basic ideas and techniques used) to find a new independent proof of the same fact than to verify the details of an existing proof.
- *Very few serious mathematical theorems have completely formalized proofs* or, for that matter, even such proofs that could be predictably completely formalized within the time, computational power and other resources accessible to the human race today.
- *Correctness of a proof does not belong to logic*, it is in the realm of Psychology and Sociology [2,3]. On the individual level, a proof is considered to be correct provided it convinces us of the validity of a result to the extent that we are prepared to persuade others. On the social level, a proof is considered to be correct provided it is accepted as correct by the majority of experts ('a proof becomes a proof after the social act of "accepting it as a proof",' [4]).
- Reliability of a proof is determined not by the absence of errors and gaps, but by a possibility to correct every error discovered in the text and answer every question arising in the process.

- Usefulness of a proof has nothing to do with the absence of errors therein. It emerges from the presence of new viewpoints, new concepts, new methods, new observations, new ideas, new tricks, new constructions, ...
- By default, mathematicians use as axioms *all* statements they ever encountered in trusted sources in the form they remember and understand these statements. Trusted sources are usually monographs and articles in the main ('refereed') mathematical journals. Owing to purely physical, physiological and psychological restrictions nobody can possibly check in detail proofs of all facts they use, and oftentimes even thoroughly ponder over their statements.

5. A pocket bestiary of mathematical mistakes

One evident offshoot of the above simplistic picture of Mathematics is that mathematicians are ashamed to acknowledge and discuss mistakes, not just their own mistakes, but even mistakes in the published work of others. They are especially reluctant to mention to their students that even the classics made scores of mistakes.

However, anyone acquainted with the history of Mathematics, and anyone familiar with the current state of affairs, is fully aware that mathematical works are swarming with mistakes of all possible sorts. Of course, one should distinguish these mistakes by how critical they are, how serious they are, and especially, by their aetiology.

As everyone knows, there are *trivial* mistakes, of computational nature. Before the advent of computers, such mistakes were extremely difficult to detect. Fortunately, proofs of serious mathematical results rarely depend on the details of calculations. On the other hand, there are much more dramatic *deep* mistakes, related to the fact that we do not appreciate some essential aspects of the situation. Also, in many cases, irreparable mistakes are produced by incorrect use of preceding results.

It seems that physicists, and some other scientists have a much healthier attitude[2]. As Walter Rudin puts it in his autobiography ([5], Misteaks): 'Mathematicians are human. Humans make mistakes. This is no cause for alarm.' Or, as Richard Feynman observed: 'It is our responsibility as scientists to teach how doubt is not to be feared, but welcomed and *discussed*'. Below I give a very scant sample of such historical mistakes of different sorts, just to illustrate my point.

- It seems that *none* of the geometric proofs in the first books of Euclid, which are even today merchandized as proofs in school Geometry textbooks, are proofs according to the modern standards. Apart from the stated axioms, these 'proofs' refer to scores of unstated ones, 'obvious' facts, pictures, etc. But, as we know from the current papers in Algebraic Topology, referring to pictures you can prove essentially anything. For instance, that any triangle is equilateral.

By purely accidental circumstances, the facts usually presented as theorems in school textbooks are true (in this case, can be deduced from Hilbert's axioms), but this does not make their *traditional* proofs any more correct. For instance, Euclid's proof of the fact that the diagonals of a parallelogram bisect each other (the one reproduced in Wikipedia) already uses much more difficult facts that they intersect inside the parallelogram or, for that matter, that they do intersect at all! As was gradually discovered between the third century B.C and the nineteenth century A.D., the reason is that Euclid's text missed a whole bunch of axioms (or, in fact, at least two such bunches, betweenness and continuity), necessary to derive his claims.

[2]Concerning this paragraph, the second anonymous referee commented that 'Physics is driven by observation and measurement, and can therefore live well with incorrect proofs'. For me, Mathematics is also largely driven by observation and experiment. In Physics, you can afford speculations, mathematical guesswork usually stays very concrete and specific. Before becoming mathematical theorems such things as the asymptotic law of distribution of primes, or Dirichlet's theorem on primes in arithmetic progressions, and thousands of other similar results were *experimental facts*. It is exactly the possibility of instantly switching the register, and checking weird speculation against irrevocable facts of life, that makes Mathematics so unique.

I am not trying to question the uppermost *historical* significance of Euclid's book, which questioned the standards of proofs accepted before that time, and which set new standards for many centuries to come. Quite amazingly, many proofs in the later books, including Euclid's proof of the infinitude of primes, *are* proofs even for us.

- Actually, even at the university level people rarely seriously contemplate the proofs they are telling to their students in the elementary courses. Thus, until very recently most of the proofs in undergraduate Analysis courses suffered from **systematic delusions** related to the incorrect use of infinity, that survived from the early twentieth century.

A crucial fact, used to establish the equivalence of continuity in terms of sequences and in terms of neighbourhoods, is that any infinite set contains a countable subset. Here is the usual proof, to which generations of students were exposed. Let X be an infinite set. Since it is infinite, it is non-empty, and thus contains a point x_1. Then the set $X \setminus \{x_1\}$ is still infinite and thus contains a point $x_2 \neq x_1$. Proceeding in the same way, we eventually get a countable subset $\{x_1, x_2, x_3, \dots\}$ of X.

This 'proof' is short and crystal clear. This is why colleagues in Analysis used to overreact when challenged by the simple fact that *in this form*, without additional assumptions, the proof is *completely wrong*—as are most other naive arguments using '...' in similar sloppy fashion.

What this argument does indeed prove is that X contains an arbitrarily large *finite* subset $\{x_1, \dots, x_n\}$, which is called mathematical induction. Using some form of the *axiom of choice*, one can within another quarter of a page derive that X contains a countable subset. However, the result is obviously **wrong** without such an assumption. In fact, Paul Cohen constructed models of **ZF** where infinite sets do not have to be Dedekind infinite. In other words, any such fake 'proof', that does not make *explicit* mention of the axiom of choice, is *grossly* misleading.

- There is no general consensus as to who was the first to give a correct and *complete* proof of d'Alembert's 1746 theorem that **C** is algebraically closed (FTHA). Subsequent proofs were proposed by Euler in 1759 and by Lagrange in 1771. However, mathematicians are rarely interested in the history of their Science. Without much thinking, most sources repeat the ridiculous claim that the first such 'satisfactory' proof was given by Gauß in 1799. To make it look slightly less preposterous, in 'Algebra' Bourbaki calls the FTHA the Euler–Lagrange theorem, and in 'Topology' the same result is called the d'Alembert–Gauß theorem.

I fully agree with Klein that on this point we should rather trust Gauß himself, who called his Thesis 'Demonstratio nova', thus showing that he does not pretend to be the first to give such a proof. Of course, Gauß starts with criticizing all preceding proofs. The objection he raises against the Euler–Lagrange proof is that they already assume that the roots of an algebraic equation exist *somewhere*, to apply the Viète formulae, and derive that *then* these roots are in fact complex. Retrospectively, after the work of Kronecker, this proof is correct to all effects. The problems in d'Alembert's proof are slightly more serious, but I would assess it as *essentially* correct, as we know the later work of Weierstrass.

Unfortunately, the same does not apply to Gauß's first proof, which if not quite false, is *severely* problematic. In fact, unlike the two preceding proofs that today can be easily patched by a first-year undergraduate student, Gauß's first proof contains a *huge* gap, that was only fixed by Ostrowski [6] in 1920, and that even today would be non-trivial to fill for a professional mathematician outside of the field. Smale [7] describes the situation as follows: 'I wish to point out what an immense gap Gauss's proof contained. It is a subtle point even today that a real algebraic plane curve cannot enter a disk without leaving. In fact even though Gauss redid this proof 50 years later, the gap remained. It was not until 1920 that Gauss' proof was completed.'

- Let me quote several momentous statements chosen *at random* from the 1821 *Cours d'analyse* by Cauchy [8], where he 'set the foundation of rigour' for modern Analysis: 'Each continuous function is differentiable almost everywhere', 'The sum of a convergent series of continuous functions is itself continuous', 'A function of several variables, continuous in each variable is continuous', 'Any essential singularity is a pole'. Browsing this textbook for a few minutes, you will easily find dozens of similar statements, 'accompanied by complete and detailed proofs.' (Of course, I know Lakatos's objections [9] that Cauchy made no mistakes, but was arguing in terms of non-standard Analysis. I am not convinced, since most early authors praise Cauchy precisely for *getting rid* of infinitesimals.)

In fact, Cauchy was notorious for making *really* bad mistakes (as Abel sarcastically puts it: 'It appears to me that Cauchy's theorems suffer exceptions'). Especially in Algebra, he made quite outrageous claims, accompanied by several dozen pages of computational proofs by obfuscation. We skip the famous Lamé–Cauchy case, where Cauchy persisted in publishing his 'proof' of the unique factorization for cyclotomic integers long after being confronted by Kummer's counterexamples [10]. However, essentially all his algebraic results I've seen are of the same nature. Thus, discussing p-elements in symmetric groups S_n, he 'proves' that any element of the wreath product $S_p \wr \ldots \wr S_p$ has order p. Even more amazing is his 1821 paper on Padé approximation, where he claims that for any natural $0 \leq m \leq n$ and for any $n+1$ distinct points $x_0, x_1, \ldots, x_n$ there exist polynomials f and g of degrees $\leq m$ and $\leq n-m$, respectively, such that f/g takes (arbitrary!) prescribed values at $x_0, x_1, \ldots, x_n$. The irredeemable mistake was indicated by Kronecker 60 years later. In fact, as any first year undergraduate student can see within 5 min, Cauchy's claim clamorously fails already in the *first* non-trivial case $m=1$, $n=2$.

- It seems that the record is set by the failed attempts to prove Fermat's last theorem. There were thousands of such wrong or defective proofs, many of them published. A person of no smaller status than von Lindemann has between 1901 and 1909 published *three* fallacious proofs of Fermat's last theorem [11]. Vandiver's 1934 criterion was long considered the most important advance towards the proof of Fermat's theorem since the time of Kummer. However, later it was discovered to contain a fatal gap. Here is how Lang [10] describes it: 'On the other hand, many years ago, Feit was unable to understand a step in Vandiver's "proof" that $p \nmid h_p^+$ implies the first case of Fermat's Last Theorem, and stimulated by this, Iwasawa found a precise gap which is such that there is no proof.' Interestingly, in this case, the publication was delayed, not to upset Vandiver, and he passed away, thinking his proof was correct.
- One of my all-time favourites is Hilbert 16. In 1923 Dulac [12] claimed that a polynomial vector field on a plane has finitely many limit cycles, thus solving the second half of this problem. In the 1950s Petrovsky and Landis even claimed they obtained a bound for the number of such limit cycles, in terms of the degree of the vector field. Soon thereafter Novikov and Ilyashenko discovered a gap in the work of Petrovsky and Landis, and in 1981 Ilyashenko discovered an abyssal gap in Dulac's work [13], after which the state of our knowledge returned to the time *preceding* 1900, when Hilbert formulated his problem!

Actually, the history of the first half of Hilbert 16 is not any less fascinating! Thus, for instance, in his PhD thesis, Gudkov has proven Hilbert's claim concerning the topological structure of real algebraic curves of degree 6, and then in his *Habilitation* constructed a counterexample [14].

Another equally amazing case is Hilbert 21, also known as the Riemann–Hilbert problem. In 1908 Plemelj published a paper that for many decades was considered a definitive solution. In 1964, he even redid this work in a monograph [15]. Soon thereafter, Ilyashenko and others started raising doubts about his proof and in 1989 Bolibrukh constructed a counterexample [16].

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If you think this is still not convincing, and that mathematicians do not make mistakes, read something on the history of the Riemann hypothesis, the four-colour problem, the Newton–Gregory problem, the Kepler conjecture, the Busemann–Petty problem, the Bieberbach conjecture, Hilbert 12 [17], . . .— whatever!—the list can be made arbitrarily long.

6. Don't worry about errors, the biographers will explain all errors

I would be rather embarrassed to leave a false impression that I am not serious about mistakes. Actually, like the vast majority of practicing mathematicians, I *very much* am. To discover a serious mistake, gap or omission in a published work of your student or a colleague, not to say in your own work, makes you feel *strong* discomfort not much different from physical pain. Fortunately, we, as mathematicians, do not have to get it right the first time we try, unlike, say, brain surgeons.

Long-term historical, social and educational benefits strongly outweigh personal feelings; one who knows the truth should speak it. Confucius always insisted on the importance of the 'correction of names' and 'disclosure of errors' (as opposed to the 'concealment of errors') as vital social mechanisms. Knowing the mistakes, delusions and traps in the field is a fundamental part of professional training. Here is what Arnold [18] says in merit: 'Mistakes are an important and instructive part of Mathematics, perhaps *as important* a part, as the proofs. *Proofs are to Mathematics what spelling* (*or even calligraphy*) *is to poetry.* Mathematical works do consist of proofs, just as poems do consist of characters.'

- It is known that the works of Hilbert himself were pestered with misprints and minor inaccuracies. Here is how Rota recounts the story in his 'Indiscrete thoughts' [19]: 'Once more let me begin with Hilbert. When the Germans were planning to publish Hilbert's collected papers and to present him with a set on the occasion of one of his later birthdays, they realized that they could not publish the papers in their original versions because they were full of errors, some of them quite serious. Thereupon they hired a young unemployed mathematician, Olga Taussky-Todd, to go over Hilbert's papers and correct all mistakes. Olga labored for three years; it turned out that all mistakes could be corrected without any major changes in the statement of the theorems. . . . At last, on Hilbert's birthday, a freshly printed set of Hilbert's collected papers was presented to the *Geheimrat*. Hilbert leafed through them carefully and did not note anything.' According to Reid [20], Hilbert was aware of the changes but was not concerned.

It is a fact of life that mathematicians do make mistakes. Another fact of life is that the majority of them are of a *trivial* nature and can be corrected 'without major changes to the statements'. There are *bad* mistakes, related to haste, or complete lack of vision, where nothing can be salvaged. But there are also *good* mistakes, which show that there is something fundamental out there, that we do not appreciate or comprehend. Such mistakes should be recognized, nourished and rationalized, and eventually lead to a deeper understanding. This is what Dalí says about good mistakes: 'Les erreurs ont presque toujours un caractère sacré. N'essayez jamais de les corriger. Au contraire: rationalisez les, comprenez les bien. Après cela, il sera possible pour vous de les sublimer.' Or, as Arnold puts it, 'The role of mistakes in Mathematics is not smaller than that of proofs: analysing their reasons and the ways to overcome them, you can move ahead much faster, than by stubbornly pushing in an unexplored direction' [14].

Actually, I consider the practice of finding mistakes where there are none, or blowing minor mistakes in a competitor's work out of proportion, and especially the practice when alleged mistakes are used as a weapon in priority issues, as more damaging and detrimental for Mathematics than the mistakes themselves. But reluctance to acknowledge your own bad mistakes can be equally damaging.

- The dramatic history of the classification of small groups up to isomorphism pleads for a novel. Here is one small episode. Already in 1896 Miller knew that there were 51 non-isomorphic groups of order $32 = 2^5$. But after mistakes in his classification of groups of orders 64 and 96 were discovered, Miller returned to this problem in 1936—exactly 40 years later!—and came up with a new value, 47 instead of 51. However, [21] confirms the correctness of the *initial* value. This shows how dangerous it is to search for mistakes in your own old works, or in old works of your colleagues, post factum, when you have managed to forget the context and many details.
- Another famous case was the proof of Gauß's 10th discriminant conjecture, when Heegner's original paper was considered defective and all credit went to Baker and Stark. Here is what Stark himself [22] writes in merit: 'Recently, Baker and Stark have independently shown that there are only 9 complex quadratic fields of class-number one. However, in 1952 Heegner had already proved the same thing. Unfortunately his proof has been regarded as incorrect or at best, incomplete. We will show here that there is in fact only a very minor gap in Heegner's proof and we will fill this gap.'
- In the opposite direction, one could mention many recent events, when top journals were unwilling to publish errata or retractions even in the case of *bad* mistakes in the published papers, which invalidated their main results, or, sometimes, initiated such retractions only after the case became public through other means, see [23], for instance.

My *Doktorvater* Zenon Borewicz advised not to rush submitting a newly finished paper, but to let it mellow, reread it after a month, then maybe after three to four months, then after a year, and only if nothing hurts your sensitivity in any way should you send it to a journal. He was a slow publisher, as I am myself and many of my co-authors. Unfortunately, the current culture of grants, impact factors and the like does not encourage this type of behaviour.

7. The $N + 1$ cultures

The belief that there are uniform crossfield standards of mathematical rigour, valid for all areas of Mathematics, seems to me even more ludicrous. In his famous essay 'The two cultures' [24], Snow discusses the enormous divergence of intellectual culture between Science and Humanities. However, in my view, it is overly simplistic to speak of *two* cultures. As a scientist, you are immediately aware of how much the culture and mentality (background, value system, basic criteria of truth, procedures, objectives,. . .) of mathematicians differs from those of physicists—which, in turn, are very different from those of other scientists, let alone engineers. Looking closer, you discover drastic dissimilarities between the cultures of pure mathematicians and applied mathematicians, then between cultures of mathematicians in different areas, then between cultures in different subfields of the same mathematical discipline, etc.

Browsing mathematical journals for a couple of days, one can easily spot that there is no obvious common standard of proofs for different subjects. Out of curiosity, I've tried to read some of the key papers in Algebraic Topology. Typically, such a paper has 30–50 pages and runs as follows. After an introduction, the statement of results, and some general remarks, four or five cases are analysed in some detail. After that, the proof is concluded by an observation that the analysis of the remaining 437 cases—or was it in fact 653?—is *similar, but easier*. What struck me most, though, was that, even for the few cases that *were* considered, their analysis referred to pictures in a highly non-trivial way. It is not that I, as someone coming from a different background, do not understand these proofs. What I do not understand is how these texts are published in top journals and are considered as valid proofs by the community. I do not expect *this* type of argument to be formalized effortlessly. To conform with the standards common in, say, Finite Group Theory, these texts would have to be made a dozen times longer, sometimes a hundred times longer. Who would write such texts, who would publish them and who would read them?

On the other hand, who am I to question the wisdom of my Topology colleagues? I do not have any doubts whatsoever that they should know what they are doing, since otherwise they would make 1000 times more mistakes than they actually do. Probably, they have developed more efficient operation and communication modes, etc. But anyway, this alone is a glaring illustration of my point, that even advanced and otherwise fairly close fields do not have a common standard as to what is normally considered a publishable proof. Of course, my humble opinion can be easily discarded, as that of an outsider.

Here is, however, what Novikov, whose work was central to the development of Algebraic Topology, writes in merit: 'To summarize our report on the period of highest flowering of classical Algebraic Topology (that is, the 1950s and 60s), we see that a considerable number of deep results were also obtained later in the 1970s and 80s. However, the community of topologists pays less and less attention to the significance of their ideas for the rest of Mathematics, restricting their interests and their horizon more and more, and making their language more and more isolated and abstract. Moreover, as we pointed out above, the community has also lost control of the extent to which even the best results of the subject are rigorously proved. The last aspect undoubtedly bears witness to a lowering level of the subject, when the central theorems are not proved, and the following generation is not even aware of this, and "trusts the classics".' [25]. In [26], Novikov expands this criticism to some famous papers in Dynamical systems, Analysis and Algebra.

8. Who understands a theorem?

A specific impetus for me to start thinking seriously about these issues has been given by an elegant and provocative paper by Brian Davies 'Whither Mathematics' [27]. He writes in particular: 'In 1875 every sufficiently able mathematician could fully absorb the proof of most theorems that existed within a few months. By 1975, a year before the four-colour theorem was proved, this was not even close to being true, but it was still the case that some mathematicians fully understood the proof of any known theorem. By 2075 many fields of pure Mathematics will depend upon theorems that no mathematician could fully understand, whether individually or collectively.'

I agree with Davies in most of his mathematical and even philosophical points, but I cannot accept his main conclusion that at the *individual* level the status of the mathematical proof is vitally different today from that of a century or two centuries ago. His claims seem to me to be overly optimistic. As far as I can judge, already in the second half of the nineteenth century the full scope of mathematical knowledge could not be mastered *in corpore* by one person. I do indeed believe that in 1775 any sufficiently competent mathematician could absorb the proofs of *all* existing theorems and that in 1875 an exceptionally qualified mathematician still could essentially comprehend the proof of *any* one of them. I can even admit that in 1975 any theorem was understood by *some* mathematician. But what Davies relegates to 2075 has overtaken us yester year : '*où sont-ils, les preuves d'antan*?'

There is no doubt whatsoever that many things concerning the status of current mathematical research are to be seriously reconsidered, but first of all, it is the traditional Philosophy of Mathematics itself. If the Philosophy of Mathematics is inadequate today, this is exactly because it was always inadequate. The pathos, ethos, spirit and values of mathematical research have changed little within the last 2500 years. Purely intellectually Archimedes, de Fermat, von Leibniz, Euler, Lagrange, Dirichlet, Jacobi, Hamilton, and Riemann are still closer to us than the majority of our contemporaries.

On a more frivolous note, my late friend Oleg Izhboldin used to specify: 'Who proved what, and *to whom*?' Essentially, at the time this could translate, for instance, into 'Voevodsky has proven Milnor's conjecture to Suslin'. It is a highly non-trivial historical issue as to what kinds of procedures and arguments counted as proofs—*when, for whom and under what circumstances*.

9. Why are mathematical results so exceptionally reliable?

Anyway, given all this one has to account for the evident truth that the overall reliability of mathematical knowledge is *exceptionally* high, by any human standards[3]. There are many contributing factors, both of a profound and a profane nature.

There are many mundane and relatively trivial aspects, of course, accounted for by historical, cultural and social circumstances. One important social ingredient is that Mathematics had *significantly* less funding than some other fields, and (at least until very recently) was considered to be outside of direct political involvement. Consequently, there was much less pressure to publish unfinished and unchecked papers, let alone to directly falsify or feign results, as is routine in more lucrative and/or politically charged human undertakings.

At a somewhat deeper level, the uniqueness of Mathematics consists not in the greater reliability of its conclusions (as some people believe) but in *procedures* used to achieve this reliability. Slightly paraphrasing Russell's [in]famous 1917 quote, one can maintain that 'Mathematics may be defined as the subject where we know what we are talking about, we know what we are saying, and we know whether what we are saying is true'. This quote refers to the mathematical Trinity: Definition–Statement–Proof.

Another important historical and cultural dimension of Mathematics is the **culture of truth** itself, absent in so many other human endeavours: 'And here I must play God and say to both Android and Mathematician: "Oh, no! Don't lie—because everybody else does".', [28].

However, mathematicians themselves know that there is a much more fundamental reason. We rarely discuss ontological or epistemological issues with lay people, but practicing mathematicians tend to develop (what they perceive as) direct contact with the world of ideas. It is virtually impossible to communicate this experience to those who do not share it. Also, you are almost embarassed to acknowledge such an attitude, which, as Manin puts it, is 'intellectually indefensible but psychologically inescapable'. Let me cite three overwhelming testimonies by three top mathematicians—either you know what these people are saying, or you don't; any further explanation is futile and to no purpose.

- 'Perhaps I can best describe my experience of doing Mathematics in terms of a journey through a dark unexplored mansion. You enter the first room of the mansion and it is completely dark. You stumble around bumping into the furniture, but gradually you learn where each piece of furniture is. Finally, after six months or so, you find the light switch, you turn it on, and suddenly it's all illuminated. You can see exactly where you were. Then you move into the next room and spend another six months in the dark. So each of these breakthroughs, while sometimes they're momentary, sometimes over a period of a day or two, they are the culmination of—and couldn't exist without—the many months of stumbling around in the dark that precede them.' Andrew Wiles [29].
- 'Being a professional mathematician, in my work I constantly have to rely not on proofs, but on feelings, guesses and conjectures, in passing from one fact to another by a special kind of revelation that forces you to perceive common features in phenomena that might seem completely unrelated to an outsider. A correct guess is accompanied by the conviction that makes any further proofs completely unnecessary, an almost excruciating feeling, that you can never forget, but which is very difficult to communicate to others.' Vladimir Arnold [14].
- 'When I'm working I sometimes have the sense—possibly the illusion—of gazing on the bare platonic beauty of structure or of mathematical objects, and at other times I'm

[3]In this connection, the second anonymous referee raised the following *extremely* important point. The real concern of Mathematics is not truth, but effectivity, and so the real question to ask is not why Mathematics is so 'exceptionally reliable', but why it is so 'unreasonably effective'. Overall, I concur with that. In the controversy of Physics vs Logic I stand with Physics. It is simply *much larger*. Specifically, in what concerns mathematical reasoning, Logic attempts to break an argument into a huge number of elementary steps. The spirit of Mathematics is exactly the opposite, to create most *efficient* ways of reasoning. Mathematical thinking consists of compressing huge bulks of arguments into tangible entities that can be perceived as a whole by the human mind, and then to operate these pieces with very high precision and certainty. This is exactly what makes Mathematics so effective: anyone, who is thinking clearly about anything, is doing Mathematics.

a happy Kantian, marveling at the generative power of the intuitions for setting what an Aristotelian might call the formal conditions of an object. And sometimes I seem to straddle these camps (and this represents no contradiction to me). I feel that the intensity of this experience, the vertiginous imaginings, the leaps of intuition, the breathlessness that results from "seeing" but where the sights are of entities abiding in some realm of ideas, and the passion of it all, is what makes Mathematics so supremely important for me. Of course, the realm might be illusion. But the experience?' Barry Mazur [30].

That's it! The realm *might be* an illusion, but *the experience is not*! After a few phrases you exchange with a person, you immediately know whether she is a mathematician, and has immediate contact with the world of ideas.

10. Formalized proofs vs traditional proof

The machine verification of the Feit—Thompson odd order theorem [31] is a magnificent and monumental achievement, and it is not my intention to downplay that. A decade before that I could not imagine something of such magnitude was feasible *soon*.

However, in my view it was not a direct computer verification of the existing proofs of the odd order theorem, nor has it increased our faith in its validity. What exactly happened? A team of 15 *highly* qualified and *unusually* dedicated experts have invested 10 years of their worktime to convert the (simplified) proof of the odd order theorem, as well as lot of background algebra, into some 150 000–200 000 lines of computer scripts in `Coq`. In the process, they had to take scores of non-trivial mathematical and programming decisions, edit, rephrase, and sometimes correct arguments in the texts they were rewriting into a formalized proof.

Of course, the positive outcome is conclusive. However, failure to compile and verify such an enormous script could be attributed to a number of reasons, not necessarily to the fallaciousness of the original theorem, nor a mistake in the original proof, and even less so to a serious such mistake. In this particular case, a substantial part of that immense effort led to the development of the proof assistant itself and is reusable. However, how much traditional stuff could a similar team produce within 100–150 human labour years?

That is not a place to discuss objections raised against computer proofs at a technical level. As we all know, initially there *were* serious problems with computer-aided proofs, several alarming instances of which are mentioned by Serre in [32]. At the same time, many of the computer/formal/formally verified proofs are admirable achievements; see a broader discussion of related issues in [33–35]. Actually, I, for myself, trust computer calculations *more* than most traditional proofs. But formalized proofs, and the present day computer proofs, are simply a completely different reality than traditional proofs, something *extrinsic* to mathematics itself.

The real mission of proofs is not to certify 'truth'. It is to **transmit understanding**: between persons and generations, above language and culture, across space and time. In all this, formalized proofs are utterly inadequate.

First of all, formalized proofs are much less robust than traditional ones. Transmitting information into eternity, beyond an ongoing historical lineage, is not easy. One of the greatest challenges in the construction of the Onkalo spent nuclear fuel repository was how to warn our distant descendants (who do not understand Finnish, Swedish or any other language we speak today, and probably do not know what radioactivity is) to keep out. Now, think of our proofs as written in clay or stone—well, '*l'airain, le marbre, le cuivre*'. Any such material is worn by time, and some of the characters fade and eventually vanish. However, even the presence of just a few misprints makes a formalized proof completely useless. If a mathematical result is not used or reproved every 10 years, it loses half of its credibility.

Larvor [36] makes another extremely important point, which is seldom mentioned in this context. Namely, a formal proof is not a genuine representation of a real-life mathematical proof not because it tells too many unnecessary details, but because it does not tell the necessary ones: 'The vital mathematical thought ... is not "buried" in the fully formal version, in the sense of being spread over too many pages or obscured by thickets of notation. It is *absent* from the formalized version. But it is precisely in grasping such thoughts that one understands a proof, and it is prior understanding of such thoughts that allows expert mathematicians to read the highly compressed proofs in Mathematics journals.' Or, as Manin puts it, 'A good proof is a proof that makes us wiser'.

It is completely misleading to promote formal proofs as something sweepingly superior to 'naive' traditional proofs. Formalized proofs are not 'higher versions' of traditional proofs, but rather completely different entities. As such, without the supporting human argument, they have no persuasive power. The difference between a traditional proof and a formalized proof is essentially the difference between an artistic drawing and an X-ray photograph. Both have their place and function in important contexts, but they are hardly interconvertible.

Overall, I'm inclined to believe that, as compared with traditional proofs, formalized proofs are

- very much longer, and less transparent (übersichtlich/überschaubar),
- not much more reliable,
- much less robust,
- contain *substantially* less information.

It is not that they are useless. I'm only saying that at the present stage, before the advent of an artificial intelligence that would be able to communicate with humans both effectively and efficiently, they are no substitute for traditional proofs. They can be *extremely* useful even now, on some occasions, about just as useful as qualified medical aid can be on some other occasions.

There is yet another aspect in which a traditional proof is very much different from a formalized proof. Namely, in many cases, it proves not just the statement of the theorem, but a different, usually stronger result. A major Taoist thinker, Winnie the Pooh, observes, 'different proofs would prove different things, otherwise it had been the same proof', [37]. In §10 of [38], I discuss one such instance, 5 different proofs of Suslin's normality theorem. When you look closer, it turns out that they all prove disparate statements (stronger, or more precise than the theorem itself), generalize in diverging directions, provide distinct bounds, etc.

But the incompatibility of a traditional and a formalized proof is much more formidable than that. A formal proof is a one-dimensional structure: a string of characters, or, at best, a tree or a graph. As opposed to that, a traditional proof is a many-splendoured multidimensional edifice. The only dimension of a formal proof is its *length*, whereas a traditional proof also has *width*, *breadth*, *depth*, *height*, *hue*, *saturation*, *brightness* and many more dimensions. Owing to the lack of time, space and expertise, I just *allude* to a couple of those: its *width* as prevised by Hilbert 24, and its *depth*, so apparent in Voevodsky's foundations.

11. Hilbert 24 vision

The way Hilbert stated his cancelled 24th problem in his mathematical notebooks (see [39,40] for the German original and an English translation) initially sounds rather tentative and generic: 'Ueberhaupt eine Theorie der Beweismethoden in der Mathematik entwickeln.' Clearly, he was anticipating something like *Beweistheorie*, what we now know as Hilbert's program, which he started to develop some 20 years later.

However, the central part of his note is of immense gravity: 'Ueberhaupt wenn man für einen Satz 2 Beweise hat, so muss man nicht eher ruhen, als man die beide aufeinander zurückgeführt oder genau erkannt hat welche verschiedenen Voraussetzungen (und Hülfsmittel) bei den Beweisen benutzt werden: Wenn man 2 Wege hat, so muss man nicht bloss diese Wege gehen oder neue suchen, sondern dann das ganze zwischen den beiden Wegen liegende Gebiet erforschen. Ansätze, die Einfachheit der Beweise zu beurteilen, bieten meine Untersuchungen ueber Syzygien und Syzygien zwischen Syzygien. Die Benutzung oder Kentniss einer Syzygie vereinfacht den Beweis, dass eine gewisse Identität richtig ist, erheblich.'

How Hilbert himself phrased the problem, mentioning syzygies, etc., clearly suggests that he was thinking purely algebraically. Today, with the hindsight of Homotopy Theory, and Higher Category Theory, we would favour a different wording. As we read it today, mathematical propositions are objects, whereas inference paths are morphisms. What Hilbert actually asks, is to study the whole domain between two such inferences. In other words, to glue 2-cells, whenever possible, or, if you like, to construct morphisms between morphisms.

But most traditional proofs do have this kind of horizontal mobility. In other words, they come with inherent *width*, which is mostly implicit, sometimes articulated. You can organize a traditional proof differently at essentially any point, and a specific presentation is a matter of taste and convenience, and more often than not the choice of a specific inference path within a certain region is purely accidental.

12. Voevodsky's vision

It is premature to casually discuss Voevodsky's foundations. I, for myself, am very impressed by the sheer scope of this enterprise. I do not expect his design of a formally verified body of mathematical knowledge to be more workable than any previous such endeavour. But its contrast with the orthodox foundations is very refreshing. Oversimplifying things, one can say that his main gadget is an explicit correspondence between what you see at different dusk levels, giving a possible precise meaning to the *depth* of a proof.

Not in a position to discuss it here at a technical level, I just quote the talk [41] in which Voevodsky explains what urged him to seriously think about foundations and computer verification of proofs. This is an *absolutely* overwhelming human document, of tremendous intellectual honesty, and maybe of about the same historical significance as the famous letter of de Fermat to Descartes, or as the last letter of Galois. An *absolute* must read for anyone wishing to understand what creative Mathematics is. It is so staggering that I indulge myself by reproducing two long passages thereof. Here is how Vladimir himself tells the story of why he converted to the computer verification.

'The field of motivic cohomology was considered at that time to be highly speculative and lacking firm foundation. The groundbreaking 1986 paper "Algebraic Cycles and Higher K-Theory" by Spencer Bloch was soon after publication found by Andrei Suslin to contain a mistake in the proof of Lemma 1.1. The proof could not be fixed, and almost all of the claims of the paper were left unsubstantiated.

A new proof, which replaced one paragraph from the original paper by 30 pages of complex arguments, was not made public until 1993, and it took many more years for it to be accepted as correct. Interestingly, this new proof was based on an older result of Mark Spivakovsky, who, at about the same time, announced a proof of the resolution of singularities conjecture. Spivakovsky's proof of resolution of singularities was believed to be correct for several years before being found to contain a mistake. The conjecture remains open.

The approach to motivic cohomology that I developed with Andrei Suslin and Eric Friedlander circumvented Bloch's lemma by relying instead on my paper "Cohomological Theory of Presheaves with Transfers," which was written when I was a Member at the Institute in 1992–1993. In 1999–2000, again at the IAS, I was giving a series of lectures, and Pierre Deligne (Professor in the School of Mathematics) was taking notes and checking every step of my arguments. Only then did I discover that the proof of a key lemma in my paper contained a mistake and that the lemma, as stated, could not be salvaged. Fortunately, I was able to prove a weaker and more complicated lemma, which turned out to be sufficient for all applications. A corrected sequence of arguments was published in 2006.

This story got me scared. Starting from 1993, multiple groups of mathematicians studied my paper at seminars and used it in their work and none of them noticed the mistake. And it clearly was not an accident. A technical argument by a trusted author, which is hard to check and looks similar to arguments known to be correct, is hardly ever checked in detail.'

Now, if you are still not impressed, here is another dramatic thread. Especially so, since it describes his famous work with Kapranov, after which Vladimir was enrolled in PhD studies at Harvard. Now, imagine for a second the mistakes therein were detected before that.

'But this is not the only problem that allows mistakes in mathematical texts to persist. In October 1998, Carlos Simpson submitted to the arXiv preprint server a paper called "Homotopy Types of Strict 3-groupoids." It claimed to provide an argument that implied that the main result of the "∞-groupoids" paper, which Kapranov and I had published in 1989, cannot be true. However, Kapranov and I had considered a similar critique ourselves and had convinced each other that it did not apply. I was sure that we were right until the fall of 2013 (!!).

I can see two factors that contributed to this outrageous situation: Simpson claimed to have constructed a counterexample, but he was not able to show where the mistake was in our paper. Because of this, it was not clear whether we made a mistake somewhere in our paper or he made a mistake somewhere in his counterexample. Mathematical research currently relies on a complex system of mutual trust based on reputations. By the time Simpson's paper appeared, both Kapranov and I had strong reputations. Simpson's paper created doubts in our result, which led to it being unused by other researchers, but no one came forward and challenged us on it.

Around the time that I discovered the mistake in my motivic paper, I was working on a new development, which I called 2-theories. As I was working on these ideas, I was getting more and more uncertain about how to proceed. The Mathematics of 2-theories is an example of precisely that kind of higher-dimensional Mathematics that Kapranov and I had dreamed about in 1989. And I really enjoyed discovering new structures that were not direct extensions of structures in lower dimensions.

But to do the work at the level of rigor and precision I felt was necessary would take an enormous amount of effort and would produce a text that would be very hard to read. And who would ensure that I did not forget something and did not make a mistake, if even the mistakes in much more simple arguments take years to uncover?'

13. The metaphor of Proof

The reason traditional Philosophy of Mathematics is grossly inadequate is that it describes mathematical knowledge as something one-dimensional, as a text. But, resuming Manin's quotation, 'Mathematics decidedly is not a text, at least not in the same sense as Philosophy. There are no authoritative books or articles to which subsequent generations turn again and again for wisdom. Except for historians, nobody reads Euclid, Newton, Leibniz or Hilbert in order to study Geometry, Calculus or mathematical Logic. The lifespan of any mathematical paper or book can be years, in the best (and exceptional) case decades. Mathematical wisdom, if not forgotten, lives as an invariant of all its (re)presentations in a permanently self-renewing discourse', [42].

Not only is mathematics as a whole not a text, in any important respect even individual mathematical proofs are not just texts, and cannot be reduced to such texts. They are of essentially multi-dimensional character. As a first approximation, you can think of a proof as a map, a chart, a blueprint. . .From the way it functions, a mathematical proof can be visualized as a fishing net. Such a net may have holes, even rather big ones, and at some point you might wish to patch or repair it. But its sole function is to catch fish: 'You need a net to catch fish, at the moment the fish is caught, you forget the net. You need words to catch thought, at the moment the thought is caught you forget the words.'

However, at the deeper personal level the decisive element of Mathematics, oftentimes more imperative than its factual, technical, pragmatical or mystical dimensions, is its *aesthetic value*. The way we experience mathematical proofs is essentially not much different from the way we behold *quattrocento* frescoes: Piero della Francesca, Filippo Lippi, Benozzo Gozzoli, Pinturicchio, or any of their contemporaries. Or, for shorter proofs, the way we savour *la pittura fiamminga*, someone like Robert Campin, Jan van Eyck, Rogier van der Weyden, or Hans Memling. Their works possess an *enormous* level of precision, and precision of Mathematics is of the same, or very similar nature.

Even if a fresco is not finished, altered by subsequent intrusions, damaged by time, poorly illuminated…at a glance you know its value, the artistic school to which the master belonged, its subject matter, the idea behind the composition, etc. Looking a bit more carefully, you can appreciate many further details, technical level, materials used, possible implications, etc. And eventually, you get a very clear idea as to master's intent and as to whether he could have completed this particular fresco with the available materials, ideas and techniques. In most cases, that's all you have to know about the proof of a specific claim, to proceed from there—that it *exists*. Not in the metaphysical sense, of course, but as an artefact.

14. Conclusion

Stewart says, 'To criticize Mathematics for its abstraction is to miss the point entirely. Abstraction is what makes Mathematics work'. I am *almost* tempted to say, that to criticize mathematicians for making mistakes in the traditional proofs is to miss the point entirely. It is precisely the possibility to commit mistakes—and then to rationalize and correct them!—that makes Mathematics work. Or, as Thom puts it: 'C'est seulement parce qu'on accepte le risque de l'erreur qu'on peut récolter de nouvelles découvertes'.

Mathematics is not that much different from other sublime manifestations of the free creative spirit, such as Language, Art or Music. The only reason why I myself became a professional mathematician was that for me, as for any cognisant individual, mathematical constructions and concepts have *supreme* intellectual and emotional appeal, or, switching from *koine* to *katharevousa*, combine the highest possible level of explicitness with the highest possible level of suggestivity. Seen as a supreme human activity, Mathematics has the same strengths and *limitations* as any other creative human activity, such as painting or musical composition.

Individual mathematical proofs have *never* been as sound as one traditionally believed, but the mathematical knowledge as such, and the world view based on the mathematical *Naturwissenschaft*, are as reliable, vigorous and rewarding today as they always have been. That's the only certainty *out there*.

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