# Remarks on the Acceptance of Proofs: the case of some Recently Tackled Major Theorems

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Ideas about mathematics as a subject, which teachers at all levels intend to convey by instruction, cannot be based solely on the logical foundations of mathematics. There is also the existence of the mathematical community as a social system which has to be taken into consideration. From this point of view one may also get valuable orientations for teaching and learning. Mathematics education as a scientific discipline shows a growing interest in this direction as, for example, H.G. Steiner has pointed out, when he claims for the didactics of mathematics a "linkage function between mathematics and society," and asks for "the elaboration and actualization of the many neglected dimensions of mathematics: the philosophical, the historical, the human, the social and — comprising all this — the didactical dimension" [ST, p. 21]

In regard to the status of proof in mathematics — a central issue of this discipline — the broadening of aspects is of special importance to mathematics education. This is because any alteration in ideas about proofs leads immediately to very concrete consequences for the construction of teaching-learning situations in the classroom. So G. Müller and E. Ch. Wittmann [MW] have shown how a greater emphasis on intuition and communication directly affects the style of practising mathematics towards becoming more informal and context-dependent ("inhaltlich-anschaulich" as they express this in German).

As an orientation and basis for such educational consequences one might ask about the role of proofs within the whole system of modern mathematics. While investigating this question through epistemological considerations, G. Hanna ([HA] elaborated the reasons why rigour is not — though often claimed to be, at least during the "new math" period — "the most important characteristic of modern mathematics" [HA, p. 3]. Against this restricted view, she emphasizes the existence of some "positive motivators" [HA, p. 71] towards the acceptance of proofs, which are essentially social phenomena. We will return later in this essay to these factors, and therefore we quote a longer paragraph from G. Hanna's book. Under the heading "factors in the acceptance of mathematical theorems," she writes:

> The development of mathematics and the comments of practising mathematicians suggest that most mathematicians accept a new theorem when some combination of the following factors is present:
>
> 1. They understand the theorem, the concepts embodied in it, its logical antecedents, and its implications. There is nothing to suggest it is not true;
>
> 2. The theorem is significant enough to have implications in one or more branches of mathematics (and is thus important and useful enough to warrant detailed study and analysis);
>
> 3. The theorem is consistent with the body of accepted mathematical results;
>
> 4. The author has an unimpeachable reputation as an expert in the subject matter of the theorem;
>
> 5. There is a convincing mathematical argument for it (rigorous or otherwise), of a type they have encountered before.
>
> If there is a rank order of criteria for admissibility, then these five criteria all rank higher than rigorous proof." [HA, p. 70]

Following [HA] we refer to these factors as the understanding, significance, compatibility, reputation, and convincing-argument factors respectively.

If these factors indeed play such an important role in the disputes about proofs of mathematical theorems, we must discover them in the public discussions about the most recently tackled — successfully or not — major mathematical theorems. This is the aim of this essay. Maybe we will find it necessary to complete or alter or relativise (or even refute) some of the these quoted above. It will turn out that the convincing-argument factor must be handled especially carefully and discriminately.

One remark should be added in advance. It is impossible to include here any technically detailed descriptions of the mathematical contents and backgrounds of the theorems in question. This is not only because of limitations of space, but also because of the difficulty of the material and therefore partly the incompetence of the author as far as the mathematical details are involved. We come to this point again later because it is also a social moment in the public adoption of proofs. But I try to quote those articles (not always the original works) which are easily accessible and give outlines of the details and hints to the original sources so that interested readers can undertake further studies.

## 1. Riemann's Hypothesis

We start with this very famous and still open conjecture (see [ED] for an outline of the mathematics surrounding this topic):

> *Riemann's hypothesis:*
> All nontrivial zeroes of Riemann's zetafunctions are


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located on the line Re $z = 1/2$

(The Riemann-zetafunction is the analytical continuation on the whole plane of the function, which is defined by

$$\zeta(z) = \sum_{n=1}^{\infty} n^{-z}$$

for the $z \in \mathbb{C}$ with Re $z > 1$ The zetafunction has the socalled trivial zeroes at $z = -2, -4, -6$ and one simple pole at $z = 1$)

With respect to this conjecture, the understanding, significance and compatibility factors are surely fulfilled. Above all, the significance of the Riemann hypothesis is quite considerable: the distribution of the prime numbers could be described more precisely if Riemann's hypothesis were to be proved. Also many other theorems in number theory depend on the validity of this hypothesis. Furthermore, Riemann's hypothesis serves as a kind of driving force in the dynamic development of mathematics, as e g Euclid's Parallel Postulate did some centuries before: one looks for consequences which are maybe provable by elementary means, or which eventually yield a contradiction to the hypothesis This underlines not only the significance factor but also gives hints how to deal with the understanding factor in this case.

But even to this day no convincing argument (factor 5) for Riemann's hypothesis has been found W. Schwarz gives in [SCH, p. 85-96] eight plausible arguments in favour of the validity of the Riemann Hypothesis. Among them are confidence in Riemann's intuition and the numerical evidence. But he also lists six arguments against the conjecture, again stressing empirical results

At first glance, these efforts support the thesis that mathematical discussion is indeed of the same type as social discussion One seeks for arguments which can eventually be accepted, either for or against. One has to realize, however, that the word "convincing" has a decisively more precise meaning in mathematics. To be convinced depends on the high standards of argumentation which mathematicians have reached during a long historical development. This makes a difference with common social disputes.

So one might draw from this example the conclusion that the convincing-argument factor is better not regarded as parallel to the other four factors. This "positive motivator" is more like a *sine qua non* condition and is therefore never the only one appearing in "some combination" of factors Then the second part of the fifth factor ([HA] — see the quotation above) becomes the essential point: the real social impact for the presenters of convincing arguments is that they must formulate their new(!) arguments in a language which is already(!) shared by the mathematical community. This seems to be independent of the degree of formality used for the communication. But it is just the other way round: the degree of formality is to be weighed according to the contribution which a more formal presentation eventually provides for an improved communication The educational consequence is then obvious, but not trivial: never can formality be a goal in itself; rather, the process of formalization has to be evaluated with respect to its improvement of the communication about and the understanding of the subject — and this evaluation should also be exhibited to the students, as far as possible. That the students may indeed be initiated into the process with suitably elementary material is shown in some of the ideas of N. Movshovitz-Hadar [MOH].

## 2. The Four-Colour-Theorem (1976)

This example is treated by G. Hanna herself in her explanation of her factors. She describes how the proof by Appel and Haken [AH1] was received by the mathematical community The controversy over the computer-assisted proof is well known These discussions fit well into the picture of acceptance given by factors 1-5 In 1986 Appel and Haken replied once more to the doubters about their proof and rejected all the (mathematical) rumors which had occurred so far (see [AH2]).

I would like to point to another problem Is the significance-factor really fulfilled in this case? One has to remember that a 5-colour-theorem is easily established, and that in no branch of mathematics and in no field of application is the actual number of colours of any great importance So the following fact may also be seen as a social phenomenon within the mathematical community: the desire of all mathematicians and scientists is to know all these things exactly even if there are no pressing outer or inner reasons to do so. For educational purposes one may learn from this observation that meaningful and applicable are not at all the same

But in the same moment, further questions arise: what is meant by "knowing" when we say that scientists want to "know" all things exactly? The proof of the Four-Colour-Theorem sharpened the questions of insight into and checkability of a proof In our context this points again to the fact that discussions among mathematicians are like social discussions: not only the subjects themselves but the ways of disputing them become questionable. Surely this must also be seen as a challenge for the construction of teaching-learning situations: namely, to include in the learning process at all levels reflections on the ways which constitute genuine mathematical working (cf [NE]).

## 3. The classification of finite simple groups (1980)

In 1980 the task of classifying finite simple groups was accomplished through the work of many authors (see [AS] for an outline of the ideas which lead to the completion of this proof) No doubt G. Hanna's factors of understanding the problem, the significance of the material, and the consistency of the results within the classical theory of finite groups, are all operating in this case While the reputation-factor is hard to prove or disprove with so many authors involved, serious problems again arise with the convincing-argument factor. What does "convincing" mean if "the proof of the Classification Theorem is made up of thousands of pages in various mathematical journals" [AS, p. 59]. Has anyone read the whole story? How could a sceptical nonspecialist mathematician convince her- or himself?

It is therefore desirable to work out an improved shorter proof to come closer to a "convincing argument." But Aschbacher dims the hope: "Still it is hard to imagine a short proof of the theorem which follows the outline just described. I personally am sceptical that a short proof of any kind will ever appear. It is perhaps time to consider the possibility that there are some natural, fundamental theorems which can be stated concisely but do not admit a short, simple proof" [AS, p. 64].

Again, what then does it mean to be convinced? Is it only a confidence in the opinions of the experts of the mathematical community? If so, the influence of the reputability-factor becomes of increasing importance. It is therefore the existence of a certain kind of general commitment to the mathematical community which assures the nonspecialist of the reliability of the specialists. Again we meet a pattern of social forces in mathematical progress. In the case of the classification theorem, the fact that some of the most renowned mathematicians have contributed to this theorem ensures the mathematical community's confidence in the result.

By the way, there arises a further problem. How should one then communicate such complex material? It is only possible to communicate by simplifying and concentrating the ideas. However this may be dangerous. Simplifications tend to become oversimplifications and may trigger the wrong associations. So one has to pay great attention to the elaboration of the sufficient ideas behind such a complex field of matter. One has in addition to realize that simplification and explanation are functions that depend on the degree of preparation of the audience with which one has to communicate.

This observation has direct educational implications. It claims that teachers have to prepare and give their explanations very carefully and seriously. This means that simplifications in a didactical context should not be allowed to turn into false oversimplifications (See A. Kirsch [KI] for an unfolding of this problem.)

## 4. Bieberbach's conjecture (1916), now De Branges' theorem (1984)

As in many other cases, in this example mathematicians first started with the consideration of special cases, restricted areas, etc., in order to convince themselves of the possibility of the validity of the conjecture — a well known behaviour not only in mathematics but also in everyday life. Now, there is established

*De Branges's theorem*

If $f(z) = z + \sum_{n=2}^{\infty} a_n z^n$ is an univalent and holomorphic function on the unit disk $|z| < 1$ ($z \in \mathbf{C}$), then for all $n \geq 2$ the inequality $|a_n| \leq n$ holds.

(The inequality $|a_n| \leq n$ is sharp, because for Koebe's function $f(z) = z(1-z)^{-2}$ one has $|a_n| = n$)

De Branges' proof of this theorem was worked out in 1984 (see [BR] for the original source), and I first heard of the proof, along with many other German mathematicians, at the 1984 meeting of Deutsche Mathematiker Vereinigung (DMV) in Kaiserslautern. At that time there was only a preliminary manuscript by De Branges, which circulated in limited numbers, and only among some specialists, a few weeks before the meeting. One of them reported on the manuscript, but many of the nonspecialized people attending this lecture felt a little bit disappointed. One got the impression that the theorem had been proved only by a lot of calculations in which, more or less incidentally, the inconvenient terms cancelled out at the right moment.

With respect to the convincing-argument factor, one learns the following from this experience: a "convincing argument" is not simply a sequence of correct inferences. One always expects some "qualitative" reason, or an intuitive capable basic idea, behind the — nevertheless necessary — single steps of the proof. However, on the one hand it takes time to unfold this idea because it is often hidden in the author's original paper. On the other hand, often this idea is present only for those people who have worked longer in the area, and such a background is not easy to explain to the general public. (See once more the remark on oversimplification in the previous section.)

Surely, the impression I got from the DMV-lecture was not correct. Russian mathematicians who worked in close connection with De Branges, reported later that: "It should bring great satisfaction to know that there is such a beautiful proof as that of de Branges." [FK, p. 47]

The article by Fomenko and Kuzmina also gives interesting hints towards an interpretation of the second part of the convincing-argument factor, namely the role of the choice of the appropriate language (see also the considerations in section 2.) Fomenko and Kuz'mina report that their research group (a mathematical seminar in Leningrad) was very sceptical at first of De Branges' proof which was originally written in the language of functional analysis. Then the group recognized "that if his proof were correct then there should exist a classical version of the proof" [FK, p. 44]. By a classical version they meant a formulation in the language of geometric function theory. Their observation could be interpreted as making an issue of a matter of convenience only. But there were serious reasons for a translation: "It was clear that for our seminar the simplest way to verify the proof was to translate it into the language of geometric function theory" [FK, p. 44].

Therefore using arguments "of a type encountered before" (see [HA]) is not only a duty of the author in order to suit his readers. Using a suitable language is a condition for communication and understanding, and above all for the acceptance of the proof. The choice of the appropriate language ensures the "intuitive coherence" [MW] of the ideas in the theorem and its proof, and only in an appropriate language is a check of the conclusiveness of the proof possible by the mathematical community.

However, one must also allow for the possibility that the formulation of the proof "in a language encountered before" is only an intermediate phase. Sometimes it turns out that new theorems are more readily understandable when expressed in new languages. But this is less a matter of the formulation than of the creation of new, richer, more appropriate contexts, which emerge around the theorem in the course of further development.

As an educational consequence of this story one must emphasize the necessity of various translation processes.

In the comparatively simple cases of elementary mathematics there exist some quite different languages, like geometric, algebraic, visual, symbolic, etc Teachers should try to foster translations from one language into another to facilitate or even generate the communication of the mathematical content, and to provide a rich background of various contexts for the subjects they teach.

## 5. Mordell's conjecture (1922), now Faltings' theorem (1983)

The problem when a proof of a mathematical theorem is accepted by the mathematical community is even further emphasized by the example of

> *Faltings' Theorem:*
> There exist only finitely many rational points on an algebraic curve of genus $g \geq 2$ over an algebraic number field
>
> (See [FA1] for the original paper, [FA2] for an outline of the various backgrounds, and [BL] for a sketch of the proof )

The reception of Faltings' proof sheds light on a further serious problem with the convincing-argument factor As I was told by some mathematicians who have deeper insights into the area of algebraic geometry, only approximately 25 people all over the world have really understood Faltings' proof thoroughly in all its details because of the highly sophisticated and abstract formulations of the concepts of modern algebraic geometry. However, the meaning and content of the theorem is very clear and easily explained to a wider audience So what do "acceptance" and "being convinced" mean when the number of experts is so limited?

First of all, we have confidence in the experts' judgement because — by a discussion process — they have filled in some gaps of Faltings' original draft. The mathematical community is therefore convinced by the achievements of both the original author and those who critically checked his proof Second, one can also stress the reputability-factor Faltings was not an "outsider" but did his work from within highly reputable German mathematical institutes.

Nevertheless it is noteworthy that only a small number of reputable and critical people seem to suffice to convince the mathematical community of the validity of a proof — a fact not very different from that operating in social life, but to be clearly distinguished from it by the degree of scepticism which every proposal of a mathematical proof is confronted with. This latter is the point we can take into educational situations, namely the necessity of fostering a rational and critical discussion of mathematical content among students and teachers.

But the history of the acceptance of Faltings' theorem also reveals some striking differences between the two social systems: the mathematicians as a scientific community and the teacher-pupils-school community. The first difference is in the difficulty of the materials dealt with While actual mathematical research work is of immense complexity in its subject structure, school mathematics theorems are comparatively easy to prove from the mathematical point of view Their complexity — for the pupils as well as for the teachers — often arises from embedding the theorems into various inner-mathematical or application-oriented contexts The pupils therefore have to learn — by rational and critical disputation — to select arguments of an appropriate type, to combine these arguments and to think about the criteria for valid inferences So the counterpart of the mathematical complexity of modern proofs lies in school in the complexity of the contexts in which pupils may see the theorems. School mathematics is therefore not to be taken as a simple replica of mathematics as a scientific discipline, though — as we have discovered here — valuable hints for educational purposes are to be found in the observation of the real work of professional mathematicians.

There is a second difference between scientific mathematics and school mathematics concerning the social status of the experts If proofs in school are also aimed at giving the students standards of rational mathematical argumentation, the teachers cannot allow "a few experts" to prove a theorem in the class, while the other pupils "are confident " The goal of settling an understanding of what mathematical communication is cannot be reached this way. As with the first difference, mathematics educators are again confronted with the fact that mathematics in school has many more aspects than those of the content itself. There is more to proofs in school than merely to become convinced of a certain theorem But, nevertheless, school mathematics remains mathematics and should therefore also reflect the work of mathematicians

## 6. Fermat's last theorem

By Faltings' theorem it is now known that the equation $x^n + y^n = z^n$ can only have a finite number of solutions in integers $x, y, z$ if $n \geq 3$ This year some attempts have been made to prove the even stronger

> *Fermat's Last Theorem:*
> There are no integer solutions of the equation
> $x^n + y^n = z^n$, for all $n \geq 3$

The discussion of these proposals for a proof also reveals the social dimension in the acceptance of proofs. In March 1988 reports in newspapers announced that a Japanese mathematician had eventually succeeded in proving Fermat's last theorem Newspaper reports (e g [LE] in German) also pointed out that mathematicians were still working on a check of that proof Two months later readers were informed that the proposed proof had turned out to be incorrect. At about the same time the newsletter of the Mathematical Association of America published an article under the heading "Furor over Fermat," saying that a further possible line of argumentation, via elliptic curves, may eventually be successful in establishing Fermat's last theorem (see [CI]). This proof also is at the moment far from being accepted by the mathematical community.

It seldom occurs that the mathematical public — in this case an even wider public — witnesses a more or less explicit discussion of a mathematical proof. However, for the details one is again referred to insiders. But in this case there is a well-documented public discussion, and the theorem in question is in its content and history so easy to grasp, even for novice mathematics students. One should therefore try to speak about this example in more elementary mathematics courses too. Such a discussion should concentrate on the social issues, not on the mathematical details. This should give a more adequate picture of the processes of acceptance of a proof than stressing only the restricted view, that proofs are chains of formal inferences.

## 7. Conclusions

The remarks made above on some aspects of the disputes about some recently tackled major theorems have shown that G. Hanna's "positive motivators" can indeed be identified. But the convincing-argument factor must be well differentiated. It is somewhat like a *sine qua non* condition and should therefore head all the other social factors. After that the most important aspect is the formulation of a proof in a language which is appropriate to ensure a rich communication between the author and the mathematical community as well as within the mathematical community itself. I will call this the "language-factor" and reformulate G. Hanna's proposal as follows:

> The process of acceptance of a proof by the community of mathematicians is initiated by the proposal of a convincing argument by an accepted member of the mathematical community, and by a careful check of the argumentation by the experts in that field. But then the existence of some combination of the understanding-, significance-, compatibility-, reputation-, and language-factors is necessary to ensure the final acceptance of the proof.

Our observations of the processes of acceptance of the proofs of some recently tackled theorems have led in the previous sections to some consequences for mathematical instruction. The main point is that in the light of these observations mathematics appears as a dynamic process, driven by the creation of new arguments and guided by critical discussions. Hence, for educational purposes, fostering a discussion-oriented and informal approach to mathematical proof, as opposed to stressing only its formality and rigor, should be espoused in order to draw a more adequate picture of our subject, mathematics. But one should also be conscious of the fact that the problem of proofs in school mathematics has many more facets to be kept in mind.


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## References


[AH1] K. Appel and W. Haken: Every planar map is four colorable (Research announcement). *Bull. AMS* 82:711-712 [1976]

[AH2] K. Appel and W. Haken: The four color proof suffices. *Math. Intelligencer* 8:10-20 [1986]

[AS] M. Aschbacher: The classification of finite simple groups. *Math. Intelligencer* 3:59-65 [1981]

[BE] P. Bender (Ed.): *Mathematikdidaktik: Theorie und Praxis. Festschrift für Heinrich Winter.* Berlin: Cornelsen 1988

[BL] Sp. Bloch: The proof of the Mordell conjecture. *Math. Intelligencer* 6:41-47 [1984]

[BR] L. de Branges: A proof of the Bieberbach conjecture. *Acta Math.* 154:137-152 [1985]

[CI] B. A. Cipra: Fermat's last corollary? *Focus*. Newsletter of MAA. 8(2). 2/6 [1988]

[ED] H. M. Edwards: *Riemann's zeta function.* New York & London: Academic Press [1974]

[FA1] G. Faltings: Endlichkeitssätze für abelsche Varietäten über Zahlkörpern. *Invent. Math.* 73:337-347 [1983]

[FA2] G. Faltings: Die Vermutungen von Tate und Mordell. *Jber. Deutsch. Math.-Verein.* 86:1-13 [1984]

[FK] O.M. Fomenko and G.V. Kuz mina: The last 100 days of the Bieberbach conjecture. *Math. Intelligencer* 8:40-47 [1986]

[HA] G. Hanna: *Rigorous proof in mathematics education.* Toronto: Ontario Inst. Stud. Educ. [1983]

[KI] A. Kirsch: Aspects of simplification in mathematics teaching. In: *Proceedings of the 3rd Int. Congress Math. Educ.*, Karlsruhe [1976]

[LE] L. Levantis: Mathematische Nuß geknackt? Japaner fand vermutlich Beweis für "Fermats letze Theorem". Süddeutsche Zeitung Nr. 61/1988, March 18 [1988]

[MOH] N. Movshowitz-Hadar: Stimulating presentation of theorems followed by responsive proofs. *For the Learning of Mathematics* 8(2): 12-19/30 [1988]

[MW] G. Müller and E. Ch. Wittmann: Wann ist ein Beweis ein Beweis? In: [BE], 237-257 [1988]

[NE] M. Neubrand: Speaking about mathematics in the classroom. In: J.A. Dossey (Ed.): Papers presented at ICME-6 Budapest. Action Group 6, to appear

[SCH] W. Schwarz: Über einige Probleme aus der Theorie der Primzahlen. *Sitzungsberichte Wiss. Ges. Johann-Wolfgang-Goethe-Universität Frankfurt* 21(2): 59-118 [1988]

[ST] H.-G. Steiner: TME: an introductory talk. In: [STAL], 16-32 [1984]

(STAL] H.-G. Steiner et al. (Ed.): *Theory of mathematics education.* IDM Occasional Paper 54, Bielefeld: Inst. Didakt. Math. [1984]


## Editorial note

Readers are reminded of Gila Hanna's article, "More than formal proof," in FLM Vol. 9 No. 1.