Chapter 5
Arguing Around Mathematical Proofs

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In the history of Mathematics, you can notice that during some epochs no damage is done to the truth of particular propositions, but their systematic linking has changed because of the rapprochements allowed by new discoveries.
(Lacroix, 1797, Preface)

Any theory of argumentation will certainly have to pronounce on the status of mathematical proofs. Formally, a typical proof is a string of regular inferential steps between statements. So, it can be seen as a string of arguments since each statement, except the first one, is supported by the reasons offered by previous statements. But is this enough to claim that argument practically matters to proof or even that proof is a kind of argument? Some authors have argued that, in spite of this strong family resemblance, mathematical proofs are not arguments: hence the temptation—and its methodological consequences for a theory of argumentation—to keep the study of mathematical proofs away from the study of arguments. But the exclusion of mathematical proof from the field of argument theory has also been disputed by philosophers who argued, mostly on practical grounds or by stressing that the border of mathematics has changed over time and place, that the use of such a proof is not incompatible with an argumentative practice (Corfield, 2002; Finocchiaro, 2003; Dove, 2007; Aberdein, 2011). This paper aims at supporting this view, and claims that a theory of argumentation should encompass at least the proof process and would benefit from looking beyond the idealized situation of the presentation of the proof to an audience of expert peers.

We shall begin with a discussion of the views of two authors—Perelman and Johnson—who claim, on different grounds, that proofs are not arguments. The

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second part of the study asserts that, even when you focus only on proofs, the practice of mathematics is full of arguments, different in styles and goals but amenable to a classification based on their temporal relation to the publication of the proof.

5.1 A Proof Is Not an Argument

Perelman is well known in the folklore of argumentation studies as one of the two founding fathers of the mid-twentieth century renewal of academic reflection on argumentation, Toulmin being the other. His very project of a "new rhetoric" (now 50 years old) is based on a radical distinction between proof and argument that will be discussed first. Then we shall turn to Ralph Johnson, a leading figure in the informal logic movement. Johnson's position is also based on a radical distinction between proof and argument. But his view is less systematic than Perelman's, for (mathematical) proof and everyday argument would belong to what Johnson calls the "spectrum" of applications of the term "argument".

5.1.1 Argument and Proof, the Two Poles of Perelman's System

Perelman's starting point was his dissatisfaction with the principle held by some philosophers—especially in the context of logical positivism[1]—that (formal) logic could provide a general theory of human reasoning and a suitable tool to analyse human inferences. He did share the positivist idea that logic is convenient for science, but denied that beyond the area of logic, mathematics and empirical sciences, human thinking is fuzzy and even irrational since it does not lend itself to logical analysis. As a jurist, Perelman could not discard value or moral judgments as irrational since they are an essential part of legal argumentation.

A consequence of this criticism is that he and Lucie Olbrechts-Tyteca, in their famous *New Rhetoric: A Treatise on Argumentation* (1958), defined and investigated a whole domain spreading beyond the field of hard sciences but precluding logic, mathematics and reasoning used in the empirical sciences. The way of reasoning specific to that domain is even characterized in contradistinction to the style that Perelman, like the positivists, held to be typical of science. The *Treatise*, like many of Perelman's other writings, states this point explicitly, its opening pages being devoted to this principle which justifies the whole enterprise:

> Formal logic constituted itself as the study of the means of demonstration used in mathematics. But a consequence is that its scope is limited, for all that is ignored by the mathematicians is foreign to formal logic. Logicians have to add a theory of argumentation to their theory of demonstration (Perelman and Olbrechts-Tyteca, 1958, 13).

[1]The term "positivism" is used loosely. Deeper philosophical subtleties are not essential for the point made here.

It is worth noticing the shift made here from logic to mathematics. The authors seem to think that mathematicians (rather than logicians?) frame the scope and program of formal logic. This tendency to take both sciences as very close to each other and, sometimes, to identify them was not unusual among some philosophers of Perelman's era.

But it is in Aristotle that the authors of the *Treatise* found two important ideas for their program. First, they explicitly borrowed a distinction between analytical, dialectical and rhetorical arguments that seemed to confirm their idea of a sharp contrast between logic and argumentation. They also granted that different domains of activity require different kinds of reasoning. As the master said in his *Nicomachean Ethics* (Aristotle, 2009, I, 1094b 25), do not expect a probable reasoning from a mathematician and a proof from a rhetorician.

This fundamental dichotomy between logico-mathematical reasoning and unscientific arguments that the *Treatise* qualified as rhetorical, after hesitating over dialectical, is supported by a whole set of other distinctions systematically used in an exclusive way. Let us mention, for instance, demonstrative/non conclusive, analytic/dialectical, true/persuasive, certain/probable, or rational/reasonable. Although Perelman does not explicitly use such an expression, his view of logic and scientific reasoning is "purely semantic", in the sense that it lacks any pragmatic dimension brought about by human interactions. Hence, in science, the only concern would be the truth of propositions. On the contrary, argumentation "never happens in a void. For it presupposes a contact between the minds of the orator and of his audience: a speech must be heard, a book read, otherwise they would not act" (Perelman, 1977, 28).

For sure, the *Treatise* and the *Realm of Rhetoric* are logic free. The most logical arguments discussed therein are the ones dubbed "quasi-logical". What makes them so? They appeal either to "logical structures" (contradiction, total or partial identity, transitivity) or to "mathematical relations" (whole/part, smaller/bigger, frequency ratios). Hence, although they are driven by logic or mathematics they keep being arguments since they are inconclusive, a distinctive feature of perelmanian arguments (Perelman and Olbrechts-Tyteca, 1958, 261). They look formal but are not and should not be taken as such. Their misplaced prestige comes from the prestige of logic or mathematics. The *Treatise* adds that "explicitly based on mathematical structures" they were cogent "in the old days, and especially among the Ancients", but now "just like formal logic allowed separating demonstration and argumentation, the development of the sciences certainly helped to limit their use to the field of calculation and measurement".

Besides providing an example of the Perelmanian confusion between logic and mathematics, this quotation confirms that the distinction between demonstration and argumentation overlaps precisely with those between conclusive and inconclusive and between scientific and practical reasoning. Although the introduction of the *Treatise* announces that examples of arguments will be borrowed from the "human sciences", a careful reading shows that no argument or, to be more careful, almost no example of argument is borrowed from any science.

Does that mean that the authors held “scientific practical reasoning” or “the logic of an unscientific discourse” to be inconsistent expressions? It is not that simple. Although they see argumentation as essentially pragmatic (interactive) and science as purely semantic since “demonstrative and impersonal” (Perelman, 1977, 28), they make some general comments about scientific meetings between peers. It is worth noting that these remarks are closely connected with their discussion of the “universal audience”, a notion that blurs their numerous dichotomies for “ultimately, the rhetoric efficient for a universal audience would be the one handling only the logical proof” (1958, 42).

Perelman’s notion of a universal audience is a bit fuzzy. It reflects his methodological hesitation between an empirical approach and a normative one, more or less inspired by Kant. The universal audience is neither the whole of humanity nor “any rational being”: in the *Treatise* it is “at least, adults and normal men” (1958, 39) and in the *Realm of Rhetoric* “at least, its competent and reasonable members” (1977, 32). So, let us ask two sets of questions. First, to the *Treatise*: Why adults? Isn’t “normal” enough? Wouldn’t normal teenagers or even children make the cut? Then, to the *Realm*: Doesn’t competence imply it is used reasonably in the field concerned? In other words, if someone becomes unreasonable (in her usual field of competence), isn’t it a good reason to think that her competence has gone astray or has been cancelled for a moment or a joke?

The fact that the very notion of competence normally implies its reasonable use certainly explains why competence alone appears in Perelman’s discussion of the cogency of arguments. According to him it depends only on two factors, usually held to be independent: the size of the audience and its “quality”, a notion he takes to be closely related (if not identified) with competence.

Who is entitled to evaluate the cogency of an argument? For Perelman, as far as argumentation is concerned, it is up to the audience to decide (1958, 32). This is why an evaluation made by a (the?) universal audience would have the last word since it is the widest competent audience. But this does not disentangle the problem of the balance between number and competence, a problem especially important in science where audiences are often small and competent.

According to Perelman, competence comes first for two kinds of audiences.

One is the elite audience. It is small because it can listen to arguments that do not convince many people. Hence its tendency to disqualify opponents, claimed to be stupid or not normal, when it sees itself “endowed with exceptional and infallible means to get knowledge” (1958, 44). Sometimes, an elite audience is widely praised by outsiders. Other times it is taken as ridiculous when compared with “the number and the intellectual value” of the people who have been rejected.

The second is the specialised audience. A paradigmatic example is a peer audience listening to a scientific talk. It is often assimilated to the universal audience by the speaker “supposing that all men with the same training, the same competence and the same information would grant the same conclusions”. But this is just the opinion of the speaker and the authors of the *Treatise* do not say if this view is shared by the members of the audience or what happens if the audience, or part of it, disagrees with the orator. Here again, Perelman seems to believe that scientific proofs cannot be controversial since they are "logical". Appealing here to the universal audience is a comfortable way to escape the pragmatic problem raised by the opposition of competent members of the audience. Finally, even when Perelman stresses the variety of actual audiences and the fragility of the link between the orator and her audience, his conception of scientific reasoning precludes the possibility of an argument among scientists, especially in mathematics.

5.1.2 Johnson: The Autonomy of Experts

Ralph Johnson's conception of argument is not based on an opposition between logic and dialectic or rhetoric, nor associated with different fields of knowledge or specific epistemic attitudes as in Perelman's system. Rather, in *Manifest Rationality* logic and dialectic are federated into a single entity, for Johnson contends that an argument has two faces. One of them is what he calls the illative core, namely a discursive structure where reasons support a thesis. But he claims that this logical aspect cannot account for the pragmatic dimension of an argument. Therefore, it has to be supplemented with a second face named the dialectical tier. It must be added that this does not mean that an argument is always put forth in a context of divergence of opinions. It suffices that "the conclusion is at least *potentially* controversial" (Johnson, 2000, 206).

The notion of dialectical tier is closely connected with the practical behaviour of the arguer. For it is the way she discharges what Johnson calls her "dialectical obligations", bound to the illative core of the argument, that makes her rationality manifest. So, the utterance of an illative core is not enough: it must be accompanied by a convenient dialectical behaviour to be evidence of rationality (2000, 164). These dialectical obligations—which have been widely discussed—are not limited to the critical attitude of examining and anticipating objections and considerations running against the conclusion of the argument, but also take into account misguided or irrelevant criticisms, because "to ignore such criticisms compromises the appearance of rationality" (2000, 270).

The main point for us is that the dialectical tier is the extra part which makes the main difference between a mathematical proof and an argument, for "no mathematical proof has or needs to have a dialectical tier" (2000, 232).

Is that true? To illustrate the potential of controversy of an argument Johnson says: "There are those who take a different view; there are adversary views; there are typically well-known objections. [. . .] An argument that does not take into account these dialectical realities is in some important sense incomplete" (2000, 206). I shall argue below that unless you presume that mathematical proofs are complete they often have to deal with these kinds of dialectical realities. Some proofs are more than "potentially controversial" and even if Johnson is right that, sometimes, they do not need a dialectical tier, they often do urgently need an explicit one. To be fair, this reading of Johnson should not be exaggerated for in other places he supports a spectrum theory claiming that the word argument can be applied to scientific theories and proofs (2000, 168). But he insists that the core notion, the prototype of the concept of argument, is outside of the scientific field. And he has a social epistemic comment about the distance between proof and argument: "The proof that there is no greatest prime number is conclusive, meaning that *anyone who knows anything about such matters* sees that the conclusion must be true for the reasons given" (2000, 232, my emphasis).

Let us take this remark about mathematics and competence as an opportunity to make a comparison with Perelman's position, which relies on a sharp contrast between his semantic view of science—in short, science is impersonal, essentially related to the world and only concerned with truth—and a conception of argumentation that is essentially audience relative, hence pragmatic and mostly concerned with agreement. We know this contrast is crucial for the intellectual and social independence of Perelman's vivid rhetorical realm spreading beyond the stark world of logic and calculus. And we remember that it is only in the context of an exchange between peers that Perelman and Olbrechts-Tyteca made a timid step toward the notion of scientific argument. But they finally canceled the possibility of a scientific argumentative idiosyncrasy by immediately calling upon the normative principle of the expertise of scientists which led them to assert that such an audience "is generally considered by the scientist, not as a particular audience, but as the true universal audience" (Perelman and Olbrechts-Tyteca, 1958, 45).

Johnson does not share Perelman's principles and introduces a concept of argument which is less exclusive. But when he is concerned by an audience consisting of "*anyone who knows anything about such matters*", his view becomes somewhat similar to Perelman's. First, a necessary condition to make an argument a candidate to become a proof is to get rid of the idiomatic and epistemic differences giving rise to the dialectical tier. Johnson's "anyone knowing anything about such matters" makes a quite acceptable equivalent to Perelman's "competent audience". Then, Johnson's quasi-pragmatist statement that a proof is conclusive when "everyone recognizes the proof as a proof and as a result, there is no longer any debate about whether the conclusion is true" (Johnson, 2000, 232) reminds us of Perelman's statement that "ultimately, the rhetoric efficient for a universal audience would be the one handling only the logical proof". A vexing question is: could there be an ultimate agreement over a proof that would not amount to a logical proof? Perelman answers no and Johnson answers that a proof is conclusive when any expert "sees that the conclusion must be true for the reasons given" (2000, 232), a claim reminiscent of Aristotle's thesis that scientific syllogisms are not open to discussion.

5.2 A Mathematical World Besides Proofs

So far, our discussion about the possibility of mathematical arguments has focused on the status of mathematical proofs and their dialectical potential. We have just seen that most of Perelman and Johnson's objections are based on an antagonism between their concepts of argument and features, for instance necessity, that they think typical of mathematical proof. Perelman's position goes even further since he uses this opposition as grounds for excluding the whole domain of the logico-mathematical sciences from the realm of argumentation.

This extreme position, although reliant on considerations about proof, forgets that proof is not all there is in mathematics. According to ethnomathematics, for instance, other cultures, especially traditional cultures having no writing, developed mathematical ideas and skills which are out of touch with the activity of proof and the focus put on it by Western professional mathematicians. In this case, mathematics is embedded in activities or general conceptions which are not traditionaly identified as mathematical, such as religious or metaphysical ideas, games, administrative tasks, economical activities, kinship relations (Ascher, 1991; D'Ambrosio, 2001). Accordingly, although proof is important in mathematics, at least for Western mathematics, it may not be a necessary condition for defining an activity as mathematical. As we shall see, loose notions of abstraction and systematicity seem to have been more important as organizing concepts for Ancient Greek mathematics than necessity and deduction. So, the extension of the field in which mathematical argumentation could be sought is far from well-defined, especially when the distinction between pure and applied mathematics is blurred (as it was for Western mathematics until the mid-nineteenth century). This matters for the very notion of a mathematical argument.

However, my focus will stay on mathematical proof since it is the core of Perelman's and Johnson's objections to the notion of mathematical argument. And, as I subscribe to their main insight that argumentation is essentially pragmatic, I suggest that it is not by looking at proof itself but at its use that its argumentative dimension can be illuminated. This is why, rather than wondering where arguments can be found in a mathematical proof, I shall address the question "When are mathematicians arguing?". The leading idea is that a proof looks like a totem-pole and that arguments can be found if you look at people bustling around it, especially at three typical moments: before, during or after its construction.

5.2.1 Before the Proof

Any beginner in mathematics knows that the trouble with proofs is that they have to be found out. It suffices that the master says: "Show that p" to infer and believe first that p is true (but beware of devilish teachers who do not hesitate to ask for the proof of a false p), second that there is a path to the solution (although there may be none), third that it can be discovered (although it may be impossible for a human mind).

Expert mathematicians usually have no omniscient master anymore except those who, like Plato, Leibniz or Cantor, live in the shadow of a mathematician God. When they try to show that p, they (usually) try to answer an open question which, sometimes, depends on definitions and methods that they have to frame or reframe. The search for a proof is an important part of the activity of professional mathematicians, maybe the essential part.

Yehuda Rav expressed a similar view by saying that there is a way to escape foundational problems, including the priority given to axioms. It suffices to "realise that *proofs rather than the statement-form of theorems are the bearers of mathematical knowledge.* Theorems are, in a sense, just tags, labels for proofs, summaries of information, headlines of news, editorial devices" (Rav, 1999). But the same move can be applied to proofs. Even if mathematical truths and proofs exist somewhere in a Platonist heaven, to contemplate them may not be the highest good. You may feel the compulsive demand to lay your hand on a methodological scale and, finally, think that the supreme mathematician is the one who finds the scale to the proof. Mathematical know-how is certainly praised by mathematicians as much as the theoretical knowledge of theorems and proofs.

Time spent hunting for a proof is a good time for arguments. First, you have to be convinced that trying to show that p is worthwhile. This granted, methodological questions matter and arguments for or against such and such an approach come to the forefront of the mathematician's activity. In the last century, philosophy of science labeled as "context of discovery" a notion reminiscent of what ancient philosophy and rhetoric called "analysis", understood as the art of invention (*ars inveniendi*), that is of discovering convincing arguments or clever starting points for a proof. This analytical stage offers many opportunities to argue. For instance, in March 1847, two rival French mathematicians, Cauchy and Lamé, claimed to have found a proof of Fermat's famous Last Theorem (Singh, 1997). Each of them published independently part of his proof after having left a complete proof in a sealed envelope at the French Academy of Science. Two months later, after having read their partial proofs, Kummer, a German mathematician, criticized their approaches. He thought they were not necessarily false but likely on a wrong path. Lamé granted Kummer's objection but Cauchy argued that his approach was less exposed to Kummer's criticism which he thought was not clearly demonstrated. Cauchy resisted for a few weeks but finally made up his mind and stopped publishing on this topic.

Lakatos's famous book (1976) about Euler's conjecture provides many other examples of arguments produced during the gestation of a proof, especially in Chap. 5 concerning hidden lemmas working as hidden premises and the fallacious appeal to what "everybody knows" or "any expert in the field knows". The history of enduring problems—and this is not unique to mathematics—is often full of arguments about methods, definitions or provisional solutions. Some of them even aim to renew the very problem at stake. Famous examples are also provided by the cantorian "continuum hypothesis" (Hallett, 1984).

Descartes's confessions about his intellectual formation and the way he came to look for a *mathesis universalis* shed some more light on the arguing process prior to the proof and the mythical status of the analysis, at least since Ramus's time (Timmermans, 1995). After stating his fourth "rule for the direction of the mind" he explains that when he began to study mathematics he read "almost everything of what is usually taught by the authors dealing with it" (Descartes, 1908). But he felt unsatisfied because "they did not show clearly enough to the mind why it [the proof] is done that way and how its invention was made". Descartes was certainly not very interested in the mere checking of the validity of a proof. He even claimed that after having “tasted” mathematics, most “men of talent and knowledge” are usually not interested anymore by such a “childish and trivial” activity. Men of talent—like him—look for something more exciting, the path to a discovery. This tends to confirm Rav’s idea that theorems are not very important and are just tags, but also that the search for proofs is more exciting for mathematicians than the proof itself. Complete and stabilized proofs are themselves milestones, totem-poles, as I said, which have become “trivial” except when they stimulate new challenges and new arguments.

Descartes shared with many of his contemporaries a dream about the future of mathematical arguments: he believed that the legendary analysis of the Ancients opened the path to what he called a “true mathematics”. This famous *mathesis universalis,* whose obviousness and simplicity should convince any rational mind, would avoid the stumbling process of critical dialectic by opening a direct path to sound demonstrations.

A new Perelman, while still borrowing from Aristotle’s classification of reasonings, could object that in spite of a partial homophony, analysis, understood as the search for a proof, should not be confused with the analytical proof that may emerge afterwards. He could claim that arguments preliminary to the proof pertain to metamathematics, for they are not so much the proof itself as about it. Hence they should be left in the surroundings of mathematical knowledge rather than incorporated into it. But aside from the slightly paradoxical fact that Descartes’s “true mathematics” would then also belong to these surroundings, a reply to our new Perelman is that a sharp distinction between language and metalanguage or theory and meta-theory presupposes a clear-cut, if not formalized, language, theory or discipline. But this is precisely the stance that opponents to the perelmanian dichotomies between argument and proof or scientific and non scientific reasoning do not want to take.

5.2.2 During the Proof

When a candidate to the status of proof has been established, the time has come for its public and critical evaluation by competent experts who may not have participated in its discovery.

If we assume that a proof is the linguistic expression of a reasoning which is the achieved and flawless production of an expert mind, as expert as any other expert mind, the distinction between producer and receiver of the proof is only a contingent matter. This presumption of equality or equivalence of expertise seems to lie behind the opinions of Johnson and Perelman when they claim that proofs do not need dialectical (Johnson) or rhetorical (Perelman) support. An expert needs nobody to grasp a proof, otherwise she is not an expert.

This view leads to the idea that anything added to a minimal version of a proof is at most mere decorum, a rhetorical ornament, a “colour” to borrow Frege’s word (see Dubucs and Dubucs, 1994). This adjuvant would bring about only side effects since the proof is supposed to be self-sufficient to be convincing. If the very notion of argument implies a human interaction—let us say a dialectical tier—a perfect proof (i.e. a proof raising no critical comment, no request for explanation and so forth) certainly differs from an argument. This is probably the idea at the core of Johnson's view.

But from a practical point of view, if you drop the normative hypothesis of equal and perfect expertise such a situation seems to be the exception rather than the rule. It is likely that perfect proofs and equal expertise exist mostly in "the world of novels" as people said in Descartes's time.[2] So, let us have a look at the conditions of our starting assumption.

How do you know that a reasoning is flawless? The correction of a mistake of reasoning is likely to come from someone other than the proponent of the proof. This offers the opportunity for a refutation. Moreover, the conversion, not to say the translation, of the reasoning into its linguistic expression may produce specific defects like confusion or equivocation. Like anybody, a mathematician may fail to express clearly a correct reasoning and this may be another occasion for an objection.

The works of Cauchy provide several examples of the case at hand (Belhoste, 1985). Besides his own innovative works, Cauchy criticised and renewed several aspects of the works of his masters. In his *Cours d'Analyse* he explains that he tried to give to his new methods "all the rigour that one can demand in mathematics so that I never rely on the reasons based on the generality of algebra". The trouble that he had with these "algebraic" reasons was that they lacked rigour: "although commonly granted ... they can only be considered as inductions which can sometimes make you feel the truth, but fit badly with the celebrated exactness of the mathematical sciences" (Cauchy, 1821, Introduction). Among these "new methods", there is the introduction of a concept of continuity based on the concept of limit and breaking with the previous notions, especially Euler's. Cauchy's definition states that "$f(x)$ will stay continuous for x taken between two given limits, if, between them, an infinitely small increase of the variable always produces an infinitely small increase of the function itself" (1821, 43).

Two years later, some of his critics are more explicit in the *Résumé* of the lectures on the calculus that he gave at the Ecole Royale Polytechnique. He argued against the systematic use of Taylor's formula that was common among mathematicians because "although the famous author of the *Mécanique analytique* [Lagrange] based his theory of derivative functions on this formula ... most geometers now grant the uncertainty of the results that can be obtained when using diverging series" (Cauchy, 1823, Avertissement). Cauchy's point was that Lagrange's approach was limited, owing to his ignorance of the notion of interval of convergence.

[2]"Mostly", for a friend told me that a celebrated mathematician visiting his university wrote his lines of proofs on the board without uttering a single word.

The new concepts introduced by Cauchy, and the demonstrations that they allowed, have framed part of the mathematical doxa since the beginning of the nineteenth century and they still inspire the corresponding notions and demonstrations taught in our schools. They are real mathematical milestones.

But unfortunately, in spite of its alleged "rigour", Cauchy's concept of continuity was not rigorous enough. It blurred the distinction between continuity and uniform continuity that our contemporary definitions, using quantifiers, make clear. And this confusion led Cauchy to a confused demonstration of the mean value theorem which required an argument to be rectified.

Up to now, we have supposed that the mathematical audience is composed of experts and that arguments occur because of mistakes, confusions or wrong moves made by the proponent. But some of them can come from the audience. A sound and correctly expressed proof can be misunderstood by some members of the audience, unless you again presume an idealized situation of communication with an audience as perfectly competent and rational as Perelman's universal audience. For instance, a member of the audience may object that the proof has not been achieved because a formula of the demonstration does not follow from the previous formulae. This is not an uncommon criticism and Cauchy's reply to Kummer can be seen as an example.

Standard logical derivations progress by making moves as short as possible, that is, in accordance with only one acknowledged rule. But many mathematical proofs do not comply with these canonical requirements of logic even if most of them can be rewritten in a format that makes their checking purely mechanical. Unfortunately, this option was not open for past mathematics and is still often practically impossible. Accordingly, a gap between two formulas may look hardly intelligible for the audience although it seems clear for the proponent. According to Johnson, the proponent will manifest her rationality by supplementing her proof with answers and replies to questions and objections from failing experts.

On this account, the outstanding romantic mathematician Evariste Galois seems to have had a hard time making his rationality manifest. But the problem may have come from his audiences. We know that the reason of his first failure when he tried to be admitted to the French Ecole Polytechnique was that his answers were too abrupt and unclear. But things got worse, for a few years later, in 1830, he had the same problem when, to win the Grand Prix of Mathematics of the Science Academy, he submitted an already improved version of his seminal *Mémoire sur les conditions de résolubilité des équations par radicaux*. In the preface to Galois's works that he published in 1846 after having deciphered his last writings, Joseph Liouville wrote that: "In their report, the Commissioners reproached the young analyst for the obscurity of his paper, and indeed, this reproach, already made to his previous communications (as we learnt from Galois himself) was justified. An exaggerated desire of brevity was the cause of this imperfection that you should avoid, especially when you deal with the abstract and mysterious topics of pure Algebra." Liouville alludes here to Poisson's comment that Galois's paper was unclear and insufficiently developed to be accepted. But more than a century later, in a new preface, another celebrated mathematician, Jean Dieudonné, expressed his admiration for Galois's conciseness that he praised much more than the "laborious presentations that his immediate followers felt obliged to make" (Galois, 1997).

5.2.3 After the Proof

Let us suppose now that the proof is professionally settled, that is accepted by acknowledged competent experts. This is not the end of the story, for mathematics is not the exclusive property of a handful of industrious shareholders but the common good of an open society.

For centuries, mathematics has been familiar to pupils from the elementary to, sometimes, the academic level. From a less institutional point of view, the didactical use of a proof can be tentatively defined as any situation in which the proponent has to convince people who stand on a lower epistemic footing, if we grant that the notion of "lower epistemic footing" is clear. A didactical situation is then similar to the previous case of an epistemic asymmetry due to failing expertise. In a didactical context, it would be harsh to qualify a very common inability or failure to take up the inferential proposal made by a proof as a defect or lack of rationality of the tutee. Nor is it always the sign of a lack of propositional knowledge, for he may know that p and grant q but not "see" that q follows from p. Even limited to a field, logical omniscience is rare and Perelman's requirement of a universal *adult* and/or *expert* audience is a convenient way to escape any didactical situation coming from epistemic or rational discrepancies.

Although they are commonly associated with pupils or students, didactical situations are not uncommon among scientists, especially since last centuries' history has taught us not to take scientific axioms or principles as obvious. This marks a shift from scientific to dialectical and even didactical argument, at least if we go back to Aristotle's classification of arguments.

In the *Sophistical Refutations* (1955, II, 165a–b) Aristotle claims there are four kinds of διαλέγεσθαι λόγων, an expression generally translated as "argument (or reasoning) involved in a discussion". But it can also be translated as "dialogue" or "dialectic", in the broad sense of "talking together". Although Aristotle here uses neither the word "syllogism", nor "enthymeme", "argument" seems a good translation since the Philosopher stresses that these discourses have premises. And these premises make the main difference between the four kinds of argument. Dialectical arguments are rooted in an *endoxa*, a common opinion. Critical arguments start from premises accepted by the answerer but also granted by the arguer whose discourse aims at "showing that he knows". Eristic arguments reason from premises that appear to be generally accepted but are not so. And finally, didactical arguments do not reason from the opinions of the answerer but from "principles appropriate to each μαθήματος". This word, μάθημα, is usually translated by "branch of knowledge" or "discipline" but it also means "lecture" or "lesson", two notions often related to an educational context. It is also close to μαθηματιχος which means "someone who studies" or "relative to a field of knowledge" and, of course, to μαθηματιχά, usually translated by "mathematics".

For Aristotle, what makes something "mathematical" is the way you consider it. It depends on the properties dropped in the process of abstraction and the principles finally taken into account, some of them being proper and some others not proper to the said science (Aristotle, 1976, I, 10, 76a, 35–40). So, a science is mathematical in the broad sense of "systematic". And a didactical argument is a deductive argument based on the principles of a field of knowledge, of a discipline. If we follow the idea that this kind of argument is based on the principles of the discipline and not on the opinions of the audience, its premises may not have the typical property of Aristotelian scientific arguments announced in the *Posterior analytics* (1976, I, 2, 71b, 20) namely "true, primary, immediate, better known than, prior to, and causative of the conclusion". To put it shortly (perhaps too shortly) the premises of a didactical argument only have to be granted, they are not necessarily "believed" (Dufour, 2011).

Granting premises is certainly the most accommodating way to share them, but it is sometimes a submissive one. Moreover, a pedagogical tragedy is likely to happen if the tutee is accommodating enough to grant a logical step because of the authority of the field instead of acknowledging its necessity or its strength. This is the case with students who learn a proof by heart to pretend they know and understand it, a situation not uncommon but notoriously fallacious.

Avoiding such cases is probably one of the reasons why there is often a long distance between the original proof and textbook or oral classroom versions, full of hints aiming at making it accessible to a wider audience. Although there may be several demonstrations of a theorem, we usually do not hesitate to qualify seemingly different proofs as versions of the same one. This allows to modify or rephrase the initial version to make it more explicit or to make its necessity more salient for people who are not experts. In my opinion, these strategic rewordings belong to the field of mathematical argumentation. Some people would certainly prefer the word "rhetoric", even if these manoeuvres aim at enhancing the process of rational persuasion, for instance by paraphrases which are not mere rhetorical embellishments of the initial proof. Some of these manoeuvres may fall under Johnson's notion of dialectical tier but only if you grant, against him, that mathematical proofs may need one.

The fate of a proof resembles the pragmatist conception of truth for although a proof is sometimes provisionally satisfactory it will attain perfection only at the end of the enquiry that is when nobody or, more faithfully to Peirce (1871), when no member of the community of inquirers will complain about it. Are didactical efforts to make it intelligible to less expert people a step in this direction? A pragmatic pragmatist could answer no, because the more you open the community the more remote perfection becomes. Perelman's small elite communities seem to be forerunners of Peirce's paradise, but unfortunately they are mostly examples of wishful thinking. A less eager pragmatist would answer that a larger paradise is a better paradise; therefore, the teaching of mathematics widens the mathematical community, the possibility of criticism and the expected paradise. But the history of mathematics also shows that conceptual distinctions and the proofs that follow are not always more intelligible for non-experts and so, are open to argument. This seems to have happened when Cauchy had some trouble with his students at the Ecole Polytechnique, although political considerations also mattered in this particular case.

The logicist episode also provides good examples. Frege, like Peirce, complained that natural languages lacked the rigour and precision required to make them decent places to harbour arithmetic. Hence, Frege's investigation in the *Begriffsschrift* (1879) of a new and purely logical language which would allow the rewriting of arithmetical proofs with no logical gaps and no appeal to the benevolence of intuition. This is the core idea that Frege and his followers perceived as improving mathematical proofs. But it required a reformulation, if not a revision, of previous demonstrations.

Unfortunately for the logicist program, not everybody agreed. Numerous arguments were raised, which were not isolated conflicts but a battle in the wider debate about the foundations of mathematics. This affair was neither purely mathematical, i.e. involving only professional mathematicians and field-dependent concepts, nor "above" mathematics, that is free from mathematical technicalities. Mathematics, logic, philosophy and their practitioners were all concerned by the logicist controversy (Heinzmann, 1986). Henri Poincaré was a famous opponent to the logicist program. Anticipating and denouncing Perelman's confusion of logic and mathematics, he contended that mathematics harbours intuitive principles—notoriously the principle of complete induction—irreducible to the principles of the new logic. For him, logicism was viciously circular. Either it stealthily called to mathematical principles to show their reducibility to logical considerations or it inflated the meaning of the word "logic" by directly incorporating mathematical principles or knowledge (Poincaré, 1905a, 808). The most interesting point for us is that one of his criticisms of logicism and formalism was a didactical objection, for he argued that their claim to improve mathematical proofs failed to make them more accessible. Poincaré ironically wrote that Peano's formal definition of "one" is "eminently apt to give an idea of it to people who would have heard nothing about it" (Poincaré, 1905b, 823), that he "would not advise it [Hilbert's mechanical formalism] to a high school student" who would very soon drop it (Poincaré, 1908, 68) and that logicists had a talent to "define what is clear by what is obscure". However, he did not disagree with any program intended to improve mathematical proofs for he thought that proofs lacking rigour have to be reworked. For him, a most significant improvement in mathematics, is an inductive move towards a greater abstraction and a greater generality. And in this case higher accessibility to beginners is not required. According to him, such an improvement had already begun around the mid-nineteenth century and he stressed that this move towards a kind of formalization was not foreign to mathematics since it was not "logicized" but "arithmeticized" (Poincaré, 1908, 69; 1905a, 32). Right or wrong, this example shows that there is room for argumentation in mathematics not only before and during the proof but also after, at least as long as it can be criticized.

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