Prologue | Historiography and history of mathematical proof: a research programme

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Pour Oriane, ces raisonnements sur les raisonnements

I Introduction: a standard view

The standard history of mathematical proof in ancient traditions at the present day is disturbingly simple.

This perspective can be represented by the following assertions. (1) Mathematical proof emerged in ancient Greece and achieved a mature form in the geometrical works of Euclid, Archimedes and Apollonius. (2) The full-fledged theory underpinning mathematical proof was formulated in Aristotle's *Posterior Analytics*, which describes the model of demonstration from which any piece of knowledge adequately known should derive. (3) Before these developments took place in classical Greece, there was no evidence of proof worth mentioning, a fact which has contributed to the promotion of the concept of 'Greek miracle'. This account also implies that mathematical proof is distinctive of Europe, for it would appear that no other mathematical tradition has ever shown interest in establishing the truth of statements.[1] Finally, it is assumed that mathematical proof, as it is practised today, is inherited exclusively from these Greek ancestors.

Are things so simple? This book argues that they are not. But we shall see that some preliminary analysis is required to avoid falling into the old, familiar pitfalls. Powerful rhetorical devices have been constructed which perpetuate this simple view, and they need to be identified before any meaningful discussion can take place. This should not surprise us. As Geoffrey Lloyd has repeatedly stressed, some of these devices were shaped in the context of fierce debates among competing 'masters of truth' in ancient Greece, and these devices continue to have effective force.[2]

[1] See, for example, M. Kline's crude evaluation of what a procedure was in Mesopotamia and how it was derived, quoted in J. Høyrup's chapter, p. 363. The first lay sinologist to work on ancient Chinese texts related to mathematics, Edouard Biot, does not formulate a higher assessment – see the statement quoted in A. Volkov's chapter, p. 512. On Biot's special emphasis on the lack of proofs in Chinese mathematical texts, compare Martija-Ochoa 2001–2: 61.

[2] See chapter 3 in Lloyd 1990: 73–97, Lloyd 1996a. Lloyd has also regularly emphasized how 'The concentration on the model of demonstration in the *Organon* and in Euclid, the one that

Studies of mathematical proof as an aspect of the intellectual history of the ancient world have echoed the beliefs summarized above – in part, by concentrating mainly on Euclid's *Elements* and Archimedes' writings, the subtleties of which seem to be infinite. The practice of proof to which these writings bear witness has impressed many minds, well beyond the strict domain of mathematics. Since antiquity, versions of Euclid's *Elements*, in Greek, in Arabic, in Latin, in Hebrew and later in the various vernacular languages of Europe, have regularly constituted a central piece of mathematical education, even though they were by no means the only element of mathematical education. The proofs in these editions were widely emulated by those interested in the value of incontrovertibility attached to them and they inspired the discussions of many philosophers. However, some versions of Euclid's *Elements* have also been used since early modern times – in Europe and elsewhere – in ways that show how mathematical proof has been enrolled for unexpected purposes.

One stunning example will suffice to illustrate this point. At the end of the sixteenth century, European missionaries arrived at the southern door of China. As a result of the difficulties encountered in entering China and capturing the interest of Chinese literati, the Jesuit Matteo Ricci devised a strategy of evangelism in which the science and technology available in Europe would play a key part. One of the first steps taken in this programme was the publication of a Chinese version of Euclid's *Elements* in 1607. Prepared by Ricci himself in collaboration with the Chinese convert and high official Xu Guangqi, this translation was based on Clavius' edition of the *Elements*, which Ricci had studied in Rome, while he was a student at the Collegio Romano. The purpose of the translation was manifold. Two aspects are important for us here. First, the purportedly superior value of the type of geometrical knowledge introduced, when compared to the mathematical knowledge available to Chinese literati at that time, was expected to plead in favour of those who possessed that knowledge, namely, European missionaries. Additionally, the kind of certainty such a type of proof was prized for securing in mathematics could also be claimed for the theological teachings which the missionaries introduced simultaneously and which made use of reasoning similar to the proof of Euclidean geometry.[3] Thus, in the first large-scale intellectual contact between Europe

proceeds via valid deductive argument from premises that are themselves indemonstrable but necessary and self-evident, that concentration is liable to distort the *Greek* materials already – let alone the interpretation of Chinese texts.' (Lloyd 1992: 196.)

3 On Ricci's background and evangelization strategy, see Martzloff 1984. Martzloff 1995 is devoted more generally to the translations of Clavius's textbooks on the mathematical sciences

and China mediated by the missionaries, mathematical proof played a role having little to do with mathematics *stricto sensu*. It is difficult to imagine that such a use and such a context had no impact on its reception in China.[4] This topic will be revisited later.

The example outlined is far from unique in showing the role of mathematical proof outside mathematics. In an article significantly titled 'What mathematics has done to some and only some philosophers', Ian Hacking (2000) stresses the strange uses that mathematical proof inspired in philosophy as well as in theological arguments. In it, he diagnoses how mathematics, that is, in fact, the experience of mathematical proof, has 'infected'

into Chinese at the time. Engelfriet 1993 discusses the relationship between Euclid's *Elements* and teachings on Christianity in Ricci's European context. More generally, this article outlines the role which Clavius allotted to mathematical sciences in Jesuit schools and in the wider Jesuit strategy for Europe. For a general and excellent introduction to the circumstances of the translation of Euclid's *Elements* into Chinese, an analysis and a complete bibliography, see Engelfriet 1998. Xu Guangqi's biography and main scholarly works were the object of a collective endeavour: Jami, Engelfriet and Blue 2001. Martzloff 1981, Martzloff 1993 are devoted to the reception of this type of geometry in China, showing the variety of reactions that the translation of the *Elements* aroused among Chinese literati. On the other hand, the process of introduction of Clavius' textbook for arithmetic was strikingly different. See Chemla 1996, Chemla 1997a.

4 Leibniz appears to have been the first scholar in Europe who, one century after the Jesuits had arrived in China, became interested in the question of knowing whether 'the Chinese' ever developed mathematical proofs in their past. In his letter to Joachim Bouvet sent from Braunschweig on 15 February 1701, Leibniz asked whether the Jesuit, who was in evangelistic mission in China, could give him any information about geometrical proofs in China: 'J'ay souhaité aussi de sçavoir si ce que les Chinois ont eu anciennement de Geometrie, *a esté accompagné de quelques demonstrations*, et particulièrement s'ils ont sçû il y a long temps l'égalité du quarré de l'hypotenuse aux deux quarrés des costés, ou quelque autre telle proposition de la Geometrie non populaire.' (Widmaier 2006: 320; my emphasis.) In fact, Leibniz had already expressed this interest few years earlier, in a letter written in Hanover on 2 December 1697, to the same correspondent: 'Outre l'Histoire des dynasties chinoises . . ., il faudroit avoir soin de l'Histoire des inventions [,] des arts, des loix, des religions, et d'autres établissements[.] Je voudrois bien sçavoir par exemple s'il[s] n'ont eu il y a long temps quelque chose d'approchant de nostre Geometrie, et si l'egalité du quarré de l'Hypotenuse à ceux des costés du triangle rectangle leur a esté connue, et *s'ils ont eu cette proposition par tradition ou commerce des autres peuples, ou par l'experience, ou enfin par demonstration, soit trouvée chez eux ou apportée d'ailleurs*.' (Widmaier 2006: 142–4, my emphasis.) To this, Bouvet replied on 28 February 1698: 'Le point au quel on pretend s'appliquer davantage comme le plus important est leur chronologie . . . Apres quoy on travaillera sur leur histoire naturelle et civile[,] sur leur physique, leur morale, leurs loix, leur politique, leurs Arts, leurs mathematiques et leur medecine, qui est une des matieres sur quoy je suis persuadé que la Chine peut nous fournir de[s] plus belles connaissances.' (Widmaier 2006: 168.) In his letter from 1697 (Widmaier 2006: 144–6), Leibniz expressed the conviction that, even though 'their speculative mathematics' could not hold the comparison with what he called 'our mathematics', one could still learn from them. To this, in a sequel to the preceding letter, Bouvet expressed a strong agreement (Widmaier 2006: 232). Mathematics, especially proof, was already a 'measure' used for comparative purposes.

'some central parts of [the] philosophy [of some philosophers], parts that have nothing intrinsically to do with mathematics' (p. 98).

What is important for us to note for the moment is that through such non-mathematical uses of mathematical proof the actors' perception of proof has been colored by implications that were foreign to mathematics itself. This observation may help to account for the astonishing emotion that often permeates debates on mathematical proof – ordinary ones as well as more academic ones – while other mathematical issues meet with indifference.[5] On the other hand, these historical uses of proof in non-mathematical domains, as well as uses still often found in contemporary societies, led to overvaluation of some values attached to proof (most importantly the incontrovertibility of its conclusion and hence the rigour of its conduct) and the undervaluing and overshadowing of other values that persist to the present. In this sense, these uses contributed to biases in the historical and philosophical discussion about mathematical proof, in that the values on which the discussion mainly focused were brought to the fore by agendas most meaningful outside the field of mathematics. The resulting distortion is, in my view and as I shall argue in greater detail below, one of the main reasons why the historical analysis of mathematical proof has become mired down and has failed to accommodate new evidence discovered in the last decades.[6] Moreover, it also imposed restrictions on the philosophical inquiry into proof. Accordingly, the challenge confronting us is to reinstate some autonomy in our thinking about mathematical proof. To meet this challenge effectively, a critical awareness derived from a historical outlook is essential.

II Remarks on the historiography of mathematical proof

The historical episode just invoked illustrates how the type of mathematical proof epitomized by Euclid's *Elements* (notwithstanding the differences between the various forms the book has taken) has been used by some (European) practitioners to claim superiority of their learning over that of other practitioners. In the practice of mathematics as such, proof became a means of distinction among practices and consequently among social groups. In the nineteenth century, the same divide was projected back into history. In parallel with the professionalization of science and the shaping of

[5] The same argument holds with respect to 'science'. For example, the social and political uses of the discourses on 'methodology' within the milieus of practitioners, as well as vis-à-vis wider circles, were at the focus of Schuster and Yeo 1986. However, previous attempts paid little attention to the uses of these discourses outside Europe.

[6] I was led to the same diagnosis through a different approach in Chemla 1997b.

a scientific community, history and philosophy of science emerged during that century as domains of inquiry in their own right.[7] Euclid's *Elements* thus became an object of the past, to be studied as such, along with other Greek, Arabic, Indian, Chinese and soon Babylonian and Egyptian sources that were progressively discovered.[8] By the end of the nineteenth century, as François Charette shows in his contribution, mathematical proof had again become the weapon with which some Greek sources were evaluated and found superior to all the others: a pattern similar to the one outlined above was in place, but had now been projected back in history. The standard history of mathematical proof, the outline of which was recalled at the beginning of this introduction, had taken shape. In this respect, the dismissive assertion formulated in 1841 by Jean-Baptiste Biot – Edouard Biot's father – was characteristic and premonitory, when he exposed

> this peculiar habit of mind, following which the Arabs, as the Chinese and Hindus, limited their scientific writings to the statement of a series of rules, which, once given, ought only to be verified by their applications, without requiring any logical demonstration or connections between them: this gives those Oriental nations a remarkable character of dissimilarity, I would even add of intellectual inferiority, comparatively to the Greeks, with whom any proposition is established by reasoning, and generates logically deduced consequences.[9]

This book challenges the historical validity of this thesis. The issue at hand is not merely to determine whether this representation of a worldwide history of mathematical proof holds true or not. We shall also question whether the idea that this quotation conveys is relevant with respect to

[7] See for example Laudan 1968, Yeo 1981, Yeo 1993, especially chapter 6.

[8] Between 1814 and 1818, Peyrard, who had been librarian at the Ecole Polytechnique, translated Euclid's *Elements* as well as his other writings on the basis of a manuscript in Greek that Napoleon had brought back from the Vatican. He had also published a translation of Archimedes' books (Langins 1989.) Many of those active in developing history and philosophy of science in France (Carnot, Brianchon, Poncelet, Comte, Chasles), especially mathematics, had connections to the Ecole Polytechnique. More generally, on the history of the historiography of mathematics, including the account of Greek texts, compare Dauben and Scriba 2002.

[9] This is a quotation with which F. Charette begins his chapter (p. 274). See the original formulation on p. 274. At roughly the same time, we find under William Whewell's pen the following assessment: 'The Arabs are in the habit of giving conclusions without demonstrations, precepts without the investigations by which they are obtained; as if their main object were practical rather than speculative, – the calculation of results rather than the exposition of theory. Delambre [here, Whewell adds a footnote with the reference] has been obliged to exercise great ingenuity, in order to discover the method in which Ibn Iounis proved his solution of certain difficult problems.' (Whewell 1837: 249.) Compare Yeo 1993: 157. The distinction which 'science' enables Whewell to draw between Europe and the rest of the world in his *History of the Inductive Sciences* would be worth analysing further but falls outside the scope of this book.

proof. As we shall see, comparable debates on the practice of proof have developed within the field of mathematics at the present day too.

First lessons from historiography, or: how sources have disappeared from the historical account of proof

Several reasons suggest that we should be wary regarding the standard narrative.

To begin with, some historiographical reflection is helpful here. As some of the contributions in this volume indicate, the end of the eighteenth century and the first three-quarters of the nineteenth century by no means witnessed a consensus in the historical discourse about proof comparable to the one that was to become so pervasive later. In the chapter devoted to the development of British interest in the Indian mathematical tradition, Dhruv Raina shows how in the first half of the nineteenth century, Colebrooke, the first translator of Sanskrit mathematical writings into a European language, interpreted these texts as containing a kind of algebraic analysis forming a well arranged science with a method aided by devices, among which symbols and literal signs are conspicuous. Two facts are worth stressing here.

On the one hand, Colebrooke compared what he translated to D'Alembert's conception of analysis. This comparison indicates that he positioned the Indian algebra he discovered with respect to the mathematics developed slightly before him and, let me emphasize, specifically with respect to 'analysis'. When Colebrooke wrote, analysis was a field in which rigour had not yet become a central concern. Half a century later in his biography of his father, Colebrooke's son would assess the same facts in an entirely different way, stressing the practical character of the mathematics written in Sanskrit and its lack of rigour. As Raina emphasizes, a general evolution can be perceived here. We shall come back to this evolution shortly.

On the other hand, Colebrooke read in the Sanskrit texts the use of 'algebraic methods', the rules of which were proved in turn by geometric means. In fact, Colebrooke discussed 'geometrical and algebraic demonstrations' of algebraic rules, using these expressions to translate Sanskrit terms. He showed how the geometrical demonstrations 'illustrated' the rules with diagrams having particular dimensions. We shall also come back later to this detail. Later in the century, as Charette indicates, the visual character of these demonstrations was opposed to Greek proofs and assessed positively or negatively according to the historian. As for 'algebraic proofs', Colebrooke compared some of the proofs developed by Indian authors to those of Wallis, for example, thereby leaving little doubt as to Colebrooke's estimation of these sources: namely, that Indian scholars had carried out genuine algebraic proofs. If we recapitulate the previous argument, we see that Colebrooke read in the Sanskrit texts a rather elaborate system of proof in which the algebraic rules used in the application of algebra were themselves proved. Moreover, he pointed resolutely to the use in these writings of 'algebraic proofs'. It is striking that these remarks were not taken up in later historiography. Why did this evidence disappear from subsequent accounts?[10] This first observation raises doubts about the completeness of the record on which the standard narrative examined is based. But there is more.

Reading Colebrooke's account leads us to a much more general observation: algebraic proof as a *kind* of proof essential to mathematical practice today is, in fact, absent from the standard account of the early history of mathematical proof. The early processes by which algebraic proof was constituted are still *terra incognita* today. In fact, there appears to be a correlation between the evidence that vanished from the standard historical narrative and segments missing in the early history of proof. We can interpret this state of the historiography as a symptom of the bias in the historical approach to proof that I described above. Various chapters in this book will have a contribution to make to this page in the early history of mathematical proof.

Let us for now return to our critical examination of the standard view from a historiographical perspective. Charette's chapter, which sketches the evolution of the appreciation of Indian, Chinese, Egyptian and Arabic source material during the nineteenth century with respect to mathematical proof, also provides ample evidence that many historians of that time discussed what they considered proofs in writings which they qualified as 'Oriental'. For some, these proofs were inferior to those found in Euclid's *Elements*. For others, these proofs represented alternatives to Greek ones, the rigour characteristic of the latter being regularly assessed as a burden or even verging on rigidity. The deficit in rigour of Indian proofs was thus not systematically deemed an impediment to their consideration as proofs, even interesting ones. It is true that historians had not yet lost their awareness that this distinctive feature made them comparable to early modern proofs.

One characteristic of these early historical works is even more telling when we contrast it with attitudes towards 'non-Western' texts today: when confronted with Indian writings in which assertions were not

10 The same question is raised in Srinivas 2005: 213–14. The author also emphasizes that Colebrooke and his contemporary C. M. Whish both noted that there were proofs in ancient mathematical writings in Sanskrit.

accompanied by proofs, we find more than one historian in the nineteenth century expressing his conviction that the assertion had once been derived on the basis of a proof. As late as the 1870s, this characteristic held true of, for instance, G. F. W. Thibaut in his approach to the geometry of the *Sulbasutras*, described below by Agathe Keller. It is true that Thibaut criticized the dogmatic attitude he attributed to Sanskrit writings dealing with science, in which he saw opinions different from those expounded by the author treated with contempt – a fact that he related to how proofs were presented. It is also true that the practical religious motivations driving the Indian developments in geometry he studied diminished their value to him. In his view, these motivations betrayed the lack of free inquiry that should characterize scientific endeavour. Note here how these judgements projected the values attached to science in Thibaut's scholarly circles back into history.[11] Yet he never doubted that proofs were at the basis of the statements contained in the ancient texts. For example, for the general case of 'Pythagorean theorem', he was convinced that the authors used some means to 'satisfy themselves of the general truth' of the proposition. And he judged it a necessary task for the historian to restore these reasonings. This is how, for the specific case when the two sides of the right-angled triangle have equal length, Thibaut unhesitatingly attributed the reasoning recorded in Plato's *Meno* to the authors of the *Sulbasutras*. As the reader will find out in the historiographical chapters of this book, he was not the only one to hold such views. On the other hand, it is revealing that while he was looking for geometrical proofs from which the statements of the *Sulbasutras* were derived, Thibaut discarded evidence of arithmetical reasoning contained in ancient commentaries on these texts. He preferred to attribute to the authors from antiquity a geometrical proof that he would freely restore. In other words, he did not consider commentators of the past worth attending to and, in particular, did not describe how they proceeded in their proofs.

To sum up the preceding remarks, even if, in the nineteenth century, 'the Greeks' were thought to have carried out proofs that were quite specific, there were historians who recognized that other types of proofs could be found in other kinds of sources. Even when proofs were not recorded, historians might grant that the achievements recorded in the writings had been obtained by proofs that they thus strove to restore. However, as Charette concludes with respect to the once-known 'non-Western' source material, 'much of the twentieth-century historiography simply disre-

[11] The moral, political and religious dimensions of the discourse on methodology have begun to be explored. See, for example, the introduction and various chapters in Schuster and Yeo 1986. More remains to be done.

garded the evidence already available'. One could add that the assumption that outside the few Greek geometrical texts listed above, there were no proofs at all in ancient mathematical sources has become predominant today. It is clearly a central issue for our project to understand the processes which marginalized some of the known sources to such an extent that they were eventually erased from the early history of mathematical proof. In any event, the elements just recalled again suggest caution regarding the standard narrative.

Other lessons from historiography, or: nineteenth-century ideas on computing

Raina and Charette highlight another process that gained momentum in the nineteenth century and that will prove quite meaningful for our purpose. They show how mathematics provided a venue for progressive development of an opposition between styles soon understood to characterize distinct 'civilizations'. In fact, as a result of this development, by the end of the century 'the Greeks' were more generally contrasted with all the other 'Orientals', because they privileged geometry over any other branch of mathematics, while 'the others' were thought of as having stressed computations and rules, that is, algorithms, arithmetic and algebra, instead.[12] Charette discusses the various means by which historians accommodated the somewhat abundant evidence that challenged this division.

This remark simultaneously reveals and explains a wide lacuna in the standard account of the early history of proof: this account is mute with respect to proofs relating to arithmetical statements or addressing the correctness of algorithms. From this perspective, Colebrooke's remarks on 'algebraic analysis' take on a new significance, since they pertain precisely to proofs of that kind. In addition, the absence of algebraic proof from the standard early history, noted above, appears to be one aspect of a systematic gap. If we exclude the quite peculiar kind of number theory to be found in the 'arithmetic books' of Euclid's *Elements*, or in Diophantus' *Arithmetics*, the standard history has little to say about how practitioners developed proofs for statements related to numbers and computations. Yet there is no doubt that all societies had number systems and developed means of

[12] From the statement by J. B. Biot in 1841 (quoted by F. Charette) to the statement by M. Kline in 1972 (quoted by Høyrup) – both cited above – there is a remarkable stability in the arguments by which algorithms are trivialized: they are interpreted as verbal instructions to be followed without any concern for justification. An analysis of the historiography of computation would certainly be quite helpful in situating such approaches within a broader context. This point will be taken up later.

computing with them. Can we believe that proving the correctness of these algorithms was not a key issue for Athenian public accounts or for the Chinese bureaucracy?[13] Could these rely on checks left to trial and error? Clearly, there is a whole section missing in the early history of proof as it took shape in the last centuries.[14]

In fact, there appear two correlated absences in the narrative we are analysing: on the one hand, most traditions are missing,[15] while on the other hand, proofs of a certain type are lacking. Is it because we have no evidence for this kind of proof? Such is not the case, and it will come as no surprise to discover that most of the chapters on proof that follow address precisely those theorems dealing with numbers or algorithms. From a historiographic perspective, again, it would be quite interesting to understand better the historical circumstances that account for this lacuna.

Creating the standard history

As Charette recalls in the conclusion of his chapter, the standard early history of mathematical proof took shape and became dominant in relation to the political context of the European imperialist enterprise. As was the case with the European missionaries in China a few centuries earlier, mathematical proof played a key role in the process of shaping 'European civilization' as superior to the others – a process to which not only science, but also history of science, more generally contributed at that time. The analysis developed above still holds, and I shall not repeat it. The role that was allotted to proof in this framework tied it to issues that extended far beyond the domain of mathematics. These ties explain, in my view, why mathematical proof has meant so much to so many people – a point that still holds true today. These uses of proof have also badly constrained its historical and philosophical analysis, placing emphasis on some values rather than others for reasons that lay outside mathematics.

[13] What is at stake today in the trustworthiness of computing is discussed in MacKenzie 2001.

[14] The failure that results from not having yet systematically developed the portion of the history of mathematical proof has unfortunate consequences in how some philosophers of mathematics deal with 'calculations', as opposed to 'proofs'. To take an example among those to whom I refer in this introduction, however insightful Hacking 2000 may be, the paragraph entitled 'The unpuzzling character of calculation' (pp. 101–3) records some common misconceptions about computing that call for rethinking. See fn. 45.

[15] As is often the case, when 'non-Western traditions' – as they are sometimes called – are missing, other traditions in the West have been marginalized in, or even left out from, the historiography. Lloyd directly addresses this fact in his own contribution to this volume.