FotFS VII

François, K., Löwe, B., Müller, T., Van Kerkhove, B., editors,
*Foundations of the Formal Sciences VII*
Bringing together Philosophy and Sociology of Science

# Sources for myths about mathematics. On the significance of the difference between finished mathematics and mathematics-in-the-making

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## 1 Introduction

Mathematical knowledge is often seen as a special kind of knowledge, namely as the very paradigm of certainty, universality, objectivity, or exactness. To some, it seems that in contrast to other forms of knowledge—including natural scientific knowledge—mathematical results are *absolutely* certain, indubitable, and infallible. For example, Pythagoras's Theorem was established over two millennia ago and still holds today. What other forms of activity can claim such definitive achievements?

> Whenever someone wants an example of certitude and exactness of reasoning, he appeals to mathematics. (Kline, 1980, p. 4)
>
> Mathematical results seem to be the paradigms of precision, rigor and certainty—from elementary theorems about numbers and geometric figures to the complex constructions of functional analysis and set theory. (Tymoczko, 1986, p. xiii)
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> [...] mathematical knowledge is timeless, although we may discover new theories and truths to add; it is superhuman and ahistorical, for

*We are immensely grateful to the lecturers and students who allowed us to observe and record the lectures and supervision meetings and who more generally helped with the research. Christian Greiffenhagen gratefully acknowledges the support through a British Academy Postdoctoral Fellowship and a Simon Research Fellowship (funded through an endowment made to the University of Manchester).

*Received by the editors:* 29 May 2009; 11 December 2009; 12 January 2011.
*Accepted for publication:* 18 March 2011.

> the history of mathematics is irrelevant to the nature and justification of mathematical knowledge; it is pure isolated knowledge, which happens to be useful because of its universal validity; it is value-free and culture-free, for the same reason. (Ernest, 1996, p. 807)

> What is that distinguishes mathematical knowledge from other forms of knowledge? Students of all ages, whether they are studying algebra, trigonometry, geometry, or calculus, would agree on one universal theme: Mathematics is beyond doubt. A mathematical statement is either right or wrong, and any true statement can be proven. Moreover, once it is proved true, it can never later turn out to be false. (Henrion, 1997, p. 235)

Mathematical reasoning can also seem to constitute a special form of reasoning, which is encapsulated in mathematical proofs. Since proofs have a logical, strict, and formal character, it may be that the reasoning that creates them shares these characteristics. Mathematical reasoning has thus been likened to the functioning of a machine, which relentlessly follows a set of (mechanical) rules. Rota has called this the "machine grinder" conception, which conceives of mathematicians as driven by logical necessity (rather than imagination) and thus wholly mechanical in their "grinding out", solely according to the dictates of formal logic, successor proposition after successor proposition until arriving at a seemingly irresistible conclusion:

> According to this myth, the process of reasoning is viewed as the functioning of a vending machine which, by setting into motion a complex mechanism reminiscent of those we saw in Charlie Chaplin's film *Modern Times*, grinds out solutions to problems, like so many Hershey bars. (Rota, 1991, p. 175)

However, are these views of mathematics actually correct or are they only myths? Though some apparently accept these myths, there is also much scepticism about them, and a variety of scholars from the humanities and social sciences have challenged these myths, denying the absolute certainty of mathematics and the formal character of mathematical reasoning.

Historians of mathematics have pointed to disagreements and controversies in the history of mathematics, some going so far as to argue that mathematics—like the experimental sciences—goes through Kuhnian "revolutions" (Gillies, 1992). The most influential contribution has been Lakatos's (1976) *Proofs and Refutations*, in which Lakatos demonstrates that mathematical concepts (such as "polyhedron") are not permanently fixed, but can have a history of continuous modification. The same argument has been applied to the very standards and forms of mathematical proof, which have been shown not to be eternally fixed but subject to historical and cultural changes (Kleiner, 1991; MacKenzie, 2001).

A variety of philosophers have questioned the status of mathematical knowledge as *absolutely* certain (e.g., Quine, 1960; Wittgenstein, 1978; Lakatos, 1976; Kitcher, 1983; Tymoczko, 1986; Maddy, 1990). Often they have done so by drawing philosophical conclusions from specific mathematical developments, in particular, the discovery of non-Euclidean geometries at the end of the 19th century and the production of Gödel's Incompleteness Theorems in the early 20th century. Non-Euclidean geometries brought into question the view that the certainty of mathematics resides in its correspondence to reality, while Gödel's Incompleteness Theorems shook the belief that it would be possible to finally (and formally) prove the consistency of mathematics from a finite number of assumptions.

Rather than emphasizing the certainty of mathematics, researchers have thus started to talk of mathematics as "fallible", "uncertain", and "contingent" (e.g., Lakatos, 1976; Kline, 1980; Davis and Hersh, 1981; Ernest, 1998). Mathematical proofs are—and remain—open to revision (as is evidenced by the fact that some of them have been revised) and therefore cannot have been statements of absolute and eternal truths (since these could not possibly be questioned). This has led some researchers to argue that the belief in the (absolute) certainty of mathematics is only a reflection of (various) *myths* about mathematics. For example, Davis and Hersh (1981, p. 322) argue that the traditional picture of mathematics constitutes "the Euclid myth"; Hersh (1991, p. 130) maintains that myths about the unity, objectivity, universality, and certainty are widely spread; Dowling (2001, p. 21) also claims that many people subscribe to the "myth of certainty", which for Borba and Skovsmose (1997) actually constitutes an "ideology of certainty".

However, despite the fact that the "loss of certainty" (Kline, 1980) occurred over a century ago, it is also assumed by researchers that these myths *still* persist and that belief in them is widespread. Some researchers have tried to explain the existence of these myths by arguing that they originate in the way that mathematics has been, and still is, *presented and taught* in journals, textbooks, lectures, and classrooms. That is to say, it may be that mathematical knowledge is communicated and taught *so as to make it seem* that it is absolutely certain. The impression that mathematical results are certain may thus be a result of the fact that mathematical publications typically contain only the final results of mathematical investigations, i.e., typically do not mention all the things that didn't work out, the reasons why certain things were tried, or the ways in which the final theorem was modified during the course of proving. In particular, the modern *definition-theorem-proof* format of presenting mathematics (cf. Davis and Hersh, 1981, p. 151; Thurston, 1994, p. 163; Weber, 2004, p. 116) could be said to misrepresent the order in which mathematicians initially work out their proofs (if the format of definition-proof-theorem is to be understood as giving an order of presentation that reproduces the order of the mathematician's investigative reasoning, i.e., as suggesting that the mathematician started with clearly stated definitions, used those to form a conjecture, and then just wrote out the successive steps of the proof that are logically implied in the initial definition unhesitatingly one after the other).

Borasi (1992, p. 161), for example, suspects that people may mistake the way that mathematical results are presented as an account of how they were found:

> Perhaps because textbooks and lectures tend to present mathematical results in a "neat" and organized way, few people realize that those results have not always been achieved in a straightforward manner. (Borasi, 1992, p. 161)

Crawford et al. (1998, p. 466) argue that the way that mathematics is presented to students hides important information and thereby misrepresents the nature of mathematics:

> Most students learn mathematics at school and university in a competitive environment where mathematics is presented as a finished and polished product and where the assessment encourages students to reproduce authoritative statements of fact [...]. In presenting mathematics in this way, students are provided with mathematical information about concepts, proofs, techniques and skills, but the processes which created this information are hidden [...]. The lack of awareness of these creative processes makes it difficult for students to experience mathematics as personally meaningful and misrepresents the nature of mathematics itself.

Powell and Brantlinger (2008, p. 428) argue that "the struggle of discovery is [...] usually missing from the narratives [students] read in mathematics texts". Livingston (2006, p. 60) wonders whether since proofs only exhibit "the reasoning that someone, or some others, had discovered, but not how they had gone about discovering it", students may mistake "the logic of a proof [as] the justification of how it was found". Similar considerations lead Campbell to speak of the "curse of Euclid":

> Euclid's axiomatic procedure is a breakthrough; it is a procedure for the unification of material. It allows key assumptions to stand out. It allows for systematic procedures of verification. But so long as students are misled into believing that the polished jewels are the actual reasoning rather than the end product of reasoning, just so long will it be that Euclidean geometry will remain a curse rather than a blessing to the teaching of reasoning. (Campbell, 1976, p. 342)

Ernest suggests that textbooks therefore constitute a "pedagogical falsification" of mathematical reasoning:

> Lakatos (1976) and others have criticized the pedagogical falsification perpetrated by the standard practice of presenting advanced learners with the sanitized outcomes of mathematical enquiry. Typically advanced mathematics text books conceal the processes of knowledge construction by inverting or radically modifying the sequence of transformations used in mathematical invention, for presentational purposes. The outcome may be elegant texts meant for public consumption, but they also generate learning obstacles through this reformulation and inversion. (Ernest, 2008, p. 67)

Overall, these quotations demonstrate a common—though vague and variable—sense that the way that mathematics is presented and written, in particular, the stress on established results (emphasising certainty) and on proofs (emphasising formal rules), may be an important source for the myth of mathematics as absolutely certain and as the province of special kinds of individuals with distinctive powers of reason. These impressions may be reinforced by the style in which mathematical reports are written, which could be described as "impersonal", "objective", or "authoritative" (Davis and Hersh, 1981, p. 36; Morgan, 1998, p. 11; Burton and Morgan, 2000, p. 435) and which could be seen to obliterate the understanding that proofs must be somebody's creation (thus conveying the impression that mathematics is a superhuman achievement with results appearing as if "untouched by human hand").

In order to demythologize mathematics, researchers have contrasted the way that mathematics is written and presented with how it is actually done and practiced. In other words, they have pointed to the differences between finished mathematics (which can be found in textbooks or lectures) and mathematics-in-the-making (when researchers are still in the process of coming up with the various definitions, theorems, and proofs). It has been argued that when the attention is turned to mathematics-in-the-making, a very different view of mathematics emerges, one that is much less alienating and therefore more welcoming to outsiders. This argumentative strategy is nicely captured in an article by Reuben Hersh.

## 2 Mathematics as divided into a front and a back

In *Mathematics has a front and a back* (1991), Hersh adopts Goffman's (1959) dramaturgical model of social establishments as divided between a "front" and a "back" region to mathematics.[1] In Goffman's terms, different areas of social establishments are separated by the need to project and sustain public impressions. A (usually more elevated) image is projected by conduct in the "front" region, since it is this area to which the audience is allowed access. However, what goes on in the "front" is supported by

[1] Cf. also Hersh (1997, pp. 35–39).

"backstage" activities, which include all sorts of things that are incongruous with (and usually diminishing of) the image being projected by the front and from which the audience—its public—is excluded. In a restaurant, for example, the "front" is the dining area where customers are offered hygienically presented dishes, which, of course, are the product of various (perhaps unhygienic) activities in the kitchen "backstage". Furthermore, while the dining area is often quiet, neat, and tidy, the kitchen is smelly, noisy, and chaotic (and there may be real discrepancies between the politely subordinate conduct of waiting staff in view of the customers and the disrespect, even contempt, on show backstage).

Hersh uses Goffman's conception of the "front" and the "back" to distinguish between the products of mathematical activities (finished mathematics) and the processes of mathematics (mathematics-in-the-making):

> [...] the "front" of mathematics is mathematics in "finished" form, as it is presented to the public in classroom, textbooks, and journals. The "back" would be mathematics as it appears among working mathematicians, in informal settings, told to one another in an office behind closed doors.
>
> Compared to "backstage" mathematics, "front" mathematics is formal, precise, ordered and abstract. It is separated clearly into definitions, theorems, and remarks. To every question there is an answer, or at least, a conspicuous label: "open question". The goal is stated at the beginning of each chapter, and attained at the end.
>
> Compared to "front" mathematics, mathematics "in back" is fragmentary, informal, intuitive, tentative. We try this or that, we say "maybe" or "it looks like". (Hersh, 1991, p. 128)

Not only are there differences between the "front" and the "back", but the audience is typically not allowed access to the "back". In other words, the activities that go on in the back are being kept from the audience:

> So its "front" and "back" will be particular kinds or aspects of mathematical activity, the public and private, or the part offered to "outsiders" (down front) versus the part normally restricted to "insiders" (backstage). (Hersh, 1991, p. 128)

Hersh takes the separation into a "front" and "back" to be the source for the various myths associated with mathematics. Although mathematicians themselves may well know that their work does not conform to this "official" image of mathematics,[2] they nonetheless sustain the mythology through their publication arrangements and educational practices:

[2]Other authors suppose that mathematicians themselves may have swallowed the myths or ideology about their own discipline.

> [. . .] the front/back separation makes possible the preservation of a myth [. . .]. By a myth we shall mean simply taking the performance seen from up front at face value; failing to be aware that the performance seen "up front" is created or concocted "behind the scenes" in back. This myth, in many cases, adds to the customer's enjoyment of the performance; it may even be essential. [. . .] Mathematics, too, has it myths. One of the unwritten criteria separating the professional from the amateur, the insider from the outsider, is that the outsiders are taken in (deceived), the insiders are not taken in. (Hersh, 1991, p. 129)

And the point of these myths is to support the social institution of mathematics:

> By calling these beliefs myths, I am not declaring them to be false. A myth need not be false to be a myth. The point is that it serves to support or validate some social institution [. . . ].
>
> [. . .] the unity, universality, objectivity, and certainty of mathematics are beliefs that support and justify the institution of mathematics. (For mathematics, which is an art and a science, is also an institution, with budgets, administrations, publications, conferences, rank, status, awards, grants, etc.)
>
> Part of the job of preparing mathematics for public presentation—in print or in person—is to get rid of all the loose ends. If there is disagreement whether a theorem has really been proved, then that theorem will not be included in the text or the lecture course. The standard style of expounding mathematics purges it of the personal, the controversial, and the tentative, producing a work that acknowledges little trace of humanity, either in the creators or the consumers. This style is the mathematical version of "the front".
>
> Without it, the myths would lose much of their aura. If mathematics were presented in the same style in which it is created, few would believe in its universality, unity, certainty, or objectivity. (Hersh, 1991, p. 130–131)

There are a number of important aspects to Hersh's adaption of Goffman's front/back dramaturgical model to mathematics:

Firstly, Hersh suggests a *strong* segregation between how mathematics is presented "in the front" and how it is actually done "in the back", insinuating that there might be two different kinds of mathematics (or mathematical reasoning), one "formal" and "logical", the other "informal" and "chaotic". As Ernest (2008, p. 66) restates Hersh:

> As Hersh [. . .] has pointed out, mathematics (like the restaurant or theatre) has a front and a back. What is displayed in the front for

> public viewing is tidied up according to strict norms of acceptability, whereas the back (where the preparatory work is done) is often messy and chaotic.

Secondly, Hersh clearly implies that this segregation of things done "in the back" that are not stated in "front" presentations is a *deliberate* and *calculated* move to keep that information from the audience:

> The purpose of a separation between front and back is not just to keep the customers from interfering with the cooking, it is also to keep the customers from knowing too much about the cooking. (Hersh, 1991, p. 129)

Thirdly, Hersh assumes that these activities are concealed or hidden from the audience in the interest of *protecting the institutional interests of mathematics* (because public awareness of them would transform, even devalue, the high status assessment that allegedly follows from the "front" presentations). Hersh speaks of the backstage activities as going on "behind closed doors", which suggests that what goes on there would be embarrassing and would detract from the merits of mathematics if made public. Henrion (1997, p. 249) similarly states:

> One reason that the mathematics community is not more active in conveying a more accurate picture of mathematics is that much of the power and prestige of mathematics comes from its claim to certainty and its image as an "exact science".

The main thrust of Hersh's front/back imagery is that mathematical results come to be treated by "the audience" or "the public" *as if* they could not possibly be questioned, which gives mathematicians an awesome authority. Those outside the mathematical community seem to have no choice but to be obedient to whatever (mathematical) directives mathematicians give them.[3] Of course, no one supposes that this authority derives from the personal characteristics of (balding, spotty, aging, shabby) mathematicians, so it must stem from the nature of the mathematics itself. Hersh suggests that the idea that mathematicians can't possibly make mistakes is—at least partially—a result of the way in which they present their work.

[3]According to David Bloor (e.g., 1976, 1994) many people (falsely) believe in the universality of mathematics, i.e., assume that 2 + 2 must equal 4, and therefore do not realize the conventional character of mathematics (for example, the use of a decimal rather than binary notation). The aim of Bloor's sociology of mathematics is thus to demonstrate that mathematical propositions "could have been otherwise" and thereby to show that no one is compelled to accept these propositions on the grounds that they could not possibly be otherwise. These views are questioned in Greiffenhagen and Sharrock (2006, 2009).

The front/back scheme is invoked to debunk myths about mathematics by showing that mathematical results and reasoning are less certain and formal than they appear to be. This is done by showing that the reality of mathematical practice relevantly deviates from its frontal presentation. Two main strategies are at play. Firstly, it is held that the "front" evades mention of mathematician's controversies or doubts about their own or others' work (since preparing things for publication includes "to get rid of all the loose ends" and to "purge [. . .] it of the personal and controversial, and the tentative"). Secondly, it is claimed that the course of a mathematical reasoning is not an inexorable progression of inevitable steps. In other words, mathematicians reason heuristically (cf. Pólya, 1945, 1954a,b) and often engage in revision of their strategies for forming their sought after proof. Again, Hersh suggests that because publications do not declare the tentativeness, uncertainties, and informality associated with working on proofs, they are occluding the fact that mathematical work is "contingent", "revisable", and "fallible". The upshot of Hersh's argument seem s to be that if the audience was admitted or given access to the "back" of mathematics—as, for example, attempted in *The Mathematical Experience* (Davis and Hersh, 1981) or *What is Mathematics, Really?* (Hersh, 1997)—they could be emancipated from the mythologized picture of mathematics.

We are certainly not going to dispute that myths about mathematics do circulate or that there are manifest differences between finished mathematics and mathematics-in-the-making. Neither do we address the "internal" question of whether the historical shifts in ways that mathematics has been written and presented have been an advantage or disadvantage to the discipline. Mathematicians of course debate the "best" way of presenting mathematical results (cf. Ulam, 1976, pp. 276–277): Is it good to have "succinct" papers that highlight the important points, or is it better to have "complete" papers that deal with every detail? Should one start with a short introduction that explains the motivations of trying to tackle the problem? Should one just state the theorem and then present the proof? Burton and Morgan (2000, p. 449), for example, quote a mathematician who calls for changes:

> I get annoyed with some of my collaborators and a lot of the papers I am sent, which are definition, theorem, lemma, proof. That seems to me to be appallingly bad. It is the sort of thing that no one is ever going to want to read. I think it is important to grab the reader from the opening sentence. Not "Let $A$ be a class of algebras such that . . . " Change it to "This paper opens a new chapter in duality theory."

Whatever the relevant pros and cons, these "internal" debates are different from the "external" question of whether the way in which mathematical papers are written implicates mythical conceptions of mathematics (although there are connections insofar as styles are condemned for being off-putting for imagined readerships). What we want to question is Hersh's suggestion that looking at mathematics "in the front" or "in the back" yields two incongruent views of mathematics. We would therefore like to take a closer look at two examples of mathematical practice, one taken from the "front" and one from the "back".

## 3 Towards a sociology of mathematics

It is surprising how very few detailed accounts of what mathematicians do *either* in the "front" *or* in the "back" can be found in the literature. The supposed features of "the front" as well as "the back" are typically not derived from the close analysis of specific examples (e.g., a particular lecture, textbook, or journal paper) or a corpus of them. This in turn makes it difficult to check whether the descriptions of the "front" or "back" are adequate.

There have been a number of very illuminating studies in the history of mathematics (e.g., Lakatos, 1976; MacKenzie, 1999, 2001; Netz, 1999, 2009; Warwick, 2003), which have in particular demonstrated the changing nature of many mathematical concepts (including the very notion of what constitutes a mathematical proof). However, there are very few studies that are based on observations of people engaged in doing mathematics. Even the recent sociological studies of mathematical disciplines (e.g., Livingston, 1986, 1999, 2006; Merz and Knorr-Cetina, 1997; Rosental, 2003, 2008—cf. also Greiffenhagen, 2008, 2010) are in no way straightforward ethnographies comparable to the laboratory studies of the experimental sciences. Part of the reason may be that it is simply more difficult to do such studies in the case of "theoretical" or "conceptual" practice:

> It is easy to study laboratory practices because they are so heavily equipped, so evidently collective, so obviously material, so clearly situated in specific times and spaces, so hesitant and costly. But the same is not true of mathematical practices: notions such as "demonstration", "modelling", "proving", "calculating", "formalism", "abstraction" resist being shifted from the role of indisputable resources to that of inspectable and accountable topics. It is as if we had no tool for holding such notions under our eyes for more than a fleeting moment, or simply no metalanguage with which to register them. (Latour, 2008, p. 444)

For the past six years, we have been conducting sociological studies of mathematics, trying to find "perspicuous settings" in which different features of mathematical practice become observable. We decided to focus on situations in which mathematicians come together to discuss mathematics. In a first study we observed and recorded three different graduate courses in mathematical logic for several months. In these lectures, an experienced mathematician demonstrates mathematical expertise to novices, showing what is treated as important and noteworthy about the "archive" of mathematics. In a second study we attended for over a year the (almost) weekly meetings between a supervisor and his doctoral students where they discuss the problems that the student has been working on.

In our first study, we video-taped different lecturers giving graduate lectures in mathematical logic (Figure 1). We do not here have the space to give a detailed example (which we plan to do in a future paper), but can only make some preliminary observations with respect to what one can see when one looks closely what happens in mathematical lectures.

The lectures that we observed followed the typical *definition-theorem-proof* format of presenting mathematics. Furthermore, like many lectures in mathematics, what the lecturer wrote on the board to a large part corresponded to the script that had been handed to students at the beginning of the course. These lectures thus are a perspicuous example of mathematics in the "front", since what is communicated

> is formal, precise, ordered and abstract. It is separated clearly into definitions, theorems, and remarks. To every question there is an answer, or at least, a conspicuous label: "open question". The goal is stated at the beginning of each chapter, and attained at the end. (Hersh, 1991, p. 128)

However, in which sense does this style of presenting mathematics contribute to myths about mathematics?

The first thing to notice is that lecturers did not simply copy the text from the script to the board. Rather, lecturers spent the majority of the time "working through" various proofs, typically without looking into the script (except on a few occasions when they wanted to check a particular detail or "got lost"), making a lot of additional comments, for example, highlighting which steps in the proof were important or noteworthy, how the steps depend on results established earlier, where the assumptions in the theorem were used in the proof, and so on.

This at least partly explains why lecturers recited theorems and proofs to students when the students could read them for themselves in the handouts of the course. Learning mathematics does involve reading a lot of theorems and proofs, but such reading requires work. Both lecturers and students are aware that reading through a complex proof (once) does not equip students with an (adequate) understanding of it. In that sense, a lecture is only the first step in a process. Lecturers expect that students will spend additional hours of individual study, together with attempting prescribed exercises, in order to understand the materials covered in the lecture (lecturers also expect that students are likely to have initial difficulties of understanding).

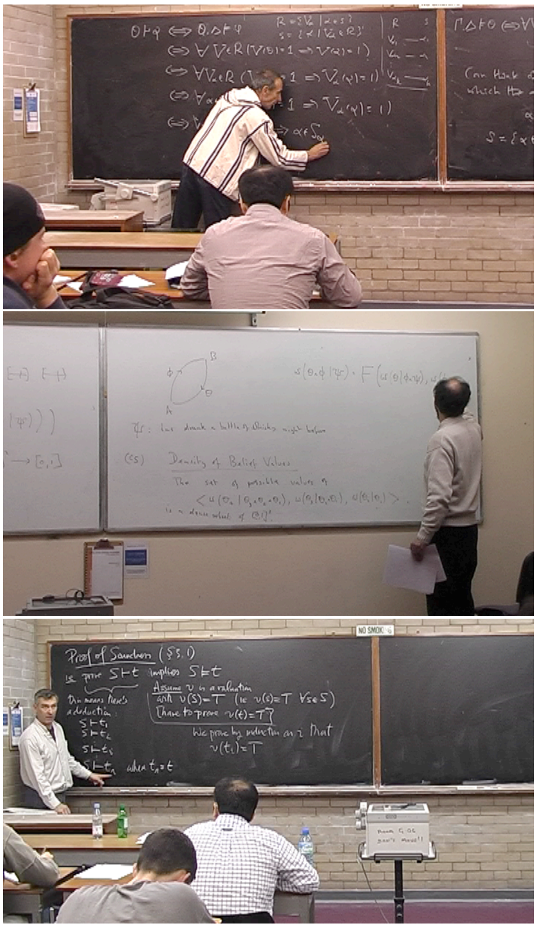


FIGURE 1. Graduate lectures

As Davis and Hersh (1981, p. 281) observe, a mathematical proof is only superficially comparable to a notated musical score and therefore only seems to be accessible to sight reading. However, this is something that students are *very* aware of. Although these students were still less than fully qualified practitioners, it is difficult to see how they would get a wrong picture of mathematics from the particular style in which the materials were presented in these lectures (a style, which does not differ from a first-year undergraduate course).

Furthermore, it is important to note that lecturers did not purport to give a report (in the form of a historical, sociological or even anecdotal description) on how the various theorems, definitions, or proofs were "found". Instead, lecturers "simply" exhibited how particular theorems or proofs "work" regardless of how they were found. The emphasis in these lectures was on intelligibility, not truthful historical reporting. The comments of lecturers were designed to make the various theorems and proofs intelligible to students, independently of knowing much, if any, biographical detail of the authors or anything about the extended and detailed work that went into developing the results. Although lecturers did not explicitly mention that the definitions, theorems, and proofs do not "fall from the sky" (but are the result of revisions, false avenues, etc.), this does not mean that they were "hiding" this from students. Students may have been frustrated that they failed to understand how a particular proof works or that they failed to solve some of the exercise problems. Even so, it would be strange to suppose that students as a consequence of the "formal, precise, ordered and abstract" way in which the results were presented in these lectures believed that it was possible for the originators of these results to achieve them without any effort or by applying a "mechanistic" procedure.

In sum, a careful consideration of looking at a concrete example of mathematics "in the front" (here: a mathematical lecture) shows that it does not necessarily lead to any of the myths about mathematics described by Hersh.

In a second study, we attended the weekly meetings between a supervisor and his doctoral students (Figure 2). In these sessions, the supervisor would discuss the work of the student, sometimes on the basis of some materials that the student had sent to the supervisor prior to the meeting. At other times, the student would provide an oral account of what he had been working on—which typically involved explaining why and where he did get "stuck" (cf. Merz and Knorr-Cetina, 1997)—and the supervisor would respond by making various suggestions on how the student could proceed.

The discourse in these meetings was indeed very different from that in the lectures and constitutes a good example of mathematics "in the back", which is described by Hersh (1991, p. 128) as "fragmentary, informal, intuitive, tentative. We try this or that, we say 'maybe' or 'it looks like'".

In these sessions, neither the supervisor nor the student would write down fully worked out proofs on the board (in fact, if the student had submitted a proof which was deemed to be complete-for-all-practical-purposes they typically did *not* talk further about it, but the conversation would move on to what could be done next, i.e., how to make further progress building on what they had proved so far). Rather than presenting finished mathematics, the supervisor and doctoral student used the board as a focus for their discussions, a place to sketch out ideas, conjectures, hunches, reasons for trying this or that.

The main aim of their meetings was to try out ideas, fully aware that they would only be able to work them out initially and partially in the

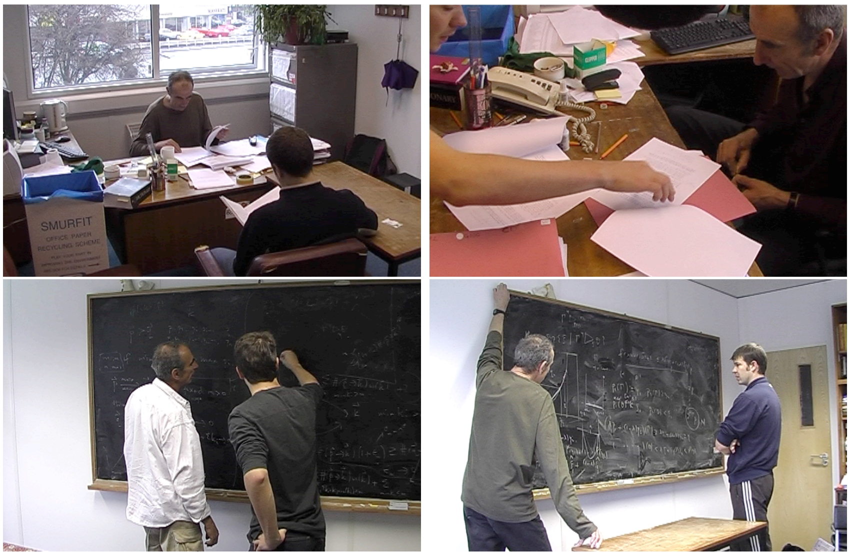


Figure 2. Supervision meetings

meeting. Sometimes it transpired relatively quickly that an idea would definitely not work and should be abandoned. However, more often than not, the idea remained to be worked through more systematically and there remained uncertainty as to whether a promising idea would work out in detail. Another aim of these meetings thus was to make assessments with respect to which problems it would be worth pursuing. These researchers, like other practical decision makers, were sensitive to economising their investment of effort as well as with the pacing of their inquiries, and so the decision as to whether to attempt to construct a proof or to search for a counterexample was to be considered in terms of the estimated likelihood that one, rather than the other, would pay off, and the amount of time that would need to be invested in getting out the correct conclusion.

Their discussions in these sessions were often not decisive, but this did not mean that there was a permanent tentativeness about determining whether an idea would or would not work in detail. Further additional work would often give a firm verdict (although some ideas remained unresolved either way). Although their work could be described as "intuitive", this does not mean that what they were doing resembled anything like arbitrary guesswork. One of the dangers of attacking a deductivist picture of mathematics is to suggest the opposite extreme. Experienced mathematicians do make guesses and rely on intuition, but do this on the basis of a vast armoury of accepted techniques and tricks.

Although the conversation in this setting did not resemble the discourse in the lecture, it was constantly oriented towards it. The aim was to come up with theorems and proofs that *could* and, if successful, would be presented in the style of the lecture. Conversely, both researchers constantly made use of the results and techniques that were taught in the lectures, since these are amongst the stock resources of the field. In that sense, it is difficult to see how the "back" was strongly separated from the "front", since the composition of their new proof is done through—in part—the use of results and techniques drawn from already established proofs (i.e., their innovation "in the back" involved the application of what they had learned from studying proofs "in the front"). In other words, there was not any simple discontinuity between "rough ideas that are good enough for our mathematical purposes" and the "formal, tidy presentation necessary to meet the needs of the audience", because, as far as these researchers were concerned, if they only had a rough idea, they did not necessarily have anything that *could* be presented in the format of a lecture.

In sum, looking at a concrete example of mathematics "in the back" (here: a meeting between a supervisor and his doctoral student) shows that it is not fundamentally different from mathematics "in the front".

## 4 Conclusion

Both Goffman and Hersh seem to suggest that the separation of a setting into a "front" and "back" can serve as the basis of various myths. Goffman writes that in the "front"

> [...] errors and mistakes are often corrected before the performance takes place, while telltale signs that errors have been made and corrected are themselves concealed. In this way an impression of infallibility, so important in many presentations, is maintained. (Goffman, 1959, p. 52)

While Hersh (1991, p. 127) states that "[a]cceptance of these myths [unity, objectivity, universality, and certainty] is related to whether one is located in the front or the back" and that "[m]ainstream philosophy doesn't know that mathematics has a back" (Hersh, 1997, p. 36).

We would be foolish to pretend that questionable conceptions about mathematics do not circulate (especially within philosophy) or that they cannot change over time ("fallibilism" was not a term widely used even in the nineteenth century). At the same time, since amongst the mythologisers are included Russell, Hilbert, and Bourbaki (and formalists generally), all of whom knew how to do advanced mathematics, we do not see that it is a lack of exposure to backregions that makes people susceptible to these mythical conceptions. People may have wrong ideas about mathematics, but we have questioned whether these are a creation of external features of the way in which textbooks, lectures, and other publications are set out in public formats, and have suggested that there is no compelling reason to think they are.

We have been arguing that whatever the origin of the various myths of mathematics might be, it cannot be the necessity to segregate one form of mathematical reasoning (that which mathematicians present themselves as using) from another (that which mathematicians *really* employ to do mathematics) so as to prevent "the audience" becoming aware of the difference. Rather than being helpful, the application of the front/back metaphor exaggerates the discontinuities between mathematics-in-publications and mathematics-in-the-making. Whilst Hersh might laudably aim to demystify mathematics, there is in our view the risk that in the end he will only service re-mystification of it.

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