PERSPECTIVES ON MATHEMATICAL PRACTICES

LOGIC, EPISTEMOLOGY, AND THE UNITY OF SCIENCE

VOLUME 5

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Perspectives on Mathematical Practices

Bringing Together Philosophy of Mathematics, Sociology of Mathematics, and Mathematics Education

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Introduction

Is mathematics finally going through the Kuhnian revolution that the sciences or, more precisely, the philosophers, historians, sociologists, economists, psychologists of science, … have been able to deal with ever since the magical year of 1962? Apart from the fact that one cannot easily identify a book that has played the part that *The Structure* has played – of course, Lakatos' *Proofs and Refutations* comes pretty close, but it does not possess the generality of Kuhn's work – there seems to be plenty of reasons why mathematicians and philosophers of mathematics are reluctant to cheer the coming of a Kuhnian revolution in their favourite domain. Instead of a full-fledged historical-philosophical analysis (actually, many papers in this volume do precisely that, so it is quite unnecessary to duplicate their efforts in this introduction), let us just repeat once more the overused quote: "Mathematics is a free creation of the human spirit". In a nutshell it expresses the cherished beliefs that many share: mathematics stands on its own, free from any societal influence, individualist and immaterial, beyond space and time, in short, it occupies a universe of its own. This view usually, though not necessarily, goes together with a belief, if not a conviction, that mathematical capacities are innate, i.e., one is born a mathematician and a mathematical training merely serves to refine the powers already present. One just needs to remind oneself of the well-known story told by G. Hardy about his reluctance to familiarise the celebrated Indian mathematician Ramanujan with the notion of a proof in mathematics for fear of ruining his innate capabilities. Add to this that to a large extent the standard account of the life of Ramanujan is a romantic invention and we consider our point made (see Kanigel [1991] for a more 'realistic' biography of Ramanujan.)

Therefore, if it is your ambition, as it is ours, to set the Kuhnian revolution in mathematics on its tracks, what to do (to quote a famous political philosopher)? It seems obvious to us that the first thing to do is to look for a good description of the subject itself: *what kind of thing is this curious process we call mathematical practice*? The aim of this book is two-fold:

- first, to bring together a number of authors who have thought and are still thinking about what mathematical practice is in general as well as in detail, how it should be studied and how theories can be formulated and,
- secondly, to incorporate existing materials from other, though related disciplines, as is, e.g., the case for mathematical education. This is a well-developed research community with its own goals, methods, and theories, but somehow it does not seem to connect all that well with the philosophical community. We wish to show in this book that such connections are indeed possible, if not necessary (if only for thought-economical reasons: duplication is rarely a time-energy saving device).

The papers presented here can thus be subdivided into three major categories:

- a first set deals with the general theme of mathematical practice,
- a second set with specific themes that arise when one takes the viewpoint from a full-blooded description of mathematical practice and
- a third set, too important to classify under the second heading and already referred to above, namely the relation between mathematical practice on research level, academic and otherwise, and education.

General theme: how to deal with mathematical practice?

In this section four papers have been brought together that show quite different ways of approaching the question above, showing thereby that the issue of how one is to study mathematical practice is itself a very difficult and complicated problem. Rather than looking for a unifying framework, it is our belief that by presenting four rather disparate approaches we will hopefully succeed in convincing the reader that it is not very likely that such a unifying theory will be easily put together, if at all.

Consider the paper by Jody Azzouni, “How and Why Mathematics is Unique as a Social Practice”. Azzouni recognizes the importance of mathematical practice as a subject worthy of philosophical reflection and he tries to identify the characteristics that distinguish, quite sharply in his mind, mathematical practice from other practices. His inspiration and arguments are drawn from mainly analytical philosophy, more specifically from philosophers such as Ludwig Wittgenstein, Michael Resnik, Saul Kripke, and Hilary Putnam. Azzouni aims to show, so we believe, that the same analytical tools that were used to deny the importance of mathematical practice can also be used to make a case in its favour.

Eduard Glas in "Mathematics as Objective Knowledge and as Human Practice" attacks the problem of the nature of mathematical practice from a different angle, namely philosophy of science, more precisely the work of Karl R. Popper and to be even more precise, the three world model Sir Karl was so fond of. We quote from Glas' paper to show the Popperian mode of thinking present: "Humankind has used descriptive and argumentative language to create a body of objective knowledge, stored in libraries and handed down from generation to generation, which enables us to profit from the trials and errors of our ancestors." It allows Glas to reach the same goal as Azzouni, i.e., to show that mathematics can be properly distinguished from other practices and to see no deep conflict between mathematics' objectivity and its being profoundly social.

A similar concern is present in the contribution of Robert Thomas, "The Comparison of Mathematics with Narrative". However, Thomas walks a different route. Combining elements of semiotic theory – Umberto Eco, Brian Rotman, and Hayden White are referred to –, and of philosophy of mathematics and science – here we find Stephan Körner, Hartry Field and Hilary Putnam as sources of inspiration –, he too aims to show that, although mathematical practice shares a number of similarities to stories and narratives, nevertheless it is at the same time quite distinct.

Finally a deeply social and to many disturbing sound can be heard in Sal Restivo's "Theory Of Mind, Social Science, and Mathematical Practice". Starting from a theory of mind that in its 'classical setting' is typically asocial, Restivo shows how it impregnates our standard view of mathematics. Socialising the mind leads to a socialisation of mathematics. His main inspiration is to be found in the work of one of the founding fathers of sociology, viz. Emile Durkheim and his seminal notion of 'practices as institutions'. It leads him to the conclusion that a number of 'old' questions have to be posed again because entirely new answers are in the making. To quote from his paper: "What are numbers (and what are all the basic concepts and processes that constitute mathematics?). What is a classroom? What are teachers and students? What is learning? What is truth? What does it mean to reason? What is a proof? The trick here is to see all of these old friends as *institutions.*"

As said above, these contributions show that a genuine theory of mathematical practice is possible. Genuine in the sense that it is not derived from any foundational theory such as formalism, logicism or intuitionism (and all varieties of constructivism that it generated), to name the three major schools of the twentieth century. Such a theory is also badly needed and the four different ways presented here of handling the subject show at the same time that much work remains to be done in terms of mutual comparisons and enrichments. This present situation however need not be an obstacle in order to have a closer look at more specific elements of mathematical practice. Indeed, they help to refine the general problems and they provide detailed accounts that can be usefully employed as test cases for the diverse general accounts. That is the justification for the next major part of this book.

Specific themes: taking mathematical practice seriously

What kind of (more) detailed problems should one expect? What seems rather obvious is that the same problems that came up during the development of the philosophy of science in the post-Kuhn era, have a fairly large chance of appearing within the (new kind of) philosophy of mathematics as well. For that reason it was to be indeed expected that someone should write something about incommensurability. After all, incommensurability – the problem of whether or not it is possible to compare different scientific theories – has been a core problem in the philosophy of science, so at least one should have a look at it from the mathematical perspective. This is precisely what Otávio Bueno in his paper "Incommensurability in Mathematics" tries to do. Rather surprisingly perhaps, Bueno presents a strong case *in favour of* incommensurability in mathematics. Unlikely as it seems – after all, is not a number a number whenever and wherever it appears, so should not comparability be guaranteed at all places and at all times? – he does a wonderful job, presenting specific case studies to back up his claim. In his own words: "Theory change in mathematics, just as theory change in science, becomes a more complex, more interesting and not a cumulative phenomenon. As with science, in mathematics sensitivity to meaning change is required. This means that a simple cumulative pattern of mathematical development doesn't seem to make sense of mathematics." The unavoidable conclusion does follow: revolutions in mathematics are possible.

Who speaks of incommensurability, unavoidably has at the back of his or her mind the problem of (mathematical) progress. As soon as some form of incommensurability, however weak, sneaks in, the problem of how to define progress poses itself. Madeline Muntersbjorn in her contribution "Mathematical Progress as Increased Scope" tries to deal with this difficult question. The suggestion she proposes is that "…, mathematicians are not like mapmakers who adhere to the environmentalist's ethic, 'take only memories—leave only footprints.' They are more like *terraformers*, science-fiction engineers who travel to inhospitable planets and struggle to make alien landscapes suitable for human settlement by adapting them to our perceptual needs and abilities via innovations in formal systems of signification.”. This to our minds powerful metaphor really begs to be further developed as it manages to steer a course between, on the one hand, the *Scylla* of strong forms of mathematical realism, including variations on Platonism, and, on the other hand, the *Charibdis* of relativism where “2 + 2 = ?” is a question on the same footing as “Did Sherlock Holmes have a homosexual affair with Watson?”.

The next step, of course, must be to provide case studies from these new perspectives. Of course, if we are thinking about mathematics of the past, then the easiest thing to do is to look at the history and historians of mathematics. Surely they must have thousands of case studies ready for use. Although we happily accept – how could one argue otherwise? – the work done by historians, nevertheless we do have something slightly different in mind. What we are talking about is a shift of focus, looking at the neglected or almost forgotten details, connecting elements that seem unrelated at first sight, so that, if successful, the philosophical relevance becomes clear(er).

A fine example of such an attempt is Brendan Larvor’s “Proof in C17 Algebra”. Although he treats the well-known mathematicians of that period – Girolamo Cardano, François Viète, Thomas Harriot, John Pell, and others –, he does look at it in a different way. He is interested in what one could describe as “proof styles”, making it possible to distinguish between a mathematical text of Viète in contrast with, say, Cardano. The connection with the philosophy of mathematics is easily made: what we count as proof today is something that took quite some time to develop and hence it is a delicate question to judge the *quality* of proofs. Hence, what are proofs? How can we distinguish proofs from arguments (if such a distinction is meaningful)? What will ‘future’ proofs look like (given that the proof concept is a mobile concept)?

Some of these questions, especially the question about proofs and arguments, are discussed in the contribution of Andrew Aberdein, “The Informal Logic of Mathematical Proof”. It is perhaps a bit surprising to see the names of Stephen Toulmin and Douglas Walton appear in a paper about mathematical proofs. After all, are these two authors not famously known for their work in argumentation theory and definitely not in proof theory? And what could be the involvement of argumentation theory in the understanding of mathematical proof? Aberdein’s claim is precisely that by looking at mathematics from an argumentation-theoretical point of view, aspects of the mathematical culture are brought into focus that, from a formal point of view, would be lost altogether. It thereby helps to refine our image(s) of mathematical practice. If proofs are situated elements of such practices, it would make much more sense to talk of *proof dialogues* as he proposes to do instead of proofs in an unqualified way, thereby inviting us to reify them.

There is however something extremely important that all contributions up to this point (mostly implicitly, sometimes explicitly) indicate: if mathematics is indeed a complex set of diverse mathematical practices, if indeed these practices are (also) shaped by social factors, dependent on societal circumstances, thus sensible to societal changes, and therefore very changeable, then to become a mathematician must also be constituted by a complex set of social processes. It cannot be a matter of 'simply' developing the faculties, capacities or powers already present in the genius' brain – "the seed is there, it merely needs to grow" –, rather it is a process that, to use a biological metaphor, aims at preparing an organism for a very specific environment. In short, any theory that takes itself seriously as a candidate for understanding mathematical practices, must deal with mathematics education. Hence the third part of this book.

The special case of mathematics education

As said above, the community of mathematics educators is a very well-established community of inquiry. However, it does not seem to connect very well with the philosophers of mathematics community (admitted that the latter group numberwise is rather small compared to the former one). Note that the intersection of the two sets is not empty: there are 'true' specialists, namely, philosophers of mathematics education. Nevertheless, it is our impression that they tend to be associated more strongly with the teachers than with the philosophers. In this third part of the book we wish to show that the two communities not only should, but really must meet more often and more intensely. In addition, it was our ambition to present the possible interactions between the philosophy of mathematics and mathematics education in as many ways as possible, from an abstract level to a very concrete level, from general considerations to case studies, from argumentation to narrative, from the institutional to the personal.

The first two papers of this part of the book are Leone Burton's "Mathematicians' narratives about mathematics – and their relationship to its learning" and Anthony and Dominic Peressini's "Philosophy of mathematics and mathematics education. The Confluence of Mathematics and Mathematical Activity". They both share the concern to show that mathematics and its philosophy on the one hand, and mathematics education and its philosophy on the other hand, have a lot to share. Do note that for both of them the idea to understand the proof concept as a social construct (we repeat, once again, without implying any form of deep relativism) is pivotal. For one thing, instead of the image whereby the ideal notion of logico-formal proof is transferred to the educational setting, we now have the image of a particular concept, viz. "proof", arising in a particular community, usually referred to as "the" mathematicians, and then being transferred into a totally different setting, namely, the teaching context. Seen thus, there is little need for an *exact copy* of the proof concept in the classroom. Hence, all kinds of questions pop up: What kind of proof is required for pupils to get a 'good' feeling for mathematics? How do other arguments (here Aberdein's paper is clearly relevant) function in the classroom? How do philosophical elements enter into the very same classroom?

It is more than obvious that answering these questions, apart from the philosophical setting, requires lots of case studies. We present here three such cases. We do know, of course, that there is wealth of materials available at the present moment. The emphasis, however, is not on a case study from the educational point of view, but from (at least) the philosophico-educational point of view. Jill Adler in her paper "Mathematical Practices in and across School Contexts" does precisely that. Analysing the situation in South Africa in the *post-apartheid* situation allows her to come to the conclusion that "… a decontextualised notion of mathematical practice makes no sense from the perspective of school mathematics, if at all. School mathematical practices are just that: practices dialectically produced by both mathematics and schooling."

The second example concerns the Belgian, more specifically Flemish situation. As both editors of this volume are working in Belgium, it seems quite normal to have a case study "close to home". However, as a case study, it is perhaps somewhat unusual and special because it involves a topic that is not often addressed, if at all, and equally often considered to be a borderline phenomenon. *In casu*, what we are talking about are journals for mathematics teachers, not to be confused with journals for educational mathematics, journals for philosophy of mathematics, journals for the philosophy of mathematics education, and so on. What is presented here in the paper by Ad Meskens, "The Importance of a Journal for Mathematics Teachers", are mathematics teachers writing for mathematics teachers. What do they write about? What do they consider to be so interesting that their colleagues should know about it? It is important to note that the author is a mathematician himself, not a philosopher. So here we have at least one example of a person-in-the-field reporting from the field.

The third and last example, also the concluding piece of this book (and the editors of this volume are truly proud to be able to include a contribution from this author) is basically about a formula and its proof, viz. Reuben Hersh's "On the Interdisciplinary Study of Mathematical Practice, with a Real Live Case Study". Formulated thus, this seems at first sight hardly innovative, refreshing or stimulating. However, in the unique style that is Hersh's own, we are invited to walk along with him and, indeed, think about a formula and its proof, but in such a way that at every step the links to education, to philosophy, and, of course, to mathematics become clear, and, in fact, refreshing and stimulating. It is worthwhile to state here his conclusion: "This much can be said. Mathematics really exists. It is going on, it is taking place, it has been around a long time and is here to stay. If your vocabulary insists that it is not real, and since in any ordinary meaning of the word it is not "fictional", then you must find some other kind of ontology, neither "real" in your sense nor fictional in any sense, to place it in."

In this sense, we have made a full circle, starting from philosophical considerations to the most concrete case study imaginable, back to philosophy with a refreshed mind.

A concluding remark and thought

First the remark: a special feature of this volume is that all the authors present here did actually meet physically at an international conference, *Perspectives on Mathematical Practices* (*PMP2002*), held at *Vrije Universiteit Brussel*, between 24 and 26 October 2002. It was organized by the *Centre for Logic and Philosophy of Science* (*CLWF*) at the same university (for the full program, see http://www.vub.ac.be/CLWF/PMP2002). This explains for a part the coherence among the diverse contributions, presented here. The other part explaining the coherence has to do with the fact that a selection has been made out of all contributions at the conference. However, the remaining contributions are not lost to the reader for these have been published in the logico-analytical journal *Logique et Analyse* (Van Kerkhove & Van Bendegem [2002]). The table of contents of this special volume can be found at: http://www.vub.ac.be/CLWF/L&A/. Finally, we like to mention that a discussion and mailing group has been launched at http://www.vub.ac.be/CLWF/mathprac/, to ensure that the debates will indeed continue.

And then the thought: there is a famous quote attributed to the famous French mathematician Jean Dieudonné (see [1982], p. 23): "Celui qui m'expliquera pourquoi le milieu social des petites cours allemandes du XVIII[e] siècle où vivait Gauss devait inévitablement le conduire à s'occuper de la construction du polygone régulier à 17 côtes, eh bien, je lui donnerai une médaille en chocolat." ("The person who will explain to me why the social setting of the small German courts of the 18th century wherein Gauss lived forced him inevitably to occupy himself with the construction of a 17-sided regular polygon, well, him I will give a chocolate medal.") Although perhaps this book is not a straightforward answer to Dieudonné's worry, at least it seems reasonable to start to think about what kind of chocolate and what kind of medal we would like to have.

the editors,
Bart Van Kerkhove
Jean Paul Van Bendegem

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I

HOW TO DEAL WITH MATHEMATICAL PRACTICE?

Chapter 1

HOW AND WHY MATHEMATICS IS UNIQUE AS A SOCIAL PRACTICE

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Abstract: Difficulties are raised for views that explain consensus in mathematics using only sociological pressure. Mathematical proof is sociologically very peculiar, when compared to other socially constrained practices. A preliminary analysis of the factors that have been at work historically in the "benign fixation of mathematical practice" are then exhumed: dispositions, implicit applications, an implicit logic, all play a role.

Key words: Consensus, mathematical proof, socially-constructed objects, drift, mature mathematics, contemporary mathematics.

1.

I'm sympathetic to *many things* those who self-style themselves "mavericks" have to say about how mathematics is a *social* practice. I'll start with the uncontroversial point that mathematicians usually reassure themselves about their results by showing colleagues what they've done. But *many* activities are similarly (epistemically) social: politicians ratify commonly-held beliefs and behavior; so do religious cultists, bank tellers, empirical scientists, and prisoners.

Sociologists, typically, study methods of attaining *consensus* or *conformity*[1] since groups act in *concert*. And (after all) ironing out

[1] Attaining "conformity" and "consensus" are mild-sounding phrases for what's often a pretty *brutal* process. Although what I say is intended to be understood generally, the reader does best *not* to think of the practice of torturing political deviants (in order to bring them and their kind into line), but of doting parents teaching children to count, to hold forks, to maneuver about in clothing, or to speak.

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mathematical "mistakes" is suppressing a form of *deviant behavior*. One way to find genuine examples of socially-induced consensus is to limn the range of behaviors *possible* for such groups. One *empirically* studies, that is, how groups deviate from one another in their (group) practices. Consider admissible eating behavior. The options that *exist* are virtually *unimaginable*: in *what's* eaten, *how* it's eaten (in what order, with what *tools*, over how much time), how it's cooked—*if* it's cooked—what's allowed to be said (or not) during a meal, and so on. To understand why a group (at a time) eats meals as it does, and why its members find variants inappropriate (even *revolting*), we see how consensus is determined by childhood training, how ideology crushes variations by making them *unimaginable* or viscerally *repulsive* (so that, say, when someone imagines a *cheeseburger*, what's felt is—nearly instinctive—disgust), and so on. Equally coercive social factors in conjunction with the ones just mentioned explain why we obey laws, respect property (in the *particular* ways we do), and so on: The threat of punishment, corporeal or financial.

Before turning specifically to mathematical practice, note two presuppositions of any empirical study of the social induction of consensus (and which have been assumed in my sketchy delineation of the sociology of eating). First, such *social* induction presupposes (empirical) evidence of the *possibility* of alternative behaviors. The best way (although not the only way) to verify that a kind of behavior *is* possible is to find a group engaged in it; but, in any case, if a behavior is biologically or psychologically impossible, or if the resources available to a group prevents it, we don't need social restraints to explain why individuals *uniformly* avoid that behavior.

The second presupposition is that the study of social mechanisms should uncover factors powerful enough to exclude (in a given population) the alternatives we otherwise know are possible; either the absence of such factors, or the presence of empirical reasons that show such factors can't *enforce* behavioral consensus, will motivate the hypothesis of *internal factors*— psychological, physiological, or both—in conforming individuals: consider, for example, the Chomskian argument that internal *dispositions* in humans strongly constrain the general form of the rules for natural languages.

2.

Let's turn to mathematical practice. There are two striking ways it seems to differ from just about *any* other group practice humans engage in. One has been repeatedly noted by commentators on mathematics; the other, oddly, is (pretty much) overlooked.

It's widely observed that, unlike other cases of conformity, and where social factors *really are* the source of that conformity, one finds in mathematical practice *nothing like* the variability found in cuisine, clothing, or metaphysical doctrine. There *are* examples of deviant computational practices: Babylonian fractions or the one-two-many form of counting; but overall empirical evidence for the possible deviation from standard mathematical practice—at least until the twentieth century—isn't rich.

Two points. First, as Kripke and others have noted (in the wake of Wittgenstein),[2] it's easy to *design* thought experiments where people, impervious to correction, *systematically* follow rules differently from us. Despite the ease of *imagining* people like this, they're not *found* outside philosophical fiction. One *does* (unfortunately) meet people who can't grasp rules at all—but that's different. In rule-following thought experiments someone is portrayed who seems to follow *a* rule but who also understands "similar," so that she "goes on" differently from us. (After being shown a finite number of examples of sums, she sums new examples as we would until she reaches a particular border (pairs of numbers both over a hundred, say) whereupon she sums differently—in some systematic way—while claiming she's still doing the same thing. This really is different from people who don't grasp generalization at all.)

Despite the absence of *empirically real* examples of alternative rule-following (in counting or summing), such *thought experiments* are often used to press the view that it's (purely) *social factors* that induce mathematical consensus. Given my remarks about *appropriate* empirical methods for recognizing *real* options in group practices, such a claim—to be empirically respectable, anyway—can't batten on thought experiment alone; it needs an analysis of social factors that arise in *every* society that counts or adds—and which force humans to agree to the same numerical claims. The social factors that are pointed to, however, for example childhood learning, are ones shared by almost every other group practice (diet, language, cosmetics, and so on) which—in contrast to mathematical practice—show great deviation across groups. That is, even when systematic algorithmic rules (such as the ones of languages or games) govern a practice, that practice still drifts over time—unlike, as it seems, the algorithmic rules of mathematics.[3]

2 E.g.: Kripke 1982, Bloor 1983, and, of course, Wittgenstein 1953.

3 I should make this clear: by "drift" I mean a change in the rules and practices which doesn't merely involve augmentation of such rules, but the elimination of at least some of them. Mathematics is always being augmented; the point of denying "drift" in its case is that such augmentation is overwhelmingly monotonically increasing.

One possible explanation for this[4] is that practical exigencies exclude deviant rule-following mathematics: someone who doesn't add as we do can be exploited—in business transactions, say. And so it's thought that deviant counting would die quickly. But this idea is sociologically naive because, even if the dangerousness of a practice did imply its quick demise, this wouldn't mean it couldn't emerge to begin with, and leave evidence in our historical record of its temporary stay among us; all sorts of idiotic and quite dangerous practices (medical ones, cosmetic ones, practices motivated by religious superstition) are *widespread*. Even *shallow* historical reading exposes a plethora of, to speak frankly, pretty dumb activities that (i) allowed exploitation of all sorts (*and* helped shorten lives), and yet (ii) didn't require too much insight to *realize* were both pretty dumb and pretty dangerous. *There's* no shortage of such practices *today*—as the religious right and the raw-food movement, both in the United States, make clear. So it's hard to see why there can't have been really dumb counting practices that flourished (by, for example, exploiting the rich vein of number superstition we *know* existed), and then died out (along with the poor fools practicing them).

Another way around the apparent sociological uniqueness of mathematical practice is the blunt response that mathematical practice *isn't* unique; there *are* deviant mathematical practices; we just haven't looked in the right places for them. Consider, instead of *counting* variants, the development of alternative mathematics—intuitionism, for example, or mathematics based on alternative logics (e.g., paraconsistent logics). Aren't *these* examples of mathematical *deviancy* every bit as breathtakingly different as all the things people willingly put in their mouths (and claim tastes good and is good for them)?

Well, *no*. What should strike you about "alternative mathematics"—unless you're blinded by an a priori style of foundationalism, where a specific style of mathematical proof (and logic), and a specific subject matter, are definitional *of* mathematics—is that such mathematics is *mathematics as usual*. One mark of the ordinariness of the stuff is that contemporary mathematicians shift in what they prove results about: they practice one or another branch of "classical" mathematics, and then try something more exotic—if the mood strikes. Proof, informal or formal, looks like the same thing (despite principles of proof being *severely* augmented or diminished in such approaches).

[4] See e.g., Hersh 1997, p. 203.

Schisms among mathematicians, prior to the late nineteenth century, prove even *shallower* than this.[5] That differences in methodology historically prove divisive can't be denied: differences in the methodology of the calculus, in England and on the continent, for example, *retarded* mathematical developments in England for over a century. Nevertheless, one finds British mathematicians (eventually) adopting the continental approach to the calculus, and doing so because they (eventually) recognized that the results they wanted, and more generally, the development of the mathematics surrounding the calculus, were easier given continental approaches. British mathematicians didn't deny the cogency of such results on the grounds that the methods that yielded them occurred in a "different (incomprehensible) tradition."

Let's turn to the second (*unnoticed*) way that mathematics *shockingly* differs from other group practices. *Mistakes are ubiquitous in mathematics*. I'm not *just* speaking of the mistakes of professional—even brilliant—mathematicians although, notoriously, they make *many* mistakes;[6] I'm speaking of *ordinary* people: they find mathematics *hard*—harder, in fact, than just about any other intellectual activity they attempt. What makes mathematics difficult is (1) that it's *so easy* to blunder in; and (2) that it's *so easy* for others (or oneself) to see—when they're pointed out—that blunders *have* been made.[7]

So? This is where it gets cute. When the factors forcing behavioral consensus are genuinely social, *mistakes can lead to new practices*. This is for two reasons at least: first, because the social factors imposing consensus are often blind to details about the behaviors enforced—they're better at imposing uniformity of behavior than at pinning down *exactly which* uniform behavior the population is to conform to. If enough people make a certain mistake, and if enough of them pass the mistake on, the social factors enforcing consensus continue doing so despite the shift in content. Social mechanisms that impose conformity are good at synchronic enforcement; they're not as good at diachronic enforcement. (Thus what's sometimes described as a "generation gap.")[8]

5 One point of this paper is to provide an explanation for why this should be so; see what's forthcoming, especially section 6.

6 This is especially stressed in the "maverick tradition" to repeatedly hammer home the point that proof doesn't confer "certainty."

7 What makes mathematics hard is both how easy it is to make mistakes and how difficult it is to hide them. Contrast this with poetry. It's as easy to make mistakes in poetry—write stunningly bad poetry—as it is to blunder in mathematics. But it's much easier to cover up poetic blunders. Why that is is extremely interesting, but something I can't fully get into now.

8 Consider school uniforms. All sorts of contingent accidents cause mutations in such uniforms; but that (at a time) the uniforms should be, um, uniform, is a requirement. It's

The second reason is that the power of social factors to enforce conformity often turns on the successful *psychological internalization* of social standards; but if such standards are imperfectly internalized (and *any* standard—however mechanical, i.e., algorithmic—can be imperfectly internalized), then the *social standards themselves* can evolve, since, in certain cases, nothing else fixes them. Two examples are, first, the drift in natural languages over time: this is often because of systematic mishearings by speakers, or interference phenomena (among internalized linguistic rules), so that certain locutions or sounds drop out (or arise). The second example is when an external standard supplementing psychological internalization of social standards is operative, and is *taken to* prevent drift—for example holy books. Notoriously, such things are open to *hermeneutical drift*: the subject population reinterprets them (often inadvertantly) because of changes in language, "common sense," and therefore changes in their (collective) view of what a given law-maker (e.g., God) *obviously* had in mind.

In short, although every social practice is easy to blunder in, it's not at all easy to get people to recognize or accept *that* they've made mistakes (and therefore, if enough of them do so, it's impossible—nearly enough—to *restore* the practice as opposed to—often inadvertantly—starting a new one).

The foregoing focus on mistakes *isn't* meant to imply that *conscious* attempts to change traditions aren't effective: of course they can be (and often are). But mathematical practice resists *willful* (deliberate) change too. A dramatic case of a conscious attempt to change mathematical practice which failed (in large part because of incompetence at the standard fare) is Hobbes[9]. Another informative failure is Brouwer, because Brouwer was *anything but* incompetent at the standard practice.

Notice the point: Brouwer wasn't interested in developing *more* mathematics, nor were (and are) the other kinds of constructivists that followed; he wanted to change the *practice*, including his own earlier practice. But he only succeeded in developing *more* mathematics, not in changing *that* practice (as a whole). This makes Hilbert's response to Brouwer's challenge, by the way, *misguided*, because Hilbert's response was also predicated on (the fear of) Brouwer inducing a change in the practice. This is common: fads in mathematics often arise because someone (or a group, e.g., Bourbaki) thinks that some approach can become *the* tradition of mathematics—the result, invariably, is just *more* (additional) mathematics. A related (sociological) phenomenon is the mathematical *kook*—there

very common for a population to slowly evolve its culinary practices, dress, accent, religious beliefs, etc., without realizing that it's doing so.

9 See Jesseph 1999.

enough of these to write *books* about.[10] Only a field in which the recognition of mistakes is extremely robust can (sociologically speaking) successfully marginalize so many otherwise competent people *without* standard social forms of coercion, e.g., prison.

So (to recap.) mistakes in mathematics are common, and yet mathematical culture doesn't splinter because of them, or *for any other reason* (for that matter)[11]; that is, permanent *competing* practices don't arise as they can with other socially-constrained practices. This makes mathematics (sociologically speaking) *very odd*. Mathematical standards—here's another way to put the point—are robust. Mistakes *do* persevere, of course; but mostly they're eliminated, even when *repeatedly* made. More importantly, mathematical practice is so robust that even if a mistake eludes detection for years, and even if many results are built on that mistake, this *won't* provide enough social inertia—once the error *is* unearthed—to resist changing the practice back to what it was originally: in mathematics, even after lots of time, the subsequent mathematics built on the "falsehood" is repudiated.[12]

This aspect of mathematical practice has been (pretty much) unnoticed, or rather, *misdescribed*; and it's easy to see how. If one focuses on *other* epistemic issues, scepticism say, one can confuse the rigidity of group standards in mathematics with the availability of *certainty*: one can claim that, if only one is *sufficiently careful*, *really* attends to each step in a proof, carefully analyzes proofs so that each step immediately follows from earlier

10 For example, Dudley 1987.

11 Philip Kitcher, during the discussion period on November 21, 2002, urged otherwise—not with respect to mistakes, but with respect to conscious disagreement on method: he invoked historical cases where mathematicians found themselves arrayed oppositely with respect to methodology—and the suggestion is that this led to schisms which lasted as long (comparatively speaking) as those found among, say, various sorts of religious believers: one thinks (again) of the controversy over the calculus, or the disputes over Cantor's work in the late nineteenth century. What's striking—when the dust settles, and historians look over the episodes—is how nicely a distinction may be drawn between a dispute in terms of proof procedures and one in terms of admissible concepts. The latter sort of dispute allows a (subsequent) consistent pooling of the results from the so-called disparate traditions; the former does not. Thus there is a sharp distinction between the (eventual) outcome of disputes over the calculus, and (some of) those over Cantor's work. The latter eventually flowered into a dispute over proof procedures which proved irresolvable in one sense (the results cannot be pooled) but not in another. See 6.

12 Contrast this with our referential practices: Evans (1973, p. 11) mentions that (a corrupt form of) the term "Madagascar," applied to the African mainland, was mistakenly taken to apply to an island (indeed, the island we currently use the term to refer to). Our discovery of this error doesn't affect our current use of the term "Madagascar"—the social inertia of our current referential practice trumps any social mechanisms for correcting dated mistakes in that practice.

ones, dutifully surveys the whole repeatedly until it can be intuited in a flash, then one can rig it so that—in mathematics, at least—one won't *ever* make any mistakes *to begin with*: one can be totally *certain.*[13]

But there's a number of, er, mistakes in this Cartesian line. First, it's a robust part of mathematical practice that mistakes are found and corrected. Even though the practice is therefore *fixed* enough to rule out deviant practices that would otherwise result from allowing such "mistakes" to change that practice, this *won't* imply that *psychologically-based* certainty is within reach. For it's compatible with the robustness of our (collective) capacity to correct mathematical mistakes that some mistakes are still undetected—even old ones.

Apart from this, the psychological picture the Cartesian recipe for certainty presupposes is inaccurate. It's very hard to correct your own mistakes, as you know, having proofread your work in the past. *And* yet, someone else often sees *your* mistakes at a glance. This shows that the Cartesian project of gaining certainty *all alone*, a strategy crucial for Descartes' demon-driven epistemic program, is quixotic.[14]

Notice, however, that the Cartesian view would explain, *if it were only true*, how individuals can disagree on an answer, look over each other's work, and then come to agree *on what the error is*. (They become CERTAIN of THE TRUTH, and THE TRUTH is, after all, THE SAME.) Without this story, we need to know what practitioners have internalized (psychologically) to allow such an unnaturally agreeable social practice to arise.[15]

[13] And then one can make this an epistemic requirement on all knowledge (and offer recipes on how to carry it off). Entire philosophical traditions start this way.

[14] One of the ways Newton is so remarkable is that he did so much totally on his own, by obsessively going over his own work. (See Westfall 1980.) Newton's work is an impressive example of what heroic individualistic epistemic practice can sometimes look like. Despite this, Newton made mistakes.

[15] Unlike politics, for example, or any of the other numerous group activities we might consider, mathematical agreement isn't coerced. Individuals can see who's wrong; at least, if someone is stubborn, others (pretty much all the competent others) see it. Again, see Jesseph 1999 for the Hobbesian example. Also recall Leibniz's fond hope that this genial aspect of mathematical practice could be grafted onto other discourses, if we learned to "calculate together." By contrast, Protestantism, with all its numerous sects—in the United States especially—is what results when coercion isn't possible (because deviants can, say, move to Rhode Island). And much of the history of the Byzantine empire with its unpleasant treatment of "heretics" is the normal course of events when there's no Rhode Island to escape to. It's sociologically very surprising that conformity in mathematics isn't achieved as in these group practices. Imagine—here’s a dark Wittgensteinian fable—we tortured numerical deviants to force them to add as we do. (Recall, for that matter, George Orwell’s 1984.)

To summarize: What seems odd about mathematics as a social practice is the presence of substantial conformity on the one hand, and yet, on the other, the absence of (sometimes brutal) social tools to induce conformity that routinely appear among us *whenever* behavior really is socially constrained. Let's call this "the benign fixation of mathematical practice."

3.

The benign fixation of mathematical practice *requires* an explanation. And (it should be said) Platonism is an appealing one: mathematical objects have their properties necessarily, and we perceive these properties (somehow). Keeping our (inner) eye firmly on mathematical objects keeps mathematical practice robust (enables us to find mistakes). The problem with this view—as the literature makes clear—is that we can't explain our epistemic access to the objects so posited.[16]

One might try to finesse things: demote Platonic objects to socially-constructed items (draw analogies between numbers and laws, language, banks, or Sherlock Holmes). Address the worry that socially-constructed nonmathematical objects like languages, Mickey Mouse, or laws, *evolve over time* (and that their properties look conventional or arbitrary) by invoking the content of mathematics (mathematical rules have content; linguistic rules are only a "semitransparent transmission medium" without content). And, claim that such content makes mathematical rules "necessary."[17]

As this stands, it won't work: we can't bless necessity upon whatever we'd like by chanting "content." Terms that refer to fictional objects have content too—that doesn't stop the properties attributed to such "things" from *evolving* over time; socially-constructed objects are *our* objects—if we take their properties to be fixed, that's something we've (collectively) imposed on them. It's a good question *why* we did this with mathematical terms, and not with other sorts of terms.

If socially-constructed objects are stiffened into "logical constructions" of some *fixed* logic plus set theory (say), this doesn't solve the problem: one

16 Current metaphysics robs Platonism of respectability. Sprinkle mysticism among your beliefs, and the perceptual analogy looks better; introduce deities to imprint true mathematical principles in our minds, and the approach also looks appealing. Deny all this, and Platonism looks bizarre.

17 See Hersh 1997, p. 206. I deny that (certain) socially-constructed objects, mathematical objects and fictional objects, in particular, exist in any sense at all. See my 2004a. Nominalism, though, won't absolve me of the need to explain the benign fixation of mathematical practice. On the contrary.