

CONTRIBUTIONS TO A SCIENCE OF CONTEMPORARY MATHEMATICS

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ABSTRACT. This essay provides a description of modern mathematical practice, with emphasis on differences between this and practices in the nineteenth century, and in other sciences. Roughly, modern practice is well adapted to the structure of the subject and, within this constraint, much better adapted to the strengths and weaknesses of human cognition. These adaptations greatly increased the effectiveness of mathematical methods and enabled sweeping developments in the twentieth century.

The subject is approached in a bottom-up ‘scientific’ way, finding patterns in concrete micro-level observations and being eventually lead by these to understanding at macro levels. The complex and intensely-disciplined technical details of modern practice are fully represented. Finding accurate commonalities that transcend technical detail is certainly a challenge, but any account that shies away from this cannot be complete. As in all sciences, the final result is complex, highly nuanced, and has many surprises.

A particular objective is to provide a resource for mathematics education. Elementary education remains modeled on the mathematics of the nineteenth century and before, and outcomes have not changed much either. Modern methodologies might lead to educational gains similar to those seen in professional practice.

This draft is about 90% complete, and comments are welcome.

1. INTRODUCTION

This work offers a detailed qualitative description of contemporary professional practice, particularly in core mathematics. The goal is to be useful to mathematicians, users of mathematics, administrators, educators, and students. Education is a particular concern.

This introduction provides context and perspective. In particular the meanings of “science”, “contemporary”, etc. in the title are explained. The table of contents follows the introduction.

1.1. About “Science”. This development began with concrete observations, and generalities were extracted from these in a gradual and disciplined way. Each stage was been challenged in every way I could find, to maximize consistency and explanatory power. The point is that the process of abstracting and synthesizing large-scale explanations will magnify small-scale confusions into large-scale nonsense, and constant challenge is necessary to prevent this. This is standard practice in science and is the sense in which the work is scientific.

Many of the observations and challenges came from personal experiences and are briefly described here. These include publication practices, §2.1.1; sociology, §2.1.2; history, §2.1.3 and §9; physics and computing, §2.2.2; education, §2.4.9; and behind

it all 40 years of toil at the research frontier, §2.2.2, 10.1, and 10.4. Other insights and challenges came from Jeremy Gray [24] §9.5, David Corfield [11] §10.4, studies in cognitive neuroscience, [67](i,j), and many other sources, c.f. §10.

The result has many of the standard features of science. First, there were many surprises and the end result is quite different from my naïve expectations. For example, strategies for resolving the tension between the technical rigidity of the subject and the limits of human cognition turned out to be a dominant theme. Next, there were a great many false starts, dead ends, and abandoned hypotheses. This is usual in science, and is the mechanism by which surprises eventually reveal themselves. And as usual, the final account has been sanitized and the dead branches pruned away. This hides the development process but is necessary for the end result to be comprehensible and useful.

Another scientific feature is that the story is richly complex with many tightly interwoven threads. Carefully understanding anything usually requires following the threads back to their roots. Questions rarely have simple answers, and often as not the conclusion is “that is not quite the right question”. On the other hand the answers provide powerful and effective guidance for education [67], and hopefully also for professional practice. As usual in science this guidance is expected to be a consequential and testable starting point for refinement, not infallible.

A final similarity to science is that this material has little in common with philosophy, either in methodology or in conclusions. Professional philosophers seem as irrelevant as Aristotle is to modern physics, though a partial exception is described in §10.4. Philosophy emerges as a villain in the historical account in §9, see §9.3, 2.4.5, and in education, §8.1. Philosophy is still the dominant approach, however, so a clarification is in order. The power of the ways in which ideas are challenged is revealed much more by the ideas that failed than by the ones that survived. The fact that failures have been expunged therefore conceals much of the filtering process, and may make the final version seem more deductive than it really is.

Finally, this essay is *Contributions to* rather than simply “A science of mathematics” because science must be a community activity, and this is the work of a single individual. I look forward to a time when this is no longer true.

1.2. About “Contemporary”. Professional mathematics changed profoundly between about 1850 and 1930 and the concern here is with practice as it stabilized and became relatively uniform by 1950.

Roughly, for most of history mathematics was guided and validated by applications to the physical world and explained by philosophy. But very little of the mathematical universe is directly visible in the physical world, and philosophical ideas about truth, reality, knowledge etc. are not suited to either mathematics or human cognition. Consequently the methodologies influenced by these points of view were not well adapted, either to the subject or the practitioners. By the mid nineteenth century they were no longer satisfactory in many areas. Genuinely well-adapted methodologies evolved and were adopted in the early twentieth century, and enabled explosive development later in the century.

This transition is analogous to the seventeenth-century ‘scientific revolution’ in physics from Aristotle to Galileo, Newton et. al., the eighteenth-century transition from alchemy to chemistry, and similar but more gradual transitions in biology and medicine. Some of the history of the transition is traced out in §9, with descriptions of the methodological changes. I also attempt to explain why the transformation is

not more widely recognized, and related to this, in §10, why modern descriptions of mathematics are so widely divergent and ineffective.

1.3. About Mathematics. First, the focus is on subject-specific features of the mathematical enterprise. General issues such as the psychology of individual discovery, the organization of research programs and agendas, and the setting of priorities are certainly important. However they are common to all sciences, if not most human activities, and the rich variety of styles seen in mathematics suggests that they are not much constrained by the subject matter. For the most part such topics are neglected here.

The second comment concerns scope. Scientists work hard to adapt themselves and their methods to their subjects, and immense variety of subjects leads to immense variety in methodology. Large-scale commonalities come mainly from human nature, and restriction is necessary to see structure. Knorr-Cetina [38] explores in detail commonalities in experimental high-energy physics on the one hand, and in molecular biology on the other, and contrasts the two. A more casual comparison of pure mathematics and theoretical physics is given here in §11. But much of the structure occurs on much smaller scales: the papers in [52], for instance, describe strong micro-adaption to different organisms in experimental biology. Lumping together fruit flies, flatworms, and a few small fish would obscure quite a bit of the subject adaption. The point is that structure is where you find it, not where you think it should be, and by default it should be expected at small scales.

There is not much commonality in the totality of things considered mathematical. The surprise—to me anyway—is that there is extensive commonality in contemporary core mathematics, and this is the main focus of this essay. “Contemporary” is explained above, and “core” indicates work done to standards necessary for use inside mathematics. Core practice is shaped by the validation criterion: criteria in other sciences are largely external (agreement with the physical world) while core mathematics uses *internal* criteria (correct proof), see §2.2. The domain in which this is possible is rigid and confined, but the payoff is completely reliable conclusions. Uniformity of practice seems to be necessary for this to be fully effective. This uniformity is now embedded in practices, ethics and norms of the community.

The complement of core mathematics is “mathematical science” §3. Mathematical science includes most of applied mathematics because applications often require use of conclusions validated by external criteria (agreement with experiment, robust numerical implementations, physical intuition etc.) rather than rigorous proof. However the core/science division is not the same as the traditional pure/applied separation: some applied mathematics is fully rigorous, and conversely pure conclusions that depend on intuition or “direct perception” qualify as science. Mathematical science is more far-ranging and occupies a larger part of the mathematical enterprise than does core mathematics. By the same token, the use of a variety of external criteria reduces commonality in the mathematical part of the activity. They lead to a variety of other commonalities, particularly with other areas in science, but these are beyond the scope of this essay.

In evolutionary terms the early twentieth-century transformation can be seen as a speciation. A new species—contemporary core mathematics—split off and was hugely successful in its niche. Mathematical science has more continuity with the past and represents the continuation of the original species. It had limited success

in what is now the core-math niche, but has prospered in environments inaccessible to core mathematics.

1.4. About Humans. Mathematics is a human activity. The subject imposes structure and constraints, and practice must be well-adapted to these before it can be successful. On the other hand human cognitive facilities also impose constraints, and successful practice must accommodate these as well.

For example it is unique to mathematics that errors can reliably be found by checking arguments for rule violations, §2.2. However the checking must (at present) be done by humans, and this limits formats that can be used, how much detail is needed to enable discovery of errors, etc., §4. Similarly, mathematical arguments require that objects be specified completely and explicitly, and modern axiomatic definitions do this. However the axiomatic format is not forced by the subject. The format is a cognitive device that has evolved to enable human use, and in fact modern definitions are one of our most powerful cognitive tools, §5.

It seems that before the twentieth-century break, mathematics was distorted to fit philosophical models of both humans and the subject. After the break, practice evolved and adapted, in ways we do not understand, to features of the subject and human cognition that we also do not understand. The result is that understanding contemporary core practice reveals as much about human cognition as about the subject.

1.5. About Education. The picture that emerges is that mathematics education in the twentieth century, and so far in the twenty-first, is modeled on practices of the nineteenth century that are now largely obsolete.

To expand on this, the new math- and cognitively-adapted methodologies of the early twentieth century were unattractive to philosophers and many traditional mathematicians. They charged that the new methods were “devoid of meaning”, “formal symbol manipulation”, “disconnected from reality”, etc. Professional mathematics changed, as it had to, but the arguments against the new methods were never answered on the philosophical level. The connection with philosophy was abandoned rather than updated. As a result, views of people who did not have to contend with the technical failure of the old methods were also not updated. Moreover during the transition some traditional mathematicians, most notably Felix Klein, were very influential in education and strongly imprinted nineteenth-century values on early twentieth-century education reforms, see §8. The result is that the nineteenth-century philosophical description of mathematics is still the dominant one outside the core professional community, and nineteenth-century arguments against contemporary practices are deeply embedded in educational philosophy and culture. This has led to a deep divide between mathematicians and educators, and between contemporary mathematics and education.

One of the objectives of this essay is to begin to bridge this divide.

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2. MATHEMATICS AND MATHEMATICAL METHODS

I follow the threads of contemporary mathematics through four levels of sophistication, beginning with reliability. The social structure of the mathematical community, publication practices, attitudes of users, and many other things point to reliability as the crucial feature of core practice. This is the topic at the first level, and some of it is expanded in §11. Deeper levels concern how reliability is obtained.

The second level describes how mathematical methods, particularly proofs, produce reliable results. Implications for education are explored later in §8 and [67]. The third level traces the evolution of reliability-producing mathematical methods, and the final level describe the real-world context for this evolution.

Computer methods are not qualitatively different from this point of view and mechanisms already in place seem robust enough to deal with them. However the quantitative differences may be great enough to make this unclear, so this is discussed in §??. Finally, the concern here is with methodology and sociology. Cognitive aspects are addressed in later sections.

2.1. Level 1: Complete reliability. *As a first approximation, the characteristic feature of contemporary core mathematics is that it gives completely reliable conclusions.*

Reliability is a feature of finished products. Informal and preliminary work—exploration, discovery, speculation, motivation, explanation etc.—is not expected to be reliable. Different people think in very different ways and as in any subject we celebrate any route to inspiration, reliable or not. However the mathematics-specific

issues, and the principal topics of this essay, concern reliability of finished products and the methods used to extract reliable conclusions from uncertain inspiration.

2.1.1. *Reliability and Publication.* My first glimpse of the explanatory power of reliability came in a study of mathematical publication practices [62, 63, 60, 64, 65], in the context of the transition to electronic publication. Special features of the mathematical literature include:

- Durability. The primary literature remains useful much longer than in other sciences, and citation half-lives of individual papers are longer.
- Extent. Key papers in other sciences tend to be concentrated in a small number of elite journals while mathematics is more widely distributed. When the JSTOR archive project [32] first started, their procedure was to digitize the half-dozen or so core journals of each academic area. However they found that a reasonable approximation to the core of mathematics seemed to require at least three times this number, and they are now up to fifty (including statistics).
- Lack of repetition. Other sciences tolerate or even encourage duplication in the literature, perhaps as an extension of the idea of “replication” of experiments. Mathematics by-and-large does not tolerate duplicate publication.
- Weak secondary literature. In other sciences the primary literature is filtered and compressed in the secondary literature (monographs, surveys, texts), and most citations point to the secondary literature. In mathematics the primary literature is already reliable so it does not need to be filtered and does not compress as much. The secondary literature is less extensive and contributing to it is not as high-profile an activity as in other sciences.

All of these require consistent reliability, and reflect a dependence on it.

2.1.2. *Reliability and Sociology.* My second encounter with reliability concerned, roughly speaking, sociology and ethics [30, 66]. The issue was whether heuristic arguments or “physical level of rigor” should qualify as *finished products* in mathematics. Two centuries ago the answer would have been “yes”, and a century ago “maybe”, but today the answer—for core mathematics—is “no”. As a human enterprise core mathematics has adapted to, and become dependent on, a degree of reliability that these methods cannot provide. This adaptation is explored in §11, through a comparison with theoretical physics.

I was led into this by experiences with physics and computing. The story is related in the next level §2.2.2 because eventually I learned more from it.

The behavior of users in other fields also reveals adaptation to mathematical reliability. If mathematical analysis of a model gives results not in agreement with observation, then users conclude that the wrong model is being used or the data is bad. Garbage *out* must mean garbage *in*, because they are confident that mathematical analysis will not turn good data into garbage.

People applying mathematics occasionally have problems with misunderstandings, or with material presented as mathematics that has not in fact been mathematically verified. These, however, are negligible compared to other sources of error. For the most part, reliability of mathematics is so deeply ingrained that it has become invisible and other scientists don’t realize the magnitude of their dependence on it. The mathematical view, of course, is that other scientists are not nearly appreciative enough of our hard work.

2.1.3. *Reliability and History.* In a study of the history of manifolds (unpublished, see [71]) I saw the influence of reliability, and the methodological transition mentioned in the introduction and §9. Erhard Scholz has a wonderful description of this area in the period 1850–1920 [75]. It comes through clearly in his account that the methodology was pre-modern in many ways, including:

- Reliance on intuition and direct insight rather than formal definitions and proof.
- The role of philosophy in providing meaning and direction.
- The role of, in Gray’s words [24], “unspoken realist assumptions”.

However the subject was considerably too subtle for this to succeed. The intuitive approach enabled Poincaré to develop deep and remarkable insights but it was inadequate for actual development, and the methods now seem clumsy and ineffective. Real success had to wait on the development and adoption of contemporary methods.

Manifolds lie at the confluence of topology, analysis and geometry. The mathematical transition in manifold theory seems to have come a little slower than in the constituent fields, no doubt due to the influence of Poincaré, Brouwer, and to some extent Felix Klein. In any case the field came into focus in the 1930s and grew explosively for the rest of the century.

My study of manifolds was too casual to be a reliable basis for conclusions and I did not make much of it at the time. Recently Gray [24] has provided a deep and thorough account of similar developments in many other areas of mathematics, and the manifold story turns out to be roughly representative of the whole discipline. This essay draws extensively on Gray’s work, see §9.

2.1.4. *Summary.* Even with the clues described above it me took about a decade and further experiences recounted in the next section, to overcome my disbelief that a real field of knowledge could be characterized in such a simple way. Indeed, contemporary core mathematics seems to be unique in this regard. However, bearing in mind that this is to be understood as a first approximation, and that “reliable” is not to be interpreted in a technical way¹, complete reliability seems to provide a robust foundation for understanding mathematics.

2.2. Level 2: Methods for achieving reliability. Mathematics provides basic facts and methods of reasoning with the following property:

If a mathematical argument produces a false conclusion then an error can be found in it.

This is shortened to ‘mathematical methods are error-displaying’. Reliability is obtained in practice by making error-displaying arguments and checking them very carefully for errors, see Proofs, §4.

We will see that there are strong limits on the conclusions that can be obtained this way, and that while these methods are very robust in some ways they are fragile in others.

¹The philosophical notion “reliabilism” [23], for instance, seems to be inappropriate here.

2.2.1. *Initial Comments.* These indicate some of the directions to be explored, and provide perspective. We add to the list in §2.2.4, after background in the next few sections.

- The error-displaying formulation is almost a restatement of ‘proof by contradiction’: an error-free argument with a false conclusion and *one* uncertain hypothesis forces that hypothesis to be false (there must be an error, and that is the only candidate).
- This formulation emphasizes that mathematics has an *internal* criterion for correctness. Mathematicians will accept things that have been proved as being correct, even when they are outrageously counterintuitive.
- This formulation identifies error detection as a crucial activity.

Proof by contradiction has had a central role in mathematics for millennia². Considering this, and with hindsight, the path from “reliability is the characteristic feature” through “how is it achieved?” to “error-displaying” seems straightforward. However I never asked “how is it achieved?” Like most mathematicians I regard meta-questions with suspicion and more likely to lead to error than to insight³, so more clues were needed. Relevant experiences are related in the next two sections; analysis of the topic resumes in §2.2.4.

2.2.2. *Adventures in Physics.* I spent a decade, roughly 1989–1999, on a computational project distantly related to physics. The goal was to detect counterexamples to the Andrews-Curtis conjecture about deformations of 2-dimensional complexes. This conjecture is expected to be false and there are many specific examples for which it is expected to fail, but for fundamental reasons the standard methods of topology are unable to detect any such failure.

I was interested in this problem because it is a good model for bizarre behavior of smooth 4-manifolds, and I was attracted to the latter because I had been quite successful in understanding *topological* 4-manifolds [19]. Standard methods of topology are also powerless to deal with smooth 4-manifolds. In that case, however, deep and subtle analysis partly aided by clues from physics had succeeded. Furthermore there was a related development in dimension three (Reshitikhin-Turaev, [73]) and an outline of a general context related to theoretical physics, “topological quantum field theory”, was proposed by Witten [85]. There was an excellent heuristic argument that a similar approach should work for 2-complexes.

The program had two components: abstract work to develop invariants and develop computational algorithms; and programming and computation. The key part was to show that prospective invariant are well-defined. It is all too easy and common to define invariants that don’t actually detect anything because a loophole lets the target phenomenon escape, and it is obviously a waste of time to compute such a thing.

I experienced serious culture shock trying to make sense of Witten’s proposal. He worked on the “physical level of rigor” using what I now see as the methodology

²Hardy [26] writes “Reductio ad absurdum, which Euclid loved so much, is one of a mathematician’s finest weapons.” My thesis is essentially that it is such a fine weapon that the rest of our methodology has evolved to support confident and aggressive use of it. Ironically most of Euclid’s methodology was eventually rejected because it was *not* precise enough, see §2.2.10.

³See Thurston [80] where careful phrasing of questions is used to justify conclusions overwhelmingly rejected by the mathematical community.

of mathematics in the nineteenth century and before: informal definition by example or by reference, and heuristic arguments based on personal intuition. This was completely alien to a mathematician trained in the late twentieth century. After consideration I decided I was willing to work in this mode—and publish in a physics journal—if it could address my problem. Unfortunately the ideas came apart immediately when I tried to modify them. Moreover this approach is inadequate for showing something is well-defined. Witten did not address this at all, and in fact his description is not quite well-defined. This may be appropriate in physics:

- A mathematical construct that manifests itself in physical reality presumably must be well-defined. The goal is to describe it in a way that exposes structure and permits calculation, and well-definition is not an issue.
- It is often better to leave details ambiguous and therefore adjustable as the theory develops. For instance an extremely precise alignment of the stars is required to produce a “topological quantum field theory” satisfying early versions of the axioms. Physically relevant theories probably won’t satisfy them, and the mathematical theories developed for smooth four-manifolds definitely do not. Too much precision too soon might lead in the wrong direction.

This approach is not effective in mathematics. Curiously, the ostensibly mathematical development (by Reshetikhin and Turaev[73]) of a theory satisfying Witten’s description also lacked significant detail related to showing the invariants were well-defined. I now see this as an instance of residual nineteenth-century methodology in the Russian school of mathematics, see 9.4.2.

It took several years to develop a rigorous abstract framework that includes 2-complexes and is sufficiently precise to support a dependable demonstration of well-definition [61]. Subsequent papers showed how to use monoidal categories to define field theories and developed algorithms for computation. If the computations had ever given an unexpected value, it would have been guaranteed to mean something. Sadly, this never happened.

2.2.3. *Insight from Education.* In the past decade I have spent many hundreds of hours providing one-on-one help to students working on computer-based materials. Students asked for help when they got stuck, and my job, as I saw it, was to diagnose specific errors and correct them in a way that would keep them from recurring. I cannot count the number of times I’ve said “let’s find the mistake”. I became adept at spotting work habits that invited and hid errors, and tried to correct them. Finally, I spent a great deal of time developing materials (e.g. solution hints) designed to make errors easier to find by maximizing “error-display”. Eventually I thought carefully about all this. The fact I was relying on, that mathematical errors *can* be found, is not obvious and is actually quite remarkable. Working in a way that displays errors, combined with active error-seeking and correction, provides reliability, not just for students but for all of us. My work with students had become a model of mathematics itself.

2.2.4. *Problematic Aspects.* I return to analysis of the topic. The error-displaying formulation as given above leaves a number of issues to be clarified:

- What are the facts and methods that mathematics “provides”, and why should we believe they are error-displaying? See §2.3.

- “Error-displaying” is supposed to mean *someone* should be able to find errors. Who, exactly? See §4.
- How can we know if an argument really is error-free, and therefore has reliable conclusions? It is usually humans that do the checking and humans are not completely reliable so this is a basic source of uncertainty. See §4.

These are serious problems at deeper levels. Before tackling them we further explore the current level.

2.2.5. *The Legal Analogy.* It is as though we have stumbled onto a language for writing contracts that Nature accepts as legally binding. If we write a valid contract (proof) then we get certain goods (conclusions of the theorem). But, as with any legal system, there is a loophole problem: a contract that is not really airtight can leave us empty-handed or with unwanted goods.

In these terms the ‘error-displaying’ property is: whenever the wrong product is delivered we can find a loophole in the contract that enabled this to happen. It seems that a loophole-free contract really does guarantee delivery of the correct product. There are obvious problems with being sure any particular contract has no loopholes, but in practice *very* careful study of the language and avoidance of sloppy or wishful thinking seems to work pretty well.

Some differences between mathematics and human law:

- Unlike commercial contracts we don’t have to *pay* for the goods, we just have to write a valid contract!
- The mathematical “laws” that govern these contracts aren’t recorded anywhere: we had to discover them by trial and error, see §2.3.
- We can’t legislate, enforce, or decide for ourselves what the laws are or the products to which they apply. Some things simply don’t work, and Nature generally regards our attempts to be too creative or greedy as just providing loopholes that justify non-delivery.
- Failures cannot be appealed.

But the goods acquired (mathematical conclusions) are both useful and free⁴, so we should accept what we can get and be grateful.

2.2.6. *Explicit Rules for Mathematics.* Genuinely error-displaying methods require explicit rules for argumentation: any method that is not made explicit cannot be used without appearing to be an error.

Parts of the descriptions of mathematical objects, and *some* of the rules of logic have been explicitly formulated for millennia. The idea that *all* of the rules should be made explicit is relatively recent and generally attributed to Frege around 1880, see [56] Development and adoption of a really effective set of rules is even more recent, see §9 for a historical discussion. Development (or discovery) of effective rules is described in §2.3; the point for the present discussion is simply that explicit rules are required. The slavish devotion of mathematicians to rigorous methodology is required by the subject and is not a character flaw.

2.2.7. *The ‘Correct Outcomes’ Formulation is Not Effective.* A common description of the mathematical process is *correct arguments give correct conclusions*. However this lacks logical force and invites confusion:

⁴Once the hard work of proof is done, anyway.

- Who is entitled to decide when an argument is correct, either methodologically or in the sense of being error-free? Leaving this unclear has been a perennial source of disagreement.
- This version is often interpreted as being to some extent a definition: arguments are *validated* by having correct conclusions. This is an *external* criterion in that it depends on things outside the argument itself. External validation is appropriate in other sciences but methods obtained this way are not sufficiently reliable to be satisfactory in contemporary mathematics.

2.2.8. *Mathematics vs. Science.* The observations about internal/external criteria provide a surprisingly sharp line between core mathematics and the other sciences.

- Mathematicians will accept an assertion on the basis of internal criteria, even when it is outrageously counterintuitive and not supported by any independent evidence. This would be highly unprofessional in any other science.
- Mathematicians, when doing core work (i.e. for use in mathematics) will not accept an assertion on the basis of external criteria, even when it seems to be incredibly well tested. See the discussion of the Riemann Hypothesis in §2.3.4 for an example. This would be highly unprofessional in any other science.

The second point has the qualification “core, or “for use in mathematics” because this is the context that requires full mathematical rigor: later work depends on complete reliability.

In fact a great deal of mathematical work is done for external use. Full mathematical rigor is not needed and this work is typically done in a more scientific mode. This gives access to a much larger world, but it is important to realize that it is a different world from mathematics. Confusion about this is a source of stress, see §3.

This distinction between mathematics and other sciences is important but in a larger sense superficial. We are all engaged in disciplined and subject-adapted searches for knowledge; mathematics just has a different source of discipline, adapted to an atypical subject. This brings me to the final point:

- Rigor plays the same role in mathematics that agreement with the physical world plays in other sciences. Relaxing rigor is like ignoring data.

2.2.9. *A Definition of Core Mathematics.* The observation in the previous section can be reformulated as a definition:

Core mathematics is the body of material that can be studied with error-displaying methods.

This is not a serious proposal to formally define the discipline; the point is to emphasize that mathematics is limited by its methods as well as empowered by them. Non-core mathematics is subject to different constraints and is much more like other sciences, see §3.

Note this is a *functional* definition in the sense that anything accessible to the methodology is fair game. Most proposed definitions have been descriptive, for example “the Science of Patterns”, c.f. Devlin, [14].

2.2.10. *Example: Euclidean Geometry.* Deficiencies in Euclid have been provoking objections for over 2,000 years quite apart from the failure of the parallel postulate, see M. Klein [36] §42.1. The perspective here provides a sharp understanding of these concerns: the methods are not error-displaying.

There are infamous proofs that all triangles are isosceles; see Klein for examples. We know the answer is wrong and after the fact we can identify the problem as unfortunate choices of diagrams. But they are identified as unfortunate using *external* criteria. They do not violate any rules and cannot be identified as erroneous using *internal* criteria. In other words, the methods do not display an error.

More generally the approach is essentially proof-by-example. It is emphasized that “generic” examples should be chosen, in other words ones that implement the universal quantifier (for every triangle ...). But, as observed above, there is no *internal* criterion for knowing whether or not any particular example is good enough. It succeeds most of the time because the topic is very simple, but it is a bad model for almost any other topic. If we don’t want students to use proof-by-example in later work we should avoid Euclid⁵.

Another problem of Euclidean geometry is that relationships such as ‘similar’ and ‘congruent’ are essentially undefined primitives. They are described operationally: triangles are congruent if one can be “moved” to coincide with the other. Taken literally this suggests that congruence is experiential and can be checked by actually moving things around. This is the feature that has drawn the most criticism over the millennia. It provides many opportunities for invisible errors and is certainly not mathematical by current standards.

A modern version of congruence is that there exists a distance-preserving function (rigid motion) that takes one thing to the other. To show things are congruent one must produce the function or logically demonstrate its existence. An error-displaying transcript of the argument pretty much requires that the function be assigned a name. Typically the function rather than the original objects becomes the center of attention. One gets drawn into the group theory of rigid motions, crystallography and other wonderful things. Moreover the definitions are appropriate for any dimension, and when formulated carefully many of the results generalize as well. The experiential Euclidean formulation blocks the way to all this.

Thirty-five years ago I attended a lecture of Dirk Struik⁶ in which he mused on the uneven pace of mathematical development. Geometry and arithmetic were both well-known to the ancients, he observed, so why did they not come together until sixteenth century Europe? In particular he felt that Omar al-Khayyam (he of the *Rubaiyat*) had all the pieces in hand; why didn’t he put them together? Struik speculated that the synthesis was inhibited by some sort of societal influence or mindset. Was the un-mathematical nature of some Euclidean methods part of the problem? Khayyam knew the Euclidean texts and identified the use of motion as a problem, but this was evidently not enough to overcome the blockage.

In §?? I analyze why Euclidean geometry has been so attractive to educators, and find another difficulty with the experiential formulation.

⁵Actual use of Euclid is now rare in the US but still common in some other countries. There is a movement to bring it back—with flaws reinforced rather than fixed—using “dynamic geometry” software.

⁶ Mathematician and historian of mathematics, 1894–2000. See the tribute in the Notices of the AMS at <http://www.ams.org/notices/200106/mem-struik.pdf>.

2.3. Level 3: Development of Methods. The error-displaying formulation of §2.2 refers to methods that “mathematics provides”. Where did they come from, and how did they come to be as effectively error-displaying as they seem to be? The contemporary answer is,

Mathematical methods evolve to maximize reliability, by maximizing the error-displaying property.

At the first level I explained that excellent reliability is so well established in core mathematics that it is even reflected in sociology and publication practices. The claim here is stronger: not only is reliability a fact but it is not an accident or byproduct of something else. There is something so powerful about reliability—or so damaging about the lack of it—that it has shaped the subject.

In short, mathematics has evolved to occupy a very special niche in the world of knowledge, one both empowered and constrained by complete reliability.

In this section I describe the sense in which methods evolve, and some of the mechanisms. The focus is on pressures that drove the subject toward complete reliability. A more global view at the fourth level in §2.4 shows that while reliability drives contemporary mathematical development, the situation was quite different in the nineteenth century and before, and there are forces that still work against reliability.

2.3.1. Short-Term Success of Erroneous Formulations. Every formulation of mathematics, at least until very recently, has been technically wrong but successful in a limited setting. It is important to understand how this happens because it is a source of confusion, and it clarifies the sense in which mathematical methods can evolve.

A formulation of mathematics is wrong if it is not error-displaying, or in other words if there is an argument without methodological errors that has a false conclusion. However this is only a problem if one actually *makes* such an argument, and it may be that lots of arguments don’t run afoul of the error. In the terminology used above, errors have not yet been magnified enough to be problematic. A related point is that believing something false does not cause problems unless the belief is used in a way that essentially uses its falseness. Some examples:

- It took the Greeks quite a while to get into trouble with the presumption that all numbers are rational.
- The example that shocked the Greeks, and that everyone knows about, is the irrationality of $\sqrt{2}$. Can this can be fixed by adding algebraic numbers (roots of polynomial with integer coefficients) to the number system? No, because π is not algebraic, but believing that irrationals are algebraic won’t harm calculus students. To get into trouble one must first of all say something like “let P be a polynomial with integer coefficients such that $P(\pi) = 0$ ” and this is far outside the usual scope of calculus⁷.
- Mathematicians in the eighteenth and early nineteenth centuries were successful with naïve infinitesimals, believing that bounded sequences converge, and that continuous functions are almost-everywhere differentiable.

⁷It is hard to get in trouble even if one says this. The existence of *any* transcendental number was only firmly established by Liouville in 1844, and the transcendence of π was first shown by Lindemann in 1882.

None of these are correct, but it took a long time to reach a depth where they became problematic.

- N ave set theory is known to be wrong by Russell’s paradox: the set-formation axiom is too strong. It is nonetheless useful to “working mathematicians” because it is rare that anyone uses the axiom in a way that could not be justified by a correct weaker version; see §10.1 for a discussion and §?? for an overview.

The historical pattern was that mathematicians worked with a formulation until it broke down. They then revised the formulation to avoid known problems, and proceeded until the next breakdown. This is an evolutionary process, and it continued until an effectively error-displaying formulation was developed. Evolution of a somewhat different sort is still going on, see §2.4.

These breakdown-and-repair cycles are similar to the ‘paradigm shifts’ of Kuhn [39], but without the extreme incommensurability of ideas. See Nickles [53] for an overview of philosophical thought on the subject.

2.3.2. Error Magnification and Extreme Testing. The breakdowns described above result from a basic strategy of mathematical reasoning. Delicate and elaborate arguments are undertaken to magnify and exploit subtle features of a situation. But they magnify errors as well as correct features. Deeper investigations give higher magnification, with the consequence that tinier errors or misunderstandings will lead to collapses. Even “really excellent” reliability is eventually unsatisfactory: only *complete* reliability provides a stable basis for unlimited exploration.

The link between elaborate arguments and error magnification applies to any science, not just mathematics. In other sciences complete reliability seems to be impossible, so the conclusion is “avoid elaborate arguments”. Wisdom and experience play important roles in knowing which arguments could be useful and which are too elaborate to trust. The question is: why should mathematics be different? Isn’t a magnification an unavoidably risky strategy, and isn’t deeper wisdom and experience the best way to minimize risk? The answer is that there is no *reason* mathematics should be different, and until the end of the nineteenth century the wisdom-and-experience approach was the standard. However it had reached its limits in several areas and it was our good fortune that humanly-accessible infinite-precision methodologies were discovered at about the same time and provided an alternative.

Some of the developers of contemporary methodology had hoped that there would be a *proof* that it is completely reliable, but this did not happen and is no longer expected to happen⁸. From the historical perspective of the previous section the contemporary formulation is just the latest in a long series, but it differs from its predecessors in the nature and degree of testing:

- The whole system, not just individual conclusions, is tested for reliability;
- Ruthless efforts have been made to find and attack potential weak points;
- Things not accessible to extreme testing are by default excluded, see §2.3.4.

In other words we vigorously crash-test everything, and discard or modify anything that can be made to crash.

⁸By me, anyway.

Basic materials stabilized around 1930 and were generally accepted by 1950. Examples of modern practice can be found much earlier, but examples of pre-modern practice are also common until about that time.

This raises another question: is it conceivable that a genuinely new method could be used and tested so extensively to be accepted? Or is the subject closed? We will see that methods are still evolving, but basic methodology is so effective and well established that compatibility with the base is now the principal criterion. This point is explored in §2.4.

2.3.3. *Methods are not Proved.* We have no external guarantee (i.e. proof) that current methods have the error-displaying property, so this is actually an experimental conclusion. However it has been so extensively and harshly tested that most of the concerns usually associated with experiment do not apply:

- Reliability of core methods is humanity’s best-tested experimental conclusion by far.
- Insanely complicated proofs by contradiction routinely hold up, and there have been absolutely no failures that could not be traced back to methodological errors.
- Since consistency is not proved it could in principle be wrong. However, that this possibility concerns the *whole system* of mathematics, not individual conclusions. Individual conclusions are uncertain because our methods for checking for errors are uncertain. The possibility that the whole system might collapse is negligible by comparison, and worrying about it is a waste of time.

2.3.4. *Exposure to Failure.* The list of testing strategies in §2.3.2 includes:

- Things not accessible to extreme testing are by default excluded.

To be accepted in the core methodology it is not enough that something has not yet led to an error; it must have been tested in every way that *might* lead to an error. This is illustrated with a comparison of standard basic material with the Riemann Hypothesis.

Turning pages in a book provides at least a weak test of the properties of the natural numbers because the pages are numbered. Even the simplest argument uses, and therefore provides a test of, basic logic. Further, there are no exceptions or special cases that might have escaped close scrutiny. Every bit of it has been heavily tested.

Some set theory also seems to be needed, though much of mathematics seems to be insensitive to the details, see §10.1. The standard choice, Zermelo-Frankel set theory, is rather minimal so again every part of it is exposed to testing.

Mathematicians take considerable comfort that no further “facts” are needed:

- The additive and multiplicative structure of the integers, the erratic behavior of primes especially, are not new facts but consequences of basic logic and the existence of the natural numbers.
- The subtle analytic structure of the real numbers and the deep magic of the complexes are not new facts but consequences of natural numbers.
- Number theory, modern abstract algebra, topology, geometry, analysis, differential equations, and even the mathematical foundations of string theory all come from the natural numbers.

These rich and complex developments provide sensitive tests of the underlying basic structure but they do not introduce new potential sources of error.

In sum, we have extraordinarily good evidence that core methods are error-displaying, and the mathematical community stopped worrying about this over a half-century ago.

2.3.5. *The Riemann Hypothesis.* We contrast the status of current foundations with the Riemann Hypothesis. Everyone expects that it is correct; there are impressive heuristic arguments that it should be correct; a century of tentative use has failed to lead to an error; and it has been shown to be correct “with probability one”. It has been checked for the first several billion zeros and spot-checked incredibly further out. This is more than enough to establish it as a scientific fact but it would be unthinkable to modern mathematicians to accept it as a fact on these grounds. Why not?

The first problem is the general point that long and delicate mathematical arguments magnify errors. An argument that deduces something remarkable from the Riemann Hypothesis therefore also greatly increases exposure to any flaw it might have. The reason the bar for accepting the Hypothesis is so high is therefore the same reason we want to know it: it has remarkable consequences.

The second concern is a more subtle version of the first. Our experience is that rare and unlikely things have special properties. Roughly speaking the constraints that make them rare also give them structure. If one writes the structure conditions for a Lie group or a category as systems of equations then they are hugely overdetermined and for many purposes it would be quite safe to assume solutions don’t exist. However the search for special structures inexorably led us to them. Similarly, if there is a failure of the Riemann Hypothesis then it’s a pretty good bet that there is an associated structure with remarkable properties, and sooner or later we will be led to it.

In sum, the Hypothesis may be correct with probability one, but if there is even a single failure then our search for significant structures will lead us to it with probability one.

2.3.6. *Doubts into Tools.* A trend in the last seventy years or so is that some concerns about basic methodology have been resolved in interesting ways: the methodology emerges unchanged, but when carefully formulated the concern provides a new tool. We give several examples.

The “constructivist school” developed in response to concerns about validity of non-constructive existence proofs, and proofs by contradiction more generally⁹. It is often much harder to construct something than to show it exists. When a construction can be done one usually learns something useful from it, but after a time it became clear that this is unsatisfactory as the only approach. Contradiction works fine and rejecting it is a self-inflicted disadvantage. A severe disadvantage in fact, and strict practitioners got selected out.

On the other hand when “constructable” is carefully defined, e.g., in terms of terminating algorithms, it turns out that some things are and others aren’t. A turning point was the Boone-Novikov proof that there is no (terminating) algorithm to determine from a presentation of a group whether or not the group is trivial.

⁹There were also philosophical concerns about what it means to know something. Today these seem quaint and irrelevant but they were troubling at the time.

Algorithmic decidability is now a tool and has been used in profound and remarkable ways, c.f. [82].

Nonstandard analysis and model theory provide another example. The infinitesimal approach to calculus was rejected as insufficiently reliable for mathematics centuries ago but works so well as a heuristic setting that educational use—particularly in engineering—persists to this day. Eventually this was explained¹⁰ in a remarkable way: it works in education because nobody was trying to do anything fancy with it, and “fancy” could be precisely formulated in logical terms. Roughly, there are restrictions on how quantifiers can be used.

The outcome is that assertions with suitably simple formulations are independent of the existence of infinitesimals. If they can be proved with infinitesimals then they must be correct even in models without them. For a while it seemed that this might justify re-introduction of infinitesimals in education, but it turned out that the necessary logical subtleties are worse than the old-fashioned $\epsilon - \delta$ fix. It does provide a useful tool for research, however.

The final example involves transfinite methods, the Continuum Hypothesis and the Axiom of Choice in particular. The Axiom of Choice was first identified as a hidden assumption in vigorous set-theoretic constructions and had to be made explicit to keep the methods error-displaying. It has a great many useful equivalent forms and—though a certain unease persisted—over time it became widely used. The unease was resolved when it was shown to be independent of the basic axioms of set theory¹¹. In other words this axiom can introduce an error only if there is already an error in other more basic axioms. With this reassurance it is now accepted as a basic tool.

The Continuum Hypothesis is also known to be independent of the other axioms so is also available for use, or denial if that seems more profitable.

2.3.7. Summary. The current core methods of mathematics are basic logic and Zermelo-Frankel set theory with the Axiom of Choice. There are minor variations and packagings that seem not to effect the mathematics built on them, see §10.1.

This core is effective and extremely well-tested, and unlikely to change in any substantial way. Qualitatively new methods are likely to be of two types:

- Subtle elaborations of what is already there, e.g. model theory or computability and complexity theory; or
- Methods that can be shown to be compatible with (or independent of) the core, e.g. the Continuum Hypothesis.

2.4. Level 4: Competition and Survival. In previous levels I described mathematics as being characterized by essentially complete reliability, explained how reliability is achieved through use of error-displaying methods, and gave a simplified, after-the-fact explanation of the evolution and testing of error-displaying methods. Here I sketch some of the nitty-gritty of the real evolutionary story.

In principle, methodology evolves by natural selection: Survival-of-the-fittest struggles occur when several modes are in use, dysfunctional methods confer disadvantages on their users and eventually die out, while effective methods confer

¹⁰By Abraham Robinson, see *Nonstandard Analysis* in [7].

¹¹We refer here to Zermelo-Frankel (ZF) set theory, see [31]. There are other approaches to set theory, most notably Quine’s [20], but ZF is clearer and much better tested.

advantages and are eventually accepted by the community. Real life is much more complicated and the outcomes less clear.

The first complication is that selection responds only to immediate differences between methods in use at the time, and only in contexts where the differences are important. The great power of contemporary mathematics depends on several components (logic, proofs, definitions) and the hindsight explanation traces the development from the perspective of the final product. In fact these components had to establish themselves more-or-less independently in various areas of mathematics.

For example by the mid nineteenth century precise formal definitions gave enough advantage to “win” in modern algebra, while more casual and intuitive definitions were still satisfactory in much of analysis. The picture in mathematics as a whole was of diversity and coexistence rather than of selection. It was only when the whole package came together in the early twentieth century that it had enough power to displace the alternatives throughout core mathematics.

A more serious complication is that there are many modes of reasoning *outside* of mathematics. Contemporary mathematical reasoning replaced the alternatives in its own niche but has had little impact outside. In this section I describe some of these other modes of reasoning and suggest some conclusions:

- There is still serious competition between mathematical reasoning and outside modes, and survival is still an issue. Consequently the mathematical niche is heavily defended against attack and customs have evolved to avoid conflicts.
- Many of the now-obsolete approaches in mathematics were inherited from outside modes of reasoning (language, philosophy, real-world intuition, etc.). The main obstacles to outgrowing them were the connection to the outside mode, not questions of technical effectiveness.
- The mathematical niche does not include elementary mathematics education. Education was already well-established as a separate domain when the main evolution of professional mathematics took place, and the survival-of-the-fittest struggle was lost in this domain.

2.4.1. *Meaning from Language.* The ancient Greeks believed that language is so closely connected to reality that one could learn about things by studying the words for them. Now we know that languages are artificial human constructs that provide crude approximations to reality at best, and natural-language reasoning is often influenced more by style than substance or logic. Language is a particularly poor way to approach science and mathematics. However it is so transparent and deeply embedded that the professional community had difficulty even seeing the problem, let alone outgrowing it. Language remains the default mode of reasoning for most people, and is still a strong competitor with subject-adapted methods of science.

I focus on problems with linguistic approaches to definitions. The logic embedded in a language also causes problems but these depend on the language and probably reflect other factors discussed later.

Euclid’s definitions, for instance, are linguistic rather than mathematical in structure.

... a point is neither a solid, nor a surface, nor a line; hence it has no dimensions—that is, it has neither length, breadth, nor thickness.

This suggests something rather than completely specifying it, and does so by reference to real-world experience. Moreover even as a reference to experience it depends essentially on language for meaning. The English version above is rather obscure, presumably because contexts implicit in the original Greek are lost in translation.

Philosophers through the ages have provided many variations on this theme. The reasoning behind Platonic ideals is essentially “if something has a name then it must exist”. Trying to disengage this from language gives “if I think of something then it exists”, which is clearly a license for creating nonsense. Descarte’s famous “I think, therefore I exist” is similar. Later philosophers were more sophisticated but have still not escaped confusions rooted in language, see §2.4.5. A significance for the evolutionary story is that for two millennia Euclid was the model for mathematics. As long as “better” meant “more like Euclid”, linguistic models and confusions (among many others, see §2.2.10, 8.7.2) were locked into the methodology.

Development of the experimental approach to physics brought with it the need for a more effective mathematics. This did not immediately change the basic approach: infinitesimals, for example, were defined following the linguistic model. They are far enough from shared experience that many (e.g. Bishop Berkeley) found this unsatisfactory and in practice they were defined by reference to examples of their use. The main significance of extensive potential applications was a new criterion for “better” methodology, and therefore new opportunities for selection to improve it.

By the mid nineteenth century significantly more-precise definitions were in use. They had not achieved contemporary standards of precision but they were in conflict with Euclidean methods and had not displaced them. The fuss over whether non-Euclidean geometries represented a *failure* of the parallel postulate may have been the outward expression of conflict between the approaches. Labeling it as a failure would discredit the methodology and provide a solid excuse to discard Euclid as a model. Seeing it simply as revealing a wider world would have reduced Euclid’s dominating position as a model but not discredit the methodology. The evolutionary interpretation is that the “failure” description won out because people using the newer methodology, and needing relief from the old, were more successful¹². This enabled change *inside* the profession but had little impact outside.

Outsiders often still try to understand mathematics from linguistic points of view.

- The implicitly Platonic view is that mathematical definitions are incantations that magically call things into existence. Mathematical objects seem to exist *because* of the language.
- The relativist view of language is that it is largely a social construct and words often do not correspond to reality. In mathematics, relativists see language without a corresponding reality because mathematical objects cannot be directly perceived without long and disciplined study. Some conclude that the language itself has value, for instance as the “language of science”, but it is common to feel that terms with no physical-world reference are only social constructs by insiders.
- Mathematics educators generally see contemporary definitions as unnatural and remain strongly committed to the linguistic model.

¹²Question for historians: does the detailed record of the debate support this interpretation?

This last is particularly unfortunate because it raises the barriers students must overcome to study advanced mathematics.

In summary, reasoning and definitions modeled on language were difficult to outgrow in the profession; are still competitors with mathematical reasoning; are still the norm outside the profession; and are still problematic for students and outside observers. See §5.1.4 for a discussion of linguistic problems with education, §10.3 for problems in popular writing, and §2.4.5 for problems with philosophy.

2.4.2. *Consistency Through Authority.* Modes of reasoning that are concerned with consistency generally rely on appeals to authority to achieve it. Scientists use the real world as the authority and perform experiments to resolve inconsistencies in theories¹³. Religions and religion-like belief systems rely on sacred texts and priests to resolve conflicts in reasoning. Legal reasoning relies on law, courts and trials.

Only in mathematics is consistency expected from slavishly following the rules. More precisely, only in mathematics were conflicts that required appeals to authority seen as indicating defects in the rules of reasoning. Eventually rules were developed whose consequences do not require adjudication. In fact rule-based mathematical conclusions now *cannot* be set aside by authority. This provides great power and clarity but many points of conflict with modes of reasoning that need authoritative control over conclusions.

The use of unqualified conditionals in religion is an example that requires authority: if one prays, meditates, etc. *enough* then one will go to heaven, prosper, achieve enlightenment, etc. By mathematical standards such statements are vacuous. In effect they define “enough” rather than make assertions about prayer, etc. Compare with: if one understands mathematics well *enough* then one can teleport to the moon. Hasn’t happened yet because—evidently—no one understands mathematics well enough. Declaring that the prayer version is valid and the teleportation version is spurious requires an appeal to authority and rejection of the mathematical analysis.

Quantifier confusions play an important in religion, pseudoscience and many alternative approaches to medicine. A small number of miracles, unexplained occurrences, unexpected recoveries, etc. can be interpreted as having significance because there is not a careful distinction between *there exists one* and *for every*, and statistical variations of them. On the other hand there are plenty of coincidences that could justify unwelcome conclusions this way. Since the rules do not determine which are “correct” and which are superstition, this must be done by authority and without interference from mathematical criteria for justified belief.

The fact that these methods are unsound by mathematical standards makes mathematics a potential enemy of religion, alternative medicine, etc. In fact the well-known conflict between *science* and religion is in some ways less problematic: this can be reduced by agreeing that science and religion are concerned with different realities, and this justifies different modes of reasoning. The conflict with mathematics concerns the reasoning directly and independently of the subject, and cannot be avoided this way.

The significance for the evolutionary story is that conflicts with religion and other belief systems reduce the selective benefits of mathematical reasoning. Even

¹³In principle. In practice wisdom and intuition developed through long technical experience often play as large a role. This complicates the picture but does not change the basic principle.

subliminal discomfort could inhibit use. Development proceeded—slowly—as benefits to science balanced disadvantages to religion, and as ways to reduce conflict were developed. In particular mathematical reasoning is, for the most part, used only in the safety of its own narrow domain. It is considered rude, if not dangerous, to mathematically analyze faith-based reasoning, and mathematical logic has certainly not replaced it.

2.4.3. *Conflicts with religion: a conspiracy?* As far as I know, no-one has looked for historical examples of math-religion conflict. The well-known conflicts between science and religion are usually presented as conflicts between conclusions rather than the modes of reasoning, and nobody cares about the conclusions of mathematics. Even if these are seen as conflicts of reasoning it would be a stretch to see them as conflicts between religion and the *mathematical* aspects of scientific reasoning. The analysis here clarifies what to look for, however, and I illustrate this with a question for historians.

The early twentieth-century Progressive education movement in the US substantially reduced mathematics in the elementary curriculum, crippled mathematical preparation for science and technology for a century, and remains the dominant approach. This was done over strong objections from the mathematical community and was, in effect, a survival-of-the-fittest contest that mathematical reasoning lost. On the face of it the damage was an inadvertent consequence of naïve psychological and social theory, and mathematics was simply not strong enough to resist. Was it also partly motivated by a desire to protect religion from the spread of mathematical reasoning?

From a religious point of view, reining in mathematics might have seemed a good idea in the US in the early twentieth century:

- Science and rationality seemed to offer a powerful alternative to religion. Nietzsche’s “God is dead” proclamation had resonated in the intellectual community, and the possibility that these attitudes might find their way into elementary education must have been alarming.
- Science in the schools seemed to be well under control: teaching evolution, for example, was illegal in many states and the Scopes trial was decades in the future. However, mathematical logic was being sharpened and freed of external influences during this time, making it potentially more dangerous, and mathematics was a major subject in the curriculum.
- The US was largely protestant Christian, with sects that—in principle—empowered people to reason about their beliefs.
- There was no systematic religious instruction in the schools that might have channeled children away from overly analytic reasoning about religion.

The chief architect of the mathematical component of the Progressive education movement was William Heard Kilpatrick: see Klein’s brief history [34], Beineke’s biography [6], and the Wikipedia entry. Kilpatrick had a rigorous Baptist upbringing (his father was a minister) and studied mathematics at Johns Hopkins among other places, so he had the right background to be aware of and uncomfortable about potential conflicts.

Kilpatrick rejected the traditional role of mathematics as mental discipline and a model for logical thought, and recommended focusing on the utilitarian value (applications). Moreover he advocated removing from the general curriculum topics

such as geometry and algebra that have few applications to everyday life. I do not see a good connection between these policies and his avowed reasons for them, and they make sense as a preemptive strike against the spread of mathematical-quality logic. On the other hand in Lagemann [40] Kilpatrick comes across as ambitious, charismatic, and intellectually undisciplined, and it is hard to see him as a devious conspirator. Napoleon’s admonition “do not attribute to malice that which can be explained by incompetence” seems to apply here.

Edward L. Thorndike’s psychological theories about “bonds” between topics also influenced the Progressive approach to mathematics. Mathematics is famously interconnected and the establishment of bonds realizing the interconnections seems a great idea. Perversely, the teaching of elementary mathematics was *fragmented* to *prevent* the formation of “incorrect” bonds. Was one objective to prevent students from seeing logic as a general-purpose tool, and perhaps applying it to religion? My impression is that Thorndike had little background or interest in mathematics. Were the damaging details someone else’s idea?

The question for historians: is there any evidence that the program developed by Kilpatrick and others was motivated in part by a desire to keep mathematical instruction faith-friendly? In other words, was this a targeted attack on mathematics, or (boring but likely) just collateral damage?

2.4.4. *Belief and Intent.* Some modes of reasoning connect validity to mental states.

The extreme version is that truth is whatever people will believe. Slogans, spin, and inflammatory rhetoric are more important than facts or logical consistency. This seems to be a major mode in current US politics: after a debate in which one candidate says “*A*” and the other says “*not A*”, the commentators ask “who won the debate” rather than “which is correct”. Presumably using this mode gives politicians a selective advantage, or perhaps eschewing it confers a disadvantage.

Belief driven by slogans lacks ways to compromise or reason out differences: slogans win or lose, not blend. This leads to polarized situations in which a subject like mathematics is a powerless spectator. Some recent examples in the US:

- The “No Child Left Behind” education policy in the US redirected resources to the weakest students. Partisans are unwilling to consider that the national welfare might depend on improving the preparation of high-achieving students as well.
- “Hold Education Accountable” shifted the focus from providing resources, to punishing educators who under-performed in some way. High-stakes testing is the primary mechanism for accountability, and now defines the curriculum in some areas.
- “If the Student Hasn’t Learned then the Teacher Hasn’t Taught” shifted the view of education from an opportunity for students, to a product delivered by teachers. The targets for accountability are therefore teachers and schools, not students. Partisans are unwilling to consider that students should share responsibility for their own learning, or that punishing teachers degrades the work environment and chases many of them out of the profession.
- Science policy influenced by “Interdisciplinary Research is the Future” puts the mathematical enterprise at a disadvantage. Partisans have little interest in suggestions that core mathematics should be supported, even if only because it is a powerful *enabler* of interdisciplinary work.

There have been technical mathematical slogans, for instance “Surgery theory is the fixed points of an involution on K -theory”, that were compelling enough to lead people to spend more time on them than really justified. This is not a threat to practice, however, since they do not replace formal justification as the final criterion.

A less extreme version of mental-state validity holds that if someone believes something, then it must be true in some way for *that person*. It is thus rude if not unethical to apply someone else’s logic to such beliefs. This is certainly appropriate for personal taste and private religion, but the prohibition against logic can make it difficult or impossible to settle disagreements. The personal-truth view also suggests that there could be different versions of mathematics or science for different people, just as there are different religions and cultures. Is standard mathematics, for example, unfriendly to females and minorities because it somehow has European white male values embedded in it? There may be problems with the way things are taught, but the basic idea is silly. Some truths are not personal, and mathematics is equally unfriendly to everyone.

A yet milder form of mental-state significance holds that what a person *intends* is important, perhaps as much as what he actually says or writes. If someone says X , which is wrong, but really means Y , is the argument valid or not? The high precision requirements of mathematics makes this particularly problematic. It is not harmful to correct errors due to sloppy writing when intent is unambiguous. It can be problematic when intent is unclear, or clues are ignored to get an interpretation that forgives errors for instance, because this promotes unreliability of the literature. Overly-friendly interpretations are also common in mathematicians’ reading of history: early glimmerings, or even just the right words, are often interpreted as essentially full insights.

2.4.5. *Philosophy and Meaning.* Philosophical modes of reasoning have many of the same defects as general language (§2.4.1), being artificial constructs not adapted to the subject. Philosophical interpretations of meaning, reality and truth, for example, are more likely to confuse than illuminate. Does deciding that certain questions “have no meaning” provide any protection from contradictions that arise when one allows them? Are mathematical constructs that seem to have nothing to do with the physical world “unreal”, and what difference would it make? Can one usefully separate something *being* true from *knowing* that it is true?

Here is a specific example:

- It is common and linguistically natural to ask “What is a natural number”, with the intention of defining “the set of natural numbers” as the set of all such things. But defining numbers one-at-a-time is mathematically backwards: it is the whole set that is important and individual numbers only have significance as elements of the set. The goal suggested by language and philosophy has been an enormous waste of time.
- Unfortunately the phrase “the set of natural numbers” also contains a linguistic trap. Mathematically it is the *properties* of the natural numbers that are important, rather than the set. Moreover there are many models

with these properties but no useful single object that qualifies as *the* natural numbers. The single-set goal suggested by language has also been an enormous waste of time¹⁴.

- The mathematically fruitful question, formulated with several millennia of hindsight, is the linguistically awkward and philosophically unattractive “what are the properties that qualify an object to be referred to as a model of the natural numbers?”

Mathematicians now have subject-appropriate solutions to these problems, and philosophy no longer has much influence on technical methodology. Problems still lurk just outside professional practice however:

- Outsiders use philosophical formulations by default, and rightly see core practices as out of step with them.
- Mathematicians usually accept philosophical formulations as appropriate for metamathematical discussion or popular accounts. Core mathematicians who do this have trouble making good sense.
- Many applied mathematicians and mathematical scientists (§3) find their practices generally compatible with philosophical formulations, and sometimes use this to attack core practices as obsessive aberrations.
- Philosophical views dominate mathematics education and this leads to programs that connect poorly with the subject.

See §9 for further discussion.

2.4.6. *Real-World Roots.* Mathematics is *physically effective* in that many physical phenomena are constrained by mathematical consequences of basic properties. I describe how this shaped and enabled early development but later became problematic.

Beginning in the late sixteenth and early seventeenth centuries physical science shifted from qualitative and philosophical to quantitative and mathematical. A sophisticated description is: there *exists* a logic so that many physical phenomena are constrained by consequences derived *with this logic*, from other relationships. This emphasizes that it was not just natural laws that had to be discovered, and not just the higher mathematics that effectively amplifies them, but the logic that underlies this mathematics as well.

In fact, physically effective mathematics evolved along with science. Science provided clues and discipline, but most importantly it provided measures of success. Methods that broke from tradition (e.g. Euclid) and philosophical preconceptions could be justified by applications. This justification reached deeper as applications became more sophisticated. A treatment of convergence that many people found unintuitive, for example, could be justified by better handling of problems with series, continuity and derivatives in electrical engineering and optics. The protection and patronage of science and applications enabled mathematics to change, and it thrived in this environment for three centuries.

¹⁴There is a unique equivalence between any two models, so the directed system of all possible models could perhaps be considered a universal object. However it is not a set in ZFC set theory, and is very awkward to use. Most of the wasted time has been spent trying to devise a set theory in which this is some sort of set, or has some sort of limit.

2.4.7. *Real-World Conflicts.* Core mathematics outgrew the dependence on science in the transformation that began in the late nineteenth century, see §9. It turned out that physically effective mathematics is consistent in the sense that it can be a completely-reliable mode of reasoning. I certainly do not want to speculate on whether the fact that mathematics effectively models nature means it must be consistent, or if nature follows our mathematics because it needs a consistent logic and this is the only one. The reliability of core methods is well-established, independent of any link to the physical world. The point is that once complete reliability was within reach it became a new measure of success that could influence the evolution of methodology. And it was so much more powerful and effective that in the core it became the primary criterion within a few generations.

The down side of this story is that when reliability replaced more physical motivations, science and applied areas changed from patrons to antagonists. Moreover the intellectual and temporal proximity, and the great vitality of science and applied mathematics, make these ideas the most difficult to resist, see §3 and the next section.

The best-known problem is that physical intuitions and standards *in proofs* are now obsolete in core mathematics and have joined language, religion, philosophy, etc., as things to be defended against. A more subtle difficulty is that physical intuitions are sometimes off-base, and if taken as authoritative can inhibit mathematical development in the same way philosophical confusions did. An example comes from the adventure described in §2.2.2:

The Witten-Reshetikhin-Turaev vision of topological field theories launched an extensive cottage industry; see Corfield [11] and §10.4 for partial descriptions. Curiously, the received wisdom from physics seems to be misleading. Specifically, the physical “insight” is that the basic theories should be defined on 3-manifolds. Accepting this has led to a theory that reflects the structure of 3-manifolds more than the structure of field theories. However, the connection to 3-manifolds (for categorical theories) is through the ‘Kirby calculus’ description of 3-manifolds as boundaries of 4-dimensional handlebodies. The field theories actually define invariants of the 4-dimensional handlebodies. In some cases these can be normalized to depend only on the boundary, and the nondegeneracy condition needed for this is embedded in standard hypotheses. The “degenerate” cases cannot be normalized and so depend essentially on the 4-dimensional manifold. This is the locale of some of the greatest mysteries in manifold theory and may be the best opportunity for profound applications, but the received wisdom from physics puts it out-of-bounds. The cases that can be normalized do have residual 4-dimensional ‘integrality’ information and this has been investigated. So far it has been too complex and inconclusive to drive evolution of a more subject-adapted viewpoint.

2.4.8. *Norms, Altruism, and the Elite.* The picture developed above is that there are many ways of thinking that are unreceptive, if not actively hostile, to mathematical precision. The community has developed norms to protect against these influences, and codes of conduct to avoid conflict. However the main function of these norms is to protect against problems from within the community. I return to discussion of norms after describing the internal problems.

Developing a complete error-displaying proof is a long and difficult process, and mathematicians focused on outcomes are often tempted to forego details. Why not be content with ingenious and compelling outlines, physically motivated heuristics,

extensive computer testing, deep expert intuition, etc. when they seem to be almost as good and much faster? This is primarily an issue with elite mathematicians: they are the ones with effective intuition, and they have strong bodies of work less likely to be damaged by an occasional error.

The answer that emerges in this work is that full proofs may not make much difference in outcomes of individual cases but they make a great difference for future work and the community. Proofs give solid reliability that others can build on, with subtle clues in unexpected details. The discipline imposed by getting it exactly right leads to sharper techniques and more insightful definitions that are more immediately useful to others. The most immediate beneficiaries are rank-and-file working mathematicians. My impression from studying the literature and editorial work (§2.4.9) is that effective tools and a solid base on which to build enables them to make useful contributions much more routinely than is possible for the rank-and-file in other sciences, see §4.3, 11.1. Original research is now accessible to average mathematics faculty while before the transformation their participation was largely limited to writing textbooks. All this also makes steady, if often slow, progress possible in a much larger range of topics relative to the size of the active community, see §11.3.2.

In other words, developing good proofs is to some degree an altruistic activity that substantially benefits the community, occasionally at some expense to individuals. In fact most of the best mathematicians don't mind working out details, and enjoy the clarity and beauty that emerges. However a few object to being held to high standards [3, 80]. Some have even used the power of their prestige and ideas to force the community to accept behavior that would result in virtual expulsion of a middle-class mathematician. I refer to this as the "elite-practitioner syndrome".

The mathematical community has evolved norms¹⁵ and codes of conduct that reinforce this altruism. Most thesis advisors, for instance, feel that instilling proper standards of rigor in their students is a vital part of their job. There are lapses: individuals misunderstand the rules; there is confusion because the concerns do not apply to much of applied mathematics (see §3); the Soviet community was problematic (see §9.4.2); but for the most part these norms have kept practice in core mathematics relatively uniform for nearly a century. Further, vulnerability of ambitious practice to flawed methods has encouraged strong and quick enforcement of these norms. By the end of the twentieth century norms were so firmly established that proposals like that of Thurston [80], §10.6 to relax them were rejected as not even interesting. The alarm expressed by Jaffe and myself in [30] about threats to mathematical norms turned out to be largely unnecessary.

I provide a bit more detail on the evolution and operation of norms.

It is a reasonable concern that strong conservative norms may block introduction of valuable new methods. Examples impeded by norms include infinite objects, nonconstructive methods, and proof by contradiction. The first response to this concern is that these examples predated contemporary reliability-based norms, and resistance was largely due to external influences. The better point, however, is that resistance from norms forced development and testing of very careful versions (with

¹⁵The norms considered here concern methodology, or in other words what sort of argumentation is, and is not, acceptable for use in a proof. The level of precision expected in the use of these methods is a separate issue and is discussed in the section on Proofs, §4.2.

careful disclaimers) that did not trigger strong reactions, see §2.4. Norms are doing their job correctly when they block sloppy versions of potentially good ideas.

Objections to high standards now usually take the form “requiring rigor is a threat to exploration and development”. As stated this is silly: rigor is expected in the final product. The quest for rigor guides and provides discipline for exploration, but no-one expects exploratory work itself to be rigorous. See §4.1.1 for the use of semi-rigorous argument. This objection also confuses the general issue of human creativity with questions about how to make it effective in mathematics, see §10.6 for further discussion.

Returning to the evolutionary story, for most of history the mathematical community was dominated by elite practitioners. Conflicts between their inclinations and the needs of the subject made development of contemporary norms difficult. Felix Klein and Henri Poincaré, for instance, saw contemporary rigor as incompatible with their personal views of mathematics and were strong opponents. However it did eventually get started, and the development of a large professional middle class of active mathematicians (i.e. direct beneficiaries) stabilized the situation by about 1940, see [24] and §9.

This account sounds like a Marxist class struggle in which the proletariat overthrew the oppressors, but this is not the case. First, the elite opponents of the new order were well-intentioned. Second, the rank-and-file is more likely to worship the elite than try to overthrow them, and this makes the elite-practitioner syndrome hard to see let alone overcome. Exactly how it happened is a question for historians but my understanding is:

- There were powerful leaders—David Hilbert especially—who supported and helped develop the new methods.
- The rank-and-file took to the new methods immediately because they could be active participants rather than spectators. When original research by average faculty went from a possibility to an *expectation*, the enabling methodology was firmly locked in place.
- Most elite mathematicians are made, not born. They develop their powers through extensive detailed work, and most of them absorb the norms that underly the material. As a result, once the process got started full adoption was a matter of waiting for the old guard to fade away.
- Exceptions tend to be the extremely few who seem to have been born mathematical (Grothendieck), those who come from outside (Witten), and those whose vision outpaces their technical ability (unnamed here).

In any case the transition went about as well as could be expected in the professional core community, and the enormous advances in the last century are a testimony to its success. Problems remain, and the approach has had little influence outside the core community.

2.4.9. *Insight From Editorial Experience.* Some of conclusions above and in [30] were shaped by my experiences as editor of the Research Announcements section of the *Bulletin of the American Mathematical Society* in the early 1990s.

I came to the job believing that publication of claims without proof speeded dissemination of knowledge in the community. After several years and around a thousand submissions, I realized there were widely divergent understandings of the editorial process. The general presumption was that complete formal proofs were

in hand and would be published shortly. As often as not, rank-and-file mathematicians already had complete manuscripts and wanted to publicize their results while refereeing and long journal backlogs ran their course. Others were much more casual, and in particular some elite mathematicians saw research announcements as an outlet for intuitive conclusions. At one point a University of California Berkeley professor, presumably in a race for priority, sent significantly different versions every few weeks for several months. There were also claims of important results that were almost certainly true but the author almost certainly could not prove. The goal—stated explicitly in one case—was to get credit but shift the burden of proof to the readers. I got so many claims of proofs of Fermat’s Last Theorem and the Four-Color Problem that I used form letters to reject them.

Over time I came to worry that presenting casual announcements as citable publications came close to betraying the trust and conscientious work of the large majority of the community. From the norms perspective described above, the announcement format itself seemed unethical. Working with Jaffe on [30] sharpened my concerns and made it impossible for me to continue. I resigned as editor and argued that announcements should be dropped from the *Bulletin*.

Predictably, the proposal to drop announcements outraged people—mostly elite—who had benefitted from the practice. Unexpectedly, it convinced enough people that announcements were in fact dropped from the *Bulletin*. Disgruntled users organized an electronic outlet through the AMS, but eventually that was discontinued as well.

2.4.10. *Fourth-level Summary.* There are many modes of reasoning in wide use and most of them conflict in some way with the mathematical enterprise. The evolutionary picture is that these conflicts obscure subject-specific benefits described at the third level and development was far from straightforward.

- Reliability essentially defines contemporary mathematics but has been the principal driver of evolution only since the early twentieth century.
- From the seventeenth through the nineteenth century, and still in some areas, science and applications to the physical world provided the primary measure of “more effective” that drove development.
- Before the seventeenth century outside influences kept the methodology essentially static for two millennia.

Conflicts with other modes of reasoning keeps mathematical logic largely limited to the niche where its benefits are compelling. Even in mathematics education it is not fully accepted because it conflicts with convention and received wisdom.

Finally, there are strong altruistic aspects to contemporary core practice. A consequence is that the behavior of elite mathematicians may be a poor guide to well-adapted practice. It is not taken as a guide in the core community, or more precisely, young people who try to emulate casual elite approaches are unsuccessful and do not survive to perpetuate the practice. It is much more influential in education, history and philosophy, where the dysfunctionality is less clear.

2.5. **Computing in Core Mathematics.** Most of the current confusion about computing in mathematics comes from a failure to distinguish between use in, and outside of, the core. Computer methods whose outcomes are (in principle) completely reliable qualify for use in core mathematics. Concerns about these are qualitatively similar to concerns about correctness of human work, because they

involve implementation rather than basic methodology, and this is the topic of the current section. Concerns about methods that are not fully certain are quite different, and these are discussed in §3 on Mathematical Science.

I further distinguish between use of computers to generate or check proofs, and calculations in a largely human proof.

2.5.1. *Computer Proofs.* [[unfinished]]

2.5.2. *Computer-assisted Proof.* [[[unfinished]]]

3. MATHEMATICAL SCIENCE

Core mathematics, as defined in the introduction, uses fully-reliable methods with sufficient precision and rigor to make the results suitable for use within mathematics. Work that, for example, relies on heuristic arguments, or is validated by intuition, experience, or external criteria, does not qualify. However I refer to such work as “mathematical science” rather than “non-core” to emphasize that it is not defective mathematics but fully valid in the different—and much larger—world of science, see 2.2.8.

3.1. **Discussion and Examples.** [[unfinished]]
[[unfinished]]

4. PROOF AND DISCOVERY

Proofs are the operational form of the description of mathematics as error-displaying: arguments that actually would display errors. The formulation reflects my conviction that proofs are first and foremost an enabling technology for users. The more usual question “what is a proof?” leads to a passive, static picture, see “spectator proofs” in §4.3.

User features are clarified by describing proofs in two stages. First, “potential proofs” use formats and methods that should display errors but they are not required to be correct. Second, a prospective proof becomes a proof when it is checked and found to be free of errors. The main complications concern who should be able to find errors, and who might be qualified to declare something error-free. This is followed by a discussion of the processes used to develop proofs, and the role of proof in mathematical discovery.

Use of proofs (broadly interpreted) as an everyday methodology in mathematics education is discussed in §8 and in more detail in [67](f). Slightly more elaborate examples are given in a proofs-for-teachers context in [67](e).

4.1. **Components of Proof.** The validation criterion used in mathematics is: *if* error-displaying methods are used, and *if* the argument is given in enough detail to display any errors, and *if* checking does not reveal errors, *then* the conclusion is reliable. “Proof” is the name given to such an argument.

Making this precise, eg. how much detail is “enough”, can be complicated and is discussed in §4.2 below. The point here is that the format and methodological requirements are as important as correctness, and in some senses more important. To clarify this I introduce another term:

4.1.1. *Potential Proof.* An argument is said to be a *potential proof* if it:

- uses only methods tested and accepted as having the error-displaying property, and
- provides a record or transcript sufficiently detailed to allow checking for errors.

In other words, a potential proof satisfies all the methodological and format requirements of a proof, but is not known to be error-free. Potential proofs are fundamental to the proof development process because they can be developed without being too anxious about whether they will hold up under checking (i.e. at this stage “potential” is more important than “proof”). Checking often does reveal errors, but then corrections are developed and incorporated to get a better potential proof. The process is then repeated, and with skill and luck may eventually lead to a real proof. Focusing too much on correctness inhibits this process.

This description makes it clear that potential proofs should be sufficiently detailed that the *person developing the proof* can genuinely check them. Checking by others is essential in the discovery process and important in everyday use, and imposes further requirements that are discussed below, but the developer has primary responsibility for checking, and feedback from checking is an essential part of the process.

4.1.2. *Actual Proof.* The criterion at the beginning of the section can now be reformulated as: a *proof* is a potential proof that has been checked and found to be error-free, and consequently can be expected to have reliable conclusions.

Most accounts of proof emphasize correctness over methodology: an argument can be considered correct even if the methodology is defective, and this correctness can somehow make up for methodological weakness. In our terms, it need not be a potential proof.

Overemphasis on correctness often leads to misuse of checkability criteria. Teachers, for instance, often accept “proofs” with big gaps if the outcome is right, there are no outright errors, and the argument more-or-less conforms to a proof known to the teacher. But the gaps may hide unreliable extrapolation from other problems or various leaps of faith. This is “teacher checkability” that does not reveal problems to the student.

At the other end of the spectrum, an argument whose checking takes twenty years of serious research and a major contribution by a Fields Medalist cannot be considered a potential proof, and therefore should not qualify as a proof even if in the end it has no outright errors, see §10.6.

Another problem involves unjustified intuitive leaps or “physical reasoning”. These may be appropriate in work that does not have to be fully mathematical, see §3, and are always appropriate in exploratory development, but they are not error-displaying. They cannot be relied on to *always* produce correct conclusions even if there are instances where they do. Again too much focus on correctness obscures this.

Finally there is a confusion involving truth and knowledge. It is common to think of arguments as either correct or wrong: checking may reveal which, but checking does not change whether or not the argument is correct. In particular an error-free argument is considered to be a proof whether or not the error-freeness has actually been verified. But this viewpoint is misleading. Something whose status is

unknown cannot be relied on no matter how it eventually turns out. Checking may not change truth, but it certainly changes our knowledge and this is what matters in practice. The picture here reflects this: a potential proof does not become a proof until it has been appropriately checked and found to be error-free.

4.2. Verifier-Specific Proofs. The objective here is to make the phrase “sufficiently detailed to allow checking for errors” more precise.

A *potential proof for a person X* is a potential proof (i.e. record of reasoning, using tested and accepted mathematical methods) that *X* can check for errors. Similarly a *proof for a person X* is a potential proof that *X* has checked and believes to be free of errors.

This idea can be used to formulate expectations. In education, for instance, the ideal goal is for students to learn to find and fix their own errors. But for this to work they must be able to diagnose their arguments, or in other words they should be expected to give “potential proofs for themselves”. This depends on the individual. If a student can reliably do several arithmetic operations without writing intermediate steps, then omitting these steps from the record still gives a potential proof for that student. Another student may make mistakes that he does not catch when he attempts similar omissions. Potential proofs for this student require the intermediate steps. Further, what qualifies as a potential proof for a given student will change over time.

Verifier-specific potential proofs can also be used to make sense of behaviors seen in the research literature. There is quite a bit of variation¹⁶ but for the most part mathematicians write papers with a particular audience in mind. In other words, the goal is potential proofs for this group. Most mathematical specialties are sparsely populated and papers are relatively detailed to make them accessible to nearby areas and later times. In densely populated or rapidly evolving areas the target verifiers are much more expert and papers correspondingly less detailed.

4.2.1. Verifier-Independent Proofs. This verifier-specific version is useful and logically unproblematic but rarely used. The general feeling is that once a potential proof has been carefully checked and found to be error-free then anyone else who checks it should also find it to error-free, and consequently there is no need to list specific verifiers.

Logically this just postpones the problem to describing what “carefully checked” should mean, and who is entitled to certify something as carefully checked. In practice there are many conventions that work pretty well, but not so well as to render the verifier-specific version unnecessary.

- *General expectation:* “appropriately qualified” people should be able to check it. This lack logical force and has been used to dodge criticism.
- *For publication:* an argument is a proof for (i.e. has been checked by) an anonymous referee.
- *Extreme high-visibility cases:* usually the community develops agreement on specific experts whose conclusions will be accepted as definitive.
- *Secondary literature:* proofs in advanced texts and monographs have usually been checked and refined by many people and errors are almost unknown. Proofs in lower-level texts are frequently unreliable.
- *In Education:* There are two issues:

¹⁶Particularly among elite mathematicians, see §??.

- (1) Spectator proofs (see below) that students are expected to read but not emulate. Standards are low, generally aiming for “understanding” rather than precision, and these frequently do not qualify as genuine proofs.
 - (2) Student proofs. Expectations are established by examples and are frequently unclear, partly because the goals of proof are poorly understood.
- *Machine proofs*: These have yet to stabilize. Computer calculations in human-designed proofs are still being treated on a case-by-case basis. Computer generated proofs are still a cottage industry that has not produced an example significant enough to force the issue, see §??.

Finally, experience shows that at the research level a *proof for the prover* (i.e. known only to, or checked only by the author) does not provide reliability and therefore should *not* be accepted as a proof. This is sometimes disputed by elite mathematicians whose intuitions are good enough to keep them out of trouble most of the time, and who don’t want to be slowed down by careful writing, see [3], §7, 2.4.8, and 11. The overwhelming majority in the community do not accept this practice.

4.3. Incidental Benefits and Spectator Proofs. It turns out that proofs often have benefits beyond providing reliable conclusions. This plays out somewhat differently at the research frontier and in education.

4.3.1. *Discovery.*

- The process of correcting and refining an argument to be a genuine proof almost always leads to deeper and sharper understanding.
- This process also leads to proofs of complexity well beyond direct human capability.
- Understanding and checking a proof frequently illuminates the significance and core meaning of an assertion or the underlying definitions.

The first two points relate to an important feature of the proof process. The immediate objective is to give an argument that uses mathematical reasoning and can be checked for errors, but without any great hope that it is actually error-free. In other words a potential proof. Next the errors are found. The process then proceeds by iteration: fixes for the errors are developed and incorporated to give a hopefully better potential proof. Then errors are found in this, Along the way ideas get sharpened, definitions often improved, and dealing with subtle errors steers thinking in new directions.

The final point is that deeper understanding is often a byproduct of the disciplined analysis needed to produce error-free arguments. It is the discipline, however, that ensures that the insights are correct and useful. There have been many casual arguments that were compellingly insightful but wrong.

4.3.2. *Spectator Proofs in Education.* Most educators think of proofs as opportunities for learning or “bearers of mathematical knowledge” (Hanna-Barbeau, [25], and [72]). This is much more passive than the view here of proofs as an enabling technology for users. In particular the focus is on what appear here as incidental benefits (understanding, insight etc.) rather than on the methodology and discipline that make them beneficial.

A name helps with exploring the difference. An argument designed to communicate understanding or evoke insight, but not intended to be emulated or deeply analyzed, is called a *spectator proof*. These suppress (or lie about) details to keep ideas clear, so cannot be checked for errors and are not potential proofs.

Spectator proofs are certainly valuable. Everyone—from elementary students to cutting-edge researchers—needs to learn, and fully rigorous proofs are usually a poor way to do this¹⁷. Complex research papers often begin with a spectator proof (a sketch) that helps readers orient themselves, and this is often sufficient for many readers.

It is important, however, to remember that a spectator proof is not the real thing. Ideally they should be condensed from real proofs to ensure that the “insights” are accurate. They should retain as much as possible of the logical precision and basic structure, and should provide a skeleton that could be expansion to a real proof.

Spectator proofs in education rarely have these virtues. Focus on “understanding” etc. to the exclusion of precision often leads to vague, unfocused, and heuristic discussions. In some approaches brevity and precision seem to be considered bad things, and the goal of “understanding” seems to be to avoid the need for discipline and precision. This leads to weak meanings for “understanding” and similar terms, see [67](c). The point, again, is that understanding and insight are *incidental* benefits of proofs, not part of the definition. They are important and precious benefits but they get their value from the connection to the more disciplined aspects of proof. Attempts to disconnect understanding and discipline weaken the benefits and undercuts further learning.

4.4. Non-Proofs. ‘Proof’ has a specific technical meaning that gives special significance to statements that have been proved. The terms

- physical proof
- visual or dynamic proof
- heuristic proof
- proof by example

indicate methods that do not do the same job. There are also terms that do not contain the word ‘proof’ but are sometimes proposed as having similar status. These include

- understanding
- physical insight
- common knowledge or experience
- intuitively clear

Less-rigorous explanation, reasoning and argumentation are essential but there is a clear priority: proof establishes goals for relaxed reasoning and relaxed reasoning is evaluated by reference to proof. In more detail,

- Explanations of things already proved, are good or bad depending on their success in promoting understanding of things revealed by proof. A proof without an explanation is a sad thing but this does not effect its status as a

¹⁷The “Moore Method”, see the Wikipedia entry http://en.wikipedia.org/wiki/Moore_method and the legacy project at <http://www.discovery.utexas.edu/rlm/method.html>, does introduce mathematics through rigorous proof. A single such course at the undergraduate or beginning graduate level can be valuable: I had one and certainly benefited from it. But this is unsatisfactory as a general approach.

proof. In contrast, an explanation that leads to understanding inconsistent with outcomes of proof is a bad explanation.

- A heuristic argument for something not yet proved can be impressive and compelling but turn out to be bad. The claim may be wrong, or it may be right for different reasons and the heuristic argument impedes rather than gets one closer to a precise understanding. The real value of heuristic reasoning cannot even be evaluated until a proof is available.

Mathematicians need and use heuristic reasoning and relaxed explanation. They celebrate insightful speculation and in fact the most influential contributions to mathematics have been conjectures and problems, not theorems. However speculation can provoke development only if it is presented as *needing* development, i.e. as a conjecture.

To put it another way, presenting heuristic reasoning as a finished product robs it of much of its value, and makes it hard to celebrate no matter how insightful it is. The claim of completeness must be withdrawn or widely rejected before development can proceed. Making such a claim or refusing to withdraw it is often seen as a parasitic attempt to cash in on the hard-earned reputation of real proof without doing the hard work. See [30] for details and examples, and §10.5 for discussion.

5. MATHEMATICAL OBJECTS

Technically, mathematical objects are ones that can be studied with error-displaying methods. The discussion begins on this level with generalities about structure of definitions, especially the use of axioms.

Deeper levels concern the way humans perceive these objects, and strategies used to develop these perceptions. It seems to me that the human aspects of objects are more basic and perhaps more complicated than the corresponding story for proofs. They are certainly more widely misunderstood. Discussion of human aspects continues in §6 on Mathematical Developments.

5.1. Level 1: Definitions. Mathematical objects are described in terms of axioms, and other objects that are in turn defined with axioms. This section provides an overview of this practice primarily as a context for later cognitive discussions.

5.1.1. Axiomatic Format. Precise and explicit descriptions of mathematical objects are required for use of error-displaying methods: any property that is not made explicit cannot be used without appearing to be an error. In contemporary practice, axiomatic definitions are used to describe objects.

The axiomatic format has an unfortunate public image. The American Heritage Dictionary listing is

Axiom: ... *Mathematics & Logic*. An undemonstrated proposition concerning an undefined set of elements, properties, functions, and relationships.

In other words, accepted without question or justification. Some otherwise very sophisticated people (including some philosophers) think of axioms this way, and believe axioms are a matter of taste and could be adjusted to get any desired outcome.

Another common belief is that axiomatic formulations rigidify a field and stifle further development, c.f. Devlin [15] and Lakatos [41], discussed in §10.3, §10.5 respectively.

Ironically, axiomatic definitions are a cognitive device that has emerged as the most effective way for people to deal with the precision required by the subject. It is true that axioms can be irrelevant, and premature fixation can hinder development, but carefully used axiomatic definitions are one of the most powerful and effective cognitive tools of modern mathematics.

5.1.2. *Development of Objects.* Standard basic definitions were developed and refined over long periods and with great effort. They were *not* simply a codification of an intuitive understanding.

In nearly every case, early attempts to codify intuitive ideas were unsuccessful in some way. Understanding the problems with an attempted definition usually leads to a sharper and more sophisticated intuitive idea. The new idea is codified and used to form new intuitions, problems with it are discovered, and the cycle repeats. Eventually a definition is found that is “right” in the sense that it is functional and captures the target idea. Quite often these right definitions have unanticipated further benefits. Substantial changes in “right” definitions are rare, though they are often reorganized as some features come to be seen as part of a wider context and are separated from features special to the object.

Development of definitions is frequently a community effort. The vision of initial discoverers may be clouded by the untidy complexity of the discovery process and leftover fragments of early versions. Clarity often comes through challenges by unencumbered minds trying to make it their own.

Finally, the primary criterion for correct definitions is functionality. Curiously this means they may not be recognized immediately. In fact, technically correct definitions of many major objects (e.g. limits, differentiability, groups, manifolds, categories, ...) appeared years before they were accepted because there was insufficient technique to demonstrate their functionality.

5.1.3. *Benefits and Usage.* Perspective gained during the discovery process is often packed into the final formulation for the benefit of later users. The ideal definition has a very concise formal statement so it can be essentially memorized and quickly reviewed. It is often accompanied by suggestions on how to think about it or what it is supposed to do. And frequently minor propositions are provided to develop basic features and let the user see how the definition functions. The objective is to develop an accurate internal representation (intuition) as quickly as possible so the definition becomes a tool and an anchor point rather than an obstacle; see the discussion below.

Expert mathematicians know this and when trying to understand a new topic usually start with the definitions, see Wilkerson-Jerde and Wilensky [83].

The deliberate identification and definition-exercise packaging of cognitive units is a relatively new development. It is probably the primary factor in the explosive expansion of complexity in the last century.

5.1.4. *Linguistic Problems.* The core problem is that words efficiently evoke pre-existing understanding, but are poorly adapted to development of precise new understanding. Mathematicians have developed some odd customs to help with this.

As noted above, when learning a new topic they often skip the explanations and begin with definitions. Mathematical lectures or texts may well begin with a technical definition and little or no explanation. To outsiders this seems bizarre if not deliberately obscure: why don't they explain what they are talking about? The reason is that mathematical vocabulary is still just words, and has the same drawbacks as any other language. Explanations give only spectator-grade understanding because unavoidable inaccuracies actually impede high-precision understanding. For user-grade understanding, descriptive terminology should be introduced *after* precise meaning emerges from development of the definition.

I have seen graduate students damaged by too much explanation too soon. Students who try to get the “big picture” without slogging through details usually learn the jargon but not the precise mathematics it refers to, and are unable to function as mathematicians. In education courses, mathematics is more often “explained” than experienced directly, so most educators have an essentially linguistic understanding of the subject. The same sometimes happens with people who write popular accounts of mathematics, see §10.3.

To sum up, the language of mathematics is not the same as mathematics. The transparency and power of language often hides this from people who try to learn mathematics through language.

5.1.5. *Non-Mathematical Objects.* Things that *cannot* be completely and explicitly described *cannot* be studied with error-displaying methods. They are in an essential sense non-mathematical.

At present all physical phenomena are excluded. Most professionals acknowledge this by distinguishing carefully between phenomena and models. Mathematical conclusions about models are reliable but say nothing about the fit between models and nature. Failing to make this distinction negates the special virtue of mathematical conclusions, see §8.6 for discussion in an educational context. It also seems to cause cognitive difficulty, see [67](i).

5.1.6. *Summary.* In brief,

- Random definitions, or mutations of standard definitions, are useless and have no consequences.
- Poorly formulated definitions are painful to work with even when they do describe natural objects.
- Well-developed definitions efficiently encapsulate properties of apparently natural objects in insightful and powerful ways; and
- they serve as solid anchors for both learning and research.

Good definitions are a precious part of our mathematical heritage and should be exploited and celebrated, not avoided or degraded!

5.2. **Level 2: Perception.** This section concerns the way humans perceive things, and some implications for teaching, learning, and understanding mathematics.

5.2.1. *Perceptual Mechanisms.* People live in the world, through their senses. People deprived of sensory input for any length of time tend to become mentally unbalanced. It is common to conclude from this that things that cannot be sensed (mathematical objects in particular) are humanly inaccessible, meaningless, or non-existent. But this is based on a misunderstanding of human perception; a more nuanced view shows that our limitations put constraints on how things should be

organized or presented in order to be comprehensible, but not on the things themselves.

The first point is that people do not see what they look at, nor hear what they listen to. We live primarily in internal worlds of pre-packaged objects that are evoked by actual perception rather than determined by them. For the most part this is a good thing: it means we perceive what we *know* about things, not just what we could deduce from their appearance, and what we know usually far exceeds what we could perceive directly.

There is a thriving area in psychology that explores curious mismatches between reality and knowledge-influenced perception. Some of these mismatches are even different at different levels of awareness. There are magic tricks, for instance, that deceive the conscious mind but, judging from eye movements, do not deceive whatever it is that controls eye movement. Presumably this means that the knowledge component is integrated into perception at a fairly high level in the brain.

The next point is that our internal models encode patterns, relationships, behaviors and facts, but with no particular restrictions on what it is that *has* these patterns, behaviors, etc. Through study and experience we can develop an enormous variety of effective models that allow us to “perceive” a great deal from very little, and very odd, input. Radiologists who look at X-rays see diseases; trackers follow nearly-invisible trails; stock traders seem to see the economy in stock prices; scientists see deep and intimate features of nature in their data streams; and art experts see the painter in a brushstroke.

A final point is that highly-developed and heavily-used models become transparent and automatic. I cannot see printed English words without reading them; I cannot hear spoken English without understanding it. As a child I spent a lot of time in cornfields looking for arrow points. Now I cannot walk along a dirt path without scanning for points, and I automatically recognize characteristic features even when very little is visible. And, for better or worse, I cannot look at student work in mathematics without automatically diagnosing errors. All this happens so transparently that it feels like primitive direct perception.

By contrast I recognize very few characters from other alphabets and I am not sure I can claim to really “see” them. I cannot even distinguish individual words in other spoken languages. I cannot see pottery fragments where I see arrow points. And it is an effort to make sense of student work in most other subjects. In other words my genuinely direct perceptual abilities are primitive and unsatisfactory, and seeing *anything* clearly depends on well-developed internal models.

5.2.2. *Mathematical Perception and Reality.* Mathematical objects are toward the abstract end of the spectrum of things that people model internally, but are evidently within reach of facilities that can learn to read X-rays or diagnose electric circuits with a voltmeter. Actually, elementary mathematical objects seem quite accessible by comparison. The point, however, is that mathematics uses standard perceptual and cognitive facilities. It is not a separate ability or qualitatively different activity. This has quite a few consequences, including:

Mathematical objects are as real as objects in the physical world.

This is a cognitive rather than a philosophical assertion¹⁸:

- Because we use cognitive and perceptual facilities that evolved to help us make sense of the physical world (nothing else is available, after all), mathematical objects *appear* just as real to us as the physical, psychological, and other things we model with these facilities¹⁹.
- Mathematical perception is not qualitatively different from what we think of as physical-world perceptions because essentially *everything* we perceive is heavily filtered through highly-developed internal models.

In other words mathematics is as real to us as anything else. Philosophical anxieties about whether it “actually exists” are irrelevant and worrying about this interferes with the doing of mathematics.

The cognitive form of the observation in §2.2.8 is that we should think of the mathematical world as independent of the physical world, as well as real in its own right. In fact from a cognitive and structural point of view the opposite dependence might make more sense: the physical world seems a murky implementation of a small fragment of the mathematical world.

5.2.3. *Historical Note.* Methods cognitively adapted to mathematics are a relatively recent development. Until the end of the nineteenth century it was an article of faith that mathematics is an abstraction of the physical world, and this enforced use of methodology better adapted to science. Separating the two was a major part of the transformation in the early twentieth century, see §9, and no doubt a major reason why so many people found it disturbing.

Traditionalists like Poincaré and Felix Klein, for instance, objected because they wanted to use physically or experientially based methodology. Many applied mathematicians are still objecting for the same reasons. Some philosophers objected (and are still objecting) because they believe mathematics only has “meaning” if it manifests in the physical world. Educators objected (and are still objecting) because they believe that their students (and normal people more generally) can only perceive and understand things that manifest in the physical world. Many physicists still feel that “important” mathematics is an abstraction of physics. It is ironic that the needs of early twentieth-century physics (relativity and quantum mechanics) helped drive the conceptual separation: the mathematics needed was too complex and abstract to be accessible to physically-oriented methodology.

When core mathematics did break away, external denial and rejection had the curious consequence that the methodology could develop without conscious direction, outside influence, or philosophical ‘guidance’. It was able to adapt, in ways we do not understand, to features of mathematics and to human cognitive facilities that are also beyond our understanding.

Finally I note that while essentially all contemporary core mathematicians *act* as though mathematics is a real, separate world, this is rarely articulated. In fact it is not uncommon to profess to believe in a dependence on the physical world. There are two points about this:

¹⁸In particular this is not support for the Platonic view of mathematics. That approach begins with vague and pre-conceived notions of “real” and claims that mathematical objects qualify. Here “real” is used to describe the way people relate to things, not some abstract quality.

¹⁹The actual neural location of these models is a very interesting and potentially important question, [67](i). Wide variation in mathematical ‘styles’ suggests great individual variation, but there may be trends in specific subject areas.

- This is understood as a philosophical issue, and the physical connection is still philosophical orthodoxy. In particular, lack of philosophically defensible alternatives means that mathematicians who address the issue at all are pretty much limited to this version.
- In §2.3.1 I observe that believing something false does not cause problems unless the belief is used in a way that essentially depends on its falseness. In particular this belief is now relatively harmless for mathematicians because the methodology no longer uses it.

Believing in the linkage to the physical world is *not* harmless for mathematics education because this methodology does still use it.

5.3. Level 3: Concept Formation. The point at level 1 is that the structure of mathematics requires that objects be described completely and precisely. The point at level 2 is that humans have to relate to such things essentially the same way they relate to features of the physical world. There is an obvious tension between these two. Educators usually try to resolve this tension by trying to soften and humanize mathematics, but the results are unsatisfactory. The mathematical approach is to develop human-adapted strategies to deal with mathematics as it really is. This has been successful but is still largely limited to the professional community.

In this section I discuss some of the professional strategies developed to help people understand mathematics. The first topic is the bottleneck: human cognitive facilities.

5.3.1. *A Model of Cognition.* For simplicity we consider human thinking as taking place on two levels: conscious and subconscious. Cognitive neuroscience is beginning to provide a much more detailed and nuanced picture (see the discussion in [67](i)), but the simplified version remains useful.

- The conscious level is where critical thinking, judgement, and effective error correction take place. The drawback of conscious thought is that it is quite limited in how many things and how much explicit complexity can be dealt with at one time. The saving grace is that these things can have great *implicit* complexity, managed by the subconscious.
- The subconscious—properly trained—can accurately represent or model very complicated constructs. These are perceived by the conscious level as cognitive units with properties or behaviors that are more-or-less transparently managed by the subconscious.
- The limitation of the subconscious is that it seems to be mostly incapable of critical thinking and judgment. It can find possible connections, generate flashes of insight, or bring concerns to the attention of the conscious level, but for the most part conscious attention is required to reliably evaluate and solidify them.

Effective thinking requires a distribution of effort so strengths of each level can compensate for weakness in the other. In mathematics the general plan is:

- (1) Organize as much as possible of the static material into conceptual units (objects, structures, definitions, standard methods, etc.).
- (2) Internalize these units, or in other words, train the subconscious to accurately model their behavior.
- (3) Consciously manipulate these units.

The following sections expand on this.

5.3.2. *Accurate Internalization.* The biggest challenge in concept formation is accuracy: successful work obviously requires accurate subconscious models, and bad models are the main source of error. The reason this is a challenge is that errors in subconscious models *are almost never corrected by the subconscious.*

- Conceptual errors usually have to be consciously diagnosed, either by the individual or a mentor.
- The concept must then be reopened to conscious processing, and changes have to be imprinted over the faulty version.

Elementary concept errors can sometimes be corrected in the subconscious but the process is long, uncertain, and painful because it is driven by trying to work with a defective version. Diagnosis and conscious correction is faster and more effective, but in any case corrections are considerably harder than initial internalizations (c.f. [16]). Therefore there is a substantial advantage to getting concepts right the first time.

Experienced mathematicians take this seriously: before releasing a concept from conscious development they exercise it and are alert to inconsistencies and other indications of error. Well-written mathematical material is designed to support this: good conceptual units are identified and packaged in ways that facilitate internalization, and are accompanied by propositions and exercises designed to expose likely errors, see §5.1.3. Serious conscious work with conceptual units is usually attempted only after the internalization process is reasonably complete and reliable.

Many experienced mathematicians can quickly absorb new material. I believe the main factor is careful attention to accuracy in concept formation.

5.3.3. *Development of Clarity.* Development of clarity—finding better ways to organize and package the material for effective human use—is a more extensive part of mathematical research than is generally acknowledged. The neglect is not just cultural bias, however:

- The search for a better understanding of known material is usually driven by a desire to extend it. The better understanding then accompanies the advance that it enables, and it is the new result rather than the new clarity that is celebrated. In other words, conceptual improvements are often found embedded in papers with big new theorems.
- “Better” is something that has to be demonstrated by improving functionality of the concept. In research this usually means enabling an advance. It can also be demonstrated by making known material more accessible, but the bar for this is now pretty high: unless the improvement is dramatic it is usually better to wait for an advance-enabling clarification.
- Another way to put this is that concept development is best done with the guidance of applications. Applications, so to speak, force us to get it right²⁰.

²⁰For example the Bourbaki project in France between 1935 and the mid-1980s tried to systematically organize and clarify core areas of mathematics. In the early period other materials were of low conceptual clarity (by current standards) and in many cases their work did significantly improve access. However some of their more ambitious organizational efforts got ahead of technical development and did not mesh well with later needs.

- “Better” also depends on context: different views of a concept might be better for different uses.

An important point here is that ambitious use forces the development of both technically effective concepts and packaging that enables accurate internalization. This suggests that educators might start with mathematical formulations and see what has to be done to adapt the material to their own context. The closer they can stay to the professional version the more effective and functional it will be, and the better it will be as a foundation for later learning. The current practice is to rely on conceptual formulations that are centuries if not millennia old: modern conceptual clarity is an untouched resource.

5.3.4. *Objects vs. Structure.* An important organizational strategy is to distinguish between “objects” and “structures” and think of them in different ways. The distinction is cognitive rather than mathematical so is imprecise and usually kept vague.

- Formats and development of the two are essentially the same. In particular “structures” are objects in the general sense used in this chapter.
- Structures at one level of sophistication may be objects at another; and
- Many topics can be approached either way.

The conceptual distinctions are:

- Structures are properties, relationships or patterns of behavior. Generally they need to be internalized (“automatic facility” acquired) and the pattern and its consequences are more important than the “meaning” in any particular instance. Thinking in terms of structure is powerful because we can transfer internalizations and work habits from simple examples.
- Objects have, are defined by, or are related by, structures. “Understanding” an object frequently means recognizing and learning to use its structures.

For example “addition” is a structure: explicitly, an associative, commutative binary operation with a unit and inverses. There is a rich collection of methods and consequences, and these apply to anything that has the structure. The corresponding “objects” are things that have the structure. These include the usual number systems; polynomials and power series; functions; rings and modules; many categories, etc.

The cognitive point is that we can internalize the properties of addition by working in the integers, and as long as it is learned *as a structure* this learning will serve us well in dealing with other additive systems. This is illustrated in [67](e) where the ring structure of the integers is used as a model for the structure of general commutative rings.

Incidentally, individual numbers do not qualify as either objects or structures in this sense. They have properties and significance mainly as members of a larger ensemble, and things with the same name may have different properties in different settings. The proper cognitive view, in other words, is that numbers are elements of a number system, *not* that a number system is composed of numbers.

The structural aspect of arithmetic is almost absent from elementary education. The connections between integer arithmetic and counting are emphasized, but these are specific to the example, not part of the structure, and do not transfer. Focusing on the “meaning” of integer arithmetic therefore interferes with internalization of

structure. It is my experience (see §8.7.1 and [67](d,i)) that current use of calculators also interferes with internalization of structure by hiding the structure of numbers and making numbers seem completely different from algebraic expressions. A fully structural approach to arithmetic may be impossible in elementary education, but surely there are approaches that are not so counterproductive.

5.3.5. *Variations on Primary Concepts.* Mathematicians deal with a complex landscape of concepts by collecting them in families around primary exemplars. The main function seems to be to help transfer internalized structure, so the development of these families seems to throw light on the scope and limitations of the transfer process. This is illustrated with examples from algebra, but these do not begin to represent the full picture seen deeper in algebra, or in topology and geometry.

The first example is the group family. a *group* (set with associative binary operation, unit and inverses) is considered a primary concept and variations include:

- *abelian* group (the operation is commutative);
- *finite* group (finite underlying set)
- *semigroup* (may not have a unit or inverses)
- *groupoid* (operation not always defined)
- special classes such as arithmetic groups, permutation groups, and linear groups.

Note that the primary concept is not the most general: two of the related concepts are obtained by *omitting* properties rather than specializing. This may seem strange logically, but the objective is to support human understanding and it seems to work. Also, the deep theories for different variations are often very different, but again the objective seems to be to provide useful perspective and reference points, not complete analogs.

The boundaries between families are also determined by human usefulness. A *category*, for instance, could be described as a “semigroupoid” but familiarity with groups does not seem to transfer in any useful way to categories. Conversely a group could be described as a category with one element and invertible morphisms, but internalization of category structure does not provide a useful basis for understanding groups. Consequently categories are in another family, and in fact are the primary concept in the family.

Rings provide another example. A *ring* has two associative binary operations: one (thought of as addition) is commutative and has inverses; the other (thought of as multiplication) distributes over addition and is usually required to have a unit. The integers are the prime example. Variations include:

- *commutative* rings (multiplication commutes)
- *semirings* (addition not required to have inverses)
- special classes such as Noetherian, Artinian, nilpotent, semisimple, Euclidean, etc.

Note that the *non*-commutative version is considered primary even though people worked in commutative rings for millennia before these made an appearance. Also rings have underlying group structures, but the distributive relationship between the two operations is so powerfully influential that the group perspective is not particularly useful.

The final example is fields. A *field* is a ring as above, but additionally the multiplication commutes, and every non-zero element is required to have a multiplicative inverse. The prime examples are the rational and real numbers. Variations include

- finite fields and fields with nonzero characteristic;
- algebraically closed fields;
- *skew* fields, in which the commutativity condition on multiplication is dropped;
- special classes such as number fields, function fields, Galois extensions, etc.

This family is interesting because even though it is a subset of rings and the structures are very similar, it is considered a separate family. Invertibility leads to a different enough theory that rings are not a useful starting point for field theory, and vice versa. It is also interesting that the primary structure in fields requires commutative multiplication, while the primary structure in rings does not. Apparently non-commutativity changes the development too much to really exemplify field theory. In fact skew fields are often called “division rings” and located in the ring family.

It is important to realize that all of these variations were developed in response to applications, not random exploration. Skew fields, for example, arise in a powerful structural result for rings: roughly, a semisimple ring is a product of matrix algebras over skew fields.

Other curiosities:

- The primary exemplar in a family is often the oldest or best-developed variation, but this probably should be seen more as winning a Darwinian fitness struggle than simple historical accident. Wussing’s account [87] of the development of the group concept describes some rather heated competition among preliminary versions.
- Different communities may have different needs and different exemplars. Semigroups, for instance, are important as time-independent dynamical systems and in theoretical computer science, and these communities have their own terminology and conceptual organization.
- Corfield [11] describes a community that feels groupoids should be considered more central. This community, however, is not large enough to develop its own conceptual tradition and their technical developments do not rival the group concept. See §10.4 for further discussion.

5.3.6. *Summary.* The third-level discussion of mathematical objects concerns concept formation. The most important point is the need for accuracy, and concise axiomatic definitions are the primary method for achieving it. There are also strategies for avoiding, finding, and correcting errors.

Strategies at a more detailed level include:

- Thinking of some patterns of behavior as “structures” rather than objects, to aid in transferring internalizations between domains.
- Grouping structural concepts in families around a primary exemplar. The rough idea is to develop an understanding of structure in the exemplar and then, as the need arises, transfer to other objects in the family.
- Evolutionary process that identify structures, families, exemplars, etc. to optimize efficiency of internalization.

Related themes in the next chapter include dealing with difficult concepts, and fitting concepts together in extended developments.

6. MATHEMATICAL DEVELOPMENTS

Most mathematical developments require multiple stages of concept formation. In this section I discuss consequences of this, including relationships with reliability and tradeoffs between difficulty of individual stages and complexity associated with multiple stages.

6.1. Cognitive Difficulty. [[unfinished]] Because it is a cognitive feature it depends on the individual and is not well-defined.

6.2. Cognitive Complexity. The *cognitive complexity* of a mathematical development is the number of distinct stages of concept formation it requires.
[[unfinished; refer to lecture notes.]]

7. MATHEMATICIANS

This section explores the ways in which mathematicians adapt themselves to the field, and some of the consequences of this adaptation.
[[unfinished]]

7.1. Adapted to Mathematics. High-achievers in any area mold themselves to the subject and a large part of the process is development of an accurate subconscious model as described in §??.
[coordinate with §5.3]

M. Gladwell in *Outliers* [21] proposes the “10,000 hour rule”: This seems to be how much fully focused professional activity is needed to become completely expert in (fully adapted to) almost any area. Gladwell’s examples are software pioneers, musicians and chess masters; mathematicians certainly fit the pattern.

The *way* people become adapted can be described in terms of the “dual process” model of thinking (c.f. Stanovich-West [78]). There is a fast, unconscious and heuristic mode, and a slower, deliberative and conscious mode.

- For most people, reliable work in mathematics requires inhibition of heuristic thinking and careful use of the slow, deliberate mode (Simpson [76]).
- Fully adapted mathematicians have trained their fast heuristic facilities to use mathematical methods. Their fast unconscious thinking is as good as most people’s careful conscious logic. This may well reflect a physical reorganization of the brain.
- Frequently expert thought processes become so transparent that they are unaware of what they are doing. Some have claimed that they no longer need careful logic or formal proof (c.f. responses [3] to Jaffe-Quinn [30]). This is silly: logic has been internalized, not bypassed.

Simultaneous chess exhibitions dramatically illustrate this sort of adaptation. There are, roughly speaking, two kinds of chess games: long games with lots of conscious deliberation, and speed chess which relies on fast heuristic thinking. Heuristic thinking is a disaster for non-experts because errors are easy to make and usually fatal. In a simultaneous exhibition one master player takes on a large number of non-experts; often twenty or thirty. The non-experts are playing carefully and deliberately and the expert is playing purely heuristically, but the expert nearly

always wins. Again, their fast unconscious thinking is as good as most people’s careful conscious logic.

Fully-adapted mathematicians are like chess masters or concert violinists in that the technical aspect of their art has become transparent and their conscious focus is on on creativity and expression. They often have trouble giving coherent explanations of what they do, and these explanations are rarely good guides for beginners. Chess masters and violinists, however, have no hesitation in identifying practice—and lots of it—as the key to proficiency, and proficiency as a necessary precursor to real creativity and expression.

8. EDUCATION

In this section I explore educational implications of the perspective described in this essay.

The first topic is why conventional educational approaches seem unable to deal with the problems observed. In a nutshell, mathematics education is based on nineteenth century mathematics, and contemporary problems in mathematics education are closely related to difficulties that led mathematics itself to change in the early twentieth (see §9). Mathematics education may not be really effective until it also goes through this transformation.

After the general introduction I offer explicit suggestions how contemporary approaches might improve student learning. Strictly speaking many of these are not “implications” in the sense that they are neither deduced from nor validated by the perspective. In fact the reverse is true: these are ideas developed through extensive direct work with students, and an important objective in writing this essay was to make sense of them.

The education topics are student use of proofs, both formal and informal; the use of abstract structure to guide teaching; the role of diagnosis; and an approach to word problems. §?? and [67](f) expand on the idea that more care with organization, and more focus on diagnosis, could bring proof-like benefits to elementary mathematics even if the word ‘proof’ is never mentioned. Another opportunity for change concerns definitions and the development of mathematical concepts.

8.1. Educational Philosophy. The basic point is that current mathematics education is based on nineteenth-century mathematics. Rather than attempt a general discussion I focus on the role of Felix Klein. Good sources are Gray [24], especially §4.2.1.1 (pp. 197–199) and 4.8.3 (p. 277), and the Klein Project of the ICMI, [37].

Klein was very influential in educational reform around 1905 through his course and curriculum designs, his book *Elementary Mathematics from an Advanced Standpoint*, [35], and his philosophical perspective. His work and point of view are still central to the current educational system. Klein was also universally recognized as a preeminent mathematician, but by 1905 the nature of his influence in mathematics was shifting. The mathematical transformation (§9) was well under way and Klein, a product of the nineteenth century, was a strong advocate for the old ways. His work was still important and admired but for the new generation he was no longer a model to be emulated.

I describe some of the issues in the transformation that remain important in education.

8.1.1. *Meaning.* Up through the nineteenth century the purpose of mathematics and meaning of mathematical ideas were philosophical rather than mathematical issues. Leading mathematicians were, perforce, involved in philosophy, and many philosophers had mathematical interests. The main theme held that mathematics is an idealization of the physical world and receives meaning from it. Mathematical things unrelated to the physical world are meaningless, if not wrong.

This is the point of view so compellingly developed by Kant, or at least the way people seem to have understood it. This is also why non-Euclidean geometries were so troubling at an earlier time. Not because the old master was wrong—many of his methods were already being discarded—but because they drove an undeniable wedge between mathematics and the physical world. Klein was not at all bothered by non-Euclidean geometry. However this may have been because he saw a place for them in a deeper understanding of the physical world; he remained committed to the physical-realist philosophy.

The new view is that mathematics is a world of its own, with wonderful things not dreamt of in their philosophy. Much of it doesn't relate to the physical world (yet anyway), and there is nothing wrong with that. The philosophical framework with its prescriptions about “meaning” inhibited rather than aided mathematical development.

8.1.2. *Intuition.* The role of intuition, particularly intuition derived from our experience in the physical world, is closely related to the philosophical connection described above. If mathematics is a faithful mirror of the world then our physical intuitions should be valid in mathematics. This point of view became increasingly problematic in the late nineteenth century and was abandoned as untenable early in the twentieth²¹.

Klein was a strong proponent of intuition. In lectures in the 1890s Klein agreed that the “arithmetization” of geometry and analysis had brought new precision and advances. He nonetheless opposed it because it came at the expense of space intuitions, understanding, perhaps even meaning. In one lecture he seemed to offer a compromise: “naive” intuition was unreliable and had to be “refined” by careful work with axioms. He still insisted on the importance of psychological and experiential aspects of intuition. His discomfort, it seems, was with the increasing tendency of people to develop “mathematical” intuition directly from their work with axioms, and to give this artificial intuition precedence over that coming from the physical world (see also §9.5.2).

8.1.3. *Modern Difficulties.* Early in the twentieth century Klein was no longer a leader in mathematics because no one would follow him: talented young people saw giving up physical intuition in exchange for technical power as a good deal and voted with their feet. It was then that he turned his focus to education, where he was more successful. To this day, and in no small part due to his efforts, mathematics education follows nineteenth century mathematical ideals.

School mathematics rarely even contains eighteenth-century material so perhaps, at least for elementary education, this is not a bad model. In fact education had many problems but the basic model seems not to have been one of them until the 1990s when three things changed:

²¹This refers to the use of intuition in official justifications (proofs). Intuition as a source of insights is unproblematic and thriving.

- Technology, particularly calculators, enabled elimination of rote and skill-oriented work that was never part of the model but could not be avoided as long as students had to work by hand. It turns out that students were learning quite a lot that wasn't being deliberately taught and doesn't fit the model, ??(d, i).
- A vigorous reform movement, particularly the National Council of Teachers of Mathematics [51] in the US, put strong emphasis on pretty much exactly the things in the nineteenth-century model that were abandoned as problematic in the twentieth.
- An educational-research community developed, both deeply committed to nineteenth-century ideals and more-or-less designed to validate them (see in [67](a)).

To be more explicit, and repeat some of the points above, the following commonalities are revealed by comparison of the NCTM Standards and the writings of Klein and other nineteenth-century proponents:

- An emphasis on intuition and experience from the physical world as a basis for mathematical learning;
- a conviction that the meaning and value of mathematics derives from the physical world (ie. from applications);
- an emphasis on heuristic understanding and intuition over rote or formal work with soulless axioms; and
- a preference for discussion of goals and methods on a high (philosophical) level rather than in terms of skills and mathematical structure.

The last point helps explain the persistent focus on Standards Documents despite plentiful evidence that they are ineffective, see [67](b). In any case these are all convictions that had to be abandoned or substantially revised before twentieth-century mathematics was possible, and may also be blocking progress in education.

These observations resolve a puzzle: current educational philosophy is coherent, attractive, and well articulated, even if deeply flawed. This is a significant accomplishment and I used to admire the NCTM writing teams for it. But it is now clear that they cribbed—knowingly or not—from the masters of nineteenth-century mathematics.

8.1.4. *Is Change Possible?* This discussion reveals some substantial barriers to change:

- (1) The coherence and attractive articulation inherited from nineteenth-century mathematics prevents piecemeal change. The whole model has to be replaced, just as it was in mathematics, before significant progress is possible.
- (2) Mathematicians have been unable to articulate convincing reasons for their objections: they typically follow current mathematical norms in focusing on technical issues and this simply does not make contact with discourse in the education community. The differences therefore appear to be a matter of opinion, and since they conflict with a coherent and well-established philosophy they are rejected.
- (3) The natural-selection pressures that drove the mathematical transformation do not apply. In mathematics the new methodology offered substantially more power, and this was clear enough to the next generation that

they abandoned the old. Modern mathematical methodologies do not offer immediate advantages to the next generation of educators.

- (4) Mathematics education has become ideological and politicized, particularly in the US. Change is now a matter of winning or losing rather than improving, and the educational establishment is far more powerful politically than the mathematical community.

A principal objective of this essay is to develop a conceptual basis for educational change. In particular, my description of the nature of contemporary mathematics in earlier sections is an effort to address (1) and (2).

Items (3) and (4) show that change cannot happen through high-level discourse, so a conceptual description alone is not sufficient. Concrete and detailed suggestions might avoid political problems and give selective advantage some purchase. The remainder of this section, [67](f), and a number of other essays in that reference are attempts in this direction.

Finally, (4) means that some changes are so firmly believed to be wrong that even concrete suggestions will be rejected. These include descriptions of ways technology could—or will not—support learning, and the use of definitions and ideas from this essay in bits of elementary education. It seems these must be approached on a very primitive level: products have to be developed and offered for use without explanations that would trigger resistance. This was the motivation for a now-discontinued problem-list project, and the EduT_EX educational software project [70].

8.2. Informal Student Proofs. According to §4 a *potential proof* is an argument that uses accepted mathematical methods and is presented in sufficient detail to be checked for errors. This becomes a proof once it *has been* appropriately checked and found to be free of errors. Remarkably, proofs and potential proofs in this sense are very common at all levels: a teacher’s instruction to “show work”, for instance, is very close to “give a proof”. I expand on this below.

This should mean that being more conscious and systematic about showing work should give students at all levels access to the procedural and cognitive benefits of proof. Good practice should include:

- write intermediate expressions correctly and carefully enough that someone else can read it;
- write steps in order or clearly indicate the order, for instance by numbering;
- explain notation used to formulate a problem (especially word problems).

The goal is work that should be reasonably easy for someone to follow and check for errors.

It is important to distinguish between checkable and “beautiful”. Crossed-out mistakes are fine as long as it is clear what is in and what is out. Students should not worry about beauty while they are trying to do math, and should not be expected to copy work over unless it is genuinely hard to decipher.

8.3. Focus on Diagnosis. The thesis of this article is that the reliability associated with mathematics is obtained by making mathematical arguments that can be checked for errors, *checking them*, and correcting any errors found. The previous section describes how checkable arguments could become a routine part of mathematics education. However they won’t produce benefits unless *checking* also

becomes a routine part. To be explicit: diagnosis and error correction should be key focuses in mathematics education.

- Answers are important mainly as proxies for the work done. Incorrect answers indicate a need for diagnosis and correction and ideally *every* problem with a wrong answer really ought to be diagnosed and corrected.
- Mathematics uniquely enables quality so the emphasis should be on quality, not quantity. In other words, doing fewer problems to enable spending more time on errors is a good tradeoff.
- An important objective is to teach students to routinely diagnose their own work. The fact that diagnosis is possible and effective is the essence of mathematics, so teaching self-diagnosis is mathematics education in the purest sense.

Ideally teachers would regularly go through students' work with them so they can see the checking process in action. Students should be required to redo problems when the work is hard to check, not just when the answer is wrong. I note that the goal is to establish work habits that will benefit students, but students respond to feedback from teachers, not long-term goals.

8.4. Complicated Problems, Formal Proofs. Formal proofs differ from informal versions in requiring explicit mention of main steps and perhaps use of a structured format [45, 55]. The purpose is still to make it possible to find errors: formality is required by complicated problems and sneaky errors, not a new purpose.

Formal proofs as an organizational tool could be introduced when first needed and then used again from time-to-time as appropriate. For example integer fractions are probably the first significant conceptual structure students have to deal with explicitly. A version of formal proof might be used until they develop enough facility to work accurately without aid. Formal structures could be used again later when a more abstract and symbolic treatment of fractions is given, see §??.

As a general point, many conceptual structures and subtle procedures must be mastered before mathematics becomes useful in a serious sense, and each of these has a learning curve. If we provide good tools—such as formal proof—for developing conceptual mastery, students will go further, more easily.

8.5. Teacher Preparation. I have two suggestions for preparation of teachers of elementary mathematics. First, ensure they have a mathematically correct understanding of the material, and second, develop an understanding of the way mathematics works, but *not* through lots of mathematics.

8.5.1. *Elementary Mathematics from an Advanced Viewpoint.* The section title comes from the important and influential book by Felix Klein, c.f. [37] though I may interpret it in a slightly different way.

Learning mathematics divides roughly into two parts: developing intuitive (subconscious) models of mathematical objects, and learning to consciously work with them, see §5.3.1. In practice models developed by elementary students are frequently inadequate for work at their current level and almost never good enough to use at the next level, so in practice there is a third part: identifying problems with internal models and retraining the subconscious to fix them. This third part is intellectually and psychologically difficult and probably accounts for much of the difficulty students have with mathematics.

I believe the greatest help teachers can give their students is by carefully—often subliminally—guiding them toward effective internal models, and by diagnosing and correcting problems before they get too deeply embedded. However for this to be possible the teacher must have very accurate and well-established models, and these should be informed by an Advanced Viewpoint.

It would also help if teachers used contemporary approaches to concept formation, §5.1.3, ??.

The essay [67](e) was written to try out these ideas. It gives perspectives on fractions, for instance, that will probably be new and useful to even the most seasoned teacher.

8.5.2. *Understanding How Mathematics Works.* To preface the suggestion, the almost universal feeling in the mathematical community is that teachers must know considerably more mathematics before good outcomes can be expected. The real issue is probably not content, but the habits and mindsets mathematicians develop while learning content. Things like understanding the need for skepticism and rigor, or appreciation of the balanced beauty and rich content of a good definition, are acquired through intense study as mathematicians adapt themselves to the subject, see §7.1.

Since these things are not learned explicitly they tend to be visceral rather than conscious. To determine if something is “mathematical” mathematicians consult their gut feelings: if the gut feelings are good the math is good, otherwise something is wrong.

I suspect that when mathematicians say “teachers need to know more math” they probably mean “my guts are unhappy about what they are doing. If they had studied enough mathematics to have effectively internalized its nature then their gut feelings would keep them from doing that.” They identify insufficient content as the problem because they themselves acquired the mindset through extensive work with content.

It is unrealistic and inappropriate to expect teachers to go through professional mathematical preparation. The mindset, however, might be developed using less-ambitious work together with explicit descriptions of how it illustrates general features. The essay [67](e) mentioned above is an attempt to flesh out this idea. It is a draft of a course for teachers, showing modern methodology (formal definitions, abstractions and proofs) at work in the contexts of fractions and area of polygons.

8.6. **Applications and Word Problems.** Word problems have two components: a physical-world formulation and a mathematical model. According to the analysis presented here the physical formulation is *not* part of the mathematics.

Material that is not strictly mathematical has an important role to play in mathematics courses. However it should not be presented as mathematics and student should be taught to distinguish between the two. In particular students should be taught to separate translation of a word problem to a mathematical model from analysis of the model. One reason is intellectual honesty: these are different activities with different rules. However I find the cognitive reasons just as compelling, and see the abstract considerations almost as guidance on where to draw the line.

- Translating a word problem to a form suitable for mathematical processing is a different cognitive process than doing the processing. Separating the two reduces cognitive interference and errors.

- Models are abstractions that frequently apply to many different physical situations. Cutting a string and sharing out a pitcher of juice can both be modeled by the same division operation. Connecting mathematics too closely to physical illustrations obscures this and can impede later learning.
- In genuine real life there are always mismatches between models and the situations they are supposed to model. This is a modeling problem, not a mathematics problem.

Word problems can engage students. Word problems with a reality/model disconnect offer further opportunities for engagement even as they promote valuable long-term perspective and work habits.

8.6.1. *Sample Problem.* Bubba has a still that produces 700 gallons of alcohol per week. If the tax on alcohol is \$1.50 per gallon, how much tax will Bubba pay in a month? [Set up and analyze a model, then discuss applicability of the model.]

I have given an example with obvious cultural bias because I am not sure I could successfully avoid it. At any rate students in my area in rural Virginia would think this problem is hilarious. We have a long tradition of illegal distilleries and they would know that Bubba has no intention of ever paying any tax.

8.7. **Counterproductive Practices.** My experiences have convinced me that some educational practices are genuinely counterproductive. The perspective in this article can help identify reasons they are counterproductive and suggest ways to avoid some problems. However these were identified as problems through direct experience with students, not by abstract deduction from the perspective.

I focus on two examples in which disconnects from the nature of mathematics are easy to see.

8.7.1. *Calculators.* I am convinced that a computational environment for mathematics education is urgently needed [?]. Unfortunately, calculators as they are currently used are not satisfactory.

- Calculators do not record steps so calculator work cannot be diagnosed for errors, see §??.
- Calculators seem not to make contact with important modes of learning [67](e). This contributes to deficiencies in symbolic skills and internalization of qualitative geometric structure that are serious disadvantages when students reach the university level.
- Calculators encourage thinking in terms of algorithmic calculation rather than logical structure and algebraic expressions, see [67](h, i).
- Calculators encourage focus on numerical work, contributing to deficiencies in symbolic skills.
- Calculators even reduce number sense, as I explain below.

Numbers have structure, and the ability to recognize and exploit this structure is the number sense needed at the university level. A basic structure encoded in our notation is that multiplication by powers of ten corresponds to shifting the location of the decimal point. Traditional students are well aware of this because it is a big time-saver, and use it automatically. However I often see students using calculators to multiply by powers of ten. Colleagues tell me they have seen students use a calculator to multiply by one.

To calculator students, all numbers are the same and their automatic response to a multiplication problem is to start pressing keys. Scanning for special cases requires a different mode of thinking, more attention, and increases exposure to errors. Even decimal-point shifting does not save time. It might save time when it works, but scanning for the opportunity to use it is not automatic for these students and the additional time and attention required for routine scanning would cause a net time loss.

Another example lies on the boundary between number sense and symbolic skills. When presented with $(56 \times 7 \times 233)/7$ traditional students will generally see the opportunity to cancel the two sevens. Many scan for things like this automatically because it is a big time-saver. Calculator students generally do all the arithmetic. Scanning for cancellations requires more attention and saves little if any time.

Canceling rarely makes much difference when dealing with numbers but becomes important with expressions such as $(56 \times a \times 233)/a$. Many traditional students are already scanning for cancellations and in this mode are dealing with numbers as symbols anyway so quickly adapt to literal symbols. Calculator students aren't scanning, and their number skills—mostly concerned with keystrokes—don't transfer to symbols.

To repeat, a good computational environment for students is necessary, even urgent. However current calculators do not provide a good environment and improvement seems unlikely until their drawbacks are acknowledged and addressed. An attempt to imagine a good environment, shaped in part by the need to avoid calculator problems, is described in [67](g). See [67](i) for discussion of neurological aspects.

8.7.2. *Euclidean Geometry.* I firmly believe that geometry is an essential part of mathematics education. However traditional Euclidean geometry is not satisfactory, and in particular it is unsatisfactory as the principal setting for proofs.

Section 2.2.10 describes ways in which Euclidean methods fail to be error-displaying and are correspondingly nonmathematical. The fact that it is essentially proof-by-example is particularly problematic. Section ?? explains how the conceptual organization is out of step with current practice and is close to dysfunctional. On top of all this there are educational shortcomings:

- In later work students will need coordinate geometry with parametric lines, function graphs, etc. The Euclidean approach of excluding coordinates and taking points and lines as primitive objects connects poorly with this.
- Students certainly need exposure to proofs. However they will need these more-careful ways of thinking in *all* mathematical work, not just a limited context. Euclidean methods do not provide good models for work in other courses even if they are upgraded to actually be proofs.
- Coordinate plane geometry is pretty obviously a baby version of three-dimensional geometry and most of the methods extend directly to higher dimensions. Euclidean plane geometry is very tied to the plane, and these days that is an uncomfortably small box.

In sum, the primitive Euclidean approach to geometry has become a barrier that hinders access to other dimensions and other mathematical worlds. These worlds are too rich and too easily accessible to modern approaches for this to be acceptable.

9. HISTORY: THE MATHEMATICAL REVOLUTION

Mathematics went through a period of turmoil known as the “foundational crisis” in the early twentieth century. It is generally known that core professional practice broke from philosophy and became much more powerful. However the conventional view of philosophers, historians, educators, popularizers, and many users of mathematics is that the new methodology is a sort of collective insanity that will eventually pass, and there is no connection between seemingly unnatural modern methods and powerful modern success.

In this section I describe the transition, contrast practice before and after, and explain what drove the change. The discussion owes much to Jeremy Gray’s historical work, and his book *Plato’s Ghost* [24] is discussed in §9.5. The conclusion is that this was essentially a ‘scientific revolution’ in the philosophy/science transition sense. Why this is not more widely understood is a puzzle: standard accounts of mathematics seem completely disconnected from the subject I know and describe here. Therefore a secondary objective in this section and the next is to look for explanations for this lack of understanding.

I also describe how mathematics was influenced by events later in the twentieth century, and speculate on upcoming challenges.

9.1. The Transition as a ‘Revolution’. There are several differences between the mathematical revolution and transitions in the physical sciences. First, philosophical mathematics was never as completely flaccid as philosophical physics. Presumably this is because Euclid got there first and philosophy developed around his work. Euclid is deeply flawed from a modern point of view but better than nothing.

A second difference is that there had already been a ‘scientific revolution’ of sorts in mathematics. The new physics of the seventeenth century depended heavily on mathematical modeling, and this enabled development of mathematical methodology evaluated by effectiveness rather than by philosophy. In particular Euclid had been locked in as the philosophical ideal of mathematics and the patronage of physical science enabled mathematics to move beyond this (see §2.4.6). However the methodological shifts were limited to lower, immediately practical levels, and philosophy continued to control ‘meaning’, ‘reality’, ‘truth’, ‘understanding’, etc. The result is that mathematical practice had not become fully mathematical, but it was a science in the sense of §2.2.8.

Finally, the new methodology did not fully displace the old. In many application-oriented areas nineteenth-century philosophical constraints were unproblematic and perhaps not inappropriate, and practice was sufficiently ‘scientific’ to be successful. Core areas needed to break free of philosophy to become fully adapted to the subject, but mathematical science for the most part did not.

9.1.1. Kuhn’s Framework. In the 1960s Thomas Kuhn [39] extended the term ‘scientific revolution’ to include “paradigm shifts” *within* science; see Nickles [53] for an overview of current status. Comparing the mathematical transition to this framework seems to clarify both. First, Kuhn’s definition includes incommensurability of ways of thinking severe enough to essentially preclude communication. This seems to be true of philosophy/science transitions. Kuhn may have extrapolated to changes internal to science because—understandably for a philosopher—he did not see a qualitative difference. It is less true in the extended setting and Kuhn’s successors have had difficulty sustaining or usefully reinterpreting this criterion.

However it seems to apply to the mathematical transition. Pre-revolutionaries, as represented by philosophers, educators, physicists, and many mathematical scientists, find a good deal of contemporary core practice alien and incomprehensible. Folland [?] and §2.2.2 describe incommensurability in the other direction. Communication across the gap has indeed been nearly impossible [67][o, p, q], §11 but this brings us to another problem.

If a revolution occurs but nobody notices, is it still a revolution? This is a variation on the old “if a tree falls in the woods but there is nobody there to hear it, does it make a sound?” Some philosophers have argued that awareness should be a criterion, perhaps because they feel ‘revolution’ is a state of mind, or perhaps because philosophers cannot imagine someone doing something revolutionary without noticing. However the historical record has not been supportive of this and it is problematic here. Mathematicians are about as conscious of what they do as fish and birds are conscious of swimming and flying, and when they try to be reflective they are about as successful as fish and birds would be. Almost none are consciously aware of the changes. Communication across the divide has been impossible not just because the methodologies are incommensurable, but because there has been no coherent voice for the new order.

A final problem has to do with replacement versus revision. In philosophy/science transitions new methodologies replace the old. In science/science transitions significant change is limited to subfields, and enough remains that it is more a revision than a replacement. Kuhnian accounts have trouble accommodating these differences. To me most of the latter transitions make more sense as progressive micro-adaptation to the subject matter, and the ‘revolutionary’ aspects have more to do with human inflexibility than deep problems with knowledge. The mathematical transition could be read this way. Viewing everything considered mathematical as a whole, most of it did not change and—incommensurability notwithstanding—much remains the same in the part that did change. Further, as mentioned above, nineteenth century mathematics was already essentially scientific. Is the mathematical transition therefore an internal-to-science subfield adaptation rather than a philosophy/science revolution?

This reading does explain the continuity in mathematics. It needs more care with the meanings of ‘mathematics’, ‘science’, and ‘philosophy’, but even with these implicit interpretations there is an interesting point. The science/mathematics transition was not really internal to science because, in profound ways related to validation criteria (see §2.2.8), core mathematics is no longer a science. In particular it seems to have caused the same human dislocations as a philosophy/science transition, so it is a good analog even if not actually an instance.

A better understanding emerges from more careful consideration of the terms involved. First, it does not make much sense to define mathematics as everyone who self-identifies as a mathematician. Work done to contemporary standards for use within mathematics really does constitute the core of the discipline, and it is only in this subset we would expect to see full methodological replacement in a philosophy/science type transition. Second, more care is needed with the difference between philosophy and science. Classical philosophy depends largely on top-down deduction from general principles. Science is more bottom-up, largely driven by (and adapted to) nitty-gritty details of the subject. Nineteenth-century mathematics was scientific in many ways but convictions about truth, meaning, knowledge,

reality, etc. still came from philosophy, were not well-adapted, and had become problematic. During the transition core practice became fully subject-driven, and the philosophical constraints were discarded as part of the process. With these interpretations the mathematical event does qualify as a philosophy/science transition.

9.2. Sketch of the Transition. Up through the early nineteenth century mathematics was thought of as an idealization of the physical world. There were no ‘mathematical objects’ per se, just abstractions or idealizations of features of physical experience. Such objects could be specified by example or by reference to the physical world, and what we now think of as mathematically-precise definitions were not considered necessary. Similarly, it was believed that reasoning about such objects could safely depend on physical intuition.

All this changed. More precisely, mathematical practice changed so this description is no longer valid even though it remains the dominant view outside the profession. I describe the change in stages; the reality was obviously not so tidy.

9.2.1. Stage One: Objects. The first stage in the transition was the development of definitions in the modern sense: concise and *definitive* formulations rather than examples or explanations intended to bring to mind or refine some feature of physical experience. This seems to have taken place mainly in Germany in the mid nineteenth century, led by Dirichlet, Dedekind, and Riemann in Göttingen. From the ‘revolution’ point of view definitions were not seriously problematic: people who needed them used them, and people who didn’t need them could ignore them.

Development of precise definitions seems to have been driven by the need to investigate objects that were so subtle and far removed from previous experience that they could only be understood this way. A good example is provided by the work of Kummer on Fermat’s “Last Theorem”, see [2] volume III p. 345. In the middle of the nineteenth century Kummer found a proof, assuming uniqueness of prime factorization in the cyclotomic integers. Uniqueness is a key tool in the ordinary integers, and consequently deeply embedded in intuition developed there. Unfortunately it fails in cyclotomic integers. Moreover, to investigate factorizations in this context one must work with prime *ideals*, not just primes in the ring. This is a complicated and subtle business, and the chances of getting it right without precise definitions are very near zero. However with these tools Kummer, Dedekind and others were able to do deep work in the subject.

Precise definitions also enable identification and rapid development of new mathematical objects, so a consequence of the new methodology was a profusion of new—or newly useable—concepts. Put another way, imprecise definitions put strong limits on complexity (in the sense of §6.2) of mathematical development. The development of precise definitions made cognitively-complex subjects more accessible, and the power of the innovation caused it to spread relatively quickly to parts of the community concerned with subjects.

Precise definitions were not universally accepted. They gave little advantage in areas of low cognitive complexity. There are very hard subjects with low complexity, see §10.2, and practitioners in these areas tended to denigrate the movement. Another barrier was the apparent lack of connection to physical reality. Philosophers and many traditional and applied mathematicians saw the new material as abstract and frivolous, done apparently “for its own sake”.

There were objections to certain types of definitions as well. The term “manifold” was introduced by Grassman in 1845 as a philosophical heuristic [75]. Around 1900 Poincaré showed these objects to have wonderful properties and they eventually became one of the main themes of the twentieth century. He still defined them by reference to examples, however, and used both analytic and combinatorial techniques on the intuitive conviction that the objects would support both and it was just a matter of choice. We now know that analytic techniques require a ‘smooth structure’ and not all manifolds have them. However early proposals to work with smooth structures were rejected on philosophical grounds: manifolds should be *objects*, not structures on an object. Furthermore, smooth structures seemed to amount to assuming what you wanted to prove, and seemed intellectually dishonest if not logically circular²². The situation was still chaotic in 1930 when van der Waerden surveyed the field and saw a “battlefield of methods”. However by then the transition was well established. In 1932 Veblen and Whitehead gave a precise definition and demonstrated that it would support rigorous development. The new generation welcomed this and it led quickly to extensive development. Curiously, many people still believed that Poincaré was right and eventually these structures would be seen to be unnecessary. Milnor’s discovery of several smooth structures on the 7-sphere in 1957 was therefore a shock. Mathematics is richer and more complex than our imaginations, and too much attachment to intuition had been a liability.

This sketch depends too much on hindsight and inference. Tracing out the early development in the historical record (e.g. Göttingen in the mid nineteenth century) would surely give a much richer story, and I think this is an important issue. I have come to see modern approaches to concept formation as significant as modern approaches to proof.

9.2.2. *Stage Two: Rigorous Proofs.* The second stage in the transition centered on more-precise ways of working, particularly proofs as described in §4. Modern proofs require precise definitions and also effectively exploit their precision. For this reason proofs came most easily to the new areas—notably “abstract algebra”—made accessible by the use of precise definitions.

The situation in analysis was more complicated and better illustrates the general situation. Modern rigor is usually considered to have its roots in the work of Cauchy, Bolzano, Gauss and others sorting out problems with convergence. They did make progress, but it was limited for quite some time by the lack of a precise definition of convergence and, more fundamentally, a precise description of the real numbers. This latter problem was recognized by Bolzano by 1817 and he and others began an effort to describe the real numbers in terms of the integers. The effort was brought to fruition by Weierstrauss, Dedekind, Cantor and several others around 1872 (c.f. Boyer, [9], §25). This development put full rigor within reach in analysis, and in the twentieth century it would be considered an essential basic part of mathematics, but it was not universally welcomed.

The first problem was that this effort revealed that the integers themselves were not fully precisely understood. Cantor, Frege and others set out to fix this with logic and set theory, and their efforts were plagued by even deeper problems. One

²²Bertrand Russell wrote ([74] p. 71) “The method of ‘postulating’ what we want has many advantages; they are the same as the advantages of theft over honest toil.” This is true about the results of toil, but not what we want to have to enable toil.

might have thought Peano’s axiomatic description of the natural numbers in 1889 would have brought some clarity, but it was one of the factors that launched the main 1890–1930 transition: it was very effective technically, and very unsatisfying philosophically (see the discussion in the next section). The outcome was that the description of the reals using the integers did in fact provide a solid base for analysis, but it took nearly sixty years for the deeper foundational crisis to work itself out so people could feel completely comfortable with it.

The second problem was that some leading analysts and geometers were hostile to this development. Felix Klein referred to it derisively as the “arithmetization” of analysis and continued to oppose it well into the twentieth century. Poincaré described it as a disease that needed a cure. Hermann Weyl described it in 1918 as “in an essential part built on sand.” He evidently felt that the foundational problems made “arithmetization” less reliable than his own intuition.

The consequence was that analysis, the ostensible nursery of modern rigor, was actually a bit tardy in becoming fully modern.

9.2.3. *Stage Three: The ‘Revolution’.* According to Gray, who mapped it out in some detail in [24], the transition became a major issue around 1890 and was pretty much over by 1930. Göttingen was again at the center of the development, this time led by Hilbert. This section gives an outline from the perspective of this essay; §9.5 describes the connection to Gray’s work in more detail.

That this was a time of turmoil is well-known even to those who do not see that anything came of it. It seems to me that the turmoil had three main factors:

- The need for uniformity of practice, and particularly the rejection of the use of intuitive conclusions in proofs;
- The rejection of philosophy and physical reality as the main sources of meaning; and
- The technical foundation crisis.

I expand on these.

First, uniformity of practice. Precise definitions and modern proofs can bring substantial benefits to a mathematical community, but only if *everyone* uses them. Work that is not scrupulously careful is not fully reliable and potentially dangerous. Consequently, once a nucleus of fully-modern material was developed the practitioners began to describe old-fashioned work as incomplete or preliminary. The elite-practitioner syndrome described in §2.4.8 and 7, where leaders want to rely on their excellent intuitions and not bother with fiddly details, came into play with full force. In some areas, e.g. analysis and geometry with Klein, Poincaré and Brower, completing the transition was literally a matter of waiting for the older generation to fade away.

Second, the distancing from philosophy and physical reality. Self-contained definitions and high-precision methods do not depend on either philosophical or physical guidance. Students trained in the new methods found meaning, insight, and intuition directly in the mathematics, and the mathematical meaning and intuition was usually deeper and more powerful.

I illustrate this with the Peano axioms for the natural numbers. In this formulation the natural numbers are a set with a “successor” function that moves everything up one place. “1” is the unique element that is not the successor of

anything. “2” is the successor of 1, and so on²³. Philosophical problems with this include:

- (1) This characterizes *models* for the natural numbers, and does not define *the* natural numbers. Any set of the appropriate size has a successor function and so gives such a model, and different models have different “1” and “2”.
- (2) This approach does not show that the numbers exist.
- (3) The connection to counting physical things is indirect and derived rather than primary.

The modern attitude toward (1) is that it doesn’t matter if each mathematician has his own private model: they are all canonically equivalent so one as good as any other. And no, you should not try to get a universal model by taking equivalence classes because this leads to ugly and irrelevant set-theoretical issues²⁴. Similarly for (2), having to assume existence separately (as existence of an infinite set) is a small price to pay for a clear and effective description of properties. Finally, the point in (3) is that counting is better seen as an *application* than a definition. Trying to turn counting into a definition leads to a dysfunctional mess (compare Peano with Russell-Whitehead, and see §2.4.5).

Returning to the items on the list, “separation from reality” means, as in (3) just above, taking the mathematical world on its own terms as primary. Clues, goals and inspiration still frequently come from physical reality, but these lead us to mathematics rather than define it. This attitude is often described as “formal and meaningless”, “divorced from reality”, etc. and these can indeed be problems. However the truth is that taking mathematics as primary gives power and simplicity, and eventually leads to deeper insights and better applications even in the physical world. This seems to be a function of human cognition more than some abstract reality of mathematics, see §5.2.2.

Finally, the foundational crisis. This was resolved in a technically effective but philosophically unsatisfactory way. First it developed that “higher mathematics” is robust in the sense that it depends on the existence of *some* set theory, but not on troublesome details of any particular set theory. This is nicely explained by Mazur in [46] using a categorical point of view; again see §10.1. Also, Frege’s attempt to derive the integers from set theory was generally thought (even by Frege) to have been severely damaged by Russell’s set-formation paradox. It turns out this is a “troublesome detail” that can be resolved in a number of ways, and none of them effect the final result²⁵.

The second peculiarity of the resolution of the crisis was the evolution of the ‘experimental’ basis described in §?? and 2.3.3. The philosophical goal of *proving* that mathematics is consistent was never realized. Instead a somewhat threadbare set theory (Zermelo-Fraenkel) was developed that was good enough to support mathematics and has (so far) resisted all attacks. This was philosophically unsatisfying, but this and robustness were good enough for mathematics.

²³For simplification and to make key issues clear, this version uses standard set theory. Peano had to include simplified versions of enough rules of set theory for it to make sense as a self-contained development.

²⁴See Mazur [46] for a similar discussion, and §10.1 for more about [46].

²⁵See Burgess [10] and Heck [27] for a resolution that should have been easily accessible to Frege. The puzzle is why Frege did not use it.

The outcome of all this was that mathematics emerged from the transition powerful and ready to undertake the vast developments of the twentieth century. The connection with philosophy had been lost, and the relationship with the physical world had been weakened and became in some ways problematic (see §2.4.6).

9.3. Confusion, Obscurity, and Philosophy. Implicit in this story is the fact that philosophers went from being important guides to methodology and meaning, to objects of ridicule, in less than forty years. How did this happen, and does it matter?

9.3.1. *Left Behind.* The mathematical side of the story is largely told in the previous section. In brief, the high-precision methodologies developed in the mid to late nineteenth century worked very well—better, in fact—without philosophical input. Philosophers seemed not to notice this. They were preoccupied with new insights into antiquity, grappling with the significance of non-Euclidean geometry, or the beginning of the foundational crisis. Furthermore the new development took place in technically-complex areas largely inaccessible to philosophers.

During the 1890–1930 transition philosophers concerned with current mathematics were largely focused on the foundational crisis. By their standards the efforts to resolve it were failures. For example Frege’s program for arithmetic and Hilbert’s algorithmic approach to foundations are both thought of as failures: Frege brought down by Russell, and Hilbert by Gödel. Further, much is made of disagreements between Hilbert and Frege; see Blanchette, [55]. It seems to me however, that the “fatal flaw” description had more to do with philosophical interpretation than with technical issues. To me the problems seem to be the kind one expects in any ambitious program: requiring adjustment of goals or techniques but not requiring starting over²⁶.

In fact these “failed” programs were both instrumental in developing contemporary methodology, and Hilbert’s was particularly successful. It seems to me that a major implicit goal of his program was a mathematical meaning for the word ‘true’. Philosophers had controlled this word for millennia and burdened it with connections to meaning and knowledge that encouraged word games and led to paralysis.

For example if an axiom system has nothing to do with “reality” then more-or-less by definition (according to philosophers) we cannot really “know” anything about it, it cannot have “meaning”, and “true” or “false” don’t even apply. This is extremely confining. I certainly “know” and find “meaning” in lots of things that as far as I can tell have no connection to *physical* reality. I see them as part of a larger reality, possibly because my brain has been rewired by a hundred thousand hours spent peering into the void. However I would *not* want to argue that because they seem real *to me* they must have some validity. This is playing the game by philosophical rules and leads to a worthless user-dependent meaning for ‘true’.

Hilbert’s proposal was to define “mathematically true” as “algorithmically deducible from the axioms”, and essentially refuse to address the “meaningfulness” of the axioms. Indeed, as a way to avoid being drawn into pointless argument he was willing to accept descriptions of axiom systems as “meaningless”. Gödel then proved, with fully-modern rigor, that in usefully complex axiom systems there are statements that are correct but that cannot be proved in Hilbert’s sense. This killed

²⁶These might be paradigm shifts and ‘revolutions’ in Kuhn’s sense, [39].

Hilbert’s technical proposal but it seems to me that it accomplished the larger goal: it established Gödel’s technical interpretation of ‘true’ as legitimate for mathematical use. Philosophers accepted this as part of the price of having Hilbert refuted, and did not realize that it cut them out of the game²⁷.

The outcome was that mathematics emerged from the transition disconnected from philosophy, and apparently much better off without it.

To paraphrase an old joke, philosophers declared the operation a failure but the patient emerged much healthier. Some philosophers have spent the last eighty years or so trying to convince the patient he is still sick (e.g. with strange and scary variants on logic or set theory), only to be ignored or met with derision. Others still go through the motions of describing mathematics but as far as I can tell very few even feel the need to try to penetrate the technical thickets of modern practice far enough to see what it is really about. Or even to see that it changed in the last century. They seem unaware that the patient left and isn’t coming back.

9.3.2. *It Does Matter.* The separation of mathematics and philosophy has unfortunate aspects. We don’t need someone to tell us what we *should* do, but we could use help in understanding and publicizing what we *are* doing.

There were vigorous debates during the transition and—unsurprisingly—the philosophical arguments were far better articulated. In particular the charges that a separation from physical reality deprived mathematics of its meaning, and that reining in the use of intuition reduced it to sterile symbol manipulation, were quite powerful. These arguments remain unrefuted on a conceptual level and the real nature of contemporary mathematics remains obscure. This causes problems, and will continue to so until an articulate and convincing account is given. The educational community, for instance, still finds the arguments against contemporary mathematics compelling, and still follows the nineteenth-century model, see §8.

Mathematicians have little objective awareness of how they work, for good reason: it interferes with the work itself (see §7). Some still subscribe to one of the old philosophical descriptions even though these are almost unrelated to how they actually work, see e.g. §5.2.3. Nearly all emphasize intuition and physical connections in non-technical descriptions of their work, and this perpetuates the confusion (see §10.3). In short it seems unlikely that mathematicians themselves will sort this out.

What we need is a “science of mathematics” community that objectively studies the field, as it really is, and explains it both to us and to the world. David Corfield makes a related observation in [12]:

... debates about the proper ordering of the goods of mathematics should be organised, not just allowed to happen in unsatisfying ways, as outbursts and releases of tension. The Jaffe-Quinn debate was the most public of these outbursts in recent years (Jaffe & Quinn 1983). There was no role for a philosopher in this debate, which, I like to think, was the poorer for it.

As one of the principals in this debate I would go further: this should have been a job for people studying mathematics from the outside, not for mathematicians.

²⁷It seems clear that philosophers (and Gödel himself, see [24], §7.3.8) intended this technical interpretation of ‘true’ to apply only in a limited context, and on higher levels intended ‘truth’ to remain connected to meaning. However, as one quickly discovers when learning proofs, intentions have no force in mathematics. Once a technical interpretation was on the table the clarity and power it offered were irresistible.

Unfortunately, contemporary philosophers do not qualify because they have no understanding of contemporary mathematical issues, and their attempts to contribute just displayed the inadequacy of their understanding; see §10.5, 10.4. In fact philosophers would have to abandon almost all of their rich heritage as irrelevant, and come to grips with modern technical practice, before progress would be possible. Help from that quarter is unlikely.

9.4. Later Developments. The internal discord that marked the transformation mostly ended about 1930, but later events effected the structure of the community just as profoundly and must be understood to fully understand the current situation. In this section I offer a very broad outline. Significant details are missing and the picture may change as these are filled in.

9.4.1. *The Second World War.* The German mathematical community was an early casualty of the war. Göttingen, the cradle of modern methodology, was gutted even before shots were fired. In the rest of continental Europe mathematics essentially came to a halt when the war started. Many of the most talented left, often for the United States. The story is well-known and does not need to be repeated here.

Long-term consequences included:

- The United States became the primary center for mathematics for the rest of the century.
- The mathematical community was homogenized and became genuinely international (mostly; see below). The days of parallel English, French, and German development with national groups either ignorant of or deliberately ignoring the others were over (mostly), and norms reflecting the new standards of precision became universal (mostly).
- English became the standard language for mathematical communication, and eventually the language of record for publication. At first this was a consequence of war damage, but the advantages proved to be so great that the practice persisted even after other national communities were reconstituted.
- Applied mathematics became a well-developed subject of major importance.

The French community retained enough coherence to attempt linguistic independence, and enough idiosyncrasy to develop the Bourbaki movement, but even they were largely internationalized by the end of the century.

After the war ambitious and talented students from many countries came to the U.S., and later Europe, for training. Most of them stayed. Japan seems to be one of the few that had enough return that they could set standards and direction, and in the second half of the century Japan developed a substantial mathematical presence well-connected to the international English-based community. It seems to be an important question for historians to explain this. Did changes during the U.S. occupation made returning to Japan more attractive?

9.4.2. *The Soviet Union.* The only major mathematical community that did not participate in the postwar homogenization was in the Soviet Union. This community was largely isolated from developments in Europe in the early twentieth century as well, so was not fully converted to the new methodologies.

In the description of §9.2 they had gone through Stage One (precise definitions) but not Stage Two (fully rigorous proofs). Great emphasis was put on intuition and “big ideas”, and intuitive leaps were accepted in published arguments. One result was an uncomfortably unreliable literature that the international community protected itself from by largely ignoring much of it.

[[this section to be expanded]]

The existence of a large and powerful but untransformed mathematical community concerned with core subjects was a potential threat to the stability of the modern international community. This was unexpectedly resolved in the early 1990s by the dissolution of the Soviet Union and the consequent collapse and dispersal of the community. There are residual groups large enough to maintain some of the customs (Moscow, Saint Petersburg, and perhaps IHES in Bures-sur-Yvette, France), but they are reduced enough that their students should eventually be assimilated into the international community.

9.4.3. *Summary.* In this work I have emphasized that real success with contemporary mathematical methods depends heavily on a reliable literature and high and reasonably uniform standards of rigor. In §9.4.1 above I suggested that homogenization and internationalization after the second world war accelerated the spread and acceptance of these standards in the major communities outside the Soviet Union. In §9.4.2 above I suggested that problems with the Soviet community were defused by its collapse following the dissolution of the Soviet Union.

Putting these points together suggests that the spectacular growth of mathematics in the second half of the twentieth century, as well as its current state of health, owe much to political events that we all hope will not be repeated. The discipline will face other challenges and it would be better to do so consciously and thoughtfully than rely on cataclysms to fix them for us.

One such challenge is the relationship with applied mathematics discussed in §??. It seems to me that core mathematics has already been overshadowed by mathematical science; will it be overwhelmed culturally as well?

Another challenge is the development of large and relatively isolated national communities, in China in particular. It is almost inevitable that the Chinese-language literature will be problematic enough that the international community will have to ignore most of it, much like the twentieth-century Russian-language literature. However the Chinese are publishing a great deal more in English than the Russians did, and in my area of geometric topology there have already been problems, [50]. If they are unable to maintain good standards then the international community may, as a matter of self-preservation, discount their English-language literature as well. India is also large enough to be problematic, though probably in different ways.

9.5. **Gray’s History.** Gray [24] describes the 1890–1930 transition in great detail, including the issues at stake; arguments as they developed over time; the convictions and contributions of dozens of participants; social context; and many other factors. He demonstrates convincingly that something happened, but does not have a good explanation *why* it happened.

This essay provides an explanatory framework for Gray’s observations. Roughly speaking the transition was a Darwinian struggle between two ways of working: one depending more on philosophy and intuition, and the other on technical definitions

and rigorous formal argument. The latter provides substantially more power in complicated or delicate situations, so increased the “fitness” of its users. Natural selection determined the outcome. This explanation is implicit in the sketch in 9.2.3 that describes the transformation as the third stage of a larger development.

This section describes how this essay and Gray’s work fit together, and will be mainly useful to those who actually read [24].

9.5.1. *Influences.* Gray describes a number of factors that influenced the transformation but rejects them as causes. They were still important influences, however, and including them enriches the picture and deepens understanding.

Gray’s observed influences include:

- The old sociological standby, a power struggle between groups. Certainly there were elements of this, and at the end there were winners and losers, but it doesn’t explain change of this magnitude in something as well-developed as mathematics. How could Hilbert overcome Poincaré, Brouwer, Klein, and other giants of the late nineteenth and early twentieth centuries, particularly if Hilbert’s explicitly articulated program “collapsed”?
- The mysterious force that also drove the modernist movements sweeping through art, music and literature at roughly the same time. There are structural similarities that aid analysis: his somewhat unfocused working definition of “modernist” ([24], §1.1.1) reflects this, and his term “modernist transformation” comes from this. However he rejects it as an explanation, and eventually seems dissatisfied with the definition ([24], §7.4).
- The political turmoil of the time. Gray credits Herbert Mehlertens [49] with the first focused discussion of the transition. Mehlertens was mainly concerned with developments in Germany, where in fact most of the action took place. Unfortunately that picture is clouded by the political destruction of the German mathematical community shortly after the transition. This leads Mehlertens to suggest a connection between politics and the technicalities of mathematics, but Gray does not find support for this in the larger picture.
- The growing importance of applications, and the professionalization of the community. Again these play roles but do not explain the changes. Why, for instance, would interest in applications favor formal argument over intuition? A professional community would stabilize the results of useful change, but why were these useful?

9.5.2. *Anxiety About Error.* Gray makes a number of observations for which he has no explanation but that make sense in this framework. I give one example; others await those who read the book.

Gray observes in [24] §4.8 (“Anxiety”):

What has not been sufficiently discussed by historians of mathematics is the note, hesitant at first but growing to a crescendo around 1900–1914, of anxiety. The mathematics of the nineteenth century is marked by a growing appreciation of error. For although mathematicians—with some notable exceptions—have traditionally had a low tolerance of errors, during the nineteenth century the awareness of errors grew and became a source of anxiety. [...]

Indeed, once the safe havens of traditional mathematical assumptions were found to be inadequate, mathematicians began a journey that was not to end in security, but in exhaustion, and a new prudence about what mathematics is and can provide.

The perspective of §2 and particularly §2.2.8 (Mathematics vs. Science) sharpens this: error is part of science, and nineteenth century mathematics was a science. The position taken by Poincaré, Klein and others can be paraphrased as: it is the role of intuition and the connection to physical reality that makes mathematics a science, and by extension meaningful. This brings with it exposure to error, but that is simply a cost of being a science and meaningful.

The real oddity is not that there were errors but that some mathematicians began to believe they could be avoided. They saw that precise definitions and rigorous proofs reduced the error rate, and more precision meant fewer errors. Increased anxiety about the remaining errors was a consequence of the growing possibility of, and need for, error-free conclusions. In the terminology of §2.1, the possibility of complete reliability. In other words, anxiety was a symptom of the evolution of mathematics from a philosophically-influenced science to something completely different. Something with a strange and physically unreal domain, but with unprecedented power within that domain.

The objections of Poincaré and Klein to the new methodology were essentially that they amounted to the end of mathematics as a science. They were right, but profoundly wrong about the consequences.

9.5.3. *Conclusion.* Gray's account weakens, as historical accounts tend to, when it turns from description to analysis. In §7 he provides clear and insightful descriptions of some of the unclear and inconclusive philosophical discussion that followed the transformation. He does not see much support for any of these themes in his own work, but rather than conclude—as I do—that none of them had much relation to actual practice, he is respectfully neutral and the situation remains murky.

In §7.4, “Did Modernism ‘Win’?”, Gray tries to identify “modernists” in the post-transition period, with very mixed results. The working definition of §1.1.1 served well enough as a vague organizing principle but it is not up to this challenge. In Britain for instance, it singles out the modernizers rather than those who were modernized. He recognizes the insufficiency and tries to use the philosophical discussion to refine the definition. He does not, however, achieve a clear formulation, and does not show that the refinement is more effective at meeting the various challenges.

Gray's final section (§7.5 “The Work is Done”) concerns the philosophical summary more than the rich historical account, so is essentially inconclusive. The Yeats poem, from which the title of the book is drawn, ends with:

“The work is done”, grown old he thought,
 “According to my boyish plan;
 Let the fools rage, I swerved in naught,
 Something to perfection brought”;
But louder sang that ghost, [Plato] “What then?”

The very title of the book (“Plato's Ghost”) suggests dissatisfaction with the outcome.

A clear central principle strongly related to the historical story would have permitted the triumphant finale I feel the work deserves. It seems to me that the “quest for complete reliability” theme developed here provides such a principle.

10. DESCRIPTIONS: OTHER ACCOUNTS OF MATHEMATICS

In this section I briefly discuss a few other accounts of mathematics. One objective is to try to understand why such accounts are so diverse and mostly—it seems to me—irrelevant when they all ostensibly concern the same thing. The mainstream philosophy of mathematics literature seems particularly irrelevant, and the reasons shallow and uninteresting, so only two are considered here. Essays by people with significant mathematical background often have useful insights, and when they seem off-base to me the reasons are revealing. The essay by Mazur is not off-base.

10.1. Barry Mazur, ‘When is One Thing Equal to Some Other Thing?’

This essay begins:

One can’t do mathematics for more than ten minutes without grappling, in some way or other, with the slippery notion of *equality*.

This leads to a beautiful riff on equality, equivalence, isomorphism, canonical isomorphism, and the ways mathematicians have found to organize it all. The language is casual, clear, and—thankfully—non-philosophical. There are quotations from Plato and Aristotle to suggest that some of the ideas have roots in their work. This is a bit like finding significance in horoscopes, but is too charming to be objectionable.

The heart of the article is a clear description of how category theory explains why so much of “higher mathematics” is robustly independent of the troublesome details of set theory. Category theory requires an underlying set theory, but it is sensitive only to certain basic features. Apparently almost any set theory will do, a situation Mazur describes as “bring-your-own”. The basic reason is that most mathematics depends on some form of equivalence rather than actual equality; categories provide a systematic framework for such equivalences; and equivalence is less sensitive than literal equality to set-theory issues.

It is important to remember that “insensitive” is not the same as “unimportant”. For instance construction of functors (Kan extensions, limits, etc.) requires careful attention to actual identity even though the end result may only be defined up to equivalence. This is still a simplification made to keep the picture clear. Here I want to re-introduce some complication to suggest a different moral: not “set theory doesn’t matter”, but “set theory does not really come into play until one goes deeply into certain subjects”.

10.1.1. *The Axiom of Choice*. The first example is the axiom of choice. There are models of set theory in which it fails in general, and category theory does not provide a substitute. There are also many mathematical constructions that depend on it. Genuinely set-theory independent statements of these results read something like “For all spaces small enough that the axiom of choice applies, ...”, rather than “For all spaces, ...”. This quickly becomes very annoying. There are also no known benefits, so in standard practice ‘choice’ is considered part of basic set theory and used without comment.

10.1.2. *Deep Structure of Spectra.* This example relates to a point made in §2.3.1 (Short-Term Success of Erroneous Formulations): it is harmless to believe or ignore something false if you never use it in a way that depends on its falsity. This is used in §2.3.1 to explain why in history we often see things going well for a while but then run into problems.

The point for the present discussion is that set-theoretic details probably seem irrelevant because we rarely do things bold enough to depend on them. I'll describe a question I've wondered about for nearly forty years, related to Mazur's discussion of representable functors. It is rather long and technical (we have to go through four or five levels of generality before set theory matters), and not particularly relevant to the rest of the essay so can be skipped without loss.

The question concerns applications of "the set of all things", illustrated here with vector bundles. There is a classifying space B_O and a bundle $E_O \rightarrow B_O$ over it that is universal in the following sense: given *any* vector bundle over *any* space, say $V \rightarrow X$, there is a map $X \rightarrow B_O$ and a bundle isomorphism of V with the pullback of the universal bundle. Moreover this map and isomorphism are well-defined up to equivalence involving homotopy of maps and bundle isomorphisms.

In many ways this is very effective. It gives an equivalence between isomorphism classes of vector bundles over X , usually denoted $K_O(X)$, and homotopy classes of maps $[X, B_O]$. Further, the well-defined aspect of the universal property specifies what the equivalence relations on bundles and maps should be to make this true. In the terminology of Mazur §7.5, $K_O(X)$ is a representable functor of X , and B_O is a representing object.

We now get more ambitious. Given vector bundles $V \rightarrow X$ and $W \rightarrow Y$ we can form the (external) direct sum to get a bundle $V \oplus W \rightarrow X \times Y$. Apply this to two copies of the universal bundle to get $E_O \oplus E_O \rightarrow B_O \times B_O$. Applying the universal property to this bundle gives a map $B_O \times B_O \rightarrow B_O$. We would like to think of this as a group structure on B_O , and in fact an abelian group structure. The reason is that direct sum gives $K_O(X)$ an abelian group structure; a group structure on B_O would give the set of homotopy classes an abelian group structure; and the representing isomorphism $K_O(X) \simeq [X, B_O]$ would then be an isomorphism of abelian groups.

The problem is that the universal property only defines a map $B_O \times B_O \rightarrow B_O$ up to homotopy. There is no chance that it will satisfy the exact identities required for a group structure. At this level this is not a big problem: the group identities are satisfied *up to homotopy*, and this is enough to give a genuine group structure on the set of homotopy classes $[X, B_O]$.

The problem grows at the next level of ambition. We want B_O to be an infinite loop space, not just a group-up-to-homotopy. For this we need something like exact group identities, not just up to homotopy.

To get exact identities we need a construction that gives specific classifying maps, not just up to equivalence. Choose a big set U and consider the vector bundles $V \rightarrow X$ with $V \subset U$. Each fiber over X is a vector space, and a subspace of U . We can get a specific model for B_O by taking "the space of all vector spaces that are subsets of U ". Then, to $V \subset U$ we get a specific map $X \rightarrow B_O$ by taking a point x to the fiber of V lying over x . Moreover there is a universal bundle $E_O \rightarrow B_O$ whose fiber over a point $[W] \in B_O$ is W itself, and the pullback of this

over classifying map $X \rightarrow B_O$ gives the original bundle V *identically*, not just up to isomorphism.

We would like for the identity map of B_O to be the classifying map of the universal bundle. This doesn't work as it stands because the total space is not a subset of U . We can get around this by relaxing $V \subset U$ to: there is a map $V \rightarrow U$ that is an injection on each fiber.

Note that we have already had to avoid a set-theoretic problem by restricting to subsets of a set U , and this has forced minor modification of the definition, but we do now have specific, well-defined classifying maps. Moreover we are dealing with actual equality of bundles, without the protection of a categorical-equivalence cushion. We are outside the safety zone described by Mazur.

The next step is to form the direct sum of the universal bundle with itself, $E_O \oplus E_O$, and consider the classifying map of that. This sum is a structure on $E_O \times E_O$ so it comes with a map $U \times U$, not a map to U , and does not immediately have a classifying map. We might try to fix this by assuming that U is an abelian group and using the composite $E_O \times E_O \rightarrow U \times U \rightarrow U$. But this won't always be injective: over the point (x, x) , for instance, we have the fiber $W \times W$ and the product will divide out by the involution that interchanges the factors.

Another approach to the sum is to suppose U is an infinite-dimensional vector space and use "the set of all (finite dimensional) vector subspaces of U " as our model for B_O . We could then define a sum operation that stays in U by taking $V + W$ to be the linear span of V and W . Again there is a disjointness problem: if these spaces have positive-dimensional intersection then the sum is not a direct sum, with the consequence that adding two bundles may not give a bundle. This is not just a technical detail. "Linear span" is an associative and commutative operation and ignoring disjointness problems seems to give a topological abelian monoid structure on B_O . However it is a theorem of John Moore that a topological abelian monoid has trivial k -invariants. This is false for B_O so being too casual with identity issues would actually lead to false numerical calculations.

I was trying to do constructions like this for my thesis and at this point gave up and used other approaches. There are elaborate homotopy structures that do this particular job (with B_O) with difficulty but without set-theoretic problems.

There are ambitions beyond this, however, that are yet harder. We can take tensor products of vector bundles as well as direct sums, and this should give B_O something like a commutative ring structure. The advantage a commutative ring has over homotopy versions is that one could define tensor products of modules. In the early 1970s I tried to use the approach above and set-theory tricks to construct some useful approximation to such a structure. Again I failed, but the attempt motivated Peter May's development of E_∞ ring spectra, [47] (see the last paragraph on p. 3). This was satisfactory for some purposes but not others, and extremely complex.

Twenty-five years later *two* useful approximations to commutative ring structures were finally obtained, [18] and [29], via commutative smash products for spectra. These are effective but again technically complex.

We finally get to the question: can the set-theoretic approach be used to get simpler and easier to use constructions of some sort of commutative ring spectra? If so it would probably involve more-sophisticated handling of actual-identity issues, but in standard set theory. I am sure there are deeper and more-ambitious levels

beyond this, and it seems possible that a set theory with “designer” features will eventually be useful.

The purpose of this digression was to give a specific example of a significant mathematical area that genuinely involves identity and set–theory issues. Set theory may matter to higher mathematics when goals are sufficiently ambitious.

10.2. Jonathan Borwein, ‘Implications of Experimental Mathematics for the Philosophy of Mathematics’. [[incomplete]]

10.3. Keith Devlin, ‘What Will Count as Mathematics in 2100?’ Keith Devlin is a masterful popularizer of mathematics: I particularly enjoyed his *Mathematics: The New Golden Age*. [14]. He has a sweeping view of mathematical history in terms of topics and conclusions, but seems to be relatively insensitive to methodology and sociological issues. I’ll describe the problem and suggest an explanation.

In [15] he describes the “Göttingen revolution” in the mid nineteenth century as the last major change in mathematics. The shift is

[from] performing a calculation or computing an answer [to] formulating and understanding abstract concepts and relationships. This represented a shift in emphasis from *doing* to *understanding*. [...] Mathematical objects, which had been thought of as given primarily by formulas, came to be viewed rather as carriers of conceptual properties.

In §9.2.1 (stage one of the transformation) I identify this proliferation of conceptual material as a consequence of the development of precise, self–contained definitions. Devlin missed the role of the enabling technology.

Furthermore it is not true that the Göttingen group lost interest in computation. Rather, they discovered that increasingly subtle and difficult computations depended on increasingly subtle use of abstract structure. The ancient practice of imprecise definition and dependence on physical–world intuition was not successful in dealing with these abstractions, but one could be successful if these objects are carefully defined and studied more-or-less on their own terms before attempting concrete applications. Finally, it was exactly studying them on their own terms (rather than reflections of physical reality) that made them “carriers of conceptual properties”.

Next, in describing this as the *last* major shift he misses the significance of the 1890–1930 transformation. He certainly knows about the sociological turmoil in the community at the time, and recognizes the later emphasis on precision and distancing from physical reality. However he sees this as a “passing fad” (see below) and feels that nothing really essential has changed. I see profound methodological changes, why does he not?

Later in [15] he extrapolates into the future:

Whereas the Göttingen revolution changed the *nature* of mathematics, but left it *looking* on the surface much as it always had, I believe the next revolution will leave the fundamental *nature* of mathematics unchanged but will lead to something that *looks* very different on the surface.

The change he expects is a much greater incorporation of conceptual and heuristic material directly in mathematics. He has learned much of what he knows through

heuristic explanations that hide a great deal of technical detail. He knows details are missing, but sees them as technical intermediaries for the concepts rather than as the actual mathematics. Eventually, he predicts, we will be able to dispense with the intermediaries and work more directly with concepts and heuristics, and thereby greatly increase the scope of mathematics. He provides a good number of interesting opportunities for such a development.

On the surface his vision is completely at odds with the picture presented here. It is a reasonably good description of actual practice in the nineteenth century, but this was already unsatisfactory for twentieth-century mathematics, and it certainly won't work in the twenty-first. On the other hand the vision is quite similar to my description of "mathematical science" in §3. In other words a complement to, rather than a replacement for, contemporary core mathematics. Moreover his vision may be a good prediction for *explanations* of even core mathematics in the twenty-first century. I am sure it will seem increasingly magical as we have to gloss over increasingly complex details and downplay the contortions needed to connect the domain of mathematics to the physical world.

Finally:

Contemporary mathematics may have declared its goal to be the formulation of precise definitions and axioms and the subsequent deduction of theorems, but that is a fairly recent phenomenon and likely just a passing fad. It is also, I suggest, a foolish one that serves no one particularly well. In the last hundred years or so, mathematics has parted company with (and even distanced itself from) theoretical physics, statistics, and computer science, and even split internally into "pure" and "applied" mathematics only to find that some of the most exciting and productive developments within core mathematics (i.e., those parts that have not been cast out) have come from other disciplines.

We both see the consequences of the transformation, but where he sees a foolish passing fad I see a difficult adaptation to deep and unexpected features of mathematics itself. He finds problematic the fact that mathematics no longer officially includes the sources of some of its inspiration. I find the same fact exciting: the domain in which mathematics is powerful is very limited, and it is wonderful that it can be enriched by clues from other areas. Again, why the difference?

I believe our different viewpoints come from our different backgrounds. I spent thirty years dealing—successfully—with complex and subtle technical details before it even occurred to me to think about mathematics as a discipline. Devlin's technical background is in logic and he came to most of the rest of mathematics essentially from the outside, as a sophisticated connoisseur and popularizer of mathematics rather than as a mathematician. He has learned what he knows—a great deal indeed—through high-level conversation that emphasized concepts and heuristics. Twentieth-century high-level conversations, concepts and heuristics are much the same as those of the nineteenth century and before. The difference is that in the twentieth century these are ways to *explain* mathematics and make it accessible by hiding details, while in the nineteenth they came close to actually *being* the mathematics. The difference is in the technical work needed to get things done: impossible to miss when you are slogging through details, nearly invisible in high-level explanations.

10.4. **David Corfield, ‘Towards a Philosophy of Real Mathematics’.** David Corfield is one of the very few philosophers who has made a serious attempt to understand modern mathematical practice, and his book [11] has useful insights and addresses real issues. However, because he is ahead of the field his ideas have not been digested and refined by his community; there are missteps due to lack of professional mathematical experience and perspective; and he has relied too uncritically on the views of participants, a practice well-known to Historians to be fraught with danger. In fact I found only a few things I felt were exactly right, and more that a few that seemed wrong. It still seems to me to be an important step in the right direction and worthy of analysis, if only to reveal pitfalls to be avoided in the next such attempt.

10.4.1. *Analogy, Axioms and Belief.* Mathematical practice is a compromise between human abilities and the demands of the subject, and confusion about what is human and what is mathematics is a perennial source of error. Corfield does better with this than most but still makes missteps.

Analogy, for instance, is a human cognitive device. Sometimes it enables us to see similarities between different areas long before we see the structural reasons for these similarities, and indeed this is often how we are led to deep structure. Nonetheless analogy is neither a feature of mathematics nor a mathematical method. In Chapter 4, Corfield confuses unexpected connections between fields (something about mathematics) with our *noticing* unexpected connections (human cognition, often using analogy) and describes these as “a riddle begging philosophical treatment” (p. 81).

At the end of the chapter (p. 98) he writes:

... we have seen mathematicians operating with ‘bite-size’ chunks of mathematical concepts... and it seems to suit them because they need only employ, in a recursive fashion, a manageable number of them.

This is a very good observation about human cognition (c.f. §??) with deep ramifications for mathematical practice. However Corfield uses it only to describe the gulf between human practice and automated (computer) proving.

In his discussion of Lakatos’s philosophy of mathematics, Corfield criticizes the idea that axiomatization has stifled development (p. 152):

I shall suggest instead that the appropriate use of rigorous definition and axiomatization has not acted as a hobble on the creativity of mathematicians, but rather an invaluable tool in the forging of new mathematical theories and the extension of old ones.

The success of twentieth-century mathematics should make this a no-brainer, but in context it is actually a big step. Essentially no philosophers understand it, and many educators see axioms as evil. However Corfield does not take it further, to understand *why* axiomatization has not acted as a hobble. It seems to me that Lakatos is a hobble (see §10.5 for a brief discussion) and though he has reservations, Corfield has not completely broken free.

Chapter five concerns Bayesianism (subjective probabilistic methods, i.e. belief) in mathematics. He correctly cites Peano arithmetic as something believed but not proved, but does not realize that mathematicians worked quite hard to eliminate such things, with the result that this (in the guise of consistency of set theory)

is essentially the *only* such example, see §?? and ??. He discusses the role of belief in the development and evaluation of new material but these are human aspects of the process, not part of the end product. He has good company in seeing Bayesian methods as part of mathematics (see §10.3 on Devlin), but not good reasons. His discussion in Chapter 6, where belief concerns modeling more than the mathematical analysis of models, is much more useful.

10.4.2. *Conceptualization.* In Chapter 9 Corfield discusses apparent cultural resistance to conceptualization in mathematics. However, as mathematicians know and philosophers endlessly point out, there are infinitely many possible mathematical ideas and most of them are worthless. There *should* be cultural resistance to unimportant ideas. A claim of *unwarranted* resistance needs evidence that the concept deserves more attention, and not just complaints from partisans. Another problem is that ‘conceptual’ is used in several different senses and careful interpretation is necessary. I discuss two of Corfield’s points from these perspectives.

On page 206 he quotes Gromov as attributing a lack of appreciation for his book on the h -principle in partial differential equations to resistance to conceptual work. I suspect Gromov is using ‘conceptual’ here as ‘lacking technical detail’: he is a product of the Russian school that puts more emphasis on ideas than precision, see §9.4.2, and the h -principle book (developed from his thesis) reflects this. Gromov’s complaint may be that the concept/precision ratio is considered too high for direct modern use rather than that the ideas themselves have been neglected.

I turn to the concepts involved. Gromov generously shares his ideas and a community has developed devoted to exploring and extending them. The ‘conceptual’ nature of the book should therefore not be much of a barrier if the ideas warrant attention. However this is doubtful:

- The basic method was already reasonably well understood and had been used in greater generality. It applies to smooth and PL structures on topological manifolds, for instance.
- Most of the main applications for the differential version had already been developed, and new applications were not compelling enough to justify the somewhat unwieldy systematic treatment.
- The information obtained is “flabby” and tends to clarify where the real problems are rather than solve them.

To illustrate the last point, Gromov’s theorem shows that symplectic structures on non-compact manifolds are classified (in a weak sense) by homotopy information. Symplectic structures on compact manifolds are much richer and more rigid, or to put it another way, much of the richness of compact symplectic manifolds unravels if one removes a single point. Gromov has contributed powerfully to this area, and said of his earlier homotopy-oriented work “I did not understand symplectic topology so I was trying to destroy it”.

A similar argument shows that smooth structures on noncompact topological manifolds are homotopic structures, but doesn’t give any information about the classifying space. The much more sophisticated work of Kirby and Siebenmann in dimensions ≥ 5 both evaluated the homotopy groups and showed the classification theorem extends to compact manifolds. I extended the calculation to dimension 4, [59], and this together with the topological h -principle showed that every noncompact (connected) 4-manifold has a smooth structure. But this is flabby in the same

way as the symplectic result: Donaldson showed that most compact 4-manifolds are not smoothable and the theory is very far from being homotopic in nature. Again removing a single point causes the subtle complications to unravel; see [19] for more discussion, and a proof that avoids the heavy h -principle machinery.

Finally, ‘conceptual’ here does not mean ‘non-axiomatic’. The basic approach is that a derivative map is defined from a space of geometric structures to a corresponding space of tangent structures. The tangent structures are essentially homotopic in nature so are characterized by a few basic properties (axioms). When one can show that the space of geometric structures also satisfies the axioms then it follows that the derivative map must be an equivalence. Gromov’s h -principle gives a criterion for the axioms to hold. The use of axioms here is essential, and this particular use is so well established that it is now considered conceptual.

The next point is Corfield’s use of groupoids as an example of cultural resistance to new concepts (§9.2, page 208). The implicit claim, again, is a value judgement: ‘groupoids’ are so important that their lack of recognition must be due to *unwarranted* cultural resistance.

An idea has to prove itself to be recognized as important, either by doing an important job particularly well, or through a rich and useful structure theory, or both. Two examples:

- Groups have a beautiful structure theory and do many jobs well. It took a long time, though, for these to emerge, and even quite a while to discover the exact formulation (definition) of the concept that has these virtues, [87].
- Categories have a great deal of structure (adjoint functors etc.) and do many organizational jobs very well. Again it took a long time for this to emerge and be accepted. When I was a student categories were still generally seen as linguistic tool for organizing complicated naturality properties. Now some people promote them as a universal setting for mathematics (see §10.1 and the next section).

Groupoids are one of the many things that fit between groups and categories. They would have to do something quite unique and special to qualify as an important distinct concept, rather than a variation on these powerful neighbors; see §5.3.5 for a discussion. Corfield found some enthusiastic partisans but I did not see evidence that the idea is undervalued. I’ll also give an example from my own work where groupoids seemed promising but were inadequate.

Many subtle invariants in geometric topology follow this pattern: start with a space X ; choose a basepoint x_0 and form the group ring of the fundamental group at this basepoint, $Z[\pi_1(X, x_0)]$. Then apply the algebraic K -theory functor to this ring. The choice of basepoint (to get a group) is artificial and interferes with naturality properties. For example suppose we have a map of spaces $Y \rightarrow Z$ and want to apply the construction coherently or continuously to the family of point inverses; see [58] and [68] for sophisticated applications of this. A coherent choice of basepoint corresponds to a section of the map and these usually don’t exist, so the fundamental-group approach is inadequate. This seems to be an opportunity for groupoids since they incorporate a multiplicity of basepoints.

Perhaps we could use the groupoid obtained by taking all possible basepoints? But then the whole space is included in the data and the groupoid structure is redundant and unnecessary. If the spaces are simplicial then perhaps we could

use the groupoid with objects the vertices. But then we get unpleasantly involved with lax maps of these objects. The problem is that the ‘group’ part of groupoids comes from taking equivalence classes (here homotopy classes of paths) and in these applications this is just as unnatural as choosing a basepoint. My experience is that when groups don’t work neither will groupoids, and I ended up working with the category of paths in the space.

10.4.3. *Higher-Dimensional Algebra.* The final chapter in [11] is concerned with higher-dimensional algebra, by which Corfield means n -categories. One of his goals is to give a glimpse of current research “at the *coal-face*”, something he notes is almost entirely missing from contemporary philosophy. He also cautions that this is “insufficiently philosophically ‘processed’”, and that he is a mathematical neophyte and learned the material mostly from a single source. Nonetheless he speculates that this subject will become the foundation of twenty-first century mathematics.

His point that philosophy desperately needs contact with actual mathematics is certainly right and his attempt to do this is admirable, but the account is problematic in several ways. I’ll discuss some of the problems, and prospects for n -categories.

The greatest problems with this account have to do with limitations of the primary source, John Baez, and others in the immediate area. Baez’s main background is in physics and he has a physicist’s penchant for sweeping extrapolation from his own experience. However he shows little awareness of large areas of mathematics relevant to his concerns and his extrapolations are correspondingly problematic. A related problem is that the n -category development is too internal. These are extremely complicated axiom systems with many “degrees of freedom” in their formulation, and developments like this tend to be sterile unless guided and challenged by applications. There is nothing particularly wrong with any of this, but it is not a good example on which to base a philosophy.

One important area of application for fancy category theory is in homotopy theory, because structures on a category lead to structures on classifying spaces. Output structures include iterated loop structures, operad actions, various kinds of spectrum structures, and symmetric (or “new age”) ring structures. Starting with one of these outputs and carefully investigating what makes it work has led to a number of special categorical structures, and identifying these has in turn led to important naturality results for the output structures. The emerging picture is of elaborate category theory as a toolkit: there is no single answer, and fine-tuning is often necessary in difficult cases. The structures useful in homotopy theory, however, do not seem to fit particularly well in the n -category context described by Corfield.

Another area of possible application is in geometry and topology, and through them, physics. Again, though, we should start with questions rather than answers. The general structure of manifolds and related objects of dimension five and above, and topological four-manifolds [19], have all been deeply and effectively explored with classical techniques. This is not to say everything is known, but there are no easy opportunities. This area is my primary specialty and the only real possibilities I see involve the homotopy theory of classifying spaces as described above.

Topics that resist classical techniques and so may offer opportunities are:

- Finite 2-dimensional complexes (the Andrews-Curtis and Whitehead conjectures);

- 3-dimensional manifolds; and
- Smooth 4-dimensional manifolds.

Special structures such as symplectic manifolds above dimension four also resist classical techniques but the theory that is beginning to take shape doesn't strike me as particularly categorical.

The main geometric motivation, and the one that informs Baez and colleagues, comes from 'topological quantum field theories' on 3-manifolds, with variations such as knots and links. Precursors were the Jones polynomial for knots and the Casson invariant for homology 3-spheres. The theory hit the big time with Witten's heuristic construction [85] of field theories with 'integrals' used in physics. Reshetikhin and Turaev gave a completely different construction with the properties predicted by Witten. It quickly became clear that the input for this and analogous constructions is a category with extra structure, and development of such things was a major industry during the 1990s. It never had major impact, however, and lags behind many other technologies including the Ricci flow, essential surfaces and laminations, exploitation of conformal structures on surfaces, and Heegard–Floer homology.

I spent several years working in this area in the 1990s, c.f. [61], and felt that there were indications that dimension three is not a good place for the method. Its greatest value to philosophy might be as an instance where received wisdom from physics impeded mathematical progress.

In the list above, therefore, 2-complexes and smooth 4-manifolds are the most promising areas for topological field theories, see [61] for an explanation. There are analytic theories on 4-manifolds (Donaldson, Seiberg–Witten, and Osvath–Szabo) with field-like behavior. I spent ten years constructing and doing numerical computations in field theories on 2-complexes, intended as a warm-up for constructions on 4-manifolds. However:

- The analytic 4-dimensional theories do not have a full composition structure, so do not come from categories.
- The known categorical field theories on 2-complexes do not do anything interesting.

I suspect that the categorical approach, and particularly the n -category approach, is not likely to have much success. I'll give two reasons beyond the failure to date.

Topological field theories on 2-manifolds can be characterized in terms of Frobenius algebras. The modular ones (roughly the ones coming from 2-categories) correspond to semisimple Frobenius algebras. Semisimple algebras are 'measure zero' in unrestricted algebras and have much simpler structure. This indicates that requiring higher-order decomposition properties (corresponding to higher categories) enormously constricts the field theories. My impression is that requiring decompositions all the way down to dimension 0 leaves only essentially classical theories.

The point is that there is probably a tradeoff between powerful structure and powerful applications. As yet there are no powerful applications, so constraints of this tradeoff are unclear, but it seems likely that we may need to *reduce* the categorical order rather than increase it.

A heuristic explanation for the potential weakness of categorical field theories is that the composition property is a form of locality. Roughly, if two points are separated by a wall then any connection between two has to pass through the wall. More precisely, the relationship between what happens at the two points can be reconstructed from their individual relationships to what happens on the wall. One

might think this would be seen as a description of ‘classical’ field theories rather than ‘quantum’ ones. In topology this corresponds to a form of excision. Excision is essentially the defining property of homology, so again is a measure of how ‘classical’ a theory is. In any case the power of the analytic 4-dimensional theories seems to be related to the *failure* of the decomposition property, i.e. the extent to which they are genuinely ‘nonclassical’, and consequently non-categorical.

10.4.4. *The Jaffe–Quinn Debate.* In section 7.6, pages 171–173, Corfield briefly discusses an exchange between myself and Arthur Jaffe, and William Thurston. The issue essentially concerns where the line is located between the human and mathematical aspects of the enterprise, and is discussed in more detail in the section on Thurston’s paper §10.6.

Corfield seems not to have fully come to grips with the issue in the book section. A later blog post²⁸ shows more clarity, unfortunately based on a misunderstanding:

... I came to realise what was really at stake in Thurston’s contribution to the debate. In essence, he and a few others were being accused by Jaffe and Quinn of acting very irresponsibly. By tossing out a conjecture and sketching how a proof might go, they were not acting for the good of mathematics. Big names shouldn’t behave like this, as it causes confusion, misleads the young, and discourages people from sorting out the field. Thurston’s response was to agree that responsibility is precisely what is at stake, but he goes on to say that responsibility involves so much more than maintaining standards of rigour. ...

In fact I believe that “tossing out a conjecture and sketching how a proof might go” is a wonderful thing, a great contribution to the field, and not at all irresponsible. My objection is that Thurston insisted on calling his conjecture a theorem, and claimed the full credit associated with a full proof. This made a huge difference to his students and followers who were trying to get jobs and promotions: one could become famous for proving a *conjecture* of Thurston, but filling in details of a *proof* of Thurston would be taken as a routine activity indicating a lack of real talent. It is nice that Thurston and philosophers can afford to think on a higher level, but arrogant and irresponsible for them not to acknowledge that it makes a real difference to less–generously endowed mathematicians.

10.5. **Michael Stöltzner; ‘Theoretical Mathematics: On the Philosophical Significance of the Jaffe–Quinn Debate’.** Stöltzner’s paper [79] concerns [30], a precursor of this article, so I begin with a brief discussion. First, however, I want to apologize for the term ‘Theoretical Mathematics’.

Our point was that misrepresenting conjectural or heuristic material as ‘proved’ is bad practice and damaging in several ways. On the other hand we value conjecture very highly when it is presented as such, and indeed think mathematics needs more of it rather than less. I had the bright idea that a dignified *name* for the activity might allow a more positive and constructive approach: instead of ‘conjecture is *not* mathematics’, we could say ‘conjecture *is* [something else]’. Unfortunately

²⁸ http://golem.ph.utexas.edu/category/2006/10/wittgenstein_and_thurston_on_u.html, Blog post October 11, 2006.

‘theoretical mathematics’ seemed to irritate people who understood the point and confuse those who didn’t²⁹.

10.5.1. *Background.* Jaffe and I were concerned that a century of difficult adaptation to precision might be eroded by a new wave of heuristic work. Witten in particular was selling his wares to the mathematical community, but insisted on playing by physics rules. Further, he seemed to be supported in this by figures like Atiyah, who had been the very model of clarity and precision in his youth but was so enthusiastic about Witten that at one point he attributed Morse theory to him.

In sounding a warning we felt we had to cite specific contemporary examples, to try to make the issues clear and show that this is a real and ongoing problem. The people we mentioned have made profound and powerful contributions to mathematics and we had no intention of denying this. Our point, rather, was that they might have been *even more* influential if incompleteness of some of their work had been acknowledged, and further, it was their power that enabled them to behave this way: lesser talents would have been essentially banished from the community.

The editor (Richard Palais) solicited responses from the persons named, and others likely to have different viewpoints. Most of the responses in [3] were in general agreement, but felt that we undervalued the importance of intuition and conjecture. Thurston [80] disagreed strongly; see §10.6 for a discussion. Outsiders who read the article and the responses got the impression that our stand was controversial. It certainly did strike a nerve among theoretical physicists. Among mathematicians in general however, and somewhat to my surprise, our view was seen as so obviously correct that there was no interest in discussing it.

10.5.2. *Our Article.* Our article was written for mathematicians and depended on shared experience for understanding. The points can now be made more explicit.

In this essay I have emphasized that contemporary methodology is an enabling technology for users, from elementary school to the research frontier. In particular it enables rank-and-file mathematicians to make significant contributions. The cumulative nature of science is often suggested by the phrase “we see further because we stand on the shoulders of giants³⁰”. In mathematics it would be more accurate to say “we see further because we stand on the shoulders of men and women of average height”. The shoulders of giants are higher but frequently not strong enough to stand on. It is true that the ‘little people’ are often working out the ideas of giants, but this is no excuse for overlooking the great value they add.

The point is that mathematical progress is more dependent on proper function of the wider community than may be the case in other sciences. In any science it would be mean-spirited to make exaggerated claims that denigrates the work of students and followers trying to get jobs and promotions. In mathematics it also damages the field.

This difference in mathematics derives ultimately from the validation criterion. Other sciences have external criteria for correctness, with the result that wisdom,

²⁹In the present development, conjectural work falls into what I call ‘mathematical science’ §3. However, having learned my lesson, I am certainly not proposing this as a name for the activity.

³⁰Attributed to Newton. Newton, however, said it of someone else, presumably thinking of himself as the giant.

insight, experience, intuition etc. play important roles in validating ideas. In mathematics, deep intuition etc. may be necessary to figure out what to *try* to prove, but the validation criterion is internal (error-free proof) and quite accessible even to those without profound intuitions. Further, *effective* mathematical intuition derives largely from the structure and details of proof. Speculation is rarely sharp enough to support further speculation, and this is the sense in which the ‘shoulders’ of speculative giants are too weak to stand on.

10.5.3. *Stöltzner’s Article*. Our article does not connect with conventional wisdom in philosophy. The usual approach is to find an interpretation of it that is easily dismissed, dismiss it, and when this is found to be unsatisfying find another such interpretation, dismiss that one, and so on. Stöltzner made a genuine effort to understand the issues from perspectives available to him. In the end he is not successful because the perspectives are inadequate, but it is a worthy effort.

The main problem is that Stöltzner relies on Lakatos’s *Proof and Refutations* [41] as a model for mathematical practice. As a baby model for the discovery process it is not too bad, but for the same reason that mathematics comes out looking like other sciences: this is the human aspect of the endeavor. Mathematicians have the same limited perceptual and cognitive facilities as other scientists. Their strategies for using these facilities to investigate nature are therefore much the same.

Lakatos’ story is a caricature of an antique that does not go past the early nineteenth century. The subject-adapted features of twentieth century practice are completely missing:

- The end result of the argument and refutation dialogue should be a completely precise, technically functional definition of the objects identified, *and* a logically complete proof that the conclusion is guaranteed by properties following from the definition.
- In particular the endpoint is *not* simply a matter of running out of refutations, with the possibility that the issue might be reopened at a later date.
- There is then the vital step of internalizing the definition and the functionality revealed by the proof. This replaces the pathetically limited cases that can be visualized directly and opens up new worlds.
- Finally, ‘island mathematics’ is sterile. It is interconnections, challenges, and applications that keep the internal verification criteria from leading to self-satisfied stasis.

Had Lakatos followed his story further he could have seen polyhedra of arbitrary dimension; cell complexes; the homological reasons an alternating sum works so well; apparently abstract but accessible examples such as projective spaces and Grassmann manifolds; apparently concrete but hard-to-access examples such as zeros of multivariable polynomials; the attempt to count cells in a specific dimension (Betti numbers, circa 1877) rather than mix them all together; the Noetherian revolution that brought clarity and power by replacing numerical invariants of matrices with abstract algebra (groups, rings, modules); the resulting view of Euler characteristics as elements of some sort of representation ring rather than mere numbers; applications of this using Burnside rings and algebraic K -theory (e.g. the Wall finiteness obstruction as an Euler characteristic); etc., etc.

The unfortunate consequence is that Stöltzner is quite wrong when he writes (p. 206):

Jaffe–Quinn and Lakatos share a common starting point: Proof stands for a thought-experiment or quasi-experiment which suggests a decomposition of the original conjecture into subconjectures or lemmas, thus embedding it in a possibly quite distant body of knowledge. (Lakatos, 1976, p. 9)

This now–obsolete meaning for ‘proof’ has many well–known drawbacks, including cultural relativism. And again, it has more to do with the human aspects of research and discovery than the constraints imposed by the subject matter. Stöltzner makes a valiant and interesting attempt to make sense of our article as an effort to deal with these drawbacks. But mathematics itself dealt with these drawbacks by evolving a new meaning for ‘proof’ in the early twentieth century, and our paper concerns new problems connected to this new meaning.

10.6. William Thurston; ‘On proof and progress in mathematics’. Thurston’s paper [80] has been influential in education and philosophy, though not in mathematics itself.

There is no question that Thurston revolutionized three–dimensional topology. Some in the older generation—Armand Borel for instance—were unhappy with his oracle–like certifications that things were true without offering proof. But most of what he claimed was true, and his outlines provided material for many dissertations and some careers. There was a price: his followers were often seen as scribes recording the thoughts of the master, rather than independent researchers, because he had already claimed a proof. Ambitious students had to distance themselves from his program rather than work on it.

The high point was his geometrization conjecture and his claim to have proved it for Haken manifolds. Over the next ten years much of his sketch was filled in, but the non–fibered case got stuck on a key fixed–point theorem on Teichmüller space. There was widespread skepticism among experts that Thurston had fully addressed the problem. R. Kirby put it back on his list of open problems, [33]. In 1989 Curt McMullen [48] proved the necessary fixed–point theorem, but even so complete proofs were not available until the late 1990s, twenty years after Thurston’s announcement, cf. [54].

[[incomplete]]

10.7. Constructivism. Constructivism begins with the observation that mathematics is a human activity and a great deal of it is influenced as much by the human aspect as anything else. This is certainly true, and is an important consideration in any account of mathematics. Constructivism goes further, to assert that the human aspect is dominant, or even the whole story.

It is useful to distinguish two levels of constructivism:

- *Strong*, or explicit constructivism: mathematics is a social construct, and different people or different societies may very well have different versions. No version should be given a privileged role.
- *Weak*, or implicit constructivism: there is indeed something special going on in mathematics. However mathematicians themselves have many different views of it (pure, applied, experimental, etc.), and *these* differences are social constructs. What constitutes mathematics could be decided by a

vote of mathematicians, and in particular contemporary pure mathematics should not have a privileged role.

The strong version is relatively rare. Supporters have to work pretty hard to align facts with their convictions, so this version is discussed in the section on religion.

Implicit constructivism is very common. Many of the accounts described in detail here (Gray, Borwein, Devlin, and Thurston) support defining mathematics by vote, and some push it strongly on the grounds that taking rigorous core mathematics as a model has inhibited development. Devlin writes in [14]:

However, the case I am trying to make in this essay is based not so much on historical precedent, but rather on a highly pragmatic view of the discipline as a human activity carried out by a human community. [...] As I see it, the question is, who gets to say what is and what is not to be called “mathematics”?

I observe in §3 that there is substantially more biomass in non-core mathematics than in the core even if one restricts to people with published work that qualifies for review in Math Reviews. A vote would indeed change the definition, and the change would be even more dramatic if the “human community” is taken to be all those who identify themselves as mathematicians.

The problem is that the subject is defined by the subject, not people. People can decide which methodologies to use but they cannot decide how effective these methodologies are. The choices in mathematics itself are:

- Contemporary core methods, highly disciplined and not particularly people-friendly but very effective; or
- Older methods, more relaxed and comfortable but significantly less well-adapted to the subject.

The conclusion is that the *solution* proposed by constructivists would undo the gains of the last century and severely damage mathematical practice. It seems to me, however, that constructivists are motivated by real problems, and rejecting the proposed solution does not make the problems go away. The motivating problems seem to be:

- There is a significant discontinuity between contemporary core methods and those used up through the nineteenth century; in education through the first few years of college; and in applied and computational areas.
- Many people are uncomfortable with these methods because they are highly disciplined; not human-friendly; and disconnected from physical reality and “meaning”.

These cannot be fixed by a vote, but there are other possibilities:

- People could realize that core mathematicians do this because they have to, not because they want to. Differences could be accepted with tolerance if not sympathy.
- People who dislike the methods could identify themselves as mathematical scientists, §3, rather than as mathematicians. This is a larger and more diverse community and has goals that do not require such unfriendly methods.
- The gap could be reduced by bringing education up to date. A goal here and in related articles ([?], [?], [?]) is to suggest that a more contemporary

approach to elementary mathematics education would not be difficult and might substantially improve outcomes.

It would also help if people who write about mathematics would understand it a bit more carefully. For instance the use of precise definitions and axioms is often described as unnecessary (“a foolish [passing fad] that serves no one particularly well”; Devlin, quoted above) and as making mathematics random and meaningless. But the methodology requires this approach, see §2.2.6, and this is not a deep observation. It is true that the axiomatic approach invites useless exploration, and entire fields (e.g. point-set topology) have drifted into irrelevance for this reason. However, ambitious mathematicians are keenly aware of the danger and have strategies for avoiding it. Further, most major definitions evolved over long periods exactly because early versions were still a bit on the random side and did not quite describe the objects of study. Definitions become standard—and highly prized—exactly because they are not random and not meaningless. Again this is not a deep observation.

10.8. **Religion.** [[unfinished]]

11. SOCIOLOGY: CULTURE IN MATHEMATICS AND PHYSICS

This is an extract from an older essay, [66], that uses a comparison with physics to illustrate the influence of reliability on mathematical culture. It duplicates material elsewhere in the essay, though I hope the different viewpoint will be useful. Finally, it is a travelogue rather than an attempt at scholarship.

By “physics” I will mean theoretical high-energy and quantum physics: areas where sophisticated mathematical apparatus is needed even to organize or interpret real data. By “mathematics” I mean strict areas in which abstract mathematical apparatus is developed. Experimental physics and applied mathematics are excluded to provide a clearer picture.

For most of history physics and mathematics were inseparable. Even after differences in subjects led to divergence there was a great deal of interaction between them. Physicists rely on mathematical work, and many mathematical structures are inspired by physics. The shared use of mathematical apparatus and long history of interaction has led to a great deal of shared language and strong superficial similarities.

Topics include reliability and its cultural consequences §11.1; efficiency as a selection force on norms §11.3; and construction of the literature §11.4.

11.1. **Reliability.** The key difference is reliability. Mathematics routinely achieves essentially complete reliability and this is a defining characteristic, see §2.1. Physical conclusions may be excellent but are—so far—never perfect.

The significance of reliability is illustrated by attitudes toward elaborate logical arguments. Such arguments magnify errors. Mathematicians exploit this in proofs by contradiction: an elaborate argument is built on a doubtful hypothesis in hopes that flaws in the hypothesis will be magnified to become obvious. However an error at the end of a long argument only indicates an error *somewhere* and one can conclude that the initial doubtful hypothesis must be wrong only if there are *no* other sources of error. Further, since the objective is to magnify errors the other ingredients must be genuinely error-free, not just well-established.

Physics does not work this way. Even if nature is “error-free” physical descriptions have limits of applicability and at least minor discrepancies, so error-magnifying methods can be counted on to produce errors. Short insightful arguments tend to be more robust than logical proofs even when they are wrong in detail.

11.1.1. *Cultural Consequences.* Because mathematical methods are sensitive to errors, customs have developed to carefully distinguish between perfect and imperfect information. The conclusion of a mathematical plausibility argument is traditionally called a “conjecture”, while the result of a rigorous argument is called a “theorem.” Theorems can be used without fear in a contradiction argument; conjectures are a possible source of error.

The theorem/conjecture distinction is not useful in physical arguments because the methods cannot take advantage of the difference. Physicists frequently do not understand the significance in mathematics either, partly because it is not part of their own culture but there are other reasons:

- When mathematics works for physicists it becomes transparent precisely because it is reliable; they don’t think much about it because they have more urgent concerns.
- Well-tested mathematical conjectures often work as well as theorems because physical methods are too robust to run afoul of subtle flaws.
- Theorems often come with a lot of fine print that sometimes includes vital restrictions. Physicists, accustomed to robust statements and arguments, tend to ignore fine print and are sometimes misled as a result. For them there is little useful difference between a theorem whose fine print is missing (a conjecture) and one whose fine print is incomprehensible.

These differences lead to culture shock when the areas interact. In physics conclusions from intuition and plausible argument have first-class status. Mathematicians tend to describe these conclusions as “conjectures” still needing proofs, or dismiss them as hopelessly imprecise. Physicists resent this. Conversely physicists tend to be disdainful of mathematical rigor as being excessively compulsive about detail, and mathematicians resent this. But there are good reasons for the values held in both disciplines. The problems come from customs adapted to the subject, not xenophobia or a power contest.

11.1.2. *Ethical Behavior.* The effects of these mutual ill-adaptations are not symmetric. Mathematical practices used in physics are inefficient or irrelevant, but not harmful. Physical practices (no distinction between conjecture and theorem) used in mathematics can cause harm: it jeopardizes standard techniques such as proof by contradiction. There is widespread feeling among mathematicians that violating these ‘truth in advertising’ customs should be considered misconduct³¹.

Presenting heuristic conclusions as finished products is productive in one field and verges on misconduct in another, not because of contingent historical development of “standard practice”, but because the subjects are different.

11.1.3. *The Interface.* The interface between mathematics and physics is populated roughly by three groups: mathematicians, physicists, and some straddlers.

³¹Not all mathematicians agree, see §7.

People working in mathematical physics are typically in mathematics departments and identify themselves as mathematicians. They are usually very careful about following mathematical norms as a matter of survival. Physics can be fast and flashy where mathematics proceeds at a glacial pace. The fact that physics has a tendency to evaporate while mathematics doesn't is highly valued by mathematicians, less so by physicists (see §11.3). Consequently if mathematical physicists carefully follow mathematics norms they can be well-respected mathematicians. If they don't they would be at best second-rate physicists.

From time to time physicists (as opposed to mathematicians inspired by physics) have direct impact in mathematics. This was rare in the mid-twentieth century but in the 1980s and 90s several lines of thought in theoretical quantum physics led to truly inspirational ideas. In fact these ideas were more celebrated in mathematics than in physics: string theory for example was ridiculed by much of the physics community as being fatally out of touch with reality, c.f. Woit [86]. The result was a number of powerful and insightful physicists trying to sell their products to the mathematical community but wanting to play by physics rather than mathematical rules.

The physicists' suggestion that more-relaxed physics rules might be a good idea even in mathematics had support from a number of eminent mathematicians [3]. Some of us were concerned that an erosion of standards might result [30] but in the end our concern was not needed. The idea was rejected without interest or comment by the overwhelming majority of the mathematical community, and students in the area realized that if they wanted jobs they would have to play by mathematical rules.

Finally, straddlers are people who identify themselves as mathematicians when speaking to physicists, and as physicists when speaking to mathematicians, thereby dodging scrutiny by either community. I have met a number of them but don't know how common they are. They seem to have little effect on either community.

11.2. Process versus Outcome. The decisive criterion for correctness in theoretical physics is agreement with experimental observation. This focuses attention on outcomes, not process. This point of view permeates even internal efforts in theoretical development that do not make direct contact with experiment. When a model is developed it is checked against others believed to be relevant: special cases, the "classical limit", etc.

Mathematicians, as explained in §§2.2.7, 2.2.8, are unscientific in their use of an internal criterion for correctness.

There are many ramifications of this difference in focus. For example mathematicians are more tolerant of apparently pointless exploration, as long as it conforms to internal standards of rigor and quality. This tolerance is amply justified by the many times that exploration turned out not to be pointless at all, but the point was beyond human ken.

Physicists tend to be more relaxed about precision and more judgmental about significance, as illustrated by Wolfgang Pauli's dismissive description of a paper as "not even wrong". In physics this attitude is justified: without an internal criterion to keep a project "correct" while waiting for a mission to appear, the chances of anything worthwhile coming out are near zero.

11.3. Efficiency. Customs well-adapted to the subject should maximize return on resource investment. How this works is not always obvious, especially when adaptations are not consciously understood.

11.3.1. *Slow but Sure.* As an illustration we consider the different paces in mathematics and physics. Years often pass between an understanding satisfactory to physicists and a rigorous mathematical demonstration. Is insistence on rigor self-indulgence enabled by being sheltered from the demands of the real world? Or is it more efficient in some way?

Explaining the efficiency of rigor begins with another fundamental aspect of mathematics: since it is (usually) right the first time, it is not discarded. Over time it may become uninteresting or insignificant, but it does not become incorrect. As a result mathematics is an accretive activity.

In physics (and other sciences) material must be checked and refined rather than simply accreting, and customs have evolved to support this. Duplication is tolerated or even encouraged as “replication”. The primary literature has to be sorted and refined; a great deal of material is discarded; and there is a strong secondary literature to record the outcome of the process. These activities use resources. Mathematics lacks many of these mechanisms: the payoff for working slowly and getting it right the first time is savings in the refinement process³². In principle the same payoff is available to physics: if complete reliability were possible then the most efficient approach would be to seek it even at great sacrifice of “local” speed. But this is not possible, and an attempt to import this attitude into physics would be ill-adapted and counterproductive.

This adaptation has produced a vulnerability in mathematics. A group or individual can disregard the customary standards and seem to make rapid progress by working on a more intuitive level. But the output is unreliable. Mathematics largely lacks the mechanisms needed to deal with such material so this causes problems ranging from areas frozen up for decades, to unemployable students, to the outright collapse of entire schools of study (see [30]).

11.3.2. *Moveable Feasts.* As a second illustration of efficiency we consider the “fad” phenomenon in (theoretical) physics. It sometimes happens that an area becomes fashionable. There is a flurry of publication, with a lot of duplication. Then most of the participants drop it and go off to the next hot area.

Physics has been criticized for this short attention span. But this behavior is probably adapted to the subject matter. First, the goal is development of intuition and understanding, and this is an effective group activity. Duplication in publication is like replication of experiments: several intuitions leading to the same conclusion increase the likelihood that the conclusion is correct. And after a period the useful limits of speculation are reached, and it is a better use of resources to move on than to try to squeeze out a bit more.

If all activity ceased after a fad then eventually all of theoretical physics would become unsuitable for further development. Different activities continue: experimentalists test the testable parts. Mathematicians clean up the logical parts. A few physicists remain to distill the material into review and survey articles. And

³²Note this is a long-term payoff for the community, not individuals. This illustrates the point in §?? that selection acts primarily on communities and norms rather than individuals.

after a period of solidification the area is ready for another round of theoretical development.

Mathematics has occasional fads, but for the most part it is a long-term solitary activity. *Mathematical Reviews* and *Zentralblatt* divide mathematics and related areas into roughly 5,000 subtopics. Some of these are outside mathematics but there are still a great many areas for the size of the community and most are sparsely populated.

Mathematicians tend to be less mobile between specialties for many reasons: a greater technical investment is needed for progress; big groups are seldom more efficient; and duplication is unnecessary and usually discouraged. These factors tend to drive mathematicians apart.

In consequence the community lacks the customs evolved in physics to deal with the aftermaths of fads. If mathematicians desert an area no one comes in afterwards to clean up. There is less tradition of review articles: since the material is already right there is less sifting to do, and less compression is possible. Shifts of fashion may be an efficient behavior in physics, but they are not a good model for mathematics.

11.3.3. *Funding.* These considerations suggest ways funding programs might be fine-tuned to mesh with cultural nuances.

- Collaborative activity is often not efficient in mathematics and it is counterproductive to try to force it.
- Physics group activity is often focused at conferences, while mathematical interactions need to be alternated with solitary digestion and exploration. This suggests that mathematical conferences should (on average) be shorter, smaller, and more focused.
- Lack of large-scale cleanup mechanisms makes mathematical areas vulnerable to quality control problems. There are a number of once-hot areas that did not get cleaned up and will be hard to unravel when the developers are not available. Funding agencies might watch for this and sponsor physics-style review and consolidation activity when it happens.

11.4. **Publication.** Papers in pure mathematics and theoretical physics often look similar, treat the same subjects, and often even reside in the same library. However publication customs and uses of the literature are quite different.

11.4.1. *Purpose of Publication.* Physicists tend not to use the published primary literature. They work from current information (preprints, personal contacts), and the secondary literature (review articles, textbooks). The citation half-life of physics papers is short, and there are jokes about “write-only” journals that no one reads. Duplication and rediscovery of previously published material are common. In contrast many mathematicians make extensive use of the literature, and in classical areas it is common to find citations of very old papers.

There are differences in the construction of the literature as well. In mathematics the refereeing process is usually taken seriously. Errors tend to get caught, and detailed comments often lead to helpful revision of the paper. In physics the peer-review process has low credibility. Reviewers are uninterested, and their reports do not carry much weight with either authors or editors. Published papers are almost always identical to the preprint version.

One view of these differences is that the mathematical primary literature is user-oriented: genuinely useful to readers. In physics it is more author-oriented, serving largely to record the accomplishments of writers.

These differences again reflect differences in the subject matter. The theoretical physics primary literature is not reliable enough to make searching it very fruitful. It records the knowledge development process rather than the end result. If material is incorporated into the secondary literature or some shared tradition then it is reasonably accessible, but it is often more efficient to rediscover something than to sift the primary literature. A consequence is that there is not much benefit in careful editing or refereeing. This leads to journals that are, in the words of one mathematician, “like a blackboard that must periodically be erased.” In contrast, the mathematical literature is reliable enough to be a valuable asset to users.

11.4.2. *Social Consequences of Quality Control.* The differences in literatures have led to differences in social structure. As noted above, mathematics has many sparsely populated specialties. More accurately these could be described as larger communities distributed in time and communicating through the literature. This works even though the communication is one-way, because the material is reliable: when puzzled one can find clarification in the paper itself even if the author is long gone.

Less-reliable material requires two-way give and take. As a consequence working groups in physics are more constricted in time and appear larger because they are all visible at once.

This also works the other way: a large working group with a lot of real-time interaction weakens the benefits of reliability, and in fact larger groups in mathematics often do become more casual about quality control. This in turn leads to a curious problem in the mathematical infrastructure. The leadership in the professional societies and top journals tends to come from larger and more active areas. As a result they tend to underestimate the importance of quality control to the community as a whole.

Twenty years ago I was concerned that electronic preprints and publication might undermine quality control mechanisms [62]. The context was that in the previous twenty-five years or so it had become much easier to circulate paper preprints that were considered “off the record” and sometimes very casual about quality. It may be that quite a few people had been depending on referees to enforce discipline in their work, and this was an unwelcome peek behind the scenes. At any rate a flood of electronic preprints with similar low standards could erode the painfully-acquired benefits of a reliable literature.

My concerns were unneeded for an interesting reason related to community norms. Paper preprints are temporary in the sense that they have limited circulation and after a time can be hard to find. Electronic preprints are available everywhere, can last forever, and are impossible to kill if they turn out to be an embarrassment. Functionally they are *not* off the record. The sheer level of exposure led most authors to use much higher standards than for paper preprints and this has, in effect, been incorporated into norms. Formal refereeing is still vital for maintaining quality but it no longer seems to be the first line of defense it once was.

12. SET THEORY: BRIEF DESCRIPTION

In this section I explain why modern set theory is a theory of logical functions, why the “set” interpretation can be misleading, and how logical issues play out in practice.

12.1. Specifying sets with logical functions. Set theory was originally envisioned as a theory of *containers*, with the idea that the behavior of containers is independent of what we put in them. The idea was also that these would be described *explicitly*, essentially by listing the contents. I might, for instance, have a set that contains the number ‘3’, the letter ‘Q’, my dirty socks, my dog, my wife, . . .

The explicit-description idea has serious flaws. First, we don’t have particularly effective methods for constructing and manipulating lists, and in a sense one of the jobs of set theory is to provide such methods. But it can’t do this if the theory itself is based on lists. Second, the very idea of lists is problematic for infinite sets. In actual practice the theory uses *implicit* descriptions via logical functions to avoid these problems.

For a first approximation, a *logical function* of one variable is something that returns either ‘Yes’ or ‘No’ when presented with an input. Comments:

- The traditional outputs are True and False. We use Yes and No because True and False have distracting philosophical and ethical associations. We think of logical functions as being a bit like members of FaceBook who like some things and not others, without intending any Good/Evil significance.
- In this view logical functions are required to be defined for *any* input. We will have to be more sensible later on, but for the moment we think of functions as returning No on questionable inputs. Roughly, functions don’t like things they don’t understand.
- Logical functions correspond to sets by: the elements in f are the a with $f(a) = \text{Yes}$. Roughly, all the things f likes.

For example the set described above corresponds to the logical function that likes the number ‘3’, the letter ‘Q’, my dirty socks, and my wife. There are other advantages: my wife might think it is sort of sweet to be liked by a logical function, but would not feel that way about being put in a container³³.

12.2. Russell’s Paradox. The Bertrand Russell function reveals a problem with the logical-function approach. Define BR by

$$\text{BR}(a) := \begin{cases} \text{Not } a(a) & \text{if } a \text{ is a logical function} \\ \text{No} & \text{otherwise} \end{cases}$$

But evaluating it on itself gives

$$\text{BR}(\text{BR}) = \text{Not}(\text{BR}(\text{BR}))$$

so either Yes = No or No = Yes. We dislike this, if only for its implications for future employment of mathematicians. This is clarified below, but first I discuss the set formulation of the problem.

If b is a logical function then $b(x) = \text{Yes}$ is interpreted as “ x is an element of b ”. In these terms, BR likes sets that are not elements of themselves, and defines “the set of all sets that are not elements of themselves”. The crisis comes when

³³This is a moot point however. Logical functions are supposed to return “No” on things they don’t understand.

we ask “is this set an element of itself?” This formulation obscures the real issue because it sounds like a problem with sets, or with using logical functions to define sets, when it is actually a problem with logical functions. In other words, thinking of sets as containers may be convenient for applications but it is not the clearest approach to set theory itself.

Returning to the theme, believing in logic requires us to conclude that BR must not be a logical function. What goes wrong? $b(b)$ is certainly a logical function of logical functions, so the problem must be with “if b is a logical function”. Specifically we conclude:

12.2.1. *“Is b a logical function?” is not itself a logical function of b .*

We explore variations on this later, but this is enough to explain some of the logical issues in everyday mathematical practice.

12.3. Practical consequences. To stay out of trouble we need to work with logical functions that can be logically recognized. Roughly, we fix a logical function sm with the property that $sm(b) = \text{Yes}$ implies that b is a logical function, and work with the functions that sm recognizes. We want to think of these as sets, but to acknowledge the role of the fixed function we call them *small* sets. More explicitly, a small set is defined by a function c such that $sm(c) = \text{Yes}$, and the elements of the set are the x with $c(x) = \text{Yes}$.

The danger is that we might construct something that sm cannot recognize, and further constructions with these are problematic. In practice we use nice sm that recognize outputs of most constructions when we use *inputs* that it recognizes, see 12.5. Thus, unless we get really ambitious, everything we do stays in the ‘universe’ of small things, and we can ignore logical issues. For the rest of the section we assume sm is nice in this way.

Categories are ambitious constructions that offer ways to get into trouble. I’ll give two examples: the categories of sets, and of models for the natural numbers.

12.3.1. *The category of small sets.* Categories have objects and morphisms. In the set case the objects are small sets, i.e. the ones defined by logical functions approved by sm . Morphisms between two objects are functions from one to the other. Functions are subsets of the cartesian product, and for nice sm the subsets of the product of small sets is itself a small set. Therefore the morphisms between two objects is also a small set. Constructions that involve small sets of sets (unions, intersections, products, etc.) produce small sets and so stay in the context.

The problem comes when someone wants to talk about the *collection* of all objects in the category. This collection is not a set; what is it? Traditionally this has been asked by troublemakers, and traditional answers include “dumb question; don’t ask it”, and “this is a *class*”. The latter is just a name devoid of content, but it satisfies many troublemakers. In fact most of the confusion results from focus on the word “sets”, and the fact that it does not display the dependence on the logical function. If we focus on the function, and ask “this is not a *small* set, so what is it?” then there is a reasonable answer. There is another, more capable, set-recognizing logical function for which it *is* a set. Before discussing this, and what it does or doesn’t do for us, I describe some real-life reasons to ask the question.

So much of mathematics stays in the small-set context that Mazur 10.1, for instance, asserts that this makes mathematics insensitive to the details of set theory. This is not completely true: there are honest reasons to ask such questions, including slick and powerful constructions of classifying spaces. For instance an n -dimensional vector bundle over a space X (small, of course) is a function $E \rightarrow X$ whose point inverses are n -dimensional vector spaces. Taking a point in X gives a vector space (the inverse in E). Does this define a function from X to “all vector spaces”? We want “all vector spaces” to be a topological space, not just a set, so we can interpret continuous variation of the structures on inverses in E as continuity of the function. If this makes sense we also get a universal bundle: over a point V in “all vector spaces” we put V itself. A vector bundle $E \rightarrow X$ is obviously *equal* on the nose to the pullback of this universal bundle by the classifying map. In contrast, the usual constructions of $B_{O(n)}$ are a lot of work, the classifying maps are not obvious, and pullback of the universal bundle only gives $E \rightarrow X$ up to equivalence. Why work so hard if you don’t have to?

The usual complicated constructions of classifying spaces stay in the category of small spaces. Functions and other constructs all make good sense and as a result the subtleties of logic do not intrude. In contrast the object evoked by “the space of all ‘things’” is not a small set, and working with it naively can be dangerous. For instance these big models often have very nice structural properties. Exploiting them vigorously—as mathematicians are wont to do—can even lead to erroneous calculations of homotopy groups [42]. How does mathematics (through the agency of logic) sort this out? First, if we cram these big beautiful constructions down into the small world then vigorous exploitation would be safe. However cramming distorts structures, so the crammed versions can no longer be exploited in the same way. Alternatively we might enlarge the ‘universe’. Recall that our set theory depended on a function sm that recognizes “small” sets. see There is another function md that recognizes “medium-sized” sets (see §12.5), including the collection of all small sets. “The set of all small ‘things’” then makes good sense as a medium-sized set, and vigorous exploitation of nice properties can be safely carried out in the enlarged set theory. The tricky point is that that this is *not* a classifying space for *medium-sized* ‘things’. Errors can result from assuming that it is, and are prevented by being careful with the logic. Occasionally there is a way around this problem, as we will see with the natural numbers, and occasionally good things can come of it. However in practice such explicit care with logic is rarely either necessary or useful, and most working mathematicians avoid it.

12.3.2. *The category of models for the natural numbers.* Following Peano, we define a *model* for the natural numbers to be a set c and a function $S_c: c \rightarrow c$ satisfying:

- (1) S_c is injective;
- (2) S_c is not onto; and
- (3) (c, S_c) is minimal with respect to these properties.

The function S_c is called the “successor function” of the model. “Minimal” means it can’t be made smaller without losing one of the properties. Specifically if $b \subset c$ then either (1) $S_c(b)$ is not a subset of b , so the restriction does not define a function; or (2) b is empty, so the restriction of S_c is a function but it is onto; or (3) $b = c$, so it is not smaller.

This definition can be packaged as a logical function that recognizes small models of the natural numbers. These therefore form a category, with morphisms $(c, S_c) \rightarrow (d, S_d)$ the functions $f: c \rightarrow d$ that commute with successor functions. A bit of structure: each model has exactly one element that is not in the image of the successor function; denote it by 1_c . A morphism $(c, S_c) \rightarrow (d, S_d)$ is determined by the image of 1_c . A corollary is that f is an isomorphism (i.e. has an inverse) if and only if $f(1_c) = 1_d$, and in particular there is a unique isomorphism between any two models.

A model of the natural numbers can be used to construct a model of pretty much all of classical mathematics, and most of the modern as well. Moreover the construction stays in the context of small sets. The mathematical view is that conclusions that depend only on the successor-function structure make perfect sense independent of the model, so who cares if each one of us has his own private model? Or put another way, mathematics is an elaboration of the *logical function* that identifies models of the natural numbers, not the models themselves, and in this sense it really is well-defined.

The problem comes when someone (usually a philosopher) objects to the “model” aspect. Shouldn’t there be ONE classical mathematics, not a bunch of equivalent models? Or when someone (always a philosopher) interprets the question “what are THE natural numbers?” as meaning there should be ONE correct universal instance, not a logical function that identifies models. Benacerraf’s “identification problem” (see Horsten [17] section 4.1) is an extreme illustration. Different models will obviously be different in ways not related to the structure. The answer to “is 2 an element of 3?” will be Yes in some models and No in others, and Benacerraf argues that, because they differ in this regard, neither of two standard constructions can be *the* one true correct version. A much better conclusion is that the word “the” in the phrase “the natural numbers” is a linguistic artifact that is out of step with mathematical reality and should not be taken seriously. Or, that philosophers who let linguistic artifacts drive their views of mathematics should not be taken seriously.

We return to efforts to define “the” natural numbers. The approach that relates it to counting proposes to define “2” as the equivalence class consisting of the second element in all possible models. A modern description is: consider the directed system with objects the models and relations the isomorphisms. Get a single object by taking the direct limit of this system. Uniqueness of isomorphisms implies that the system is contractible, so the canonical morphism from any element in the system to the direct limit is an isomorphism. The limit should therefore be a canonical universal model.

The problem with these approaches is that the collection of small models is not small. In particular, the collection of all second elements is not small, and it is not logically sensible to use it to define an equivalence relation. The direct system in the modern limit formulation is not small and the limit (defined using the product over the index set) is not small. As with set theory, this can be resolved by using a theory of medium-sized sets in which the collection of all small sets is medium-sized. The equivalence relation or the direct limit make sense in this theory, and they give a canonical medium-sized object that is universal for small ones. In this particular case uniqueness on the single-model level implies that every medium-sized model

has a unique isomorphism to the small-model limit. This is still not completely canonical because it depends on the small theory, but it is pretty close.

The general mathematical attitude, again, is that trying to get a sensible definition of “the” natural numbers is not worth the trouble. It seems to be doomed to failure, and the multiple-personal-models or single-logical-function view works fine.

12.4. **Logical operations and notations.** [[incomplete]]

12.5. **ZFC axioms.** [[incomplete]]

12.6. **Application: Stone-Čech compactification and ultralimits.** The Stone-Čech compactification of a topological space is an ambitious construction; enough so to need care with set theory. Ultralimits of sequences are an application. It is a fundamental fact that any sequence in a compact space has a limit point. Usually it has more than one, and in a separable space there is (by definition) a sequence whose limit points are the whole space. Ultralimits provide a coherent way to pick out one of these limit points for every sequence in every (small) compact Hausdorff space. As an application we get an ultra-integral defined for all functions from an interval to an interval.

12.6.1. *The construction.* Fix a finite closed interval I in some model for the real numbers. If X is a small topological space let $C(X, I)$ denote the continuous functions from X to I . Now let $\Pi_{C(X, I)} I$ be the product of copies of I , one for each continuous function.

Recall that the product is the collection of set functions $C(X, I) \rightarrow I$, and this is nonempty by the axiom of choice, and small by assumption that the set-detection function is ZFC closed.. Note that we are using all functions, not just continuous ones, so there are a lot of them. Further, this set is to be given the product topology, with basis of open sets given by open sets in some finite sub-product, times the remaining factors. It is a theorem of Tychonoff (surprising but not hard) that this is a compact Hausdorff space.

There is a natural function $ev: X \rightarrow \Pi_{C(X, I)} I$ defined by: $x \in X$ goes to the function $\Pi_{C(X, I)} \rightarrow I$ that evaluates functions at x . This is easily seen to be continuous.

- The Stone-Čech compactification, denoted by βX , is defined to be the closure of the image of ev in $\Pi_{C(X, I)} I$.

12.6.2. *Universality and functoriality.* This construction is functorial in the following sense: if $f: X \rightarrow Y$ is continuous then composition induces a continuous function $f \circ: C(Y, I) \rightarrow C(X, I)$. Composition with this induces a function on products that gives a commutative diagram

$$\begin{array}{ccc} X & \xrightarrow{f} & Y \\ \downarrow & & \downarrow \\ \Pi_{C(X, I)} I & \xrightarrow{(f \circ) \circ} & \Pi_{C(Y, I)} I \end{array}$$

In particular the induced function restricts to give a continuous function of closures

$$\beta(f): \beta X \rightarrow \beta Y$$

that extends f . This description makes it clear that $\beta(f \circ g) = \beta(f) \circ \beta(g)$, etc.

Next, since the product of intervals is a compact Hausdorff space, βX is compact Hausdorff. Moreover, if X is itself compact Hausdorff (and small) then $ev: X \rightarrow$

βX is a homeomorphism. A corollary of this is that the natural maps give an equivalence of functors $\beta^2 \simeq \beta$, so we might say that β gives a retraction of the category of all spaces to the subcategory of compact Hausdorff spaces. Another corollary is that any map $X \rightarrow Z$, with Z small compact Hausdorff, factors uniquely as $X \rightarrow \beta X \rightarrow Z$.

12.6.3. *Ultralimits.* The smallest nontrivial example is also one of the most interesting. Let N denote a model of the natural numbers with the discrete topology, and let βN denote the Stone-Ćech compactification. Let α be a point in βN that is not in N , then for any sequence in a compact Hausdorff space $s: N \rightarrow Z$ define

- $\text{ultralim}_\alpha s = \beta s(\alpha)$.

We consider this as a point in Z via the inverse of the canonical homeomorphism $Z \rightarrow \beta Z$.

Note this is *always* defined: no nonsense about divergence. Ultralimits in a finite-dimensional real vector space V are linear in the sense that if s, t are bounded sequences in V and r is a bounded sequence of real numbers then

- $\text{ultralim}_\alpha (s + t) = (\text{ultralim}_\alpha s) + (\text{ultralim}_\alpha t)$ and
- $\text{ultralim}_\alpha r s = (\text{ultralim}_\alpha r)(\text{ultralim}_\alpha s)$

But some of the other properties may make divergence seem not so bad.

First, a simple observation:

- The permutations of N act transitively on the limit points in βN .

This means that if α, γ are limit points then there is a permutation (bijection) $p: N \rightarrow N$ so that the induced βp takes α to γ . To find p begin with sequences a_i and g_i in N that converge to α, γ respectively. We may assume these are increasing, and (by taking subsequences if necessary) that $a_i, g_i \geq 2i$. Define $p(a_i) = g_i$, and extend to $N - \{a_i\} \rightarrow N - \{g_i\}$ by the unique order-preserving bijection. We then have

$$\gamma = \lim_{i \rightarrow \infty} g_i = \lim_{i \rightarrow \infty} p(a_i) = \beta p(\lim_{i \rightarrow \infty} a_i) = \beta p(\alpha).$$

One conclusion is that the limit points of βN are all spiritually equivalent. How many of them there are depends on the set theory, and in particular whether or not the continuum hypothesis is true, but this won't make any difference for applications. In particular:

- Suppose z is a limit point of a sequence x_i in a compact Hausdorff space Z . Then there is a reindexing p so that $\text{ultralim}_\alpha x_{p(i)} = z$.

Therefore ultralimits are independent of the indexing of the sequence only if there is a single limit point. In other words, only if the sequence converges in the traditional sense. As a result they require more careful handling than ordinary limits, but they can still be extremely useful. The next section is *not* an example of their usefulness, however.

12.6.4. *Ultra-integrals.* Fix a limit point α of N in βN . For any bounded real-valued function on the interval $[0, 1]$ define a sequence (of Riemann sums) as follows:

$$S_n(f) = \frac{1}{2^n} \sum_{i=0}^{2^n} f(i/2^n)$$

These are bounded, so are all in a compact interval in R , and the ultralimit is defined.

- Define $\int_\alpha f = \text{ultralim}_\alpha S_n(f)$

This integral is defined for *every* bounded real-valued function. It is linear in f , and agrees with the usual integral when f is continuous. It is very unreliable in other ways, however. For instance for $0 < t \leq 1$ define

$$f_t(r) = \begin{cases} f\left(\frac{r}{t}\right) & \text{if } \frac{r}{t} \leq 1 \\ 0 & \text{otherwise} \end{cases}$$

This compresses f down to $[0, t]$, and ideally would multiply the integral by t . Unfortunately, for discontinuous f there need be no relationship at all between these integrals for t not related by a power of 2.

The moral of this story is that when standard texts refer to “non-integrable functions” this does not mean there is *no* way to define integrals of them, but that it is a bad idea because we would have to give up beloved properties of the integral to do it. This is similar to the confusion about dividing by zero. It is not impossible, but it is a bad idea because it causes the whole number system to collapse ($1 = 0$).

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