A Revolution in Mathematics? What Really Happened a Century Ago and Why It Matters Today

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The physical sciences all went through "revolutions": wrenching transitions in which methods changed radically and became much more powerful. It is not widely realized, but there was a similar transition in mathematics between about 1890 and 1930. The first section briefly describes the changes that took place and why they qualify as a "revolution", and the second describes turmoil and resistance to the changes at the time.

The mathematical event was different from those in science, however. In science, most of the older material was wrong and discarded, while old mathematics needed precision upgrades but was mostly correct. The sciences were completely transformed while mathematics split, with the core changing profoundly but many applied areas, and mathematical science outside the core, relatively unchanged. The strangest difference is that the scientific revolutions were highly visible, while the significance of the mathematical event is essentially unrecognized. The section "Obscurity" explores factors contributing to this situation and suggests historical turning points that might have changed it.

The main point of this article is not that a revolution occurred, but that there are penalties for not being aware of it. First, precollege mathematics education is still based on nineteenth-century methodology, and it seems to me that we will not get satisfactory outcomes until this changes [9]. Second, the mathematical community is adapted to the social and intellectual environment of the mid- and late twentieth century, and this environment is changing in ways likely to marginalize core mathematics. But core mathematics provides the skeleton that supports the muscles and sinews of science and technology; marginalization will lead to a scientific analogue of osteoporosis. Deliberate management [2] might avoid this, but only if the disease is recognized.

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The Revolution

This section describes the changes that took place in 1890–1930, drawbacks, objections, and why the change remains almost invisible. In spite of the resistance, it was incredibly successful. Young mathematicians voted with their feet, and, over the strong objections of some of the old guard, most of the community switched within a few generations.

Contemporary Core Methodology

To a first approximation the method of science is "find an explanation and test it thoroughly", while modern core mathematics is "find an explanation without rule violations". The criteria for validity are radically different: science depends on comparison with external reality, while mathematics is internal.

The conventional wisdom is that mathematics has always depended on error-free logical argument, but this is not completely true. It is quite easy to make mistakes with infinitesimals, infinite series, continuity, differentiability, and so forth, and even possible to get erroneous conclusions about triangles in Euclidean geometry. When intuitive formulations are used, there are no reliable rule-based ways to see these are wrong, so in practice ambiguity and mistakes used to be resolved with external criteria, including testing against accepted conclusions, feedback from authorities, and comparison with physical reality. In other words, before the transition mathematics was to some degree scientific.

The breakthrough was development of a system of rules and procedures that really worked, in the sense that, if they are followed very carefully, then arguments without rule violations give completely reliable conclusions. It became possible, for instance, to see that some intuitively outrageous things are nonetheless true. Weierstrass's nowhere-differentiable function (1872) and Peano's horrifying space-filling curve (1890) were early examples, and we have seen much stranger things since. There is no abstract reason (i.e., apparently no proof) that a useful such system of rules exist, and no assurance that we would find it. However, it does exist and, after thousands of years of tinkering and under intense pressure from the sciences for substantial progress, we did find it. Major components of the new methods are:

> **Precise definitions:** Old definitions usually described what things are supposed to be and what they mean, and extraction of properties relied to some degree on intuition and physical experience. Modern definitions are completely self-contained, and the only properties that can be ascribed to an object are those that can be rigorously deduced from the definition.
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> **Logically complete proofs:** Old proofs could include appeals to physical intuition (e.g., about continuity and real numbers), authority (e.g., "Euler did this so it must be OK"), and casual establishment of alternatives ("these must be all the possibilities because I can't imagine any others"). Modern proofs require each step to be carefully justified.

Definitions that are modern in this sense were developed in the late 1800s. It took awhile to learn to use them: to see how to pack wisdom and experience into a list of axioms, how to fine-tune them to optimize their properties, and how to see opportunities where a new definition might organize a body of material. Well-optimized modern definitions have unexpected advantages. They give access to material that is not (as far as we know) reflected in the physical world. A really "good" definition often has logical consequences that are unanticipated or counterintuitive. A great deal of modern mathematics is built on these unexpected bonuses, but they would have been rejected in the old, more scientific approach. Finally, modern definitions are more accessible to new users. Intuitions can be developed by working directly with definitions, and this is faster and more reliable than trying to contrive a link to physical experience.

Logically complete proofs were developed by Frege and others beginning in the 1880s, and by Hilbert after 1890 and (it seems to me) rounded out by Gödel around 1930. Again it took awhile to learn to use these: the "official" description as a sequence of statements obtained by logical operations, and so forth, is cumbersome and opaque, but ways were developed to compress and streamline proofs without losing reliability. It is hard to describe precisely what is acceptable as a modern proof because the key criterion, "without losing reliability", depends heavily on background and experience. It is clearer and perhaps more important what is *not* acceptable: no appeals to authority or physical intuition, no "proof by example", and no leaps of faith, no matter how reasonable they might seem. As with definitions, this approach has unexpected advantages. Trying to fix gaps in first approximations to proofs can lead to conclusions we could not have imagined and would not have dared conjecture. They also make research more accessible: rank-and-file mathematicians can use the new methods confidently and effectively, while success with older methods was mostly limited to the elite.

Drawbacks

As mathematical practice became better adapted to the subject, it lost features that were important to many people.

The new methodology is less accessible to nonusers. Old-style definitions, for instance, usually related things to physical experience so many people could connect with them in some way. *Users* found these connections dysfunctional, and they can derive *effective* intuition much faster from precise definitions. But modern definitions have to be used to be understood, so they are opaque to nonusers. The drawback here is that nonusers only saw a loss: the old dysfunctionality was invisible, whereas the new opacity is obvious.

The new methodology is less connected to physical reality. For example, nothing in the physical world can be described with complete precision, so completely rule-based reasoning is not appropriate. In fact the history of science is replete with embarrassing blunders due to excessively deductive reasoning; see the section "Hilbert's Missed Opportunities" for context and illustrations. Professional practice now accommodates this through the use of mathematical models: mathematics applies to the model but no longer even pretends to say anything about the fit between model and reality. The earlier connection to reality may have been an illusion, but people saw it as a drawback that had to be abandoned. In the other direction, core mathematics no longer accepts externally verified (experimental) results as "known" because this would bring with it the same limitations on deductive reasoning that are necessary in science. Even the most seemingly minor flaw will sooner or later cause proof by contradiction and similar methods to collapse. In practice this led to a division into "core" mathematics and "mathematical science". For instance, if numerical approximations of fluid flow seem to reproduce experimental observations, then this could be taken as evidence that the approximation scheme converges. This conclusion does not have the certainty of modern proof and cannot be accepted as "known" in the core sense. However, it is a reasonable *scientific* conclusion and appropriate for mathematical *science*. Similarly the Riemann hypothesis is incredibly well tested. For scientific purposes it is a solid fact, but it is unproved and remains dangerous for core use. Another view of this development is that, as mathematical methods diverged from those of science, mathematics divided into a core branch that separated from physical science in order to exploit these methods and a mathematical science branch that accepted the limitations in order to remain connected. The drawback here is that the new power in the core and the support it gives to applied areas are invisible to outsiders, whereas the separation from science is obvious. People wonder: is core mathematics a pointless academic exercise and mathematical science the real thing?

Opposition

Henri Poincaré was the most visible and articulate opponent of the new methods; cf. [6]. He felt that Dedekind's derivation of the real numbers from the integers was a particularly grievous conceptual error because it damaged connections to reality and intuitive understanding of continuity. Some of the arguments were quite heated; the graphic novel *Logicomix* [1] dramatically illustrates the turmoil (though it muddles the issues a bit). Scholarly works [3] are more dignified but give the same picture.

As the transition progressed, the arguments became more heated but more confined. At the beginning traditionalists were deeply offended but not threatened. But because modern methods lack external checks, they depend heavily on fully reliable inputs. Older material was filtered to support this, and as the transition gained momentum some old theorems were reclassified as "unproved", some methods became unacceptable for publication, and quite a few ways of looking at things were rejected as dangerously imprecise. Understandably, many eminent late nineteenth-century mathematicians were outraged by these reassessments. These battles were fought by proxy, however. For instance, Poincaré's monumental development of the theory of manifolds was quite intuitive, and we now know that some of his basic intuitions were wrong. But, in the early twentieth century, only a fool would have openly criticized Poincaré, and he could not respond to implicit reproaches. As a result the arguments usually concerned abstractions such as "creativity" and "understanding", often in the context of education.

On a more general level, scientific concerns about the new methods were reasonable. The crucial importance of external reality checks in physics had been a hard-won lesson, and analogous revolutions in biology and chemistry were still in progress (Darwin's *Origin of Species* appeared in 1859, and Mendeleev's periodic table in 1869). How could mathematical use of the discredited "pure reason" approach possibly be a good thing?

Most of the various schools of philosophy were, and remain, unconvinced by the new methods. Philosophers controlled words such as "reality", "knowledge", "infinite", "meaning", "truth", and even "number", and these were interpreted in ways unfriendly to the new mathematics. For example, if a mathematical idea is not clearly manifested in the physical world, how can it be "real"? And if it is not real, how can it have "meaning", and how can it make sense to claim to "know" something about it? In practice mathematicians do find that their world has meaning and at least a psychological reality. If philosophy were a science, then this would qualify as a challenge for a better interpretation of "real". But philosophy is not a science. The arguments are plagued by ambiguity and cultural and linguistic biases. "Validation" is mostly a matter of conviction and belief, not functionality, so there are few mechanisms to correct or even expose the flaws. Thus, rather than refine the meaning of "reality" to accommodate what people actually do, philosophers split into Platonists and non-Platonists, depending on whether they believed mathematics fit their own interpretation. The Platonic view is hard to defend because mathematics honestly does not fit the usual meanings of "real" (see the confusion in Linnebo's overview [5]). The non-Platonic view is essentially that mathematicians are deluded. Neither view is useful for mathematics. To make real progress mathematics had to break with philosophy and, as usual in a divorce, there are bad feelings on both sides.[1]

The precollege-education community was, and remains, antagonistic to the new methodology. One reason is that traditional mathematicians, most notably Felix Klein, were extremely influential in early twentieth-century educational reform. Klein founded ICMI [4], the education arm of the International Mathematical Union. His 1908 book *Elementary Mathematics from an Advanced Viewpoint* was a virtuoso example of nineteenth-century methods and did a lot to cement their place in education. The "Klein project" [4] is a contemporary international effort to update the *topics* in Klein's book but has no plan to update the *methodology*.[2] In brief, traditionalists lost the battle in the professional community but won in education. The failure of "new math" in the 1960s and 70s is taken as further confirmation that modern mathematics is unsuitable for children. This was hardly a fair test of the methodology because it was very poorly conceived, and many traditionalists were determined that it would succeed only over their dead bodies. However, the experience

[1] *There are exceptions, but I wonder whether many of these might not be instances of another thing seen in divorces: one partner remains in love with a fantasy assembled from the good times they had together. See the "Other Views" section in* [7] *for instances.*

[2] *For detailed explanation see the essay* "Updating 'Klein's Elementary Mathematics from an Advanced Viewpoint': Content only, or the viewpoint as well?" *in* [10].

reinforced preexisting antagonism, and opposition is now a deeply embedded article of faith.

Many scientists and engineers depend on mathematics, but its reliability makes it transparent rather than appreciated, and they often dismiss core mathematics as meaningless formalism and obsessive-compulsive about details. This is a cultural attitude that reflects feelings of power in their domains and world views that include little else, but it is encouraged by the opposition in elementary education and philosophy.

In fact, hostility to mathematics is endemic in our culture. Imagine a conversation:

A: What do you do?

B: I am a _____.

A: Oh, I hate that.

Ideally this response would be limited to such occupations as "serial killer", "child pornographer", and maybe "politician", but "mathematician" seems to work. It is common enough that many of us are reluctant to identify ourselves as mathematicians. Paul Halmos is said to have told outsiders that he was in "roofing and siding"!

Obscurity

Like most people with some exposure to history of mathematics, I knew about the "foundational crisis" that occurred roughly a century ago. However, my first inkling that something genuinely revolutionary happened came at an international conference on proofs in mathematics education.[3] Sophisticated educators described proofs in ways that I did not recognize, while my description [8], based on an analysis of modern practice [7], was alien to them. The picture that emerged after a great deal of reading and study is that these educators were basing their ideas on insightful analysis of professional practice up through the nineteenth century. They were not misunderstanding modern mathematics but correctly understanding pre-modern mathematics. The disconnect stems from a change in mathematics itself, a change of which they were unaware.

No one is unaware of the scientific revolutions. The first subsection suggests that high-profile publicity had a lot to do with this, and obscurity of the mathematical transition is in a sense a public relations failure. To make this more concrete, the second section describes some public relations opportunities that Hilbert had but did not use.

Proxies and Belief

Scientific revolutions were methodological, but it was conclusions that attracted attention. The Copernican revolution, for instance, is known for the then-controversial conclusion that the earth orbits the sun, and the Darwinian revolution in biology is known for controversial conclusions about human origins. In both cases the real advances were methodologies effective enough to make alternative conclusions untenable, but methodology is complex and technical. High-profile conclusions served as public proxies for the methodology.

This proxy picture suggests several difficulties for mathematics. First, mathematical conclusions are not exciting enough to provide highly visible proxies. Second, conclusions used to promote mathematics are almost always applications to science, medicine, and engineering. They are proxies for *mathematical science* and have raised visibility of these areas, not the core. For the core, these efforts to use proxies may have actually backfired. Finally, when core results such as the Fermat conjecture or the Poincaré conjecture are described, it is—of necessity—in heuristic terms that are compatible with nineteenth-century viewpoints. The descriptions hide the crucial role of modern methodology, so they are not proxies for it. We will see that there are metamathematical conclusions that at one time might have served as proxies for modern methods, but they were not used.

The science examples also suggest a problem with belief. Users adopt more effective methods, but nonusers often reject things they do not like (e.g., evolution) regardless of benefits to the technical community. Core methods such as completely precise definitions (via axioms) and careful logical arguments are well known, but many educators, philosophers, physicists, engineers, and many applied mathematicians reject them as not really necessary. There are cases in which physical science has been unable to overcome rejection based on dislike, so even a very clear case for modern mathematics may not succeed.

Hilbert's Missed Opportunities

David Hilbert was the strongest and most highly visible proponent of the new methods during the transition, and as such he was frequently involved in controversies. I describe several situations in which Hilbert might have reframed debates and provided metamathematical proxies that could have led to a much clearer view today. The historical context is used to make the discussion more concrete, not to reproach Hilbert. After all, these opportunities are still just barely visible even with a century of hindsight. The first controversy occurred early in Hilbert's career and concerned his vigorous use of the "law of the excluded middle" (proof by contradiction). His response to the objections was that denying mathematicians use of this principle was "like denying boxers the use of their fists"; true but not a clear claim or challenge. If he had said the following, it would have caused an uproar:

> Excluded-middle arguments are unreliable in many areas of knowledge, but absolutely essential in mathematics. Indeed we might *define* mathematics as the domain in which excluded-

[3] *ICMI Study 19, Taipei, May 2009.*

> middle arguments are valid. Instead of debating whether or not it is true, we should investigate the constraints it imposes on our subject.

At the time mathematics was generally seen as an abstraction of physical reality, and it would have been outrageous to suggest that a logical technique should have higher priority in shaping the field. But in fact nothing physical can be described precisely enough to make excluded-middle arguments reliable, and this as much as anything drove the division of mathematics. In applied areas these arguments continued to be tempered by wisdom and experience. In the core the link to reality became indirect, with modeling as an intermediate, primarily to provide a safe environment for excluded-middle arguments.

Such a statement would have redirected the debate by making successful use of excluded-middle arguments a proxy for core methods. It would also have enabled the issue to be settled in a coherent way. As it was, this issue was a constant pain for Hilbert; Brouwer's Intuitionist school kept it alive into the 1930s; and it died out more from lack of interest than any clear resolution.

Next, Hilbert's axiomatic formulation of geometry in 1899 precisely specified how points, lines, and so forth *interacted*, rather than specifying what they "were" and extracting the interactions from physical intuition. Hilbert himself pointed out that this disconnected mathematics from reality because one could interpret "point" as "chair" and the axioms would remain valid. Again this provoked objections. He might have pointed out that it is a powerful advantage to be able to use a single mathematical construct to model many physical situations. This would have made the disconnect a proxy for mathematics as an independent domain. Widespread acceptance of explicit modeling would then have carried with it acceptance of mathematical independence. As it happened, modeling became widespread in the professional community without being seen as having any such significance.

Hilbert's famous 1900 problems were powerful technical challenges that did a lot to drive development of infinite-precision methods. However, the few that were actually seen as proxies for new ways of thinking (e.g., the second, on consistency of arithmetic) did not fare well, and the changes that the problems helped drive remained mostly invisible.

Another debate concerned the use of axiomatic definitions and detailed logical arguments. These provoked strong objections about lack of reality and meaning, artificial rigidity, and content-free formal manipulation. Hilbert might have replied:

> Axiomatic definitions can be artificial and useless, but they can also encapsulate years, if not centuries, of difficult experience, and newcomers can extract reliable and effective intuitions from them. Similarly, fully detailed arguments can be formal and content-free, but fully confronting all details usually deepens understanding and often leads to new ideas. Fully detailed arguments also give fully reliable conclusions, and full reliability is essential for successful use of the powerful but fragile excluded-middle method.

This would have acknowledged the dangers of formality but established reliability as a proxy for high-precision methodology and implicitly staked a claim to a nonphysical sort of meaning. Instead, Hilbert accepted the slanders by saying "mathematics is a game played according to certain rules with meaningless marks on paper." Hilbert also suggested that these mathematical methods might be prototypes for similar developments in other sciences. Such things were in vogue at the time. Arthur Conan Doyle, for instance, set his enormously popular Sherlock Holmes stories in a world where excluded-middle logic actually worked:

> …when you have eliminated the impossible, whatever remains, however improbable, must be the truth…
> —*The Sign of the Four*, ch. 6 (1890)

It was probably not widely known that this sort of logic led Doyle himself to a strong and expensive belief in fairies. Blondlot's "N-ray" debacle in France around 1904 was not yet seen as a cautionary tale. Since then there have been quite a few embarrassing failures due to excessively deductive reasoning in science. In the "cold fusion" episode in 1989, for instance, electrochemists Fleischmann and Pons observed excess energy in some of their experiments. After eliminating electrochemical explanations, they deduced that the only alternative they could imagine—hydrogen fusion in the electrodes—must be the truth. This is a standard move in mathematics and in Doyle's fiction, but bad science because there is no way to ensure that all alternatives have been imagined. Good *scientific* practice would have required them to test the fusion deduction, for instance by looking for the radiation that would have been a byproduct of fusion. Not seeing radiation would have turned them back to interesting electrochemistry. Presumably they had stumbled on a previously unimagined way to make a battery, and it was releasing energy accumulated during earlier experiments. But their reliance on the power of deduction led instead to crashing ends to their careers and reputations.

The modern view is that Hilbert's proposal— that mathematical deduction might be a general prototype for science—is a failure. His linkage ended up casting doubt on mathematical developments instead of justifying them. Meanwhile, very high reliability has been achieved in mathematics without drawing attention or having significance attached to it. The axiomatic-definition approach also made mathematics more accessible. A century ago original research was possible only for the elite. Today it is accessible enough that publication is required for promotion at even modest institutions, and an original contribution can be required for a Ph.D. Again this is a profound change that had no significance attached to it.

The final missed opportunity concerns disagreements about knowledge, meaning, and "true". By 1920 the search for secure foundations had bogged down in obscure philosophical arguments. Hilbert had proposed a precise technical meaning for "true", namely, "provable from axioms that themselves could be shown to be consistent". But ten years later Gödel showed that in the usual formulation of arithmetic there are statements that are impossible to contradict but not provable in Hilbert's sense. In particular, consistency of the system could not be proved within the system. This was seen as a refutation of Hilbert's proposal. Ironically, it had the same practical consequences because it established "impossible to contradict" as the precise mathematical meaning of "true". Hilbert might have been explicit about deeper goals, for instance:

> Mathematics needs a precise definition of "true" that is internal and accessible to mathematical verification, and in particular unconstrained by philosophy or imagined connections to physical reality. We can worry about what such a definition "means" after it has been developed and shown to be successful in actual practice.

In this light Gödel's work would have been seen as a successful modification rather than a refutation.[4] Since that time a precise internal meaning for "true" has been enormously liberating for professional work, but its benefits have gone unnoticed.

Summary

The mathematical transition had such a low profile that no one understood its significance. Felix Klein was still denouncing the new methods in the 1920s, and because his views were not only unrefuted but almost unchallenged, outsiders accepted them as fact. Historians, educators, and philosophers went forward largely unaffected, propelled by the momentum of three thousand years and rebuffed by the technical complexity of modern practice.

Strangely, mathematicians are also unaware that their field changed so profoundly. Newcomers found philosophical arguments incomprehensible and irrelevant, and philosophy went from a respectable pursuit to an object of ridicule and evidence of senility in just a few decades. But this replaced bad understanding with no understanding at all. Mathematicians have joined fish and birds in doing something very well without any idea how!

The Core at Risk?

For most of the twentieth century, mathematics was mainly supported in the higher educational system. Core mathematicians dominated this system, so mathematics had a secure niche that did not depend on understanding what it was about. However, this niche is eroding, and the security is gone.

A large-scale problem is that resource constraints are eroding the ability of the higher education system to support basic research. There is pressure to increase instructional productivity by replacing researchers with teaching faculty at half the cost. Mathematics departments with large service loads are particularly vulnerable. There is also pressure to increase research productivity, with consequences discussed below.

There is a problem with external research funding. In the United States, external support for core mathematics comes almost exclusively from the National Science Foundation. A desire to have something to show for the money has led the NSF to want "wider impacts", and the use of applications as proxies to promote mathematics has led to "applications" being the default interpretation of "wider impacts". The result is a steady shift of funding toward applied areas (and education; see below). Because external funding is a major indicator of productivity, a decline in NSF support for core activity has contributed to the decline in core activity in academic departments.

Yet another problem comes from changes in applied mathematics. Up through the late twentieth century, applied mathematicians were trained in mainstream graduate programs and had foundations in modern methods and values. Today many are several generations removed from these core mathematical foundations. Many are scientists rather than mathematicians in the modern sense, and some are actually hostile to core methodology. At the same time, demand from science and engineering and pressure for more highly visible research have caused many academic departments to shift toward applied areas. The result is culturally divided departments in which core mathematics is increasingly at a disadvantage.

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[4]*It is doubtful that either Hilbert or Gödel would have accepted this formulation. Both felt that the core axioms of mathematics should be "concrete intuitions", an extra-mathematical criterion. Their interpretations of "finitistic" were also less well defined and less internal than those used today; see Tait* [11]. *In these ways Hilbert and Gödel were still not fully modern.*

The final problem concerns the disconnect between school mathematics and higher education. School mathematics is still firmly located in the nineteenth century, so student success rates in modern courses have been very low. There is a great deal of pressure to improve this situation, but recent changes, such as use of calculators and emphasis on vague understanding over skills, have actually worsened the disconnect. Something has to change. Ideally, school mathematics could be brought into the twentieth century. Unfortunately the K–12 education community is better organized, more coherent, and far more powerful politically. External funding agencies are committed to the K–12 position. At the NSF this means funds have shifted from research to educational programs that are actually hostile to the research methodology. It seems possible that the K–12/college articulation will be "improved" by forcing higher education to revert to nineteenth-century models.

The point in all these examples is that the nature of modern core mathematics must be much better understood to even see the problems. And if the problems are not recognized and addressed quickly, then—in the United States anyway—core mathematics may well be marginalized, and the mathematical Golden Age that began in the twentieth century will end in the twenty-first.

The big question is: Why would marginalization of the core be a problem, if one is not particularly interested in the subject itself? In fact, core mathematics provides a rigid skeleton that supports the muscles of science, engineering, and applied mathematics. It is relatively invisible because it cannot interact directly with the outside world; it grows slowly; and it would not cause immediate problems if it stopped growing. Premodern mathematics and contemporary mathematical science, on the other hand, are more like exoskeletons: in direct contact with reality but putting strong constraints on size and power. The long-term consequence of mathematical osteoporosis is that science would have to go back to being a bug!

Solutions for Education?

The point briefly addressed here[5] is that modern methods were adopted because they are much more effective at advanced levels. If the reasons for their success are clearly understood, then some of these methods might be adaptable to elementary levels. This is the meaning of "brought into the twentieth century" in the discussion above, and at the very least it would improve K–12/college articulation. But it might do far more.

To be specific, consider fractions. Currently these are introduced in the old-fashioned way, through connections with physical experience. This is philosophically attractive and "easy" but follows the historical pattern (see the discussion in "Drawbacks") of being dysfunctional for most students. If we want students to be able to actually *use* fractions, then core experience points a way: use a precise definition that looks obscure at first but that can be internalized by working with it and that is far more effective once it is learned. Such an approach is suggested in [9] and elaborated in some of the essays in [10]. Similarly, in [8] I explain how a careful understanding of the nature of modern proofs might improve success even with arithmetic. (These are detailed and specific illustrations but are given as starting points rather than "classroom ready").

The big question is: Can any version of these approaches be used by real children? Children are attracted to rule-based reasoning (think *games*), and rich applications and success downstream should more than compensate for initial obscurity. I suspect that it is a bigger challenge for educators to think this way than it is for children. The starting point would be to acknowledge the significance of the mathematical revolution a century ago and to see the new methods—properly understood—as profoundly rich resources rather than alien threats.

[5] *And at great length in* [9] *and* [10].

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