CMS NOTES

Rigor in Context

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One of the defining features of modern mathematical proof is rigor. We can spell out what rigor is in formal derivations where the acceptable inferences, axioms, and formal language are all defined. Of course, mathematical proofs in practice are rarely like logical derivations—they're gappy, in a natural language, and often use diagrams. So, rigor in practice has a "know it when you see it" quality. It has often been thought that the rigor of mathematical proofs ought to be defined in terms of formal rigor. What I'd like to suggest in this column is that rigor evaluation might have a much more social nature.

According to the "standard view" of rigor proposed by philosophers of mathematics, a mathematical proof is rigorous only in the instances in which it could be translated or fleshed out into a valid formal derivation in a suitable system. Numerous philosophers of mathematics have raised objections to the standard view. The objections arise since the standard view fails to make sense of how a mathematician could ever judge historical and diagrammatic proofs to be rigorous. Euclid, for example, seems to give some rigorous proofs long before there was a definition of a formal language. In addition, many proofs in geometry and topology rely on diagrammatic manipulations. It's unclear whether those proofs even can be formalized at all. Altogether, the standard view of rigor fails to identify many rigorous proofs as rigorous.

Part of the issue is that proof evaluations seem to vary across practice. Standards of rigor seem to depend on the mathematical subfield, location, and time period. We also tend to adjust our demands for rigor based on our pedagogical and communicative goals. There may be some way to reconcile formalizability with these facts about practice. But I think the more natural direction is to explore the context-sensitive and social aspects of rigor. I am not alone in this approach to rigor—for instance, Tanswell (2017) has argued for a virtue-theoretic account of rigor. According to that view, rigor is a kind of characteristic of a mathematician, much like curiosity or intellectual humility. Larvor (2012) has also argued for a context-sensitive account of rigor under which a rigorous proof is one that conforms to the permissible actions in a domain. In soccer only certain movements are allowed, but those same movements wouldn't be permissible in gymnastics. Likewise, in knot theory, only certain inferences are deemed permissible, which might be impermissible in probability theory.

Like Tanswell and Larvor, I'm interested in examining rigor through a context-sensitive and social lens. But I'm mostly motivated by claims, like Hersch's (1993), that argue proof really aims at conviction and explanation. The difficulty is in spelling out who we are aiming to convince. I've argued before (2021) that the universal audience is the target audience based on the goals of proof. A universal audience is an imagined audience constructed to represent all reasonable people. This audience is a mental fiction that will be specific to each mathematician. In everyday life, people imagine arguing to certain kinds of audiences. Before meeting your class, you might imagine a general audience of students in order to develop a presentation they will understand. You use your past experiences with real audiences of students to do so. A universal audience is developed in a similar fashion. We imagine a general audience of reasonable people based on our past experiences with reasonable people.

I argue that a proof is completely rigorous when each step is one to which the mathematician's universal audience assents. Each inference is judged to be rigorous when it convinces one's universal audience. For the mathematician, this amounts to the judgment that "this inference would convince everyone." This account is consistent with historical judgments of rigor, since the concept of an audience dates back far longer than that of a formal language. It also is consistent with diagrammatic proofs, since it doesn't require a translation into another language to determine whether or not a field-specific diagrammatic move is performed correctly. For example, a proof in knot theory that employs a Reidemeister move, one of three simple deformations for knot diagrams, will not need to be translated in order to judge whether or not it is rigorous. Moreover, my account allows one to make sense of comparative rigor judgments in a straightforward way. Mathematicians frequently give "more rigorous" or "less rigorous" proofs depending on the context.

"Non-standard" accounts of rigor don't gild proof evaluation in objective terms. This may seem problematic, but I think it gives us a richer framework to understand unjust parts of the history of mathematics. In particular, the audience view allows us to examine how subjective perceptions of groups might affect rigor judgments. To close this column, I'll outline such a case.

von Neumann (*Wikipedia*); Bell (Linda Hall Library); Hermann (*Physics Today*).

In his 1932 *Mathematical Foundations*, John von Neumann published a purported proof of the impossibility of hidden-variable theories in quantum mechanics. It was accepted as a rigorous proof until 1966, when John S. Bell published a devastating objection to the proof, noting that it contains a problematic assumption—that of the linearity of expectation for all possible observables—which does not actually hold. For example, there are well-known counterexamples involving eigenvalues in the quantum mechanical case. As a consequence, von Neumann's proof was revealed to be a fundamentally circular argument. It seemed that there had been a lapse in rigor unnoticed in the entire community. On its own, this is an interesting story about how long it might take to correct incorrect rigor judgments. Von Neumann clearly thought his own proof was rigorous. And at least a few mathematicians also judged it to be so. Bell managed to point out an assumption that most reasonable people would not agree with, which resulted in the community recognizing the von Neumann proof as non-rigorous. The counterexamples highlight that the proof is not correct. But rigor doesn't entail correctness since a circular proof shouldn't be regarded as rigorous even if it happens to have a correct conclusion. In fact, the circularity of the proof could be identified without counterexamples. And, John Bell was not the first to point out that circularity in von Neumann's proof!

In 1935, Grete Hermann (2016) published an essay entitled "Natural-Philosophical Foundations of Quantum Mechanics," which had a section called 'The Circle in Neumann's Proof.' She thus had already argued that von Neumann's proof is circular because it attempts to rule out the existence of dispersion-free states by assuming the additivity rule, but the additivity rule only holds when there are no such states. Hermann's argument was almost entirely ignored in the years between von Neumann and Bell. Seevinck (2016) and Paparo (2012) have both tried to determine why Hermann's argument was ignored. Some of their proposed reasons are that the paper was not published in a popular journal, von Neumann was an almost prophet-like figure, she was young, she was a woman, she was a political outsider, she typed that part of her article in a small font, she did not ascribe significance to her own argument, and she "did not desire the status of a revolutionary or radical" (Seevinck 2016, p. 116).

The purported reasons seem to fall into three categories. Reasons in the first category suggest that Hermann's work was ignored because of her failure to realize its own importance. I include purported explanations involving the size of the font, the unpopular journal, and her desire to not be revolutionary in this category, but I think these reasons are not particularly strong. It seems fairly obvious that she did attribute importance to her argument since she went through the trouble of having it published and she subtitled the von Neumann section in a clear, non-conciliatory way. The second category of reasons suggests that the work was not read since it was not published in a popular journal, but we know the paper was read by important contemporaries. As Seevinck (2016) points out, Carl Friedrich von Weizsäcker wrote a review of the 1935 essay. Further, Hermann composed the argument while working with Werner Heisenberg at his institute in Leipzig. The journal she published in was not the strongest, but the arguments within the paper were clearly read by important physicists at the time.

Having ruled out the first two categories, we are left with the sad but obvious conclusion that she was ignored due to some social characteristic. It doesn't matter for the purposes of this column whether that social factor was her youth, sex, political orientation, or perceived status relative to von Neumann. Prejudice based on one of those factors allowed mathematicians and physicists to ignore her circularity objections. Another potential explanation is that the community wanted von Neumann to be right, and so for Hermann to be wrong, which caused them to ignore Hermann's objection. Either way, rigor judgments surrounding von Neumann's proof and Hermann's objections were not objective or based on formalizability. They were products of a more complicated, and unjust, milieu that influenced mathematical judgments.

Hermann's story is not a happy one. But it is useful in examining how intertwined our mathematical practices can be with our social experience. It seems to me that an important step to avoiding these injustices is to have philosophical accounts that allow us to discuss them. The "standard view" of rigor leaves very little room to explore cases such as Hermann's. Instead, we should turn to "non-standard" views of rigor like those explored in Tanswell (2017) or Ashton (2024).

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