(Suspense novels must contain the prerequisite amounts of sex, violence, and endings that result in an explosion, narrowly averted.) Editors tell you this. Any novel that doesn't conform to these conventions is deemed unbelievable. And so the reality of the genre has replaced the reality of the real world.

In some respects the relationship between the sciences and the humanities long ago fell into the pattern of genre fiction. This is unfortunate and does not adequately reflect the reality of the world. In fact, mathematics and science have influenced art more, perhaps far more, than is usually acknowledged. In the late 19th century, speculation on the meaning of the "fourth dimension" was extremely popular and influenced the work of futurist and suprematist artists, who in turn influenced world architecture through Bauhaus. Einstein's relativity prompted artists and musicians of the 1920s to speculate on the nature of space and time, which resulted in the machine esthetic. Marcel Duchamp's famous "Large Glass" in Philadelphia was actually based on his musings about physics. Some historians argue that Girard Desargues invented projective geometry as a result of concern with perspective in art.

And so on. That is the way civilization is created, not by streams running each in its own course, but by streams coursing together. I can't help think that it is long past time on the part of both writers and scientists to emphasize their commonality of experience rather than their separateness of existence.

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Social Constructivism as a Philosophy of Mathematics. By Paul Ernest. State University of New York Press, 1998, xiv + 315 pp., $19.95 softcover, $59.50 hardcover.
What Is Mathematics, Really? By Reuben Hersh. Oxford University Press, 1997, xxiv + 343 pp., $35.00.

Reviewed by **Bonnie Gold**

In the early years of this century, Platonism (by which I mean the belief that mathematics is the science of certain mind-independent, non-physical objects with determinate properties) was dethroned as the dominant philosophy of mathematics. Since then, there's been a struggle to replace it with an alternative that avoids the philosophical problems of Platonism while accurately reflecting the working mathematician's daily experiences of doing mathematical research.

None of Platonism's immediate successors—logicism, formalism, intuitionism—has proved satisfactory. The first two fail to account for the role of the mathematician in the establishment of mathematical knowledge, as if mathematical knowledge were possible without any mathematicians. They also don't allow for the development of our knowledge of mathematics over time. The third, intuitionism, is unpopular because it rejects large parts of mathematics. As the philosophy of science started studying how scientific knowledge develops and the reasons for accepting new scientific theories, Lakatos and others began similar inquiries about mathematics. In the last 25 years, new candidates for philosophies of mathematics have become popular, including fictionalism, conventionalism, structuralism, and social constructivism.

In the books under review, both Paul Ernest and Reuben Hersh propose versions of social constructivism, which has been gaining adherents recently. However, their writing styles and viewpoints are completely different. Although each author speaks warmly of the other, they represent opposite extremes of the school. Mathematicians will almost universally find Hersh's version palatable and Ernest's unpleasant to read and at odds with actual mathematical practice.

Part of the problem with Ernest's book is really the fault of mathematicians, especially teachers of logic. Philosophy students often take a required mathematics course without really understanding it—they never internalize DeMorgan's laws (the negation of "p and q" is "not p *or* not q"), and they don't appreciate the importance of correct hypotheses in theorems—but they work so hard that we give them a passing grade. Alas, some of them go on to graduate school and beyond, and start writing books about the philosophy of mathematics that make us shudder with embarrassment as they betray their total lack of understanding of mathematics.

Ernest is a philosopher who thinks he knows about mathematics because he's taken a few courses in mathematical logic. He hasn't understood them. He bases a part of his philosophical position on an incorrect statement of the Craig Interpolation Lemma. The lemma says that if one has two formulas A and B, possibly involving different symbols, and a proof that $A \to B$, then there is a formula X involving only symbols that occur in both A and B such that there are proofs that $A \to X$ and $X \to B$. That is, if one formula implies another, then there is an interpolant in a language that they have *in common*. Ernest first states the lemma incorrectly (p. 204, footnote 13), saying that X "*includes* the mathematical concepts occurring in both A and B"—that is, it might be in a larger language! Then, he uses this bizarre misstatement of the theorem to claim that "no step from A to B in a proof is above further analysis, and there are no ultimate basic proof steps into which a published mathematical theorem can be analyzed." Yet, once one has interpolated an X that is in the common language, there's nothing further to be done! Certainly one can trivially add X arbitrarily often as an interpolant: $A \to X$, $X \to X$, $X \to X$, and $X \to B$; but this adds nothing, whereas the original act of interpolation in some sense gets at what A and B have in common that *makes* A imply B. After that, there is nothing *further* to analyze.

This confusion is not a central point in Ernest's argument (although he does refer to it twice, using it to claim that it's not possible, even in principle, to reduce all proofs to a standard format), but it is typical of the lack of understanding of mathematics found throughout the book. He gives few concrete mathematical examples of what he is talking about, and when he does, frequently they are either incorrect or a misinterpretation of the mathematical result.

The audience for the book is clearly professional philosophers of mathematics, not mathematicians. Since the book is full of terms such as "reification" and "hermeneutic" and references to philosophers such as Gadamer, Bakhtin, and Collingwood, mathematicians will find it extremely tough slogging unless they have a good knowledge of philosophy and have closely followed the writings in philosophy of mathematics over the last 30 years.

The philosophical point of view Ernest puts forth is that mathematics is simply whatever the community of mathematicians chooses to call "mathematics", and mathematical truths are simply what we decide to baptize as truths. If tomorrow we decide that $\mathbf{Z}_6$ has a subgroup of order 5, then it will, and that will be as "objectively" true as the fact, today, that it doesn't. Recognizing that he has to account for the apparent "objectivity" of mathematics, Ernest simply uses Orwell's Newspeak as a model and says that if a community agrees something is true, that's "objectivity." He accounts for the universal agreement on the facts of mathematics by the observation that we bully our students in grade school to accept such "conventions" as "$2 + 2 = 4$," so that by the time they get to college they view it as a law of nature.

The basis of Ernest's views is what he sees as extensions of the work of Wittgenstein and Lakatos. From Wittgenstein he takes the view that mathematics is a verbal game, played by rules. The rules are looked after by the community of mathematicians, and we accept new members into our community when they show that they can follow those rules. What the rules are, he declines to specify, although they include "proof," which he views as a kind of conversation (again, unspecified, but varying over time) to "warrant" mathematics (a philosophical term roughly meaning to give a basis for something to count as an object of knowledge). Proof is by no means the *only* rule of the game: there are rules for discovery, for writing or discussing mathematics, etc., but no details beyond general principles are given. There is nothing even conceivably fixed in any of these rules —rather, Ernest insists that NO text can have a unique meaning, but simply has the meaning the community chooses to give it at present. As has become popular among philosophers of mathematics, he cites the Lowenheim-Skolem Theorem in support of this view, although this theorem is about first-order logic only, not all of mathematics.

From Lakatos, Ernest takes the view that the philosophy of mathematics must be considered from a historical perspective, and revolutions in mathematics are a regular part of mathematical practice. Ernest takes this to mean that no facts are permanently true; "facts" true today may become false tomorrow. This is simply false. There are changes in the meanings of some words and different levels of rigor over time, so our philosophical interpretations of the mathematics change. But the facts themselves simply don't change. Although other sciences and philosophical theories change their "facts" frequently, 2 + 2 remains 4.

Hersh's book is an attempt to set forth in greater detail the philosophy mentioned cursorily in his books written with Philip J. Davis, *The Mathematical Experience* and *Descartes' Dream*. These books have done a substantial service to the mathematical community, and to the world at large, by attempting to bridge the gap between mathematicians and everyone else. There is far too little of this kind of expository writing by mathematicians. Hersh targets the same wide audience in this new book, but in this case he serves no one well by the ambiguity of audience. There are too many references to specific mathematical facts for anyone without an undergraduate degree in mathematics fully to understand the book (despite a final section that briefly explains various mathematical topics). On the other hand, the lack of specific references for his myriad quotes and paraphrasings makes it a Herculean task for the mathematician or philosopher to examine the contexts of the quotes.

The title "What is Mathematics, *Really*" is in response to "What is Mathematics" by Courant and Robbins **[1]**, which "answered" the question by providing some very nice examples of mathematics, but which never summed up these examples in a clear, concise statement. Hersh's book, an attempt to provide that statement, consists of three parts. The first part sets forth his philosophy of mathematics in a brief, well-written 90 pages. While I don't believe that this is *the* correct view of mathematics, Hersh makes the case well. He has the best account (pp. 61–62) I've seen of the role of intuition in mathematics: the various ways mathematicians use the word and its importance in both the development of new mathematics and decisions about the correctness of this new mathematics. It's worth getting the book simply to read this discussion. The second part is his particular take on the history of the philosophy of mathematics, in which he divides philosophers of mathematics into mainstream or humanist. I'm not sufficiently well-versed in this history to judge the accuracy of his division; the lack of detailed references is especially annoying in this part of the book. His classification of Brouwer with the traditionalists is particularly bizarre. Any reasonable understanding of intuitionism would place Brouwer as a forerunner of the social constructivist school: for intuitionists, mathematics is that which the community of mathematicians constructs; mathematical objects don't exist until constructed by mathematicians. The final part of the book begins with an extremely offensive classification of philosophers of mathematics into leftists and rightists, resulting in a body count of far more leftists on the humanist side and more rightists on the traditional side. (Of course, moving Brouwer to the correct side of the traditionalist/humanist controversy makes the count less one-sided.) I see it as a desperate argument to sway mathematicians (who mostly tend to be generous souls) toward his philosophy. This is followed by 65 pages of mathematical notes and comments, an attempt to make the various mathematical references through the body of the text accessible to the lay reader.

Thus, the main new contribution of this book is in its first 90 pages. Hersh begins by discussing the problems of Platonism. The traditional Platonist view is that "mathematical entities exist outside space and time, outside thought and matter, in an abstract realm independent of any consciousness, individual or social" (p. 9). The traditional philosophical difficulties with this view are (1) it requires a belief in some abstract, non-physical, non-psychological realm, which might have been fine when God was central to our world-view, but which is unattractive to modern intellectuals, and (2) even if such a realm exists, how do we, physical beings, have any contact with, or knowledge of, this realm? To the best of my knowledge, there haven't been any serious recent attempts by mathematicians to modify Platonism to meet those objections. This leaves the field open for philosophers to declare victory over Platonism, and to offer alternative philosophical descriptions of mathematics.

It's not clear that any modern mathematicians who view themselves as Platonists (or "realists") actually subscribe to Hersh's full statement. Most mathematicians are not particularly committed to where or what mathematical objects are, just as long as they do have objective properties. Therefore Hersh's view is attractive: mathematical objects are constructed by the community of mathematicians. Generally a new concept is suggested by one mathematician, but often the idea is developed and modified over a period of time by the community until it settles down to have a fixed definition.

Mathematical objects, then, are neither mental nor physical; rather, they are social entities, in the same general category as monetary systems, or the Supreme Court (not the people, but the institution). So far, Hersh and Ernest are in rough agreement. For Hersh, however, once a mathematical object has been constructed by the community of mathematicians, it takes on a sort of life of its own. Its properties are no longer dependent on people, but are discovered; some properties, not apparent when we first define the object, can be difficult to discover. This is the point at which Hersh and Ernest part company. For Ernest, only the current rules of the mathematical game make x^2 an even function, and it could become odd at any time. For Hersh, once the function has been defined, mathematicians no longer control it.

Hersh's version has several very attractive features. It fits fairly well with mathematics as done by mathematicians. Our knowledge of mathematics develops over time, as well it should if mathematicians invent new mathematical objects or discover connections among those already invented. Mathematics is no longer infallible, but that's much more consistent with our actual experience than is the purported infallibility of traditional Platonism. After all, false proofs are often published, and mistakes are common when one works on new mathematics. Under Hersh's view, the mathematical community determines what constitutes a proof, and proof becomes the standard set by the community for acceptance of new mathematical knowledge. This describes actual mathematical practice far better than formalism's view that proofs are deductions in first-order logic, of which no human can comprehend any non-trivial examples. Further, there's a role for mathematics education: it brings new members into the community.

However, there are two important ways in which Hersh's social constructivism is inadequate as a philosophy of mathematics. The first is that it doesn't account well for the usefulness of mathematics in the world. Why should mathematics developed *before* any application was known turn out to be useful, and often in a variety of unrelated contexts? Certainly social constructivism can explain the parts of mathematics developed in response to some societal need. But the majority of applications, especially in this century, came from mathematics developed for the pure interest of the mathematical question—so why should it later be found to have anything to do with the real world?

The second, and, I think, the more serious problem with social constructivism is that it doesn't distinguish between the facts of mathematics (the function x^2 is even, the group $\mathbf{Z}_6$ has subgroups of order 1, 2, 3, and 6) and *our knowledge* of that mathematics. Plato's bizarre suggestions on recollection from past lives aside, it's clear that as children we are taught mathematics; the mathematical community's *knowledge* of mathematics *develops* over time; and it involves a communal effort. That social constructivist philosophies do recognize this development, and credit it, is a principal reason they are attractive. But this recognition is not an argument against the independence of mathematical objects from human society. Our knowledge of physics also develops over time. Nonetheless, the facts of physics exist before their discovery, and they are independent of errors the community of physicists makes or debates within that community. Furthermore, the concepts of physics—atoms, velocity, force, and so on—are developed gradually by the community of physicists. Yet physical objects either are or are not made up of atoms, and it's not the community of physicists that makes that true or false: it's the actual state of the world. That is, people develop and clarify concepts, but the *objects* those concepts attempt to describe either are or are not present in the world. The same is true of the objects of mathematics. Symmetry properties of chemicals affect how those chemicals behave, and did so even before mathematicians discovered symmetry groups. Ropes didn't start hanging in catenaries only after mathematicians "invented" that curve.

Hersh consistently ignores this distinction between the facts of mathematics and our knowledge of those facts, and many of his statements become obviously false if one reads them with this distinction in mind. "From living experience we know two facts: Fact 1: Mathematical objects are created by humans..." (p. 16). In what way does our "creation" of $\mathbf{Z}_6$ differ from our creation of quarks? Clearly, we single out the concept as something worth applying to part of our experience, but that isn't *creating* the object. "Point 1 is that mathematics is a social-historic reality. This is not controversial" (p. 23). No, it's not controversial, just false. Our *knowledge* of mathematics is a social-historic reality, though, and *that* isn't controversial.

There are several other problems with Hersh's account of mathematics. "A realistic analysis of mathematical intuition should be a central goal of the philosophy of mathematics" (p. 62). Hersh asserts that our intuition of mathematical objects comes from our education, from doing problems and getting checked for correctness by our teachers. We check that we have the same representation of a concept by seeing if we give the same answers to questions about it. That is, it's a social activity. But this doesn't square with experiences such as that of Ramanujan, who developed his results in complete isolation. Certainly, without a community of mathematicians—and especially without Hardy—to recognize the importance of his work, it would have been lost to the world. But Ramanujan's intuition came from working with the mathematical objects themselves, not from the community. While Ramanujan's case is certainly the extreme, the history of mathematics has numerous other examples of mathematicians (Desargues and Abel come to mind) who were so far ahead of their time that there was no *community* developing the intuition behind their discoveries.

In the past, a requirement of a philosophy of mathematics has been that it account for the peculiar level of certainty we appear to have in mathematics. The three philosophical schools that developed around the turn of the century—logicism, intuitionism, and formalism—were the result of trying to regain the level of certainty for which mathematics was notorious before the discovery of non-Euclidean geometry, contradictions in set theory, and problems in the foundations of analysis. Hersh spends a lot of time trying to debunk the notion of "mathematical certainty" on the grounds that formal proofs don't give certainty, because nontrivial ones aren't surveyable, and whence does our certainty come, if not through formal proofs? Yet once the definitions are understood, *any* two mathematicians, no matter what their cultures, should come to the same conclusions. That is different even from other sciences, not to mention studies (e.g., economics, politics) of the "socio-historical objects" Hersh would place mathematics among. Even if proofs don't give absolute certainty, proofs are central to the establishment of a mathematical result. I'm happy to view proofs among the activities that concern human knowledge of mathematics, not the mathematics itself. But nothing in Hersh's account explains the special role of proof in mathematics—why it's so central for establishing the truth of mathematical facts. An adequate philosophy of mathematics must do a better job of incorporating the role of proof, beyond explaining it as a peculiar social custom.

I'm a mathematician. I work every day with such mathematical objects as the function x^2 and the group (or ring) $\mathbf{Z}_6$. I'm not really sure where or what these objects are. But I am sure that *nothing* the human community (mathematicians or otherwise) does will make x^2 into an odd function or make $\mathbf{Z}_6$ have a subgroup of order 5. And any philosophy of mathematics that says otherwise must simply be rejected.

What is needed is an account of mathematical objects, and facts of mathematics, and our knowledge of these facts, that parallels the account of physical objects and facts of physics and our knowledge of them. This may be roughly what Gödel meant when he wrote, "Despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true.... This, too, may represent an aspect of objective reality" [**2**]. There's no reason to believe, as Hersh asserts, that Gödel believed this perception true only of sets and not of other mathematical objects.

There is one alternative philosophy of mathematics that rivals social constructivism in growing popularity, but that Hersh touches on only briefly and trivially dismisses. It is the closest to a current attempt to account for mathematics in a Platonist manner while meeting the philosophical objections to older Platonist accounts. This is the view of mathematics as "the science of pattern." While many mathematicians have supported this view (Lynn Steen and Peter Hilton among them), I'm unaware of any attempt on the level of the two books under review to present this view carefully and in detail. Hersh's objection that mathematics doesn't study *every* kind of pattern is trivially correct. That is why this view needs further exploration. However, the view that mathematics is the science of *certain kinds* of patterns allows mathematics to be independent of society and yet accounts for the ability of physical beings to contact objects in the world of mathematics.

One thing human beings do extremely well is to observe patterns. We're far superior to computers in this ability. With great difficulty we can program computers to recognize very simple patterns, but human beings recognize some patterns when we are born and very quickly learn to recognize and work with many kinds of patterns. Furthermore, patterns partake of both the physical and the non-physical. In fact, we can see our ability to classify *anything* as a recognition of patterns. One collection of atoms, of leaves and branches in certain patterns, is a tree. We recognize the general pattern by giving it this collective name. Another pattern is a table. Some patterns are man-made, others are out there in the world, others are abstract, but our minds are what give them a collective name. To the extent that we say that trees are objective and not dependent on the human mind, we can start attributing that same objectivity to the patterns mathematicians study. It's certainly not the case that all patterns are mathematics. But at least somewhere in that region there seems a place to carve out the realm of mathematics—neither physical, mental, nor social, but certainly associated with the world, independent of us, and accessible to human beings.

Such a view does a better job of accounting for the applicability of mathematics, for many of our patterns come from the physical world. That our knowledge of mathematics develops over time becomes no more surprising than that our knowledge of physics develops over time. That different cultures have been especially hospitable to the discovery of certain parts of our mathematical knowledge is almost inevitable, as the patterns that a culture surrounds itself with are particular to it—and so our mathematical *knowledge* is very much a socio-cultural artifact. How proof helps guarantee mathematical truth remains to be explored, but it's reasonable that a link can be found in the connection between patterns in the world and mathematicians looking for them.

Although in the end I don't believe Hersh has yet shown us *what is mathematics, really*, he has made an important contribution to the discussion. Any acceptable philosophy of mathematics must be consistent with actual practice of mathematicians—that we make errors, that our proofs are *not* exercises in formal logic, that our knowledge changes over time. Whatever we finally decide mathematics is, it is still discovered by humans; what is discovered may depend on social or cultural factors; and the discovering, teaching, and sharing of our knowledge of mathematics remains something to be shared by all, both for the benefit of the growth of that knowledge and for the human race's ability to rise above petty fraternal feuds.

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Today Devin catches me at it.
"Mommy, what're you trying to do?"
"Oh," I say, "well, these lines.
I'm trying to fix these lines."
And then I explain triangles.
And then I explain transitive.
"Sometimes it can't be done," I tell him, "and other times it can.
I'm trying to figure out when it can."
"I get it," he says. "I get it, Mommy."
And later he catches me at it again.
"*That* one *worked*, right?"

'Cause maybe, if that one worked, we can go play Parchesi.
Or cards. Or ice cream. Or hanging out.
Or at least Mommy won't
keep staring at those lines.

Contributed by Marion Cohen, Drexel University, Philadelphia, PA