18 Unconventional Essays on the Nature of Mathematics. Edited by Reuben Hersh. Springer, New York, 2006, xxi + 326 pp., ISBN 0-387-25717-9, $49.95.

Reviewed by Edward Nelson

Although several of the essays in this collection are by sociologists, none addresses the central question of the sociology of mathematics: why is it that mathematicians are such nice people? We are no respecters of persons (in that curious phrase that means we do respect persons but pay little attention to the trappings of age, position, or prestige), we take equal delight in fierce competition and collaborative effort, and we are quick to say "I was wrong." Perhaps some of us know an exception that proves the rule, but by and large I speak sooth, especially when one compares mathematicians to our colleagues in the humanities.

But alas! one major qualification is still needed. The bias against women in our field is yet with us, equally pernicious when it is unwitting. To take just one example, the wide-ranging and all-too-brief commentary by Mary Beth Ruskai on the decline of science [1] raises a number of issues quite relevant to the collection under review. Its inclusion would have enriched this all-male book.

How does one explain that we are so lovable? Is there something in the nature of mathematics that attracts gentle souls? Possibly, but another explanation is more convincing. We are singularly blessed in that the worth of a mathematical work is judged largely by whether the *proof* is correct, and this is something on which we all agree (eventually), despite the fact that we may have divergent views on the nature of mathematics, as the volume under review amply shows. This is a singular fact. In art, projection of personality may prevail; in the humanities, the power of position may prevail; in science, the prevailing fad may prevent the publication even of excellent work—but we are extraordinarily fortunate that in our field none of this matters.

So what is a proof? Like obscenity, we all know it when we see it, but it is hard to define—unless one is a formalist. In my opinion, the gap between rigorous argument and formal proof in the sense of mathematical logic is one that will close. In the lifetime of most of my readers, it will be common for a referee to submit the paper to a computer program to verify the correctness of the proof, thus freeing the referee to evaluate the work in terms of originality, depth, and importance.

The nature of proof is one of the themes of the essays in this book. But the typesetting of the book is a disgrace. Here are a few verbatim examples, though they will give my spelling checker fits. (This is the spelling checker that suggested replacing *finitist* by *dentist*, and *Fock space* by—but the *Monthly* is a family magazine.)

> the derivative of f o g is f' o g * g' [p. 39]
> as in Figure 1 [p. 57; there is no figure]
> miracle-as Rényi had Hippocrates say-that [p. 78]
> Hausdorff1 space [p. 105]
> 'the necessary residue of' the extinction of the ego' [p. 107]
> in mathema tics was held to he the [p. 112]
> in terms of the amalgamations al' thinking/scribbling [p. 121]
> ofprevious [p. 227]

I could go on, but these are more than enough to show the careless and uncaring treatment of this book by the publisher. Is this what is meant by "value added" by which the publisher justifies a charge of $49.95 for a paperback? It is true that all of these passages, except the one on p. 121, are understandable, but that is not the point. When my brother John was taking freshman French, the professor corrected a student's mistake and the student said, "But a Frenchman would have understood me." The professor replied, "Yes, and dogs understand each other by sniffing one another's behinds." The responsibility for such messes is ours. We have the right and the obligation to require of publishers that they not mangle manuscripts submitted to them, that they look at the book at least to catch glaring errors before offering it for sale, that they have some skill at mathematical typesetting, and even that the manuscript be read by someone with sufficient literacy to catch errors such as *affect* for *effect* [p. 159].

This book is not, and is not intended to be, a source book such as the very valuable selection [2] by Benacerraf and Putnam. Rather the essays are chosen to be diverse and provocative. The chief pleasure in reading such a book is mentally arguing with the essayists. Gentle Reader, double your pleasure, double your fun: argue with the present reviewer as well.

Rényi. The first essay is a delightful Socratic dialogue, perhaps no more unhistorical than those of Plato, on the nature of mathematics. It ends with an impassioned speech by Socrates on the virtue of using the mathematical method in philosophy—in strong contrast to Rota's views (see below).

Celluci. In this introduction to his book on philosophy and mathematics, the author presents 13 points of the "dominant view" of the philosophy of mathematics, points that he says he will counter. Most of these points of the dominant view are quite sensible, and one wishes they were expressed in greater detail, especially when he quotes Dummett.

Thurston. This is a humane, balanced, and deeply personal account of the author's experience and views of mathematics. I found it well worth pondering.

Aberdein. This essay contains an interesting discussion of the computer-assisted proof of the four-color theorem by Appel and Haken, if one ignores the jargon about patterns of argument.

Rav. The author says, "It is an intellectual scandal that some philosophers of mathematics can still discuss whether whole numbers exist or not," thus dismissing one of the central problems of the field. As do several of the other essayists, he emphasizes the importance of a solid orientation towards the practice of mathematics. In short, he offers us a descriptive rather than a normative philosophy of mathematics, based in his account on "evolutionary epistemology." But is a descriptive account what is needed? Are we so sure of the essential correctness of current mathematical practice that no critical study of it is required? A descriptive philosophy of law written in the nineteen-thirties might well have included a description of lynch law without comment.

Brian Rotman. Once I led a junior seminar on the foundations of mathematics. This was a mistake: the students did not have enough experience of mathematics to appreciate the foundational problems. But we had fun; the twelve students read in the literature and made presentations on Platonic realism, constructivism, and formalism. In the last meeting I asked the students to guess what my position on foundations was. Ten thought I was a Platonist and two thought I was a constructivist; not one guessed the awful truth. At least I knew I was innocent of any charge of proselytizing.

Rotman's essay achieves the astonishing feat of making me wish to leap to the defense of Platonism against his attacks. This is because it contains passages such as "Frege's anti-psychologism and his obsession with eternal truth correspond to his complete acceptance of the two poles of the subjective/objective opposition - an opposition which is the *sine qua non* of nineteenth-century realism." Or again, "Whether one sees realism as a mathematical adjunct of capitalism or a theistic wish for eternity, the semiotic point is the same. . . ."

MacKenzie. The frightening possibility is raised that the question of what is a proof may, and almost did, reach the law courts.

Stanway. The most interesting passage here is the description [p. 152] of Hardy and Littlewood's highly civilized four axioms for successful collaboration. When I was a graduate student, I heard a lecture by Littlewood on the art of work. The audience was mesmerized. Apart from his emphasis on the need for vacations, what I chiefly remember is his advice to finish an evening's work in mid-thought to provide an entry point in the morning.

Stanway discusses the effects of digital technology, but sensibly concludes, "There are good reasons to believe, however, that despite changes in patterns of collaboration, doing mathematics in the twenty-first century, will not be too unlike doing mathematics in the twentieth century."

Núñez. The author's thesis is that "by finding out that real numbers 'really move,' we can see that even the most abstract, precise, and useful concepts human beings have ever created are ultimately *embodied*." But he provides little evidence that this thesis is true or even interesting.

I once had an experience that at first sight might be seen as supporting the author's argument. One morning I was preparing for a graduate class in dynamics and worked out the formula for the flow generated by the Lie product of two vector fields. This was already in the literature, in a paper by Helgason and possibly elsewhere, but it was harder to search the literature in those days. A simple calculation produced the answer:

$$\lim_{n\to\infty}\left(U\left(\sqrt{\frac{t}{n}}\right)V\left(\sqrt{\frac{t}{n}}\right)U\left(\sqrt{\frac{t}{n}}\right)^{-1}V\left(\sqrt{\frac{t}{n}}\right)^{-1}\right)^n$$

where U and V are the flows produced by the two vector fields. As I stared at this formula—do something, do something else, undo the first, undo the second, repeat many times—something about it seemed familiar. And then I felt the similarity in the muscles of my arms: drive, steer, reverse drive, reverse steer, repeatedly. I went to Woolworth's and bought a toy car to illustrate the formula for the class, for which I received a sitting ovation. (But when I wrote this up in lecture notes I got the illustration all wrong, as my brother Jim pointed out: I drew a wagon rather than a car.) This was fun, but does it say anything of the slightest significance about mathematics? Are we to believe that Sophus Lie in his seminal investigations was expressing his embodied experience of parking a car?

The article is illustrated by some funny pictures of people gesturing while lecturing about mathematics, with their eyes blocked out by black rectangles. I was reminded of my most memorable encounter with gesture at a mathematics lecture. Will Feller was a great showman in addition to being a deep and subtle mathematician. Once at a colloquium talk he gave the audience *roared* with laughter. I won't attempt to say what was so funny because it wasn't; only Feller's showmanship made it so. On another occasion in a seminar Feller was discussing an intricate combinatorial problem about random walks. It was quite hard to follow. (But the purpose of a mathematical lecture is not to convey technical knowledge but to entertain and impart a feeling for what is important and exciting. A class attempts to do both, which is one reason that teaching is so difficult and rewarding.) At one point, Feller said, "Now follow the trajectory until the first time it goes *down*," accompanied by a dramatic gesture *up* with the chalk.

Núñez objects to the notion of continuity as used in mathematics, wishing to replace it by "natural continuity," which seems to mean something like "continuous with a locally rectifiable graph and crossing each horizontal line in a locally monotonic fashion." One gets the impression that he dislikes mathematics—certainly he dislikes mathematics as mathematicians practice it.

Gowers. The author expounds a non-extreme version of formalism in an engaging way. By "non-extreme" I mean, for example, that he wants to say that there is a fact of the matter as to whether the decimal expansion of π contains a string of a million sevens. But his feeling is less strong about the twin primes conjecture, and he points out that the first question is a "there exists" problem while the second is a "for all there exists" problem. Since the argument that the natural numbers form a model for arithmetic depends on the cogency of there being a fact of the matter as to whether an *arbitrary* closed formula of arithmetic is true, it would be quite interesting to hear the author's views on his reasons for believing arithmetic to be consistent (if he does).

Azzouni. The author gives a witty and instructive discussion of what he calls, in a happy phrase, "the benign fixation of mathematical practice."

Rota. The Italian word *geniale* means possessed of genius. Certainly GianCarlo Rota was genial in both the Italian and the English meanings of the word. I wish he were still with us to counter my comments on his essay.

His main concern is with philosophy rather than mathematics. It is an angry attack on what he calls "mathematicizers of philosophy." And the attack is angry: *snobbish symbol dropping, preposterous, bewitched, enslaved, absurd pretense, unable or afraid, slavish and superficial imitation, damage to philosophy, dictatorial regime, resorted to the ruse, derelict in their duties, outrageous proposition, failed mathematicians, today's impoverished philosophy, catastrophic misunderstanding* are some of the terms he uses. (But yes, GianCarlo *was* genial.)

In the course of this jeremiad he makes some strange comments about mathematics. The strangest is this: "No mathematician will ever dream of attacking a substantial mathematical problem without first becoming acquainted with the *history* of the problem." I have been doing mathematics for sixty years and I never encountered this idea before. None of my teachers ever said anything similar, and I certainly never gave any of my students such misguided advice. The way to begin work on a substantial problem is with a fresh idea; it is not very important in the beginning whether it be right or flawed, since the important thing is to begin. Many young people hesitate to begin research feeling that they do not *know* enough, but much study is a weariness of the flesh. It is a mistake to become indoctrinated with the methods of the past, which by definition were insufficient for the substantial problem at hand.

Another is this: "Suppose you are given two formal presentations of the same mathematical theory. The definitions of the first presentation are the theorems of the second, and vice versa. ... Which of the two presentations makes the theory 'true?'" I have tried to imagine a presentation in which the list of finite simple groups is the definition and the definition of simple is the theorem, but I can't seem to make this work. I just don't know what he is saying here.

One final gnomic utterance without comment: "Not only is every mathematical problem solved, but eventually every mathematical problem is proved trivial."

Schwartz. Under the pretext of discussing the harm that mathematics does to science, the author launches an all-out attack on mathematical physics as an intellectual discipline. He says, "The mathematician turns the scientist's theoretical assumptions, i.e., convenient points of analytical emphasis, into axioms, and then takes these axioms literally. This brings with it the danger that he may also persuade the scientist to take these axioms literally." Does he really believe that we need to be careful not to lead gullible physicists astray?

Anyone who has devoted years of effort to mathematical physics is aware of the abrasive, but at its core mutually respectful, relationship between theoretical physicists and mathematicians. Our goals are different, but the goal of the mathematical physicist, to find what consequences follow rigorously from what explicit assumptions, is a goal worthy of respect even from those who choose to follow a different path. The work enriches physics to some extent and it greatly enriches mathematics.

Schwartz writes, "The sorry history of the Dirac Delta function should teach us the pitfalls of rigor." ("Pitfalls of rigor"? Yes, that is what this mathematician says.) He goes on to say, "This function remained for mathematicians a monstrosity ... until it was realized that [it] was not literally a function but a generalized function." *Generalized function* is Courant Institute-ese for distribution; implicit in Jack Schwartz's account is a belittling of Laurent Schwartz's fundamental contribution to analysis.

Later in the article he belittles the Birkhoff individual ergodic theorem, writing "The Birkhoff theorem in fact does us the service of establishing its own inability to be more than a questionably relevant superstructure...." It is correct to say that the ergodic theorem is not essential to statistical mechanics, as Khinchin argued before Schwartz. But this theorem is a deep result, simple to state and pure in its generality. Historically it had its origin in a central problem of physics and today it plays a central rôle not only in many problems of mathematical physics but in number theory as well. Schwartz calls it "intellectual prestidigitation" and "glittering deception."

Ávila del Palacio. In this essay we find the statement "The critical work of Berkeley on Analysis provoked Weierstrass' mathematical work." This is fascinating and I would like to know more about it. Berkeley's objections to the calculus were absolutely correct, and think of the time lapse from Berkeley to Weierstrass! Bishop Berkeley was no unsophisticate in technical mathematics, by the way. He objected to the procedure of first assuming that $h \neq 0$, drawing certain conclusions, and then setting $h = 0$ while retaining the conclusions. When Newton tried to counter this by calculating the derivative of x^2 by using *symmetric* difference quotients, Berkeley said in effect, all right, my friend, now let's see you do that with x^3.

Pickering. The account of Hamilton's invention of quaternions is easy reading. The surrounding sociological argumentation is harder going but interesting.

Glas. I found this the most interesting essay in the collection, and the one most likely to change my views on the nature of mathematics. I'll not try to say how, since I'm still ruminating. But I will make one technical comment. Citing Popper, Glas poses three questions: "Is any even number greater than 2 the sum of two primes [the Goldbach conjecture]? Is this problem solvable or unsolvable? And if unsolvable, can its unsolvability be proved?" The answer to the third question is no. For if the Goldbach conjecture is false, it is provably false: just exhibit the even number and check all possibilities. Therefore if the problem is unsolvable, the conjecture is true. Hence if we could *prove* that the problem is unsolvable, we could prove that the conjecture is true, thereby solving the problem.

White. The author's location of mathematical reality in culture is unsatisfying because he gives no explanation for the *universality* of mathematics, which distinguishes it from all other cultural phenomena, even music.

Hersh. The final essay, by the editor, is a brief and persuasive contribution to the problem raised by Wigner of the unreasonable effectiveness of mathematics in the natural sciences. I would like to add a comment that, taken with a grain of salt, could supplement Hersh's. It is based on the observation that Wigner's title is mistaken; it should read *physics* rather than *natural sciences*.

Mathematics is the invention and investigation of formal patterns, and good mathematics is the invention and investigation of deep and beautiful formal patterns. Let us call *physics* that portion of science that can be described, to a great extent, by a formal pattern, and call the rest of science *biology.* Then by definition mathematics is successful in physics.

[1] Mary Beth Ruskai, The decline of science, *Notices Amer. Math. Soc.* **45** (1998) 565.

[2] Paul Benacerraf and Hilary Putnam, eds., *Philosophy of Mathematics: Selected Readings*, Cambridge University Press, 1964.

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