ℵ Modern Logic ω

Stewart Shapiro, *Foundations without Foundationalism: A Case for Second-Order Logic* (New York/Oxford, Oxford University Press, 1991)

Reviewed by

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In a nutshell: Shapiro likes second-order logic and feels it has been unfairly neglected by proponents of first-order logic. In the book under consideration, he attempts to prove that first-order logic has terrible inadequacies that are removed only by second order logic. At the same time, he realises that second-order logic has its own problems.... The net result is the conclusion that the Foundations of Mathematics must do without the demand that there be a single best foundation, which demand he calls *foundationalism.* Actually, he distinguishes between *strong* foundationalism (there is a unique foundation for mathematics) and *moderate* foundationalism (there is at least one foundation for mathematics). Anyway, the book is divided into three parts—an Orientation (philosophy), Logic and Mathematics (a technical survey of second-order logic punctuated by an occasional philosophical aside), and a closing section on History and (more) Philosophy.

In the preface, Shapiro acknowledges that many of those who contributed their time and comments found his programme to be "seriously misguided." Let me say at the outset that I agree with them. I certainly agree with Shapiro that Foundations of Mathematics needs no foundationalism. At the same time, however, I must say that I see no need for an argument. (If one does need an argument, why not simply point out the simultaneous legitimacy of classical and intuitionistic mathematics and their obvious foundational incompatibility?) To argue that second-order logic deserves more attention is reasonable, and, perhaps, even necessary. To argue for such by attacking first-order logic is unseemly, smacking more of politics than of philosophy. But, if one is going to carry out such an argument, one ought at least to present an argument that is clear, fair, and informed. I would not care to say that Shapiro argues through confusion, cheating, and ignorance, but he could have done a better job of it.

The central confusion in my mind—and I take it as axiomatic that confusion in a reader is entirely the fault of the author—is what he is talking about. There are Mathematics (M), Foundations of Mathematics (FM), Mathematical Logic (ML), Philosophical Logic (PL), Philosophy of Mathematics as practised by Philosophers (PMP), Philosophy of Logic (PoL), Foundations of Logic (FL), and Epistemology (E). While there are relations among these subjects (e.g., PMP E), no two of them coincide (except possibly PoL and FL), and what may be inadequate for one may be perfectly adequate for another. What I never understand in the book is which of these perspectives is operational. Is first-order logic inadequate for M, FM, ML, PL, PMP, ... or what? On page 15 we read that "it may come to pass that logicians advocate and study only classical first-order systems. We cannot rule out the possibility that the 'triumph' of first order logic will be complete. However, if I may be permitted to be smug, both towards this possibility and towards those who currently hold that first-order logic is all there is, it might be recalled that it was once held that Aristotelian logic is the only logic there is. The considerations that toppled this view were of the same nature as those advanced in this book. The subsystems in question are not good models of important aspects of mathematics as practised". Now, who advocates only the study of first-order logic? Shapiro does quote Quine on this. So, are we discussing PL, PoL, or FL? We cannot be discussing M, FM, or ML because Quine ignores mathematical practice—his system of set theory refutes the axiom of choice, an axiom so appealing that even its opponents couldn't resist using it. On page 25, we read, "It seems that philosophical movements spawn tendencies that remain long after the views themselves are dismissed, at least publicly. One of these tendencies, I believe, is a preference for first-order logic". In which field is the "preference for first-order logic" merely an unjustified tradition? Well, he does say something about philosophy—I assume PL, PoL, or FL. In any event, he cannot be speaking of ML, where compactness and the Löwenheim-Skolem Theorem are powerful tools: Can anyone imagine developing non-standard analysis using second-order logic with its categorical set of real numbers? Or basing a computer language like PROLOG on a logic in which Herbrand's Theorem (i.e., compactness) fails? What other logic allows the calculation of explicit bounds from proofs in Analytic Number Theory— as announced by Kreisel in the late '50s, and currently demonstrated by Luckhardt and his students? In M, as well as in ML, the preference for first-order logic is well-founded. On page 37, he refers to "natural language" and "ideal justification". These concepts are foreign to M, FM, and ML; they do occur in PL, PoL, FL, PMP, and E. But mathematical practice is never considered in these fields, and in the first quote cited above Shapiro referred explicitly to "mathematics as practised" (which I take to mean *actual* practice). What is Shapiro talking about?

I am quite serious about this confusion. Throughout the book, Shapiro ignores actual mathematical practice, preferring instead some idealised version in which mathematicians do not ritually produce proofs, but they "reason", make "ideal justifications" and argue in "natural language". On page 47, for example, he says, "Historically, the codification of correct deductions is an central task of logic". *Bene disserere est finis logicis. I* cannot argue with that. But, he continues, "and it remains so in current studies". Really? I think the *Journal of Symbolic Logic* has more articles on complexity theory than on codifying correct deductions, which task has already been accomplished. It may be informative to recall—while on the subject of such codification—that Heyting obtained his axiomatisation of the intuitionistic propositional calculus empirically, by listing principles used in practice and eliminating redundant ones. On page 50, Shapiro says that the recursiveness of logical derivations (whether first-order or Heyting-propositional or whatever) "is particularly reasonable if the deductive system is to model the *process* of ideal justification". Now, this is entirely different from being adequate for actual practice. Heyting could not have gotten away with a survey (*á la* Aristotle's approach to politics), but would have had to analyse intuitionistic truth (*á la* Plato's approach to politics) if that were the goal. The fact is that modeling the process of ideal justification is unnecessary to the Foundations of Mathematics (however, desirable it may be for Philosophy of Mathematics or Epistemology). Mathematicians use proofs that can be communicated. They use surprisingly few rules of inference, write in a narrowly restricted language (not in full natural language), and give (at least in classrooms and many textbooks, if not in journals with limited space) rather detailed proofs. The formalisation of such proofs in first-order logic is largely a routine matter. Indeed, the logicians at Eindhoven ran Landau's *Grundlagen der Analysis* through first-order logic on the computer to check the proofs. The point is not merely that Shapiro ignores mathematical practice—almost all philosophers of mathematics do—but that he doesn't seem to realise this and bases part of his argument on the necessity of using second-order logic to capture the "process of ideal justification" and on this stronger logic's thus offering a "good model of important aspects of mathematics as practised". The fact is that first-order logic captures as much of the "process of ideal justification" as necessary to provide as good a "model of important aspects of mathematics as practised" as does second-order logic—it is even better: how do you check a semantic, second-order "proof" for correctness on a computer?

On page 43, following a remark that neither Aristotelian nor propositional logic is adequate for mathematical practice (did anyone ever believe they were?), he says, "The main theme of this book is to argue that first-order languages and semantics are also inadequate models of mathematics". There follows the "seriously misguided" discussion of ideal mathematics touched on above. Much later in the book Shapiro tries a different approach. On pages 119 *ff.*, he follows a lead of Kreisel's and criticises first-order formalisations in languages that are (not acknowledged to be) overly restricted. On page 121, for example, we read that there are "good reasons why the first-order theory [of real closed fields] is far too impoverished to be an adequate formalisation of classical analysis". One of these reasons, uncited by him, is that the theory was never intended to be such! The theory of real closed fields concerns the algebraic properties of the real numbers and is adequate for its purpose. Classical analysis is about real-valued functions, not real numbers, and any theory of classical analysis—first- or second order—must be in a language in which one can refer to these functions. His discussion of the first-order languages $\{0, s\}$, $\{0, s, +\}$, and $\{0, s, +, \bullet\}$ (s denoting the successor function) as languages for "number theory and elementary syntax" is similarly disingenuous: Dedekind's characterisation aside, number theory is not concerned with the successor function, but with the additive and multiplicative properties of natural numbers, with finite sequences of natural numbers (consider the Fundamental Theorem of Arithmetic), with finite sums and products (Möbius Inversion, factoring again), and with various numerical functions (the Euler totient, σ, τ, d, etc.); Gödel's β-function allows one—with a bit of effort—to replace the *obvious* language by the familiar $\{0, s, +, \bullet\}$. The adequacy of Peano Arithmetic is, given the artificially restricted language, simply amazing: as Takeuti first showed, and as becomes increasingly evident with the successes of Reverse Mathematics, Peano Arithmetic is adequate for deriving all results of traditional Analytic Number Theory—contrary to the misinformation of footnote 28, page 132. As to adequacy for elementary syntax, this was never the goal of number theory; that one can discuss such—to some extent—in arithmetic is again a matter of Gödelian coding.

I have already cited a number of points favouring first-order logic not acknowledged in Shapiro's attempted proof of its inadequacy—e.g., PROLOG, bounds, Landau. Whether Shapiro didn't know these facts or willfully withheld them I leave to the reader's judgment. I note only that, as any true believer in first-order logic will assume the latter explanation, I will offer—in the author's defense, mind you—a couple of examples of his astonishing ignorance. (One day, perhaps, he will return the favour.) The first is a footnote, the second an equally off-hand remark. On page 24, footnote 16 reads, in part, "as far as I know, Hilbert never considered a universal all-encompassing domain. Thus, according to the thesis at hand, he should not have been bothered by Russell's paradox. Yet after learning of the paradox, he took a cautious view... In 1905, he wrote…" In his paper for the 1900 International Congress of Mathematics in Paris, Hilbert quite explicitly stated that there was no set of all alephs, i.e. no set of all sets. It should come as no surprise to one who cites Zermelo's prior discovery of Russell's paradox (as does the author) and who knows Zermelo to have been a student of Hilbert (does the author know this?) to read now that Hilbert had been aware of some paradoxes for several years before making that address. The "thesis at hand" was right in predicting that Hilbert should not have been bothered by Russell's paradox. By 1905, however, Russell had published the paradox and the *reaction* to it took Hilbert by surprise and convinced him there was something deeper involved than he had realised. From 1904 on Hilbert addressed this reaction. A second example is the throw-away comment on page 30: "Perhaps reservations concerning negative or irrational numbers were alleviated when it was shown that they can be modeled in, or reduced to, natural numbers." Were there ever any widespread doubts about irrational numbers? Their existence certainly destroyed a central tenet of Pythagorean philosophy, but this existence was immediately accepted by the Pythagoreans nonetheless. The problem the Pythagoreans faced was not the justification of irrational numbers, but the construction of proof-methods that applied to them. For example, on assumption that triangles of equal height and base have equal area, it is easy to prove that, if two triangles of equal height have commensurable bases, then the ratio of their areas equals the ratio of their bases. How does one prove this if the bases are not commensurable? The Eudoxian Theory of Proportion answered ~~this pragmatic~~ question; it did not address any ontological issue. As to negative numbers, the Victorian exceptions prove the rule that they were widely accepted before a reduction to natural numbers was made. If there was an asymmetry, it was in two areas—the non acceptance of negative numbers as solutions to problems which generally called for positive solutions, and their absence from geometrically-based algebra where negative numbers have no meaning.

The last two examples are minor, and whatever point the latter was attempting to make should have been made with imaginary numbers rather than irrational or negative ones. They are, however, indicative of the overall quality of argument in the book. My admittedly harsh judgment is that the author's argument that first-order logic is inadequate is very weak (it is even more inadequate than first-order logic) and the mature reader will learn little from it. The central portion of the book, however, does have a nice overview of second-order logic and the difference between its metatheory and that of first-order logic, and I can recommend it to anyone unconcerned with the proofs of the results stated.