

## Book Review

Shaughan Lavine. *Understanding the Infinite*. Harvard University Press, Cambridge, 1994, ix + 372 pages.

This ambitious book is shaped around Mycielski's "finite mathematics" and Lavine's way of placing it in the foundations of mathematics. Lavine sees this formal theory of the indefinitely large (yet always with finite models) "as a codification of the actual historical and psychological source of our intuitions concerning the infinite" (p. 9). Thus he is drawn all the way to the historical roots of set theory. Further, he argues that "extrapolation from the theory of indefinitely large size provide[s] rational justification for believing our current theory of [infinite] sets" (p. 249). To support this he goes into Primitive Recursive Arithmetic and schematic versus second-order axiomatization. Finally he offers finite mathematics as a strategy to settle open questions in set theory such as the continuum hypothesis: "Anything that helps to clarify the sources of our axioms may help to suggest more axioms or help to adjudicate between the additional ones that have already been proposed" (p. 9). He says "It is not at all clear" how to apply the method beyond the currently standard axioms and "That is, I claim, partially why we are genuinely unclear about various questions of set theory" (p. 313). But he offers some possibilities which "are, I believe, the sorts of considerations that will be relevant to arguing for new axioms of set theory" (p. 314).

With all this in place, Lavine argues, "the apparently mysterious character of knowledge of the infinite is dissolved" (p. 10). Yet the particulars of that knowledge remain elusive. Lavine says, citing a similar view by Fraenkel, "Our understanding of the foundations of set theory is not much better than d'Alembert's understanding of the foundations of analysis was in the latter half of the eighteenth century" (p. 153).

The scope of the argument is thus extremely broad, and Lavine offers it as a general introduction to the philosophy of set theory. He discusses Gödel's, Quine's, and Putnam's views, and argues extensively with recent work of Kitcher, Maddy, Parsons, Shapiro, and others. He says "The reader who is innocent of mathematical knowledge beyond that taught in high school should be able to read . . . enough for all the major ideas to be presented," and "A reader who learned freshman calculus once, but perhaps does not remember it very well, and who has had a logic course that included

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a proof of the completeness theorem will be in fine shape throughout the book, except for [a few specified passages]" (p. v). I am not sure the book will work for so wide an audience as that. It seems to me to require rather more of calculus, logic, and set theory than Maddy [8]. But it will be interesting, enlightening, and occasionally maddening to anyone who studies the foundations of set theory.

***Historical background*** Starting with the Pythagorean discovery of irrationals, Lavine quickly gets to Newton and Leibniz on the calculus. Then twenty pages look at problems around continuity and series convergence. He contrasts the purely philosophic search for rigor to efforts arising out of mathematical practice, though in historical practice these are not so easily distinguished.

For example, he says Dedekind had philosophic motives for seeking "a foundation for analysis," and was perhaps "an exception to the usual rule that rigorization is not undertaken for its own sake" (p. 47). But Dedekind and Weber used similar infinitary set theoretic methods for an algebraic proof of the Riemann-Roch theorem in complex analysis. That theorem was already proved to some people's satisfaction and not to others, by analytic means (cf. McLarty [11]). Was this rigorization "for its own sake" or actually to check the theorem? Their abstract methods gave a more general theorem in principle, but the generality had no known interest at the time. Was that a philosophic advance or a mathematical one? (cf. Tappenden [23]). And Dedekind used similar algebraic methods to recast Galois theory in the field- and group-theoretic form taught today. These infinitary, algebraizing methods parallel Dedekind's foundation for analysis yet the connection between them seems to have gotten no philosophic attention beyond brief remarks in Dugac [2] and [3], and Stein [21].

Lavine (p. 46) recognizes Dedekind's importance in the origins of set theory. And I will add that Dedekind is central to the historical continuity of set theory in the broadest sense with category theory. Yet Lavine talks only of Dedekind's foundations of analysis. While the logicians who created the Zermelo-Fraenkel and related axioms looked largely to the foundations of analysis, set theory got its practical importance from much wider applications.

Lavine offers an original second-order axiomatization of set theory, based on ordinals and functions, in which a set is explicitly defined as the image of some function from an initial segment of the ordinals. That is, a set is defined to be well orderable. The axioms aim to capture Cantor's conception of sets prior to about 1891 (p. 80–82, with theorems in ff.). But Cantor rejected this conception as he realized the importance of power sets (p. 96–97). With power sets, some principles which had seemed obvious came to seem problematic (on this, Lavine cites Moore [12]).

In this connection, Lavine finds that "The Axiom of Extensionality, which is often taken to be constitutive of the notion of set, is curiously difficult to locate in Cantor's writings" (p. 86). Like Hallett [4] he tries to find extensionality implicit in various things Cantor said but has to admit it is a strain. In fact extensionality is only constitutive of the notion of set adopted by later logicians including Zermelo. The elements of a Cantorian cardinal have no properties but distinctness from one another, and no identity outside of that cardinal, so that extensionality is meaningless for these cardinals. Zermelo objected to this as unworkable (Cantor [1], n. 1, p. 351) but it does

work today in categorical set theory (see Lawvere [5]).

The book describes Cantor's relation to Lebesgue integration (p. 49–51) as well as to Dedekind, Frege, and Peano. Later chapters have much to say on Russell and Zermelo, and less extensive remarks on many others.

***The historical argument*** Lavine claims finite mathematics “shows *how* non-trivial principles concerning the infinite are self-evident: they are extrapolated from principles concerning the indefinitely large” [emphasis added] (p. 303). His historical chapters aim to show *that* some principles on the infinite are self-evident. The key to this is “disposing of . . . the argument based on the supposed historical facts that there have been strong disagreements about what is self-evident and that supposedly self-evident principles have led to contradictions” (p. 156).

Of course he describes Cantor and Russell as disagreeing. Most pointedly, Russell found paradoxes in set theory where Cantor found none (p. 63). But Lavine takes this not to be a disagreement over what is self-evident on the infinite—because Cantor and Russell were not talking about the same concept of sets or collections. I think his argument proves far too much, if it proves anything at all.

As various people have said in various terms, Cantor had a combinatorial notion of set where existence of sets is independent of specification. Peano and Russell had a logical notion, where set existence is closely tied to the means of set specification. Lavine argues that “The Cantorian notion of a combinatorial collection is not merely different from the Peano-Russell notion of a logical collection—it arose in opposition to it” (p. 77).

Contrast another disagreement:

It is, after all, possible that you could come to believe the continuum hypothesis, while I came to believe its negation. (I am assuming, of course, that the continuum hypothesis is independent of the axioms we agree on, as will almost certainly be the case given the present state of knowledge about sets.) If that were to happen, it would then be clear that we were not using the words set and member in the same way. (p. 236)

We can drop the hypothetical. Maddy ([7], p. 500) notes that “while established opinion among more mature members of the Cabal [group of set theorists] is against CH, younger members are sympathetic to [various arguments for CH].” Are these people not disagreeing about what is or is not evident (after long expert consideration) about the infinite?

Lavine does not hold that difference of conception precludes all real disagreement. He contrasts the *iterative conception* of sets, expressed by the Axiom of Foundation, to noniterative conceptions which allow, say, for sets to be members of themselves. He argues that no serious mathematical or pragmatic features favor either one. But he says “Whatever defense the iterative conception has must be philosophical” (p. 147) and goes on to look at such defenses.

He even says “Zermelo's collections are of a different sort from Cantor's,” because of the way Zermelo coordinates power sets and choice. (I would add Zermelo's rejection of Cantor's cardinals.) And yet “I see the one sort as an outgrowth of the other, and so I have used the same term,” that is, he calls both combinatorial collections (p. 114).

Lavine owes us a clear criterion to decide which differences of conception preclude disagreement over evidence, and which do not. Until then he has not “dispos[ed] of . . . the argument based on the supposed historical facts that there have been strong disagreement[s] about what is self-evident” on the infinite (p. 156).

His other historical thesis is that “The paradoxes never caused any trouble for the combinatorial conception of set. Our sense of what is true has been quite reliable” (p. 214). But this is defensible only if “trouble” is taken to mean specifically a “crisis of foundations.” Paradoxes troubled Hilbert and his protégé Zermelo enough to produce the modern axioms. Lavine says “Zermelo discovered [Russell’s] paradox independently, but little is known about the details” (p. 60). Now details are in Peckhaus [17] and [18]. Around Göttingen it was called “Zermelo’s paradox.” Hilbert lectured on it and the related “Hilbert paradox” as important problems for logic, demonstrating the need for an axiomatic set theory. As Zermelo produced that theory he deviated considerably from Cantor’s original conception.

So I cannot accept Lavine’s theses. But his arguments are provocative reading, covering a great deal of ground in mathematics, history of logic, and contemporary logic.

**Arbitrary functions** Lavine puts a lot of emphasis on “the general definition of an arbitrary function” (p. 35). By this he means a set of ordered pairs with the familiar properties. He calls this a “function given by its graph alone” (p. 35) and I will call it a “point-set function.” Of course this is routine in histories of set theory. But precisely because Lavine brings in so much of the analytical context, I will take a moment to mention the inadequacy of the routine.

The two most widely used kinds of non-point-set functions come straight from Lavine’s favorite example, which he mentions often:

The paradigm example [of an arbitrary function] is surely Fourier’s: the temperature of a bar as a function of distance from a given point on the bar. The values are not computed but determined by factors external to the mathematician, and they are not constrained in any way [e.g., they need not be continuous]. (p. 242)

Temperature today is not defined at any single point. It is defined by mean kinetic energy in some region, or energy exchange between finitely extended systems. The theory uses *measurable functions* and *generalized functions* (or *distributions*), both closely related to Fourier’s work.

A measurable function  $f$  from the real numbers to the real numbers has an average value over any interval of real numbers, but no specific value  $f(x)$  for any specific real number  $x$ . It can be interpreted as an equivalence class of Lebesgue measurable point-set functions, where among other things any two point-set functions differing at only finitely many points are equivalent. Lavine (pp. 46–51) discusses *Lebesgue measurability* and its ties to Fourier series and Cantor’s work.

Rudin [20] is typical of introductory analysis books in straining between mathematical usage and the point-set definition of “functions.” He defines certain well-known spaces of measurable functions and then says such a space is actually

*not a space whose elements are functions, but a space whose elements are equivalence classes of functions.* For the sake of simplicity of language, it is, however, customary to relegate this distinction to the status of a tacit understanding

and to continue to speak of [such a space] as a space of functions. We shall follow this custom. [his emphasis] (p. 69)

He follows custom for the rest of the book. Outside the opening pages of introductory texts, the term “measurable function” is almost invariably used as I use it here. Certainly this suits the applications. No matter how you define your mathematical terms, physical thermodynamic quantities are means over regions and not values at points (see, for example, Martin and England [9]).

Fourier’s work on temperature also used a kind of Fourier transform, and work with those transforms later led to generalized functions. A generalized function  $T$  from the reals to the reals has no specific value for any specific argument. But for any suitable point-set function  $g$  the product  $g \cdot T$  has a specific integral over the real line—and the point is that formal rules of the calculus apply freely to generalized functions, ignoring continuity and so forth. The famous example is Dirac’s  $\delta$ , defined by saying: for any suitable point-set function  $g$  from the reals to the reals, the integral of  $g \cdot \delta$  over the whole line is  $g(0)$ . These are central to work in partial differential equations today. Liverman [6] briskly motivates the ideas.

There are two practically important ways to interpret generalized functions by point-set functions. A generalized function  $T$  from the reals to the reals can be construed as an equivalence class of suitable series of point-set functions from the reals to the reals. Or it can be construed as an operator, so that if  $g$  is a suitable point-set function from the reals to the reals then  $T(g)$  is a real value, which we regard as the integral of  $g \cdot T$  over the line.

Another kind of non-point-set function lies close to Cantor’s original term for cardinality, which was “power (Machtigkeit).” Cantor wrote

I have borrowed the expression “power” from J. Steiner, who used it in a very special but related sense, to say that two figures can be put in projective relation to one another, so that each element of the one corresponds to one and only one element of the other. ([1], p. 151)

Steiner ([22], p. 1) shows any two conic sections have the same power. For example, you can place a circle inside a parabola. Each half-line out from the center projects a point of the circle to a unique point of the parabola, and vice versa. At least, that is so if we add a “point at infinity” where the two sides of the parabola rejoin. But nineteenth century geometers knew that comparable studies of higher degree figures—such as cubics and quartics—need rational functions.

A rational function  $f$  from a figure A to another B has a well-defined value  $f(x)$  for nearly every point  $x$  of A, but usually cannot be defined for all points of A even allowing points at infinity. So Reid [19] says they are not really functions. Yet he says the established term “rational function” is unavoidable and “students who disapprove are recommended to give up at once and take a reading course in Category Theory instead” (p. 4). In the simplest cases a rational function to the real numbers (or another field, such as the complex numbers) is patched together out of ratios  $f/g$  of polynomials, with  $g$  not constant 0. More general notions of algebraic figure need more complicated versions of rational functions, right up to Grothendieck’s scheme theory where we will not follow.

Among these examples, only measurable functions are coeval with their interpretation by point-set functions. They were invented to give an explicitly arithme-

tized foundation for the theory of integration. Generalized functions and rational functions had geometric/analytic origins long before they got set theoretic explanations.

These and other examples are called functions with good mathematical right, despite textbooks nodding to the point-set definition as stipulative. Even on thoroughly set theoretic foundations of mathematics, the functions in widespread use go far beyond point-set functions.

**Finite mathematics** Mycielski calls a theory locally finite if every finite set of its theorems has a finite model. Of course ZF set theory is not locally finite. But Mycielski [14] shows that for every first-order theory T there is a locally finite theory Fin(T) and a simple translation between T and Fin(T) preserving provability and consistency.

Sentences of T translate into Fin(T) by relativizing the quantifiers to a series of domains  $\Omega_0, \Omega_1$ , and so on, each seen as larger than the ones before, but all as finite. Take the ZF axiom of infinity stated in the form

$$\exists x(0 \in x \ \& \ \forall y(y \in x \longrightarrow \{y, \{y\}\} \in x)).$$

This becomes the Fin(ZF) axiom that Lavine calls “Zillion”:

$$(\exists x \in \Omega_0)(0 \in x \ \& \ (\forall y \in \Omega_1), (y \in x \longrightarrow \{y, \{y\}\} \in x)).$$

That is, from the viewpoint of  $\Omega_1$  the postulated “x” contains zero and is closed under successor. But  $\Omega_1$  itself need not be closed under successor, so “x” may be finite by ending with some element not in  $\Omega_1$ .

The axioms of Fin(T) are (1) translates of all axioms of T and (2) axiom schemes on handling the domains  $\Omega$ . In fact the domains have positive rational subscripts, to make it easier to shuffle them together. One axiom scheme yields any sentence

$$(\forall x \in \Omega_p)x \in \Omega_q$$

with  $p$  less than  $q$ . So domains are successively larger. Another says in effect that if  $S$  is a theorem of Fin(T) then so is any sentence  $S'$  gotten by replacing the domains of  $S$  with new ones, respecting the order of subscripts.

Full details are in Lavine’s book or [14] but a rough example from Fin(ZF) may be helpful. In Fin(ZF) the axiom Zillion has companions

$$(\exists x \in \Omega_p)(0 \in x \ \& \ (\forall y \in \Omega_q), (\in x\{y, \{y\}\} \in x))$$

for any rational numbers  $p$  less than  $q$ . Call the displayed sentence “Zillion<sub>pq</sub>” to indicate the domains. Consider a finite model for Zillion<sub>01</sub> and Zillion<sub>02</sub> plus corresponding axioms of extensionality and pair set. It turns out that  $\Omega_2$  must be strictly larger than  $\Omega_1$ , in such a way that the set “x” postulated by Zillion<sub>12</sub> must be larger than that postulated by Zillion<sub>01</sub>.

A proof in ZF may call on the axiom of infinity some finite number of times. Then it corresponds to a proof in Fin(ZF) calling on finitely many sentences Zillion<sub>pq</sub> with successively larger subscripts postulating successively larger sets. This works to prove translates of all ZF theorems, yet any finite list of Fin(ZF) theorems has a finite model. In fact Mycielski proved for any first-order theory T:

T is consistent if and only if every finite subset of the theorems of  $\text{Fin}(T)$  has a finite model.

This is Theorem 3.4 in Lavine's book, and we will cite it that way.

Translation in the opposite direction is partial but very simple. In any formula of  $\text{Fin}(T)$  such that the domains  $W$  occur only to relativize quantifiers as described above, you simply drop the relativizing domains to get a formula of  $T$ . Every  $Zillion_{pq}$  translates to the usual axiom of infinity of ZF. Lavine calls this *extrapolation* from  $\text{Fin}(T)$  to  $T$  and says "this process . . . is my proposed formal analysis of what we actually do" when we pass from experience of the indefinitely large to theories of the infinite (p. 257).

A key step towards Lavine's interpretation is Pawlikowski's [16] finitary refinement of Theorem 3.4. Mycielski proved it in Peano Arithmetic. Pawlikowski proved it in Primitive Recursive Arithmetic, PRA. Lavine restates the theorem in the Primitive Recursive theory of Words on a given finite alphabet, PRW, which is equivalent in strength to PRA. Lavine gives and discusses the axioms of PRA and PRW in an Appendix (pp. 203–12), and outlines Pawlikowski's proof in a Technical Remark (p. 275).

Lavine also shows how to interpret PRA as the theory of hereditarily finite pure sets by, in effect, Gödel numbering the hfp sets. He sketches an axiomatization of PRS, the Primitive Recursive theory of hereditarily finite pure Sets, without using PRA. He says "The details seem to be uncontroversial" but the axiomatization would "require a tediously large number both of primitives and axioms" so he does not give it (p. 212).

For Lavine, even the finitary proof of Theorem 3.4 is not enough to make finite mathematics epistemologically significant. "For that,  $\text{Fin}(T)$  must have *reasonable models*, not just crazy ones" (p. 276, his emphasis). That is, if we want to justify our claim to know infinitary facts of  $T$  by saying we have experience of finite models for finite parts of  $\text{Fin}(T)$ , those finite models must be suitably contained in models of  $T$ . In fact such finite models are always available, but this cannot even be stated in general in finitary terms as there is no general finitary description of models for a first-order theory  $T$ .

Here Lavine makes his main technical contribution to finite mathematics: He argues that the suitable models for parts of  $\text{Fin}(ZF)$  are those containing only hereditarily finite pure sets and interpreting the empty set and membership by the actual empty set and membership. He calls these the *natural* models. Then he sketches a proof in PRS that ZFC is consistent if and only if every finite part of  $\text{Fin}(ZFC)$  has a natural model. Lavine uses ZFC (ZF plus Axiom of Choice) where Mycielski uses ZF, but this seems to make no particular difference. The last two chapters include extensive metatheoretical investigation of this use of natural models, with comparison to Peano Arithmetic and its natural models.

Natural models are Lavine's starting point for judging new axiom candidates for set theory. He suggests considerations that might show one model is *more natural* than another—so we could argue that sentences extrapolated from more natural models are more plausible axiom candidates (e.g., pp. 313–14). This is a big difference between Lavine's take on finite mathematics and Mycielski's. Mycielski is interested in using finite mathematics to investigate consistency but not the truth of new candidate

axioms for set theory. For example, [14] announced a paper (cited on Lavine's p. 293) on "Finite intuitions supporting the consistency of ZF and ZF+AD" where AD is the Axiom of Determinacy (see [7]). But from this he would not conclude that the Axiom of Determinacy is true, since it contradicts the Axiom of Choice and he believes that "ZFC and its extensions define all of mathematics" (Mycielski [13], p. 626).

Lavine, like Mycielski, makes passing remarks on Non-Standard Analysis. This should be pursued further. Besides giving far more substantial applications, Nelson [15] offers nearly opposite motives to Lavine's for a kind of finitism. Lavine makes his point "as strongly as possible" by saying "suppose that Peano Arithmetic is inconsistent and there are really only finitely many numbers." He says he cannot take the idea very seriously, but even if it were so "it would be perfectly possible for all of finite mathematics to survive" since finite mathematics needs only finite models (p. 276). For Lavine, finitism lies in the metatheory.

Nelson implies a very different argument. If PA is inconsistent (as Nelson thinks it might be) the problem will surely lie in the induction scheme. This would not show there are only finitely many numbers. It would show induction cannot hold for all properties. Let us suppose it is so for a moment, and take any purported simply infinite system, that is, any model for the successor axioms plus induction for some properties. By assumption there are other properties for which induction fails in that model. Using induction on one of those other properties we get a smaller ersatz for a simply infinite system inside the first one. The interplay between the two systems gives Non-Standard Analysis.

Even if PA is consistent (as I suspect it is) we can still set up such pairs of systems for fragments of the induction scheme—or as Nelson does by adding a new predicate and contrasting induction with and without that predicate. We used infinite models to describe this. But the Non-Standard Analysis actually lives in an axiomatic theory, which is finitist in the sense that it is itself a kind of arithmetic.

***Does this explain our intuitions of the infinite?*** Much of Lavine's work is to interpret finite mathematics philosophically: "Though the theory is Mycielski's, he did not interpret it as a theory of indefinitely large size, and he has not commented on its relation to any intuitive picture" (p. 250). Lavine takes indefinitely large size as the link between our experience and the infinite:

The idea of indefinitely large size is a familiar one that comes out of daily experience. It is the idea of too many to count...the idea of a zillion...There is no skeptical problem comparable to that concerning the infinite since we encounter indefinitely large quantities all the time: zillions of grains of sand on a beach, zillions of stars, zillions of homeless people, zillions of books we wish we had time to read . . . a zillion dishes that need to be washed. (p. 248)

Of course this is relative to context and interests: those zillion dishes will be easy to count after they are washed. Astronomers estimate the number of stars in our galaxy. Lavine claims "Infinite is nothing more than too large to count—too large in a context independent sense, too large for anyone to count independent of context, or abilities, or interest" (p. 248).

Thus far I agree. But this invokes nothing like  $\text{Fin}(\text{ZF})$ . I will call this the idea of the "bare infinite." The harder problem is to explain the "Cantorian finite": How did

we discover that the size of some barely infinite collections is nonetheless “finite” in the classical sense (from the Ionian philosophers through Hegel) of determinate and knowable? Specifically, how did Cantor come to believe he could get determinate results from “counting” beyond the arithmetically finite? Lavine likes the idea of the Cantorian finite but unfortunately calls it “Hallett’s courageous turn of phrase” (p. 289) since Hallett [4] uses it well. It is due to Mayberry and is a major theme in his ([10], §2).

When Lavine says “fully developed set theory as a matter of psychological and historical fact rests on Cantor’s progression and extrapolation from the idea of indefinitely large size” (p. 249) he refers to the Cantorian finite. The concept of the barely infinite is no fully developed set theory.

Yet he says “I am not claiming that Cantor was self-consciously relying on a picture of the indefinitely large, though . . . he was indeed self-consciously relying on the method of extrapolating from the finite” (p. 289). In fact

I am not claiming that anyone ever consciously set out to obtain a theory of the infinite based on experience of indefinitely large size. I am claiming that the picture of the infinite—of the use of the ellipsis—with which the founders of set theory started was one which developed out of their experience of indefinitely large size, even though that source was largely unconscious. (p. 251)

And

in the absence of a finite set theory that incorporates the indefinitely large, the creators of set theory had ordinary set theory as the only apparent option for making sense of their intuitions. (p. 265)

These quoted passages seem to undercut Lavine’s argument. According to them, the indefinitely large in Lavine’s and Mycielski’s specific sense supplied only a picture, the picture in the ellipsis “. . .”. This picture seems to me to involve nothing like Mycielski’s finite mathematics. I find it a picture of the bare infinite—a picture of going on forever. And Lavine himself says that when the creators of set theory sought a determinate theory they did not and could not draw on any theory of the indefinitely large finite.

Finite mathematics is a fascinating link between the finite and the infinite, but I think it is not at the psychological or historical origin of infinitary set theory. That origin lies in specific work by Cantor and others, beginning with the ideas of equipotence and well-ordering. It is a story of specific mathematical insights, as recounted by Moore, Hallett, Maddy, and others—though I think we still need a broader view of the context, as for example of Dedekind’s role and of functions.

Lavine argues that “the ‘explanation’ that [the Axiom of] Choice holds because the Omnipotent Mathematician has the power to pick members out of infinitely many nonempty sets is not so much an explanation of Choice as an exhortation to believe that Choice is true.” What “does indeed explain the self-evidence of Choice” (p. 290) for Lavine is the fact that it clearly holds for finite sets and extrapolates in the precisely defined sense. But besides the Omnipotent Mathematician there is a very different, and by now classical line of arguments drawn from the role of Choice in mathematics. These are expounded by Maddy [7] and [8], for example, along with arguments for the other standard axioms and a number of controversial new axiom candidates. I find these much more explanatory than invoking Fin(ZFC).

But I have not at all exhausted Lavine's arguments. He claims the distinction between sets and proper classes has a more "natural and inevitable" motivation in finite set theory than in ZF (p. 318). He claims to solve "the problem of why Limitation of Size and Limitation of Comprehensiveness are principles about the same things, the combinatorial sets, instead of principles of two separate set theories about two different kinds of sets" (p. 290). He gives arguments on the role of the Axiom of Foundation and alternatives to it.

Alongside his main argument, he has a lot of fun working with finite mathematics. He goes into formalism and various constructivisms. He discusses first-order schemata versus second-order logic, concluding that "we should reject any foundational role for full second-order logic" (p. 325). He gives a dual argument that: finite mathematics "shows that one could coherently deny the existence of infinite sets without doing violence to mathematical practice" and yet we should not do so (p. 252ff). More topics come up than I can mention here. The book is sweeping, often debatable, but always informed and involving.

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