

Abraham Robinson and Nonstandard Analysis: History, Philosophy, and Foundations of Mathematics

Mathematics is the subject in which we don't know* what we are talking about.

—Bertrand Russell

* Don't care would be more to the point.

—Martin Davis

I never understood why logic should be reliable everywhere else, but not in mathematics.

—A. Heyting

1. Infinitesimals and the History of Mathematics

Historically, the dual concepts of infinitesimals and infinities have always been at the center of crises and foundations in mathematics, from the first “foundational crisis” that some, at least, have associated with discovery of irrational numbers (more properly speaking, incommensurable magnitudes) by the pre-socratic Pythagoreans¹, to the debates that are currently waged between intuitionists and formalist—between the descendants of Kronecker and Brouwer on the one hand, and of Cantor and Hilbert on the other. Recently, a new “crisis” has been identified by the constructivist Erret Bishop:

There is a crisis in contemporary mathematics, and anybody who has

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not noticed it is being willfully blind. *The crisis is due to our neglect of philosophical issues. . . .*²

Bishop, too, relates his crisis in part to the subject of the infinite and infinitesimals. Arguing that formalists mistakenly concentrate on the "truth" rather than the "meaning" of a mathematical statement, he criticizes Abraham Robinson's nonstandard analysis as "formal finesse," adding that "it is difficult to believe that debasement of meaning could be carried so far."³ Not all mathematicians, however, are prepared to agree that there is a crisis in modern mathematics, or that Robinson's work constitutes any debasement of meaning at all.

Kurt Gödel, for example, believed that Robinson more than anyone else had succeeded in bringing mathematics and logic together, and he praised Robinson's creation of nonstandard analysis for enlisting the techniques of modern logic to provide rigorous foundations for the calculus using actual infinitesimals. The new theory was first given wide publicity in 1961 when Robinson outlined the basic idea of his "nonstandard" analysis in a paper presented at a joint meeting of the American Mathematical Society and the Mathematical Association of America.⁴ Subsequently, impressive applications of Robinson's approach to infinitesimals have confirmed his hopes that nonstandard analysis could enrich "standard" mathematics in important ways.

As for his success in defining infinitesimals in a rigorously mathematical way, Robinson saw his work not only in the tradition of others like Leibniz and Cauchy before him, but even as vindicating and justifying their views. The relation of their work, however, to Robinson's own research is equally significant, as Robinson himself realized, and this for reasons that are of particular interest to the historian of mathematics. Before returning to the question of a "new" crisis in mathematics due to Robinson's work, it is important to say something, briefly, about the history of infinitesimals, a history that Robinson took with the utmost seriousness.

This is not the place to rehearse the long history of infinitesimals in mathematics. There is one historical figure, however, who especially interested Robinson—namely, Cauchy—and in what follows Cauchy provides a focus for considering the historiographic significance of Robinson's own work. In fact, following Robinson's lead, others like J. P. Cleave, Charles Edwards, Detlef Laugwitz, and W. A. J. Luxemburg have used nonstandard analysis to rehabilitate or "vindicate" earlier infinitesimalists.⁵ Leibniz, Euler, and Cauchy are among the more promi-

nent mathematicians who have been rationally reconstructed—even to the point of having had, in the views of some commentators, “Robinsonian” nonstandard infinitesimals in mind from the beginning. The most detailed and methodically sophisticated of such treatments to date is that provided by Imre Lakatos; in what follows, it is his analysis of Cauchy that is emphasized.

2. Lakatos, Robinson, and Nonstandard Interpretations of Cauchy’s Infinitesimal Calculus

In 1966, Lakatos read a paper that provoked considerable discussion at the International Logic Colloquium meeting that year in Hannover. The primary aim of Lakatos’s paper was made clear in its title: “Cauchy and the Continuum: The Significance of Non-standard Analysis for the History and Philosophy of Mathematics.”⁶ Lakatos acknowledged his exchanges with Robinson on the subject of nonstandard analysis as he continued to revise the working draft of his paper. Although Lakatos never published the article, it enjoyed a rather wide private circulation and eventually appeared after Lakatos’s death in volume 2 of his *Mathematics, Science and Epistemology*.

Lakatos realized that two important things had happened with the appearance of Robinson’s new theory, indebted as it was to the results and techniques of modern mathematical logic. He took it above all as a sign that metamathematics was turning away from its original philosophical beginnings and was growing into an important branch of mathematics.⁷ This view, now more than twenty years later, seems fully justified.

The second claim that Lakatos made, however, is that nonstandard analysis revolutionizes the historian’s picture of the history of the calculus. The grounds for this assertion are less clear—and in fact, are subject to question. Lakatos explained his interpretation of Robinson’s achievement as follows at the beginning of his paper:

Robinson’s work . . . offers a rational reconstruction of the discredited infinitesimal theory which satisfies modern requirements of rigour and which is no weaker than Weierstrass’s theory. This reconstruction makes infinitesimal theory an almost respectable ancestor of a fully-fledged, powerful modern theory, lifts it from the status of pre-scientific gibberish and renews interest in its partly forgotten, partly falsified history.⁸

But consider the word *almost*. Robinson, says Lakatos, only makes the achievements of earlier infinitesimalists *almost* respectable. In fact,

Robinson's work in the twentieth century cannot vindicate Leibniz's work in the seventeenth century, Euler's in the eighteenth century, or Cauchy's in the nineteenth century. There is nothing in the language or thought of Leibniz, Euler, or Cauchy (to whom Lakatos devotes most of his attention) that would make them early Robinsonians. The difficulties of Lakatos's rational reconstruction, however, are clearer in some of the details he offers.

For example, consider Lakatos's interpretation of the famous theorem from Cauchy's *Cours d'analyse* of 1821, which purports to prove that the limit of a sequence of continuous functions $s_n(x)$ is continuous. This is what Lakatos, in the spirit of Robinson's own reading of Cauchy, has to say:

In fact *Cauchy's theorem was true and his proof as correct as an informal proof can be*. Following Robinson . . . Cauchy's argument, if not interpreted as a proto-Weierstrassian argument but as a genuine Leibniz-Cauchy one, runs as follows: . . .

$s_n(x)$ should be defined and continuous and converge not only at standard Weierstrassian points but at *every* point of the "denser" Cauchy continuum, and . . . the sequence $s_n(x)$ should be defined for infinitely large indices n and represent continuous functions at such indices.⁹

In one last sentence, this is all summarized in startling terms as follows:

Cauchy made absolutely no mistake, he only proved a completely different theorem, about transfinite sequences of functions which Cauchy-converge on the Leibniz continuum.¹⁰

But upon reading Cauchy's *Cours d'analyse*—or either of his later presentations of the theorem in his *Résumés analytiques* of 1833 or in the *Comptes Rendues* for 1853—one finds no hint of *transfinite* indices, sequences, or Leibnizian continua made "denser" than standard intervals by the addition of infinitesimals. Cauchy, when referring to infinitely large numbers $n' > n$, has "very large"—but finite—numbers in mind, not *actually infinite* Cantorian-type transfinite numbers.¹¹

This is unmistakably clear from another work Cauchy published in 1833—*Sept leçons de physique générale*—given at Turin in the same year he again published the continuous sum theorem. In the *Sept leçons*, however, Cauchy explicitly denies the existence of infinitely large numbers for their allegedly contradictory properties.¹²

Moreover, if Lakatos was mistaken about Cauchy's position concerning the actually infinite, he was also wrong about Cauchy's continuum

being one of Leibnizian infinitesimals. If, by virtue of such infinitesimals, Cauchy's original proof had been correct all along, why would he then have issued a *revised* version in 1853, explicitly to improve upon the earlier proofs? Instead, were Lakatos and Robinson correct in their rational reconstructions, all Cauchy would need to have done was point out the nonstandard meaning of his infinitesimals—explaining how infinitely large and infinitely small numbers had given him a correct theorem, as well as a proof, all along.

Lakatos also draws some rather remarkable conclusions about *why* the Leibnizian version of nonstandard analysis failed:

The downfall of Leibnizian theory was then not due to the fact that it was inconsistent, but that it was capable only of limited growth. It was the heuristic potential of growth—and explanatory power—of Weierstrass's theory that brought about the downfall of infinitesimals.¹³

This rational reconstruction may complement the overall view Lakatos takes of the importance of research programs in the history of science, but it does no justice to Leibniz or to the subsequent history of the calculus in the eighteenth and early nineteenth centuries, which (contrary to Lakatos) demonstrates that (i) in the eighteenth century the (basically Leibnizian) calculus constituted a theory of considerable power in the hands of the Bernoullis, Euler, and many others; and (ii) the real stumbling block to infinitesimals *was* their acknowledged inconsistency.

The first point is easily established by virtue of the remarkable achievements of eighteenth-century mathematicians who used the calculus because it was powerful—it produced striking results and was indispensable in applications.¹⁴ But it was also suspect from the beginning, and precisely because of the question of the contradictory nature of infinitesimals.

This brings us to the second point: despite Lakatos's dismissal of their inconsistency, infinitesimals were perceived even by Newton and Leibniz, and certainly by their successors in the eighteenth century, as problematic precisely because of their contradictory qualities. Newton was specifically concerned with the fact that infinitesimals did not obey the Archimedean axiom and therefore could not be accepted as part of rigorous mathematics.¹⁵ Leibniz was similarly concerned about the logical acceptability of infinitesimals. The first public presentation of his differential calculus in 1684 was severely determined by his attempt to *avoid* the logical difficulties connected with the infinitely small. His article in the *Acta Eruditorum* on maxima and minima, for example, presented the differen-

tial as a finite line segment rather than the infinitely small quantity that was used in practice.¹⁶

This confusion between theoretical considerations and practical applications carried over to Leibniz's metaphysics of the infinite, for he was never committed to any one view but made conflicting pronouncements. Philosophically, as Robinson himself has argued, Leibniz had to assume the reality of the infinite—the infinity of his monads, for example—or the reality of infinitesimals not as mathematical points but as substance- or force-points—namely, Leibniz's "monads" themselves.¹⁷

That the eighteenth century was concerned not with doubts about the potential of infinitesimals but primarily with fears about their logical consistency is clear from the proposal Lagrange drew up for a prize to be awarded by the Berlin Academy for a rigorous theory of infinitesimals. As the prize proposal put it:

It is well known that higher mathematics continually uses infinitely large and infinitely small quantities. Nevertheless, geometers, and even the ancient analysts, have carefully avoided everything which approaches the infinite; and some great modern analysts hold that the terms of the expression *infinite magnitude* contradict one another.

The Academy hopes, therefore, that it can be explained how so many true theorems have been deduced from a contradictory supposition, and that a principle can be delineated which is sure, clear—in a word, truly mathematical—which can appropriately be substituted for *the infinite*.¹⁸

Lakatos seems to appreciate all this—and even contradicts himself on the subject of Leibniz's theory and the significance of its perceived inconsistency. Recalling his earlier assertion that Leibniz's theory was not overthrown because of its inconsistency, consider the following line, just a few pages later, where Lakatos asserts that nonstandard analysis raises the problem of "how to appraise *inconsistent* theories like Leibniz's calculus, Frege's logic, and Dirac's delta function."¹⁹

Lakatos apparently had not made up his mind as to the significance of the inconsistency of Leibniz's theory, which raises questions about the historical value and appropriateness of the extreme sort of rational reconstruction that he has proposed to "vindicate" the work of earlier generations. In fact, neither Leibniz nor Euler nor Cauchy succeeded in giving a satisfactory foundation for an infinitesimal calculus that also demonstrated its logical consistency. Basically, Cauchy's "epsilonotics"

were a means of avoiding infinities and infinitesimals. Nowhere do Robinsonian infinitesimals or justifications appear in Cauchy's explanations of the rigorous acceptability of his work.²⁰

Wholly apart from what Lakatos and others like Robinson have attempted in reinterpreting earlier results in terms of nonstandard analysis, it is still important to understand Robinson's own reasons for developing his historical knowledge in as much detail—and with as much scholarship—as he did. For Robinson, the history of infinitesimals was more than an antiquarian interest; it was not one that developed with advancing age or retirement, but was a *simultaneous* development that began with his discovery of nonstandard analysis in the early 1960s. Moreover, there seem to have been serious reasons for Robinson's keen attention to the history of mathematics as part of his own "research program" concerned with the future of nonstandard analysis.

3. Nonstandard Analysis and the History of Mathematics

In 1965, in a paper titled "On the Theory of Normal Families," Robinson began with a short look at the history of mathematics.²¹ He noted that for about one hundred and fifty years after its inception in the seventeenth century, mathematical analysis developed vigorously on inadequate foundations. Despite this inadequacy, the precise, quantitative results produced by the leading mathematicians of that period have stood the test of time.

In the first half of the nineteenth century, however, the concept of the limit, advocated previously by Newton and d'Alembert, gained ascendancy. Cauchy, whose influence was instrumental in bringing about the change, still based his arguments on the intuitive concept of an infinitely small number as a variable tending to zero. At the same time, however, he set the stage for the formally more satisfactory theory of Weierstrass, and today deltas and epsilons are the everyday language of the calculus, at least for most mathematicians. It was this precise approach that paved the way for the formulation of more general and more abstract concepts. Robinson used this history to explain the importance of compactness as applied to functions of a complex variable, which had led to the theory of normal families developed largely by Paul Montel. There followed the qualitative development of complex variable theory, such as Picard theory, and, finally, against this background, more quantitative theories like those developed by Rolf Nevanlinna—to whom Robinson's paper was dedicated as part of a *Festschrift*.

The historical notes to be found at the beginning of Robinson's paper were echoed again at the end, when he turned to ask whether the results he had achieved using nonstandard analysis couldn't be achieved just as well by standard methods. Although he admitted that because of the transfer principle (developed in his paper of 1961, "Non-Standard Analysis") this was indeed possible, he added that such translations into standard terms usually complicated matters considerably. As for nonstandard analysis and the use of infinitesimals it permitted, his conclusion was emphatic:

Nevertheless, we venture to suggest that our approach has a certain natural appeal, as shown by the fact that it was preceded in history by a long line of attempts to introduce infinitely small and infinitely large numbers into Analysis.²²

And so the reason for the historical digression was its usefulness in serving a much broader purpose than merely introducing some rather remote historical connections between Newton, Leibniz, Paul Montel, and Rolf Nevanlinna. History could serve the mathematician as propaganda. Robinson was apparently concerned that many mathematicians were prepared to adopt a "so what" attitude toward nonstandard analysis because of the more familiar reduction that was always possible to classical foundations. There were several ways to outflank those who chose to minimize nonstandard analysis because, theoretically, it could do nothing that wasn't equally possible in standard analysis. Above all, nonstandard analysis was often simpler and more intuitive in a very direct, immediate way than standard approaches. But, as Robinson also began to argue with increasing frequency and in greater detail, *historically* the concept of infinitesimals had always seemed natural and intuitively preferable to more convoluted and less intuitive sorts of rigor. Now that nonstandard analysis showed *why* infinitesimals were safe for consumption in mathematics, there was no reason not to exploit their natural advantages. The paper for Rolf Nevanlinna was meant to exhibit both the technical applications and, at least in part through its appeal to history, the naturalness of nonstandard analysis in developing the theory of normal families.

4. Foundations and Philosophy of Mathematics

If Robinson regarded the history of infinitesimals as an aid to the justification in a very general way of nonstandard analysis, what contribution did it make, along with his results in model theory, to the founda-

tions and philosophy of mathematics? Stephan Körner, who taught a philosophy of mathematics course with Robinson at Yale in the fall of 1973, shortly before Robinson's death early the following year, was doubtless closest to Robinson's maturest views on the subject.²³ Basically, Körner sees Robinson as a follower of—or at least working in the same spirit as—Leibniz and Hilbert. Like Leibniz (and Kant after him), Robinson rejected any empirical basis for knowledge about the infinite—whether in the form of infinitely large or infinitely small quantities, sets, whatever. Leibniz is famous for his view that infinitesimals are useful fictions—a position deplored by such critics as Nieuwentijt or the more flamboyant and popular Bishop Berkeley, whose condemnation of the Newtonian calculus might equally well have applied to Leibniz.²⁴ Leibniz adopted both the infinitely large and the infinitely small in mathematics for pragmatic reasons, as permitting an economy of expression and an intuitive, suggestive, heuristic picture. Ultimately, there was nothing to worry about since the mathematician could eliminate them from his final result after having infinitesimals and infinities to provide the machinery and do the work of a proof.

Leibniz and Robinson shared a similar view of the ontological status of infinities and infinitesimals. They are not just fictions, but well-founded ones—“*fictiones bene fundatae*,” in the sense that their applications prove useful in penetrating the complexity of natural phenomena and help to reveal relationships in nature that purely empirical investigations would never produce.

As Emil Borel once said of Georg Cantor's transfinite set theory (to paraphrase not too grossly): although he objected to transfinite numbers or inductions in the formal presentation of finished results, it was certainly permissible to use them to discover theorems and create proofs—again, whatever works.²⁵ It was only necessary to be sure that in the final version they were eliminated, thus making no official appearance. Robinson, however, was interested in more, especially in the reasons *why* the mathematics worked as it did, and in particular why infinities and infinitesimals were now admissible as rigorous entities despite centuries of doubts and attempts to eradicate them entirely.

Here Robinson succeeded where Leibniz and his successors failed. Leibniz, for example, never demonstrated the consistent foundations of his calculus, for which his work was sharply criticized by Nieuwentijt, among others. Throughout the eighteenth century, the troubling foundations (real-

ly, lack of foundations) of the Leibnizian infinitesimal calculus continued to bother mathematicians, until the epsilon-delta methods of Cauchy and the "arithmetic" rigor of Weierstrass reestablished analysis on acceptably finite terms. Because, as Körner remarks, "Leibniz's approach was considered irremediably inconsistent, hardly any efforts were made to improve this delimitation."²⁶

Robinson was clearly not convinced of the inconsistency of infinitesimals, and in developing the methods of Skolem (who had advanced the idea of nonstandard arithmetic) he was led to consider the possibility of nonstandard analysis. At the same time, his work in model theory and mathematical logic contributed not only to his creation of nonstandard analysis, but to his views on the foundations of mathematics as well.

5. Robinson and "Formalism 64"

In the 1950s, working under the influence of his teacher Abraham Fraenkel, Robinson seems to have been satisfied with a fairly straightforward philosophy of Platonic realism. But by 1964, Robinson's philosophical views had undergone considerable change. In a paper titled simply "Formalism 64," Robinson emphasized two factors in rejecting his earlier Platonism in favor of a formalist position:

- (i) Infinite totalities do not exist in any sense of the word (i.e., either really or ideally). More precisely, any mention, or purported mention, of infinite totalities is, literally, *meaningless*.
- (ii) Nevertheless, we should continue the business of Mathematics "as usual," i.e. we should act *as if* infinite totalities really existed.²⁷

Georg Kreisel once commented that, as he read Robinson's "Formalism 64," it was not clear to him whether Robinson meant 1864 or 1964! Robinson, however, was clearly responding in his views on formalism to research that had made a startling impression upon mathematicians only in the previous year—namely, Paul Cohen's important work in 1963 on forcing and the independence of the continuum hypothesis.

As long as it appeared that the accepted axiomatic systems of set theory (the Zermelo-Fraenkel axiomatization, for example) were able to cope with all set theoretical problems that were of interest to the working mathematician, belief in the existence of a unique "universe of sets" was almost unanimous. However, this simple view of the situation was severely shaken in the 1950s and early 1960s by two distinct developments. One of these was Cohen's proof of the independence of the continuum hypothesis,

which revealed a great disparity between the scale of transfinite ordinals and the scale of cardinals—or power sets. As Robinson himself noted in an article in *Dialectica*, the relation “is so flexible that it seems to be quite beyond control, at least for now.”²⁸

The second development of concern to Robinson was the emergence of new and varied axioms of infinity. Although the orthodox Platonist believes that in the real world such axioms must either be true or false, Robinson found himself persuaded otherwise. Despite his new approach to foundations in “Formalism 64,” he was not dogmatic, but remained flexible:

The development of “meaningless” infinitistic theories may at some future date become so unsatisfactory to me that I shall be willing to acknowledge the greater intellectual seriousness of some form of constructivism. But I cannot imagine that I shall ever return to the creed of the true platonist, who sees the world of the actual infinite spread out before him and believes that he can comprehend the incomprehensible.²⁹

6. Erret Bishop: Meaning, Truth, and Nonstandard Analysis

Incomprehensible, however, is what some of Robinson’s critics have said, almost literally, of nonstandard analysis itself. Of all Robinson’s opponents, at least in public, none has been more vocal—or more vehement—than Erret Bishop.

In the summer of 1974, it was hoped that Robinson and Bishop would actually have a chance to discuss their views in a forum of mathematicians and historians and philosophers of mathematics who were invited to a special Workshop on the Evolution of Modern Mathematics held at the American Academy of Arts and Sciences in Boston. Garrett Birkhoff, one of the workshop’s organizers, had intended to feature Robinson as the keynote speaker for the section of the Academy’s program devoted to foundations of mathematics, but Robinson’s unexpected death in April of 1974 made this impossible. Instead, Erret Bishop presented the featured paper for the section on foundations. Birkhoff compared Robinson’s ideas with those of Bishop in the following terms:

During the past twenty years, significant contributions to the foundations of mathematics have been made by two opposing schools. One, led by Abraham Robinson, claims Leibnizian antecedents for a “non-standard analysis” stemming from the “model theory” of Tarski. The other (smaller) school, led by Errett Bishop, attempts to reinterpret

Brouwer's "intuitionism" in terms of concepts of "constructive analysis."³⁰

Birkhoff went on to describe briefly (in a written report of the session) the spirited discussions following Bishop's talk, marked, as he noted, "by the absence of positive reactions to Bishop's view."³¹ Even so, Bishop's paper raised a fundamental question about the philosophy of mathematics, which he put simply as follows: "As pure mathematicians, we must decide whether we are playing a game, or whether our theorems describe an external reality."³² If these are the only choices, then one's response is obviously limited. For Robinson, the excluded middle would have to come into play here—for he viewed mathematics, in particular the striking results he had achieved in model theory and nonstandard analysis, as constituting much more than a meaningless game, although he eventually came to believe that mathematics did *not* necessarily describe any external reality. But more of Robinson's own metaphysics in a moment.

Bishop made his concerns over the crisis he saw in contemporary mathematics quite clear in a dramatic characterization of what he took to be the pernicious efforts of historians and philosophers alike. Not only is there a crisis at the foundations of mathematics, according to Bishop, but a very real *danger* (as he put it) in the role that historians seemed to be playing, along with nonstandard analysis itself, in fueling the crisis:

I think that it should be a fundamental concern to the historians that what they are doing is potentially dangerous. The superficial danger is that it will be and in fact has been systematically distorted in order to support the status quo. And there is a deeper danger: it is so easy to accept the problems that have historically been regarded as significant as actually being significant.³³

Interestingly, in his own historical writing, Robinson sometimes made the same point concerning the triumph, as many historians (and mathematicians as well) have come to see it, of the success of Cauchy-Weierstrassian epsilon-delta over infinitesimals in making the calculus "rigorous" in the course of the nineteenth century. In fact, one of the most important achievements of Robinson's work in nonstandard analysis has been his conclusive demonstration of the poverty of this kind of historicism—of the mathematically Whiggish interpretation of increasing rigor over the mathematically unjustifiable "cholera bacillus" of infinitesimals, to use Georg Cantor's colorful description.³⁴

As for nonstandard analysis, Bishop had this to say at the Boston meeting:

A more recent attempt at mathematics by formal finesse is nonstandard analysis. I gather that it has met with some degree of success, whether at the expense of giving significantly less meaningful proofs I do not know. My interest in nonstandard analysis is that attempts are being made to introduce it into calculus courses. It is difficult to believe that debasement of meaning could be carried so far.³⁵

Two things deserve comment here. The first is that Bishop (surprisingly, in light of some of his later comments about nonstandard analysis) does not dismiss it as *completely* meaningless, but only asks whether its proofs are “significantly less meaningful” than constructivist proofs. Leaving open for the moment what Bishop has in mind here for “meaningless” in terms of proofs, it seems clear that by one useful indicator to which Bishop refers, nonstandard analysis is year-by-year showing itself to be increasingly “meaningful.”³⁶

Consider, for example, the pragmatic value of nonstandard analysis in terms of its application in teaching the calculus. Here it is necessary to consider the success of Jerome Keisler’s textbook *Elementary Calculus: An Approach Using Infinitesimals*, which uses nonstandard analysis to explain in an introductory course the basic ideas of calculus. The issue of its pedagogic value will also serve to reintroduce, in a moment, the question of meaning in a very direct way.

Bishop claims that the use of nonstandard analysis to teach the calculus is wholly pernicious. He says this explicitly:

The technical complications introduced by Keisler’s approach are of minor importance. The real damage lies in his obfuscation and devitalization of those wonderful ideas. No invocation of Newton and Leibniz is going to justify developing calculus using [nonstandard analysis] on the grounds that the usual definition of a limit is too complicated! . . .

Although it seems to be futile, I always tell my calculus students that mathematics is not esoteric: it is commonsense. (Even the notorious ϵ, δ definition of limit is commonsense, and moreover is central to the important practical problems of approximation and estimation.) They do not believe me.³⁷

One reason Bishop’s students may not believe him is that what he claims,

in fact, does not seem to be true. There is another side to this as well, for one may also ask whether there is any truth to the assertions made by Robinson (and emphatically by Keisler) that "the whole point of our infinitesimal approach to calculus is that it is easier to define and explain *limits* using infinitesimals."³⁸ Of course, this claim also deserves examination, in part because Bishop's own attempt to dismiss Keisler's methods as being equivalent to the axiom " $0 = 1$ " is simply nonsense.³⁹ In fact, there are concrete indications that despite the allegations made by Bishop about obfuscation and the nonintuitiveness of basic ideas in nonstandard terms, exactly the *opposite* is true.

Not long ago a study was undertaken to assess the validity of the claim that "from this nonstandard approach, the definitions of the basic concepts [of the calculus] become simpler and the arguments more intuitive."⁴⁰ Kathleen Sullivan reported the results of her dissertation, written at the University of Wisconsin and designed to determine the pedagogical usefulness of nonstandard analysis in teaching calculus, in the *American Mathematical Monthly* in 1976. This study, therefore, was presumably available to Bishop when his review of Keisler's book appeared in 1977, in which he attacked the pedagogical validity of nonstandard analysis. What did Sullivan's study reveal? Basically, she set out to answer the following questions:

Will the students acquire the basic calculus skills? Will they really understand the fundamental concepts any differently? How difficult will it be for them to make the transition into standard analysis courses if they want to study more mathematics? Is the nonstandard approach only suitable for gifted mathematics students?⁴¹

To answer these questions, Sullivan studied classes at five schools in the Chicago-Milwaukee area during the years 1973-74. Four of them were small private colleges, the fifth a public high school in a suburb of Milwaukee. The same instructors who had taught the course previously agreed to teach one introductory course using Keisler's book (the 1971 edition) as well as another introductory course using a standard approach (thus serving as a control group) to the calculus. Comparison of SAT scores showed that both the experimental (nonstandard) group and the standard (control) group were comparable in ability before the courses began. At the end of the course, a calculus test was given to both groups. Instructors teaching the courses were interviewed, and a questionnaire was filled out by everyone who has used Keisler's book within the last five years.

The single question that brought out the greatest difference between the two groups was question 3:

Define $f(x)$ by the rule
$$\begin{cases} f(x) = x^2 & \text{for } x \neq 2; \\ f(x) = 0 & \text{for } x = 2. \end{cases}$$

Prove using the definition of limit that $\lim_{x \rightarrow 2} f(x) = 4$.

	<i>Control Group</i> (68 students)	<i>Experimental Group</i> (68 students)
Did not attempt	22	4
Standard arguments:		
Satisfactory proof	2	14
Correct statement, faulty proof	15	14
Incorrect arguments	29	23
Nonstandard arguments:		
Satisfactory proof		25
Incorrect arguments		2

The results, as shown in the accompanying tabulation, seem to be striking; but, as Sullivan cautions:

Seeking to determine whether or not students really do perceive the basic concepts any differently is not simply a matter of tabulating how many students can formulate proper mathematical definitions. Most teachers would probably agree that this would be a very imperfect instrument for measuring understanding in a college freshman. But further light on this and other questions can be sought in the comments of the instructors.⁴²

Here, too, the results are remarkable in their support of the heuristic value of using nonstandard analysis in the classroom. It would seem that, contrary to Bishop's views, the traditional approach to the calculus may be the more pernicious. Instead, the new nonstandard approach was praised in strong terms by those who actually used it:

The group as a whole responded in a way favorable to the experimental method on every item: the students learned the basic concepts of the calculus more easily, proofs were easier to explain and closer to intuition, and most felt that the students end up with a better understanding of the basic concepts of the calculus.⁴³

As to Bishop's claim that the *d,e* method is "commonsense,"⁴⁴ this too is open to question. As one teacher having successfully used Keisler's book remarked, "When my most recent classes were presented with the epsilon-delta definition of limit, they were outraged by its obscurity compared to what they had learned [via nonstandard analysis]."⁴⁵

But as G. R. Blackley warned Keisler's publishers (Prindle, Weber and Schmidt) in a letter when he was asked to review the new textbook prior to its publication:

Such problems as might arise with the book will be *political*. It is revolutionary. Revolutions are seldom welcomed by the established party, although revolutionaries often are.⁴⁶

The point to all of this is simply that, if one take *meaning* as the standard, as Bishop urges, rather than *truth*, then it seems clear that by its own success nonstandard analysis has indeed proven itself *meaningful* at the most elementary level at which it could be introduced—namely, that at which calculus is taught for the first time. But there is also a *deeper* level of meaning at which nonstandard analysis operates—one that also touches on some of Bishop's criticisms. Here again Bishop's views can also be questioned and shown to be as unfounded as his objections to nonstandard analysis pedagogically.

Recall that Bishop began his remarks in Boston at the American Academy of Arts and Sciences workshop in 1974 by stressing the crisis in contemporary mathematics that stemmed from what he perceived as a misplaced emphasis upon formal systems and a lack of distinction between the ideas of "truth" and "meaning." The choice Bishop gave in Boston was between mathematics as a meaningless game or as a discipline describing some objective reality. Leaving aside the question of whether mathematics *actually* describes reality, in some objective sense, consider Robinson's own hopes for nonstandard analysis, those beyond the purely technical results he expected the theory to produce. In the preface to his book on the subject, he hoped that "some branches of modern Theoretical Physics might benefit directly from the application of non-standard analysis."⁴⁷

In fact, the practical advantages of using nonstandard analysis as a branch of applied mathematics have been considerable. Although this is not the place to go into detail about the increasing number of results arising from nonstandard analysis in diverse contexts, it suffices here to mention impressive research using nonstandard analysis in physics, especially

quantum theory and thermodynamics, and in economics, where study of exchange economies has been particularly amenable to nonstandard interpretation.⁴⁸

7. Conclusion

There is another purely theoretical context in which Robinson considered the importance of the history of mathematics that also warrants consideration. In 1973, Robinson wrote an expository article that drew its title from a famous monograph written in the nineteenth century by Richard Dedekind: *Was sind und was sollen die Zahlen?* This title was roughly translated—or transformed in Robinson's version—as “Numbers—What Are They and What Are They Good For?” As Robinson put it: “Number systems, like hair styles, go in and out of fashion—it's what's underneath that counts.”⁴⁹

This might well be taken as the leitmotiv of much of Robinson's mathematical career, for his surpassing interest since the days of his dissertation written at the University of London in the late 1940s was model theory, and especially the ways in which mathematical logic could not only illuminate mathematics, but have very real and useful applications within virtually all of its branches. In discussing number systems, he wanted to demonstrate, as he put it, that

the collection of all number systems is not a finished totality whose discovery was complete around 1600, or 1700, or 1800, but that it has been and still is a growing and changing area, sometimes absorbing new systems and sometimes discarding old ones, or relegating them to the attic.⁵⁰

Robinson, of course, was leading up in his paper to the way in which nonstandard analysis had again broken the bounds of the traditional Cantor-Dedekind understanding of the real numbers, especially as they had been augmented by Cantorian transfinite ordinals and cardinals.

To make his point, Robinson turned momentarily to the nineteenth century and noted that Hamilton had been the first to demonstrate that there was a larger arithmetical system than that of the complex numbers—namely, that represented by his quaternions. These were soon supplanted by the system of vectors developed by Josiah Willard Gibbs of Yale and eventually transformed into a vector calculus. This was a more useful system, one more advantageous in the sorts of applications for which quaternions had been invented.

Somewhat later, another approach to the concept of number was taken by Georg Cantor, who used the idea of equinumerosity in terms of one-to-one correspondences to define numbers. In fact, for Cantor a cardinal number was a symbol assigned to a set, and the same symbol represented all sets equivalent to the base set. The advantage of this view of the nature of numbers, of course, was that it could be applied to infinite sets, producing transfinite numbers and eventually leading to an entire system of transfinite arithmetic. Its major disadvantage, however, was that it led Cantor to reject adamantly any mathematical concept of infinitesimal.⁵¹

As Robinson points out, although the eventual fate of Cantor's theory was a success story, it was not entirely so for its author. Despite the clear utility of Cantor's ideas, which arose in connection with his work on trigonometric series (later applied with great success by Lebesgue and others at the turn of the century), it was highly criticized by a spectrum of mathematicians, including, among the most prominent, Kronecker, Frege, and Poincaré. In addition to the traditional objection that the infinite should not be allowed in rigorous mathematics, Cantor's work was also questioned because of its abstract character. Ultimately, however, Cantor's ideas prevailed, despite criticism, and today set theory is a cornerstone, if not the major foundation, upon which much of modern mathematics rests.⁵²

There was an important lesson to be learned, Robinson believed, in the eventual acceptance of new ideas of number, despite their novelty or the controversies they might provoke. Ultimately, utilitarian realities could not be overlooked or ignored forever. With an eye on the future of non-standard analysis, Robinson was impressed by the fate of another theory devised late in the nineteenth century that also attempted, like those of Hamilton, Cantor, and Robinson, to develop and expand the frontiers of number.

In the 1890s, Kurt Hensel introduced a whole series of new number systems, his now familiar p -adic numbers. Hensel realized that he could use his p -adic numbers to investigate properties of the integers and other numbers. He also realized, as did others, that the same results could be obtained in other ways. Consequently, many mathematicians came to regard Hensel's work as a pleasant game; but, as Robinson himself observed, "Many of Hensel's contemporaries were reluctant to acquire the techniques involved in handling the new numbers and thought they constituted an unnecessary burden."⁵³

The same might be said of nonstandard analysis, particularly in light

of the transfer principle that demonstrates that theorems true in ${}^*\mathbf{R}$ can also be proven for $\mathbf{R}$ by standard methods. Moreover, many mathematicians are clearly reluctant to master the logical machinery of model theory with which Robinson developed his original version of nonstandard analysis. This problem has been resolved by Keisler and Luxemburg, among others, who have presented nonstandard analysis in ways accessible to mathematicians without their having to take up the difficulties of mathematical logic as a prerequisite.⁵⁴ But for those who see nonstandard analysis as a fad that may be a currently pleasant game, like Hensel's p -adic numbers, the later history of Hensel's ideas should give skeptics an example to ponder. For today, p -adic numbers are regarded as coequal with the reals, and they have proven a fertile area of mathematical research.

The same has been demonstrated by nonstandard analysis. Its applications in areas of analysis, the theory of complex variables, mathematical physics, economics, and a host of other fields have shown the utility of Robinson's own extension of the number concept. Like Hensel's p -adic numbers, nonstandard analysis can be avoided, although to do so may complicate proofs and render the basic features of an argument less intuitive.

What pleased Robinson as much about nonstandard analysis as the interest it engendered from the beginning among mathematicians was the way it demonstrated the indispensability, as well as the power, of technical logic:

It is interesting that a method which had been given up as untenable has at last turned out to be workable and that this development in a concrete branch of mathematics was brought about by the refined tools made available by modern mathematical logic.⁵⁵

Robinson had begun his career as a mathematician by studying set theory and axiomatics with Abraham Fraenkel in Jerusalem, which eventually led to his Ph.D. from the University of London in 1949.⁵⁶ His early interest in logic was later amply repaid in his applications of logic to the development of nonstandard analysis. As Simon Kochen once put it in assessing the significance of Robinson's contributions to mathematical logic and model theory:

Robinson, via model theory, wedded logic to the mainstreams of mathematics At present, principally because of the work of

Abraham Robinson, model theory is just that: a fully-fledged theory with manifold interrelations with the rest of mathematics.⁵⁷

Kurt Gödel valued Robinson's achievement for similar reasons: it succeeded in uniting mathematics and logic in an essential, fundamental way. That union has proved to be not only one of considerable mathematical importance, but of substantial philosophical and historical content as well.

Notes

1. There is a considerable literature on the subject of the supposed crisis in mathematics associated with the Pythagoreans. See, for example, (Hasse and Scholz 1928). For a recent survey of this debate, see (Berggren 1984; Dauben 1984; Knorr 1975).

2. (Bishop 1975, 507).

3. (Bishop 1975, 513-14).

4. Robinson first published the idea of nonstandard analysis in a paper submitted to the Dutch Academy of Sciences (Robinson 1961).

5. (Cleave 1971; Edwards 1979; Laugwitz 1975, 1985; Luxemburg 1975).

6. (Lakatos 1978).

7. (Lakatos 1978, 43).

8. (Lakatos 1978, 44).

9. (Lakatos 1978, 49).

10. (Lakatos 1978, 50). Emphasis in original.

11. Cauchy offers his definitions of infinitely large and small numbers in several works, first in the *Cours d'analyse*, subsequently in later versions without substantive changes. See (Cauchy 1821, 19; 1823, 16; 1829, 265), as well as (Fisher 1978).

12. (Cauchy 1868).

13. (Lakatos 1978, 54).

14. For details of the successful development of the early calculus, see (Boyer 1939; Grattan-Guinness 1970, 1980; Grabiner 1981; Youshkevitch 1959).

15. (Newton 1727, 39), where he discusses the contrary nature of indivisibles as demonstrated by Euclid in Book X of the *Elements*. For additional analysis of Newton's views on infinitesimals, see (Grabiner 1981, 32).

16. See (Leibniz 1684). For details and a critical analysis of what is involved in Leibniz's presentation and applications of infinitesimals, see (Bos 1974-75; Engelsman 1984).

17. See (Robinson 1967, 35 [in Robinson 1979, 544]).

18. In (Lagrange 1784, 12-13; Dugac 1980, 12). For details of the Berlin Academy's competition, see (Grabiner 1981, 40-43; Youshkevitch 1971, 149-68).

19. (Lakatos 1978, 59). Emphasis added.

20. See (Grattan-Guinness 1970, 55-56), where he discusses "limit-avoidance" and its role in making the calculus rigorous.

21. (Robinson 1965b).

22. (Robinson 1965b, 184); also in (Robinson 1979, vol. 2, 87).

23. I am grateful to Stephan Körner and am happy to acknowledge his help in ongoing discussions we have had of Robinson and his work.

24. For a recent survey of the controversies surrounding the early development of the calculus, see (Hall 1980).

25. Borel in a letter to Hadamard, in (Borel 1928, 158).

26. (Körner 1979, xlii). Körner notes, however, that an exception to this generalization is to be found in Hans Vaihinger's general theory of fictions. Vaihinger tried to justify infinitesimals by "a method of opposite mistakes," a solution that was too imprecise, Körner suggests, to have impressed mathematicians. See (Vaihinger 1913, 511ff).

27. (Robinson 1965a, 230; Robinson 1979, 507). Nearly ten years later, Robinson recalled the major points of "Formalism 64" as follows: "(i) that mathematical theories which, allegedly, deal with infinite totalities do not have any detailed meaning, i.e. reference, and (ii) that this has no bearing on the question whether or not such theories should be developed and that, indeed, there are good reasons why we should continue to do mathematics in the classical fashion nevertheless." Robinson added that nothing since 1964 had prompted him to change these views and that, in fact, "well-known recent developments in set theory represent evidence favoring these views." See (Robinson 1975, 557).

28. (Robinson 1970, 45-49).

29. (Robinson 1970, 45-49).

30. (Birkhoff 1975, 504).

31. (Birkhoff 1975, 504).

32. (Bishop 1975, 507).

33. (Bishop 1975, 508).

34. For Cantor's views, see his letter to the Italian mathematician Vivanti in (Meschkowski 1965, 505). A general analysis of Cantor's interpretation of infinitesimals may be found in (Dauben 1979, 128-32, 233-38). On the question of rigor, see (Grabiner 1974).

35. (Bishop 1975, 514).

36. It should also be noted, if only in passing, that Bishop has not bothered himself, apparently, with a careful study of nonstandard analysis or its implications, for he offhandedly admits that he only "gathers that it has met with some degree of success" (Bishop 1975, 514; emphasis added).

37. (Bishop 1977, 208).

38. (Keisler 1976, 298), emphasis added; quoted in (Bishop 1977, 207).

39. (Bishop 1976, 207).

40. (Sullivan 1976, 370). Note that Sullivan's study used the experimental version of Keisler's book, issued in 1971. Bishop reviewed the first edition published five years later by Prindle, Weber and Schmidt. See (Keisler 1971, 1976).

41. (Sullivan 1976, 371).

42. (Sullivan 1976, 373).

43. (Sullivan 1976, 383-84).

44. (Bishop 1977, 208).

45. (Sullivan 1976, 373).

46. (Sullivan 1976, 375).

47. (Robinson 1966, 5).

48. See especially (Robinson 1972a, 1972b, 1974, 1975), as well as (Dresden 1976) and (Voros 1973).

49. (Robinson 1973, 14).

50. (Robinson 1973, 14).

51. For details, see (Dauben 1979).

52. See (Dauben 1979).

53. (Robinson 1973, 16).

54. (Luxemburg 1962, 1976; Keisler 1971).

55. (Robinson 1973, 16).

56. Robinson completed his dissertation, *The Metamathematics of Algebraic Systems*, at Birkbeck College, University of London, in 1949. It was published two years later; see (Robinson 1951).

57. (Kochen 1976, 313).

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- . 1853. Note sur les séries convergentes dont les divers termes sont des fonctions continues d'une variable réelle ou imaginaire, entre des limites données. *Comptes Rendus* 36: 454-59. In *Oeuvres complètes d'Augustin Cauchy*, vol. 12 (1st ser.). Paris: Gauthier-Villars, 1900, pp. 30-36.
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