Chapter 5

INCOMMENSURABILITY IN MATHEMATICS

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Abstract: In this paper, as part of an argument for the existence of revolutions in mathematics, I argue that there is incommensurability in mathematics. After devising a framework sensitive to meaning change and to changes in the extension of mathematical predicates, I consider two case studies that illustrate different ways in which incommensurability emerge in mathematical practice. The most detailed case involves nonstandard analysis, and the existence of different notions of the continuum. But I also examine how incommensurability found its way into set theory. I conclude by examining some consequences that incommensurability has to mathematical practice.

Key words: Incommensurability, nonstandard analysis, set theory, mathematical practice.

1. INTRODUCTION

To make sense of mathematical practice, it is important to determine whether there are—or there aren’t—revolutions in mathematics.\(^1\) For depending on the existence of such revolutions, mathematical practice is understood in substantively different ways. For example, together with the existence of revolutionary changes in mathematics comes the existence of incommensurable mathematical theories. But is it possible that there is incommensurability in mathematics? If it is, we should be sensitive to radical meaning changes and changes in the extension of mathematical predicates, and we should identify the strategies developed in mathematical practice to deal with these changes.

In this paper, as part of an argument for the existence of revolutions in mathematics, I argue that, just as there is incommensurability in physical theories (see Kuhn [1962]), there is also incommensurability in mathematics.

\(^1\) For several perspectives on the issue of revolutions in mathematics, see Gillies (ed.) [1992].

This may seem unexpected, for reference to mathematical terms is often taken not to change in time. But I argue that this widespread assumption about mathematics is not warranted.

To make this case, I will consider a couple of case studies. The most detailed one involves nonstandard analysis, and the existence of different notions of the continuum (see Robinson [1974] and Lakatos [1978a]). As Lakatos and Robinson argued, in the 19th century there were two conceptions of the continuum: a Leibnizian conception, which was rich and included infinitesimals, and a Weierstrassian conception, which was simpler and excluded infinitesimals. These different conceptions had different implications about which results were true in analysis. That is, depending on the notion of the continuum that was adopted, different results were obtained. In particular, assuming the Leibnizian conception, it follows that every convergent series of continuous functions always has a continuous limit function. But this result cannot be extended to the Weierstrassian conception.

Although both Lakatos and Robinson have discussed the existence of these two conceptions of the continuum, they failed to appreciate the implication that this fact has for the incommensurability of mathematical notions. The existence of these distinct conceptions of the continuum highlights the existence of differences in the process of fixing the reference of mathematical terms and, consequently, differences in the results that are true in analysis.

I will also discuss, much more briefly, how incommensurability found its way into set theory. When Zermelo developed the first axiomatic formulation of set theory (see Zermelo [1904], [1908a] and [1908b]), he was explicitly trying to vindicate Cantor’s conception of set. What he produced, however, was a significantly different notion of set—a broader and more abstract notion than Cantor’s. Despite the fact that Zermelo was able to get some of the results that Cantor was trying to establish (such as the well-ordering theorem), the results were established in a framework that incorporated a notion of set that was non-Cantorian in significant ways (e.g. Cantor would never accept a unrestricted power set axiom; see, e.g., Lavine [1994]).

To make sense of the existence of incommensurable notions in mathematics, it’s crucial to have an account of mathematical practice—an account that is more sensitive to meaning change as well as change in the extension of mathematical predicates. I’ll sketch below a framework that should help making sense of these aspects of mathematical practice—highlighted in the case studies under discussion—and that should be general enough to be applicable to other cases. As will become clear, the framework incorporates important insights provided by Lakatos (in
particular, Lakatos [1976] and [1978c]) as well as by Azzouni [1994]. I’ll start by outlining the framework, and examining the bearing of the incommensurability issue on mathematical practice. This will pave the way to the discussion of the case studies later in the paper.

2. THE INCOMMENSURABILITY PROBLEM

What is the problem of incommensurability of mathematical theories? Briefly, the problem is that different mathematical theories—about the same domain (for example, the domain of sets)—may characterize the relevant objects in radically different ways. And there is no common standard to decide which of the two characterizations (if any) is the correct one. Depending on the framework that is assumed, a different answer regarding the correctness of the characterization is provided. If we assume the framework of one theory, we get one answer; using the framework of the other theory, we get a different one. And there’s no common measure to decide between them.

Consider, for example, two different set theories: Zermelo-Fraenkel (ZF) and von Neumann-Bernays-Gödel (NBG). The former quantifies only over sets and is not finitely axiomatizable; the latter quantifies not only over sets but also over proper classes, and is finitely axiomatizable. Despite these differences, the two set theories are equiconsistent (that is, ZF is consistent if, and only if, NBG is also consistent). Given ZF and NBG, which sets are we referring to when we refer to sets? How can we distinguish ZF sets from NBG sets? It’s only in the context of each set theory that one can try to make such a distinction. But sets play exactly the same role in each theory. Given the equiconsistency between the two theories, there will always be interpretations of the formalism of the theories in which counterparts to sets in one theory play the role of sets in the other theory. ‘Set’ thus becomes indeterminate between ZF sets and NBG sets. There are no common standards to determine to which sets we are referring when we refer to sets. ZF and NBG are incommensurable.

But if there are incommensurable mathematical theories, what is the significance of this fact? What difference would it make to mathematical practice, and to our understanding of this practice? I think the existence of incommensurability in mathematics—similarly to its corresponding counterpart in science—has significant implications, not only to the practice of mathematics, but, in particular, to our understanding of the latter. First, if there are, for example, incommensurable theories of sets (as I think there

\footnote{Of course, I’m not considering proper classes that are not quantified over in ZF.}
are), the mathematician cannot claim that the sets characterized in one set theory are actually the same as those characterized in another theory. The fact that both theories have enough in common to be considered theories of sets is not enough to guarantee that they characterize the same objects. (It's not even clear that the theories in question characterize the same type of objects, given that some set theories quantify over objects that are not sets.) Similarly, the fact that classical and relativistic mechanics have enough in common to be considered mechanics is not enough to guarantee that they characterize, and refer to, the same objects: Newtonian mass is not dependent on velocity, Einsteinian mass is.

Moreover, if the mathematician cannot claim that the sets studied in different set theories are different (or the same), how can he or she identify such sets with each other? That is, how can the mathematician identify sets across different set theories? It might be completely arbitrary to carry out such identification, and as a result, the nature of the sets in question is left unspecified. For example, it won't be determined whether an acceptable set theory should distinguish sets and proper classes. The answer would ultimately depend on the set theory one accepts, but ZF and NBG provide conflicting approaches to the issue.

Problems about identification of mathematical objects across different theories have motivated the development of certain forms of structuralism about mathematics. According to Michael Resnik (1997), for example, there is no fact of the matter as to whether objects in different mathematical structures are the same or not. The mathematical theories in question—and the resulting structures—are simply silent about the issue. As long as we follow the structuralist's insight that all that matters in mathematics are the structures in question (rather than the objects that exemplify such structures), the fact that we might be unable to identify the objects is no longer significant. Objects, as it were, "drop out" of the picture.

This means, in turn, that the structuralist may end up being committed to the incommensurability of mathematical notions—at least at the level of mathematical objects. The problem, however, is that once this first step is taken, the incommensurability will be extended to the level of structures as well. After all, if there is no way of identifying mathematical objects across different mathematical structures, there's no way of identifying the corresponding structures either. Any identification of structures presupposes, at least, the existence of some mapping from the objects of one structure to the objects of the other. But, given the emphasis on objects, this is exactly the presupposition that the structuralist doesn't grant. Moreover, even if the existence of such mappings were stipulated, it wouldn't guarantee that the structures in question are the same. For example, in the case of first-order structures with infinite domains, there will always be structures that satisfy
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exactly the same statements (they are elementarily equivalent), but which are not isomorphic. So there will be differences in structure, and the structuralist would acknowledge that. But which of these structures are we referring to? Without simply assuming that the structures in question can be identified, there's no way of providing a positive answer to this question. But making this assumption amounts to assuming the point in discussion.

Note that advocating the existence of incommensurability in mathematics does not entail that there's no genuine disagreement in mathematics. If incommensurable mathematical theories exist, then such theories may—or may not—be about different objects. Of course, if it turns out that the theories are not about the same objects, there need not be any disagreement between them. They are not rival theories after all. (Different mathematical theories may deal with different entities, or, at least, not necessarily the same ones.) But, and this is the significant case, incommensurable mathematical theories can still be about the same objects, in which case there might be disagreement between them. The point is only that incommensurability leaves the issue about the existence of disagreement in mathematics open. And any account of mathematical practice has to accommodate the existence of disagreement in mathematics.

But things are still more complex. After all, often mathematicians identify objects across different mathematical theories—e.g. by stipulation (see Azzouni [1994]). Making such identification often simplifies the description and the development of mathematical theories. In this way, connections across different mathematical theories are established, and information from different domains can be transferred. But stipulation wouldn't be enough to overcome the incommensurability problem. On what grounds is the stipulation made? Is it warranted? Any answer to these questions presupposes that we have already correctly identified the relevant objects, and that's exactly what is at issue.

3. A DILEMMA

Despite being fruitful, the attempt to identify mathematical objects from different theories seems to raise the following dilemma. Either the notions from different mathematical theories are identified or they are not. If they are not identified, we are unable to claim, say, that the old notions are being explained by the new ones—in particular, we cannot claim that whatever turned out to be right about the old notions can be recaptured by the new formalism. If the notions are identified, we have incoherence, given that the old and the new notions are not the same. After all, even though they may seem to lead to the same results in certain contexts, they have different
properties (for example, ZF sets are not finitely axiomatizable, NBG sets are). Moreover, even in the cases in which the same results can be derived in each theory, the meaning of these results change from one theory to the other. Which theory should be used to interpret the results: the new or the old one? Given that sets do not have the same properties in each theory, the interpretation of the results changes when we move from the old to the new theory. As a result, the incommensurability issue returns. Thus, it's misleading to identify old and new notions. As noted above, examples of this scenario are found both in mathematics (different conceptions of set, different conceptions of the continuum), and in physics (Newtonian and Einsteinian notions of mass).

Someone may try to respond to this dilemma by examining the process through which mathematical objects are identified across different theories. Certainly, stipulation plays an important role here (Azzouni’s emphasis on this point is exactly right). But, as Azzouni recognizes, mere stipulation cannot be the whole story. There are pragmatic reasons to stipulate the identification of certain mathematical objects with other such objects (say, natural numbers with certain sets). These reasons range from the development of more systematic, and in some cases, simpler mathematical theories to the formulation of new mathematical results. It’s not surprising, then, that mathematical objects are routinely identified across different theories. Thus, if the identification process turns out to depend ultimately on stipulation, this emerges from good, pragmatic reasons.

But the identification of mathematical objects (via stipulation) is also guided by structural considerations. Isomorphism, and often partial isomorphism, play a significant role in motivating the chosen stipulation. What such isomorphisms allow is, as it were, the transferring of structure from one domain into another, the transferring of information about properties of certain objects into information about others. The existence of an isomorphism between two structures—or the embedding of one structure into the other—justifies the identification of certain mathematical objects based on the underlying structures.3

However, to stipulate that certain mathematical objects should be identified doesn’t overcome the incommensurability problem. After all, despite the identification, there are still multiple instantiations of the resulting structures. Which of them are we referring to? It’s not enough to claim (with the structuralist) that it doesn’t matter; as long as the same

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3 Moves of this sort certainly lend plausibility to structuralism in the philosophy of mathematics. But structuralism doesn’t provide the whole picture, given that, as noted above, (i) there is still underdetermination at the level of structures, and (ii) mathematical objects are still being identified with each other, despite the difficulties of making the nature of these objects precise.
properties are satisfied in each structure. After all, different structures may satisfy exactly the same mathematical properties (as non-standard models of arithmetic and analysis clearly illustrate). Thus, even if we stipulate that, say, natural numbers are such and such sets, there will be other models of set theory in which different sets will exemplify the same properties as the sets with which we have initially identified the numbers. Which sets are we referring to? Looking at their mathematical properties alone, there’s no way to tell. And what else could we look at?

In trying to overcome this difficulty, one could examine the mechanisms of referential access to mathematical entities employed in mathematical practice. Is there anything in this practice that undermines the incommensurability of mathematical notions? I don’t think so. After all, there is no way of linking, or tying in a unique way, mathematical notions and their referents, as the presence of nonstandard models and different interpretations of mathematical formalisms clearly indicate. This generates, as a result, referential indeterminacy. But referential indeterminacy leads to incommensurability, given that with the lack of any tight connection between mathematical notions and their referents, it’s not clear that there is any common measure (or standard) to determine uniquely which object a given mathematical notion refers to.

Let me elaborate on this point. The mechanism of reference to mathematical objects articulated in mathematical practice emerges from the exploration of mathematical systems. A mathematical system is a collection of mathematical principles and a logic (often left implicit) to draw conclusions from such principles. The mathematical principles need not be formulated in any particular formal language (usually, they are expressed in mathematical English, or whatever extension of natural language that includes appropriate mathematical terms). Intuitively, the objects (whatever they may be) that satisfy such mathematical principles are the ones mathematicians refer to. Typically, there will be many different objects satisfying the same description. For example, in the case of (first-order) mathematical theories with infinite domains, there will always be nonstandard models of the theories in which the same results will come out true, even though the extension of the mathematical predicates of these theories is radically different from model to model. Clearly, there is no other way to refer to mathematical objects but through the language of the

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4 Even in the case of second-order mathematical theories, there will be such nonstandard models. As is well known, besides standard semantics, second-order logic also has Henkin models, that is, models in which the second-order variables do not range over the full power set of the universe of individuals. With Henkin models, second-order logic has the same metalogical features as first-order logic, including the existence of nonstandard models via the Löwenheim-Skolem theorem (for details, see, e.g., Shapiro [1991]).
relevant mathematical system, and it’s through this language—and only through it—that we can “grasp” such objects. (Of course, there’s no causal contact or instrumental access to mathematical objects.) As a result, and for the reasons discussed above, there is no unique way of specifying the objects we are referring to—and incommensurability follows.

Of course, “fixing” the reference up to isomorphism doesn’t help here, given that despite the existence of an isomorphism between two structures, the objects that are referred to in each of them can be radically different. To which of these objects are we referring? Going structuralist doesn’t help either, because, as we saw, there is incommensurability at the level of structures too (e.g. different types of set instantiate the same number-theoretic properties). Hence, the incommensurability problem still stands.

Perhaps a different perspective on incommensurability in mathematics is provided by a particularly strong form of platonism: full-blooded platonism (FBP; see Balaguer [1998]). According to FBP, every logically possible mathematical theory is actually true of some part of the mathematical realm. The mathematical realm is a realm of plenitude, where, for example, Cantorian and non-Cantorian sets, Hilbert spaces, nonstandard models, differential equations and unusual topologies are all found “side by side”, as it were. The only requirement for the existence of a mathematical entity is that it is referred to by a consistent mathematical theory.

Can FBP solve the problem of the incommensurability of mathematical theories? If every logically possible mathematical theory is actually true of some part of the mathematical realm, then the fact that, say, ZF and NBG provide different notions of set is no longer a problem. Each set theory will correctly describe the relevant part of the mathematical realm. The FBP-ist need not select a unified picture of the mathematical world. No matter how

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5 To use Azzouni’s terminology, there’s no thick epistemic access to mathematical entities (see Azzouni [1994] and [1997]). An epistemic access to an object is thick if (i) it is robust (e.g. you blink, you move away and the object is still there); (ii) the access can be refined (e.g. you can get closer for a better look); (iii) the access enables us to track the object in question, and (iv) we can use properties of the object in question to get to know other properties of the object. Mathematical objects clearly fail to satisfy (at least) conditions (i) and (iii). To the extent that we have access to such objects at all, it is only a thin form of access. That is, the access is through a theory that has the usual methodological virtues: it is simple, familiar, has large scope, is fecund, and is successful under testing. Using van Fraassen’s [1980] distinction between acceptance and belief, we can say that having a thin form of epistemic access to mathematical entities gives us reason to accept such entities, although not a reason to believe in their existence (reasons for acceptance are pragmatic reasons, after all).

6 It should be noted that Mark Balaguer himself is not a FBP-ist. He thinks that there’s no fact of the matter as to whether platonism or nominalism is true. But as part of his case for the latter claim, he provides the most interesting and thorough defense of FBP.
dissimilar the mathematical objects are from one another, they will all be there, and the consistency of the mathematical theories is enough to guarantee reference to the relevant objects.

This is a very interesting proposal. But it's not clear that the proposal solves the incommensurability problem. Rather, it embraces the issue raised by the incommensurability of mathematical notions. For the FBP-ist, different (consistent) mathematical theories are about different mathematical objects. But to embrace this conclusion, the BFP-ist seems to be committed to the impossibility of genuine disagreement in mathematics. After all, distinct (but consistent) mathematical theories deal with distinct parts of the mathematical realm. So, the theories are about different objects, and hence, strictly speaking, they cannot be in conflict with each other. The problem, however, is that, as a feature of mathematical practice, there seems to be genuine disagreement in mathematics. Even a cursory look at the history of mathematics makes this clear. Mathematicians systematically disagree about a number of issues, from the introduction of new mathematical entities (consider the debates surrounding the formulation of irrational numbers, imaginary numbers, or different notions of set), through the appropriate standards of proof (are informal arguments enough?), to the adequacy of mathematical techniques (consider the debate between constructivists and classical mathematicians). Without being able to make sense of these debates at least for what they seem to be—that is, disagreements about how to conceptualize the objects and standards in question—it is not clear that FBP provides an adequate picture of mathematical practice. As a result, in the end, FBP doesn't yield an acceptable account of the incommensurability of mathematical notions.

4. A SIMPLE PATTERN

To examine the case study about incommensurability in mathematics, it will be useful to have a pattern to guide the discussion. To do that, I'll use some ideas that Lakatos put forward for a different purpose.

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7 Of course, the FBP-ist can always claim that the disagreement among mathematicians is simply an appearance; in fact, there is no such thing as a controversy in mathematics. Mathematicians that seem to disagree are simply describing different aspects of the mathematical universe. Even if this were the case, the FBP-ist owes us at least an explanation as to why mathematicians seem to be disagreeing if they are really talking past each other. But it's not clear that the FBP-ist has an adequate explanation—sensitive to mathematical practice—to offer here. Inflating the ontology, by requiring the existence of all mathematical objects that logically possibly could exist, does all the work for the FBP-ist. This move is simply silent with respect to mathematical practice.
In a very interesting piece on mathematical reasoning, Lakatos discusses three types of mathematical proofs: pre-formal proofs, formal proofs and post-formal proofs (Lakatos [1978b]). Most of mathematical practice is carried out in terms of pre- or post-formal proofs, given that the bulk of mathematical reasoning is done in a somewhat informal way. In particular, the underlying logic typically is hardly ever made explicit, and hence inferences are drawn informally. Unless the subject under consideration is particularly controversial, or there are genuine doubts about the consistency of the theories in question, the proofs provided are never formal.

Lakatos [1978a] also develops a simple (Hegelian) pattern of development of mathematical conjectures, and this pattern helps to illuminate important features of mathematical practice. First, an initial conjecture is advanced (thesis). Second, attempts are made both to prove the conjecture and to refute it (antithesis). Finally, as the result of this double-headed process, a refined conjecture is obtained and eventually established. This new conjecture incorporates what was learned from possible counterexamples to the original conjecture as well as the new concepts introduced in the attempt to formulate and establish the refined conjecture (synthesis).

This simple pattern beautifully illustrates the development of several mathematical results. For example, the formulation and eventual proof of the well-ordering theorem in set theory can be clearly described in terms of the pattern. Let me briefly sketch why this is the case.

As part of his study of trigonometric series, Cantor was led to provide a formulation of real numbers in terms of sequences of rational numbers, and to study infinite iterations of certain operations over collections of real numbers. This latter study led him to develop a mathematical theory of the infinite. To develop that theory, Cantor needed a mathematical principle to justify carrying over properties of finite numbers into the transfinite. This idea—of transferring some properties from finite domains to infinite ones—provided a guiding heuristic motivation for the construction of Cantor’s theory (see Hallett [1984]). The mathematical principle Cantor introduced, which clearly holds for finite numbers, was the well-ordering principle. According to this principle, “it is always possible to bring every well-defined set into the form of a well-ordered set” (Cantor [1883], p. 550; see Kanamori [1996], p. 5).8 Interestingly enough, Cantor considered this principle “fundamental”, since it is “rich in consequences and particularly

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8 According to Cantor [1883], a set $M$ is well-ordered by a relation $<$ if: (a) $M$ is linearly ordered by $<$; (b) $M$ has a first element $m_0$ (according to the relation $<$); and (c) whenever $N \subseteq M$ and $\exists m \in M-N$ such that $\forall n \in N (n < m)$, then there is a $<$-smallest $m \in M-N$ such that $\forall n \in N (n < m)$. This definition is equivalent to the standard definition. (For a discussion, see Hallett [1984], pp. 51-52.)
remarkable for its general validity”. On his view, the principle was actually a “law of thought”. It clearly yielded the justification for unifying the finite and the infinite that Cantor needed. But, alas, Cantor never gave a proof of the principle (why should one try to prove a “law of thought” anyway?).

In 1904, at the Third International Congress of Mathematicians in Heidelberg, König presented what he took to be a counterexample to Cantor’s principle, offering an argument to the effect that there is no well-ordering of the continuum. If this result were correct, it would of course have seriously damaged Cantor’s program. But it turned out that König’s “proof” contained a mistake, and he had to withdraw it. The error was found by Zermelo, who showed that König relied on a theorem of Bernstein that was incomplete in the relevant case.9

Motivated in part by this incident, and in order to provide support for Cantor’s case, Zermelo eventually proved a theorem to the effect that every set can be well-ordered, the well-ordering theorem (Zermelo [1904]; for a discussion, see Kanamori [1996], pp. 10-13, and Kanamori [1997]). But in order to prove this theorem, Zermelo had to use what was soon to be called the Axiom of Choice (AC).10 It is only based on this axiom that the theorem holds. On Zermelo’s view, however, AC was nothing but a “logical principle”, and although it cannot “be reduced to a still simpler one”, it is “applied without hesitation everywhere in mathematical deduction” (Zermelo [1904], p. 141). A crucial point here is that AC is consistent with Cantor’s requirement that the finite and the transfinite should be approached uniformly, since the axiom extends to infinite sets an unproblematic feature of finite sets (Kanamori [1996], p. 10; see also Moore [1982], and Lavine [1994], pp. 103-118). However, in introducing AC, and in developing his approach to set theory, Zermelo ended up formulating not exactly a Cantorian view about sets. Zermelo’s notion of set is much more abstract and general than Cantor’s, given that it includes, in particular, an unrestricted power set axiom, something Cantor would never endorse (see, e.g., Lavine [1994]).11

This case can be clearly described in terms of Lakatos’ pattern. Cantor’s initial conjecture (the well-ordering principle) is the thesis, the initial conjecture that Cantor never actually proved. The antithesis is given by

9 For a discussion of this episode, see van Heijenoort (ed.) [1967], p. 139; Moore [1982], pp. 86-88; Lavine [1994], p. 103; and Kanamori [1996], pp. 9 and 49, note 22.
10 In one of its formulations, this axiom states that: If T is a set whose elements are sets that are different from ∅ and mutually disjoint, its union ∪T includes at least one subset S having one and only one element in common with each element of T (see Zermelo [1908b], p. 204, and Moore [1982], pp. 5-11, and 321-334).
11 I discuss this episode further in Bueno [2000] and Bueno [2002], but I draw a different moral in the present context.
König’s attempt to refute Cantor’s conjecture, trying to establish that the continuum cannot be well ordered. The synthesis is finally obtained with Zermelo’s proof of the well-ordering theorem. But given the use of AC in Zermelo’s proof, and the abstract treatment of sets articulated in his axioms for set theory, the meaning of Cantor’s original conjecture is changed. After all, Cantor’s original “law of thought” is only valid assuming a particularly strong principle about sets (namely, AC). In other words, as opposed to Cantor’s initial conjecture, it’s not the case that every set can be well ordered in general. Every set can be well ordered—as long as AC holds for such sets. Now, whether the latter condition is, or is not, satisfied depends, ultimately, on the context (the mathematical system) one considers, and the type of sets in question. (As is well known, the properties of sets differ quite dramatically in the presence of AC.) In the attempt of refining and proving Cantor’s initial conjecture, Zermelo ended up establishing a different, more restricted result.

But Lakatos’ simple dialectical pattern needs to be used with care. With the possibility of incommensurable mathematical theories, the result established in the final synthesis can be more than a simple refinement of the original conjecture. As the discussion above of Cantor’s case illustrates, the final result may mean something different from what was meant with the initial conjecture. After all, with Zermelo’s work, a more abstract notion of set is in place. But note that this is not a problem. It’s actually an advantage of this pattern, given that it is able to accommodate the development of richer and more complex bodies of mathematics. What emerge from the interaction between the initial conjecture and its refinement are new concepts and mathematical results that, in the end, are more interesting.

There’s no doubt that the simple pattern described by Lakatos accommodates an important feature of mathematical practice: the interplay between attempts to prove and refute a given conjecture, leading to the formulation of new notions and the refinement of the initial guess. But given that the original conjecture and the refined one may not have the same meaning, the original conjecture might be right in the context in which it was originally formulated, and the final theorem may actually establish a different result than the one that was initially formulated. This seems to introduce a lack of continuity in the development of mathematical theories.

Thus, and Lakatos certainly emphasizes this point, we have a challenge to the received view about the history of mathematics, according to which the development of mathematics is the result of the accumulation of truths about mathematical objects. Rather, a closer look at the history of mathematics reveals that radical discontinuities are indeed possible. To accommodate this possibility, Lakatos’ pattern does not require a tight connection between the initial conjecture and the final theorem. Once the
process of concept revision and refinement is in place, there is always the possibility of radical meaning change.

5. INCOMMENSURABILITY IN NONSTANDARD ANALYSIS

The impact of the incommensurability of mathematical notions in mathematical practice is not restricted to set theory. To consider an additional illustration of the incommensurability phenomenon, I'll examine the construction of nonstandard analysis by Abraham Robinson, who, among other achievements, put infinitesimals back into calculus (see Robinson [1974], [1979a] and [1979b]). The reason for focusing on nonstandard analysis is that it provides an interesting example of the bearing of the incommensurability issue on mathematical practice—at least in the case of Robinson’s work. In this way, we can appreciate the overall relevance of the issue.

It is well known that the early formulation of the calculus, due to Leibniz and Newton, was heavily dependent on infinitesimals, which were employed, for instance, in the derivation of the rules of Newton's method of fluxions (see Lavine [1994], pp. 15-26). Intuitively, infinitesimals were taken to be indefinitely small quantities, smaller than any finite quantity. But lacking a precise mathematical definition, infinitesimals were received with harsh criticism. Discussing Newton’s method of fluxions, Berkeley pointed out:

And what are these fluxions? The velocities of evanescent increments? And what are these evanescent increments? They are neither finite quantities, nor quantities infinitely small, nor yet nothing. May we not call them the ghosts of departed quantities? (Berkeley [1734], p. 88)

As to Leibniz’s infinitesimals, Berkeley was no less caustic:

[Our modern analysts] consider quantities infinitely less than the least discernible quantity; and others infinitely less than those infinitely small ones; and still others infinitely less than the preceding infinitesimals, and so on without end or limit. (Berkeley [1734], p. 68)

Now, Leibniz was trying to devise a program of construction of numbers that would include infinitesimals in a suitable way (see Leibniz [1701]). The idea was to introduce the latter, by appropriate arithmetic rules, as ideal numbers into the system of real numbers, in such a way that the resulting system would have the same properties as the real number system. Nevertheless, given that neither Leibniz nor his followers managed to
produce such a system, infinitesimals gradually fell into disrepute, and were eventually eliminated in the classical theory of limits elaborated in the nineteenth century (see Lavine [1994], pp. 26-41; see also Robinson [1974], pp. 260-282).

However, as Lakatos would say, with sufficient heuristic resources every program can be revived. And in 1960, with the work of Robinson, Leibniz’s program was brought back. In fact, what Robinson argued ([1974], p. xiii) is that the model-theoretic techniques developed in the 20th century provided the adequate framework in which Leibniz’s intuitions could be properly articulated and vindicated. Robinson showed that the ordered fields that are nonstandard models of the theory of real numbers could be thought of, in the metamathematical sense, as non-archimedean ordered field extensions of the reals, and they included numbers behaving like infinitesimals with regard to the reals. Moreover, since these ordered fields are models of the reals, they have the same properties as the latter. As a result, Leibniz’s problem was solved.

The crucial notion Robinson used to provide nonstandard models of analysis was that of enlargement. Given a structure \( R \) (say, the real number structure), an enlargement \( ^*R \) of \( R \) is an expansion of \( R \) (in technical parlance, \( R \) is a substructure of \( ^*R \)), such that a sentence \( \alpha \) is true in \( R \) if and only if \( \alpha \) is also true in \( ^*R \) (that is, \( R \) and \( ^*R \) are elementarily equivalent). In other words, an enlargement \( B \) of a given structure \( A \) is an extension of \( A \) that preserves the truth-values of the sentences that hold in \( A \). The decisive result from model theory that Robinson employed in the development of nonstandard analysis was the compactness theorem, according to which if \( K \) is a set of sentences such that every finite subset of \( K \) is consistent, then \( K \) is also consistent. What the compactness theorem allowed him to prove is that for any structure \( A \) there is an enlargement \( B \) of \( A \)\(^{14}\).

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12 A field over the real numbers is archimedean if for any pair of real numbers \( a \) and \( b \), \( 0 < a < b \), there exists a natural number \( n \) (in the ordinary, standard sense) such that \( b < na \). This postulate is not true in Robinson’s nonstandard models, and in this sense, the latter are non-archimedean (see Robinson [1974], pp. 266-267).

13 Robinson’s original formulation of nonstandard analysis, in 1960, was articulated in type theory (for an overview, see Robinson [1974]). His account was subsequently reformulated by Luxemburg [1962], using an ultrapower of the first-order structure of real numbers, and by Machover [1967] in set-theoretic terms (the latter account is also presented in Bell and Machover [1977], pp. 531-575, which provides a concise and helpful introduction to nonstandard analysis). Certainly the last two formulations, especially Luxemburg’s, were crucial for spreading nonstandard analysis among mathematicians, who typically are not used to the background in logic required by Robinson’s type-theoretic version (see Dauben [1995], pp. 393-396).

14 Given Robinson’s use of type theory, a point should be noted. It is well known that type theory is only complete and, in particular, compact if we consider Henkin models. Again,
pointed out, the enlargement is by no means unique. The real number structure \( \mathbb{R} \) has several enlargements \( \ast \mathbb{R} \), and any of them provides a nonstandard model of analysis. However, once an enlargement has been chosen, “the totality of its internal entities is given with it” (Robinson [1967], p. 29). As a result:

corresponding to the set of natural numbers \( \mathbb{N} \) in \( \mathbb{R} \), there is an internal set \( \ast \mathbb{N} \) in \( \ast \mathbb{R} \) such that \( \ast \mathbb{N} \) is a proper extension of \( \mathbb{N} \). And \( \ast \mathbb{N} \) has “the same” properties as \( \mathbb{N} \), i.e. it satisfies the same sentences of \( L \) just as \( \ast \mathbb{R} \) has “the same” properties as \( \mathbb{R} \). \( \ast \mathbb{N} \) is said to be a Non-standard model of Arithmetic [just as \( \ast \mathbb{R} \) is called a nonstandard model of Analysis]. From now on all elements (individuals) of \( \ast \mathbb{R} \) will be regarded as “real numbers”, while the particular elements of \( \mathbb{R} \) will be said to be standard. (Robinson [1967], pp. 29-30).

Now, the crucial feature of \( \ast \mathbb{R} \) is that it is a non-archimedean ordered field. Therefore, \( \ast \mathbb{R} \) contains infinitely small numbers (infinitesimals), that is, numbers \( a \neq 0 \) such that \( |a| < r \) for all standard positive \( r \) (Robinson [1967], p. 30).

Since the structures \( \mathbb{R} \) and \( \ast \mathbb{R} \) satisfy the same set of sentences, the properties of relations and functions in one structure can be “transferred” back into the other, and vice-versa. This provides the main heuristic move used by Robinson, namely transfer principles. These principles are straightforward consequences of the model-theoretic framework in which Robinson worked, given the elementary equivalence of the structures under consideration. In other words, there are significant (model-theoretic) interconnections between \( \mathbb{R} \) and \( \ast \mathbb{R} \), and the decisive trait of nonstandard analysis is to explore them. Although we may not know whether a given result holds in \( \mathbb{R} \), by embedding it into \( \ast \mathbb{R} \), we have “more structure” to

in these models, the interpretation of the quantifiers takes into account not the totality of all relations of a given type, but only a subset of them. These are the so-called internal relations. As Robinson points out, “if \( S \) is a set or relation in \( \mathbb{R} \) then there is a corresponding internal set or relation \( \ast S \) in \( \ast \mathbb{R} \), where \( S \) and \( \ast S \) are denoted by the same symbol in \( L \) [the higher-order language in which statements about the structure \( \mathbb{R} \) are formulated]” (Robinson [1967], p. 29). However, Robinson insists, “not all internal entities of \( \ast \mathbb{R} \) are of this kind” (ibid.). Indeed, since there is an infinite set in \( \mathbb{R} \), there is a set in \( \ast \mathbb{R} \) which contains an internal relation which is not a standard relation, that is, which is not denoted by any constant in the fixed set of sentences \( K \) (see Robinson [1974], pp. 42-45).

Note Robinson’s use of quotation marks when he describes the relation between the nonstandard models \( \ast \mathbb{N} \) and \( \ast \mathbb{R} \) and their standard counterparts \( \mathbb{N} \) and \( \mathbb{R} \). The quotations highlight the fact that \( \ast \mathbb{N} \) and \( \mathbb{N} \) (as well as \( \ast \mathbb{R} \) and \( \mathbb{R} \)) are actually not the same—as they couldn’t be, given that they have different elements—even though they satisfy the same sentences. Different extensions for the same predicates are found in each model.
work with, and in this way, we may be able to establish the result. Using a transfer principle, we then establish that this result also holds in \( \mathbb{R} \).

By systematically exploring this heuristic strategy, Robinson was not only able to simplify several proofs of established theorems, but also to prove new results. For example, in a joint work with Bernstein, he solved an invariant subspace problem, using nonstandard techniques. This was an open problem, which hasn’t been solved yet with the resources of classical analysis (see Bernstein and Robinson [1966]). Moreover, he also provided results in general topology, theory of distributions, topological and metrical groups, applied mathematics etc. (see Robinson [1974]). Robinson also showed how analysis could be reformulated with infinitesimals. For example, he established that the real-valued function \( f \) is continuous at \( x_0 \) in the real number structure \( \mathbb{R} \) if and only if \( f(x_0 + \eta) \) is infinitely close to \( f(x_0) \) in the nonstandard model \( \mathbb{R}^* \), that is, \( f(x_0 + \eta) - f(x_0) \) is infinitesimal, for all infinitesimal \( \eta \) (see Robinson [1967], pp. 30-31, and Robinson [1974], pp. 49-88). In other words, a completely new approach was devised.

Robinson’s formulation of nonstandard analysis was not only heuristically fruitful (given, for example, the solution of the invariant subspace problem), but it also led to a re-evaluation of the history of mathematics, in particular of the calculus (see Robinson [1974], pp. 260-282, and Robinson [1967]). After all, until the construction of nonstandard analysis, the history of mathematics has been written on the assumption that infinitesimals were, in Cantor’s words, “the cholera bacillus of mathematics”, or in Berkeley’s view, “the ghosts of departed quantities” (Berkeley [1734], p. 88). As a result, the insightful ideas of Leibniz and many others about infinitesimals were disregarded as mere eccentricities. As Robinson points out, with nonstandard analysis it’s possible to devise a more sympathetic evaluation.

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16 Bernstein and Robinson proved the following theorem: if \( T \) is a bounded linear operator on an infinite-dimensional Hilbert space \( H \) over the complex numbers, and if \( p(z) \neq 0 \) is a polynomial with complex coefficients such that \( p(T) \) is compact, then \( T \) leaves invariant at least one closed subspace of \( H \) other than \( H \) or \( \{0\} \) (Bernstein and Robinson [1966], p. 421). The main idea is to associate with the Hilbert space \( H \) a larger space \( \mathbb{R}^*H \) which, given the construction of the enlargement, has the same properties as \( H \). The problem is then solved by considering the invariant subspaces in a subspace of \( \mathbb{R}^*H \), whose number of dimensions is a nonstandard positive integer (ibid.). After seeing the nonstandard proof of this result, Halmos obtained a proof using standard techniques, essentially translating Bernstein and Robinson’s model-theoretic argument into ordinary mathematics (see Halmos [1966], and for a discussion, Halmos [1985], pp. 204 and 320, and Dauben [1995], pp. 327-329).

17 Cantor expressed this evaluation in a letter to the Italian mathematician Vivanti, December 13, 1893 (see Dauben [1979], p. 233).
As an illustration of the historical fertility of nonstandard analysis, Robinson considered the celebrated case of Cauchy on (uniform) continuity (see also Lakatos [1978a]). In 1821, Cauchy proved a theorem to the effect that the sum of a convergent series of continuous functions was continuous. In the proof, although not in the statement of the theorem, Cauchy made full use of infinitesimals. Five years later, Abel provided an example of a convergent series of continuous functions whose sum was not continuous, namely \( \sin x + \frac{1}{2} \sin 2x + \frac{1}{3} \sin 3x + \ldots \). It turns out that Cauchy's theorem would be entirely correct if we consider that the convergence holds in an interval containing not only standard, but also nonstandard points (that is, points of the form \( x + \eta \), where \( x \) is real and \( \eta \) infinitesimal). In other words, Cauchy held a richer conception of the continuum than Abel (and later Weierstrass), since it included infinitesimals. In fact, Robinson showed that if nonstandard points are introduced, uniform convergence of an infinite series in an interval—that is sufficient for the continuity of the sum—is equivalent to pointwise convergence. In this way, in the light of nonstandard analysis, we can understand why Cauchy presented his theorem, and why it is inadequate to read it assuming the Weierstrass continuum that excludes infinitesimals.\(^{18}\)

In other words, in the 19th century, there were two conceptions of the continuum, and they yielded very different results regarding the properties of real numbers. The conceptions were actually incommensurable in that (i) depending on the conception one adopts, the *meaning of 'real number' is different* (e.g. one conception includes infinitesimals, the other doesn't), and (ii) there is *no common standard* to assess the adequacy of the different notions (after all, different results about the continuity of series of functions are established by each conception).

The existence of these different conceptions changes the practice of mathematicians. The Leibnizian mathematician (such as Cauchy) has more structure to work with, given that the Leibnizian continuum incorporates infinitesimals and infinitely large numbers. As a result, he or she is able to obtain results that couldn't be obtained on the Weierstrassian conception of the continuum. The latter conception, despite being less rich, had the initial benefit of not depending on the introduction of infinitesimals. But this came with a cost: some results about real numbers *cannot* be obtained in this framework, such as Cauchy's result regarding convergent series.

Now, it's important to note that Robinson's notion of the continuum, although being in spirit close to Leibniz's, is in important respects different from Leibniz's too. The fact that there are objects that 'behave like

infinitesimals’, or ‘could be taken to be infinitesimals’ in an enlargement doesn’t entail that these objects are infinitesimals. As Robinson acknowledges, nonstandard analysis provides a framework to represent infinitesimals. This is very different from actually incorporating or invoking infinitesimals as Leibniz conceived of them. The model-theoretic framework that Robinson employed lets reference to mathematical objects completely loose. The plasticity provided by the various reinterpretations of mathematical principles that the model-theoretic approach offers is both the strength and the weakness of the program. It’s the strength in that a rich variety of structures is generated in this way, and several results are then obtained. It’s the weakness in that, on the model-theoretic account, there’s no close tie between mathematical notions and their referents. It’s always possible to find deviant interpretations of the formalism of analysis in which the relevant results come out true, even though the extension of the predicates in question changes dramatically. This means that there’s no way in which Robinson could claim that the objects that play the role of infinitesimals in his framework are Leibnizian infinitesimals, even though such objects may share some of the properties of the latter. There is incommensurability even here.

But what exactly are Leibnizian infinitesimals? To the extent that Leibniz himself was clear about the nature of these entities, the constraints he imposed on them were minimal. Clearly, he assigned an instrumental role to infinitesimals, and was explicit in taking them only to be “useful fictions” (Leibniz [1716]). That doesn’t entail, of course, that infinitesimals could be anything one pleases. They had some properties, lacked others, and had otherwise to behave in certain ways. But whatever Leibniz and his followers had in mind when they entertained the notion of infinitesimal, it was certainly not a particular kind of object in a Henkin model that is an enlargement of certain type-theoretic structures. This is further grist for the incommensurability theorist’s mill.

The incommensurability, though, is not restricted to Leibniz’s and Robinson’s view of the continuum. It also applies to Robinson’s framework and standard analysis. It was initially thought—and Robinson certainly presented the issue in this way—that nonstandard analysis is simply a conservative extension of standard analysis, and so any result obtained via nonstandard techniques can also be obtained via the standard approach. In this sense, although useful, nonstandard analysis is ultimately dispensable. It turns out, however, that things are not so simple. As Henson and Keisler [1986] have shown, there are theorems that can be proved with nonstandard analysis but which cannot be proved without it. The source of the confusion about the issue is the following. On the one hand, we have the correct statement (essentially the one made by Robinson): if a theorem can be
proved using nonstandard analysis, it can be proved in Zermelo-Fraenkel set
theory with the Axiom of Choice, and therefore it is acceptable as a theorem
of (standard) mathematics. However, from this it doesn’t follow that there is
no need for nonstandard analysis. As Henson and Keisler indicate, almost all
results in classical mathematics employ methods available in second-order
arithmetic, given appropriate comprehension and choice axioms. In other
words, mathematical practice is articulated in a conservative extension of
second-order arithmetic, and the higher level of sets available in ZFC is not
used. What the authors then show is that nonstandard analysis (that is,
second-order nonstandard arithmetic) with a saturation principle (often used
in nonstandard arguments) has the same strength as third-order arithmetic.
Therefore, “there are theorems which can be proved with nonstandard
analysis but cannot be proved by standard methods” (Henson and Keisler
[1986], p. 377).

This means that the frameworks of standard and nonstandard analysis are
importantly different, particularly in the context of everyday mathematical
practice. How could we choose between them? Again, there’s no common
standard to make the choice. One could prefer the standard approach
because it’s familiar and does not invoke “suspicious” entities like
infinitesimals. But these considerations clearly presuppose the adequacy of
the standard account and the inadequacy of the way infinitesimals are
characterized in nonstandard analysis. As a result, the considerations simply
beg the question. One could prefer, instead, the nonstandard approach
because it’s richer and can be used to obtain new results. But this also begs
the question, now against the standard approach. The alleged new results
from nonstandard analysis might be obtained using standard techniques, in
which case the results are not exactly new and the nonstandard framework is
not necessarily richer than the standard one.19

This is not an argument against nonstandard analysis, of course. It’s only
an argument for the incommensurability of the standard and the nonstandard
approaches. This fact has a significant impact on mathematical practice.
Given the strength of nonstandard techniques (and the strength of the
mathematical framework they presuppose), the incorporation of nonstandard

19 How can one claim that the alleged new results from nonstandard analysis are actually
results in standard analysis? An argument (which I don’t exactly endorse) could go like
this. If the new results are established using resources beyond the scope of the standard
framework, they simply do not belong to the latter. If the new results are established based
on standard resources, they can be obtained without nonstandard techniques. So, in either
case, there’s no need for the nonstandard approach. What is established properly is
established using the standard approach. (Of course, this argument fails to note that the
results in question may have different meanings in the standard and the nonstandard
frameworks.)
techniques into everyday mathematical practice would increase the overall strength of the mathematical systems used in that practice. The rejection of these techniques, in turn, would amount to an important loss to mathematical practice. In either case, important changes emerge.

Finally, note that the development of nonstandard analysis illustrates the simple dialectical pattern discussed by Lakatos. Leibniz’s initial conjecture about the possibility of developing a system of real numbers incorporating infinitesimals was the thesis. The difficulties of actually implementing such a system, which eventually led to the exclusion of infinitesimals from the calculus with Weierstrass, illustrate the antithesis. Robinson’s work on nonstandard analysis, that motivated a reassessment of the whole debate, eventually provides the synthesis. Interestingly enough, the synthesis changes substantially the meaning of the original Leibnizian project, given the nature of the infinitesimals put forward by Robinson and the model-theoretic techniques he devised to achieve that. Moreover, note that throughout this development, the proofs provided have always been rather informal (in most cases, they were actually pre-formal proofs in Lakatos’ sense). The arguments given have typically been rather intuitive, even in Robinson’s case, despite his use of type theory. Given the way in which mathematical practice is actually conducted, that’s the expected outcome.

6. CONCLUSION

If the considerations above are correct, a case for the incommensurability of mathematical notions can be made. Incommensurability is not restricted to scientific theories after all, but is also part of mathematical practice. Theory change in mathematics, just as theory change in science, becomes a more complex, more interesting and not a cumulative phenomenon. As with science, in mathematics sensitivity to meaning change is required. This means that a simple cumulative pattern of mathematical development doesn’t seem to make sense of mathematics. This, in turn, opens up the possibility for the existence of revolutions in mathematics—radical cases of mathematical theory and conceptual change. In other words, as Lakatos would put it, the usual Euclidean model of mathematical development according to which mathematical theories are a body of eternal, immutable truths, has to go. A more fine-grained model is in order.

While I have no intention of providing such a model here—the aim of the paper is only to make the initial case for the incommensurability of certain mathematical notions—I’d like to conclude by highlighting the impact that incommensurability has on mathematical practice. Mathematicians are
typically sensitive to meaning changes, and as part of their practice, they develop strategies to track such changes. One simple strategy is to identify whether new results can be established once a new conceptual setting is advanced. If new results can be obtained, this provides *prima facie*, but certainly not conclusive, evidence for meaning change. But, alas, as the considerations above indicate, things are never that simple. There may be meaning change even when *the same results* are established in the old and the new frameworks. The case of nonstandard analysis beautifully illustrates this. On the usual reading (the one favored by Robinson), given that nonstandard analysis is essentially a conservative extension of standard analysis, all new results proved by nonstandard techniques could always be obtained without the latter, but they need not have the same meaning (given the quantification over infinitesimals in one framework and their absence in the other). So meaning change need not be correlated with the presence, or not, of new results.

This leaves the incommensurability of mathematical notions as a feature—an important feature—of mathematical practice. The existence of incommensurability, in turn, makes mathematical practice a much more interesting, rich and complex phenomenon to study. In the end, there’s much more to that practice than meets the eye.

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