Review of "From Dedekind to Gödel"

Thomas Forster

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The title of this book is an allusion to "From Frege to Gödel: a source book in mathematical Logic," and reveals the publisher's hope that everyone who has a copy of it on their shelves will want to have a copy of the book under review next to it. This is optimistic: van Heijenoort's volume is specifically a collection of period texts, translated and annotated and in paperback, whereas the current volume consists exclusively of secondary literature and is available—so far—only in hardback. The judicious obscurity of the subtitle—"'Essays on the development of the foundations of mathematics"—is carefully designed to ensure that no potential reader can legitimately complain that this curious book does not contain what they had been led to expect. And a curious mixture it is too, written by historians, logicians, mathematicians and philosophers. The unifying factor is not a particular topic or even a particular point of view but the fact that the large majority of contributors are based in Boston. This provides a wonderful defence for the charge that the volume fails to cover topics that individual reviewers consider important. To get my complaint on this score out of the way: I would like to have seen someone discuss Klein and the Erlanger Programm, and its implications for Logic. Such a major figure as Tarski (whose background is discussed in an eponymous article in this volume by Jan Woleǹski) wrote about this ("What are Logical Notions?") posthumously in *History and Philosophy of Logic* **7** (1986) 143-154) and the topic deserves more attention. To the extent that there is a slant here it is philosophical rather then mathematical: this is betrayed by the much greater space devoted to Wittgenstein than to Brouwer.

That is not to say that the book does not contain good material, but it is not the place to start for someone who wants to capture what it was like to be a philosophically concerned mathematician 100 years ago. When we try to compare late 20th century mathematics with late 19th century mathematics we find that whereas 19th century mathematics worried a great deal about whether mathematical objects had to be constructed or could be viewed as arbitrary objects existing independently of the thinker, 20th century mathematics has a fairly relaxed pluralistic attitude to these questions: we distinguish between constructive and nonconstructive proof, but we use both. Nineteenth century mathematics had no satisfactory symbolic logic; no concept of model, of algorithm, of proof. 20th century mathematics has extended the reach of formal mathematics to these areas and others. These are important developments, and it is instructive for modern readers to relive the pain and puzzlement that accompanied them. Although this book does not explicitly set out to recreate this experience (and there is very little directly concerning one of the biggest causes of friction at the time, the axiom of choice)[1], the determined reader will find much in it to help. Some of the articles are discussed in what follows.

The first article in the book is Judson Webb's on the prehistory of independence proof and the discovery of non-euclidean geometry. It is the only one to explicitly treat material that is outside the period announced by the book's title. If we are trying to determine whether or not the axiom of parallels follows from Euclid's other axioms, what do we do? If it does, life is easy, for it will be sufficient to exhibit a proof. If it doesn't, we need to demonstrate that there is no proof. One way to do this would be to show that every proof fails to be a proof of the parallel axiom from the other axioms. For this to be persuasive we have to have a sufficiently formal notion of proof for demonstrations of this kind to be as convincing as—for example—the demonstration that every natural number is the sum of four squares. To do this we have to make proofs into mathematical objects as straightforward as numbers: logicians' slang speaks of "the arithmetisation of metamathematics". We turn the syntax that we use to describe and execute mathematics into, itself, another suite of mathematical objects. On the other hand we could show that there is a model universe in which the parallel axiom is false but all the other axioms of Euclid are true. That way we do not have to make proofs into mathematical objects, but we do have to develop a ro-

[1]But see Gregory Moore's book on this period: "Zermelo's axiom of choice

bust concept of a *formula being true in a structure* and a fairly robust concept of what it is for a structure to exist. (The less tenuous the existence of the model, the more convincing the independence proof.) That is to say, we need semantics. This step—of thinking of a symbolism separately from the subject matter it was devised to describe, and as having a life of its own—creates a division between syntax and semantics without which no independence proofs can be had. The most characteristic products of twentieth-century logic, the completeness theorems, arise like the Greek conception of sexuality from the need to rejoin the two halves of this beast. A completeness theorem is something that identifies a syntactic property of a formula (like having an even number of occurrences of every variable) with a semantic property (like being true under all interpretations). It is no exaggeration to attribute the development of logic along these lines ultimately to the need to produce and understand independence proofs. Anyone who wishes to understand twentieth century completeness proofs and how they arose must sooner or later look at the topics covered in Webb's article.

David McCarty writes about Dedekind. Dedekind in known to logicians for the definition of infinitude that bears his name: a set is infinite if it is in bijective correspondence with a proper subset of itself. The point is not that this is Dedekind's definition, the point is rather that Dedekind argued that since each object is distinct from the thought of that object, and since each object can be thought of, there are infinitely many things. Demonstrating the truth of the axiom of infinity is not on modern mathematicians' agenda but also

Dedekind is claimed as the father of what one might call the *anti-ontological* view of the naturals. This is the view that what they are made of is not a useful question: all that matters is that there is 0, and everything is made from it by S. In categorese: all there is to say about $\mathbb{N}$ is that it is a recursive datatype.[2] Naturally the truth is more complicated—the history of ideas is littered with esurient movements inventing spurious pedigrees for themselves—and readers who want to learn more can read McCarty's article. In fact the bulk of the material McCarty discusses is available as Dover's edition of two of Dedekind's essays, translated and bound together under the title "Essays on the theory of numbers", ISBN 0-486-21010-3.

An independence proof of a formula ϕ from a formula ψ (both belonging to a language $\mathcal{L}$) can be provided by exhibiting models in which ψ is satisfied but ϕ isn't. In the normal course of events all such models are likely to be odd in some way, at the very least in the sense of being evidently and disturbingly different from the universe that the language $\mathcal{L}$ was developed to describe. This is because independence questions are likely to arise only in circumstances where the models of $\mathcal{L}$ that lie closest to hand all satisfy ϕ as long as they satisfy ψ. Models with an air of contrivance are liable to be called *non-standard*. This is really a piece of mathematical slang, and deserves examination because of it and not despite it. Models in which the equality symbol '=' is interpreted by

[2] As Quine put it: "The subtle point is that any progression will serve as a version of number so long and only so long as we stick to one and the same progression. Arithmetic is, in this sense, all there is to number: there is no saying absolutely what the numbers are; there is only arithmetic."—*Ontological Relativity* p. 45.

something other than actual equality are *prima facie* odd, for equality is not just any old predicate letter but a logical symbol. Models in which higher-order quantifiers range over not *all* subsets of the domain but only some designated subcollection of it are nonstandard too. The concept of a standard model for higher-order theories is notoriously obscure. At the end of Chang and Keisler's epic textbook on Model theory is a list of open problems. Item 24 (the last) is: *Develop the model theory of second- and higher-order logic.* To judge by the amount of puzzled correspondence on electronic media nowadays about the difference between "higher-order" and "many-sorted" the more explanations that are available the better. But the hackers will not find Hintikka's article what they want: it is designed for the the *cognoscenti*. Hintikka covers Henkin's 1950 discussion of this kind of nonstandardness but doesn't mention the contemporary article of Rosser and Wang *Journal of Symbolic Logic* **15** (1950) pp 113-29, nor the subsequent literature contributed by that adoptive Bostonian curiously absent from this book: W.V.Quine.

The story of Russell alarming Frege by discovering a contradiction—the paradox that now bears Russell's name—in his three-volume *magnum opus* has become part of the folklore of 20th century logic. Readers who are not mathematicians will perhaps recognise Russell's paradox more readily in the form of the paradox of the village barber who shaves every man in the village who does not shave himself—and shaves nobody else. Paradoxes of this flavour have been a source of puzzlement since the time of the Greeks and probably the Ancient Egyptians, but they could not cause serious havoc in mathematics until the growing tide of formalisation in mathematics reached territory where these paradoxes could be applied. This happened at the end of the nineteenth century, and the story of how it happened is told by Gregory Moore. The effect was devastating. As Russell wrote in his autobiography: "It seemed unworthy of a grown man to spend his time on such trivialities, but what was I to do? There was something wrong, since such contradictions were unavoidable on ordinary premisses. ...Every morning I would sit down before a blank sheet of paper. Throughout the day, with a brief interval for lunch, I would stare at the blank sheet. Often when evening came it was still empty." Thus it came about that the phrase "The crisis in foundations" was invented, and not with hindsight, but by those who lived through it. The phrase gradually dropped out of use, not because the crisis went away, but because mathematicians did—in the words of an American general—"...what we did in Vietnam: declare a victory and go home". Although the paradox looks straightforward enough to modern eyes, Russell discovered it only in the course of an examination of the proof of another oddity called *Cantor's paradox* and the fact that both these paradoxes—and indeed others—could be deduced from principles of set existence adumbrated by Frege (see Heck's essay: Frege's Principle, in this volume) was not evident to Frege either. For those of us interested in the recent history and philosophy of mathematics there are many aspects of 19th century mathematics that make it hard for us to understand what they were thinking: they were worried by ideas of continuity and lacked a satisfactory notion of arbitrary mathematical object. But for those of us interested in logic it is the difficulty they had in recognising the paradoxes for what they were that is most baffling. From that period the paradox that we now recognise as the (chronologically) first (the Burali-Forti paradox) was not thought of as a paradox at all for quite some time. Russell and Whitehead at one point thought of it as a *reductio ad absurdum* proof of the axiom of infinity, but mostly it was taken as a sign that there was something going that they understood so poorly that they didn't know whethere there was a problem or not. Gregory Moore (whose work on the axiom of choice has been alluded to above) contributes an illuminating article to this volume on this topic ("The origins of Russell's paradox [...]") but this kind of intellectual climate is very hard to reconstruct.

Kanamori's "Emergence of Descriptive Set Theory" in this volume is really a companion piece to his much longer leading article "The mathematical development of Set theory from Cantor to Cohen" from the *Bulletin of Symbolic logic* **2** pp 1-71, which readers of this volume may also want to consult. Two articles, by Mathieu Marion and Juliet Floyd, concern Wittgenstein: the first his debates with Ramsey (which the people at the Wittgenstein archive in Cambridge are examining closely) and the second Wittgenstein's views on the incompleteness theorems. The remaining articles are Harold Edwards on Kronecker, Claire Ortiz Hall on Frege's letters and on Husserl and Hilbert on completeness; Philip Ehrlich writes on Hahn's 1907 book on nonarchimedean number systems, Jan von Plato on constructive probability theory, Follesdal on Gödel and Husserl and Boos on Skolem and Weyl.

It would be churlish to complain that each article has its own separate bibliography. I have found no sense-threatening missprints. In answer to the challenge that books like this always invite—"Why were the contents of this book not dispersed among suitable journals?"—the publishers can reply that at least they produced a general index and an index of names that covers all the articles. And since this is largely the product of a single city, they have shown us that the historical study of the foundations of mathematics is flourishing.