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# REVIEWS

The Association for Symbolic Logic publishes analytical reviews of selected books and articles in the field of symbolic logic. The reviews were published in ***The Journal of Symbolic Logic*** from the founding of the JOURNAL in 1936 until the end of 1999. The Association moved the reviews to this BULLETIN, beginning in 2000.



In a review, a reference "JSL XLIII 148," for example, refers either to the publication reviewed on page 148 of volume 43 of the JOURNAL, or to the review itself (which contains full bibliographical information for the reviewed publication). Analogously, a reference "BSL VII 376" refers to the review beginning on page 376 in volume 7 of this BULLETIN, or to the publication there reviewed. "JSL LV 347" refers to one of the reviews or one of the publications reviewed or listed on page 347 of volume 55 of the JOURNAL, with reliance on the context to show which one is meant. The reference "JSL LIII 318(3)" is to the third item on page 318 of volume 53 of the JOURNAL, that is, to van Heijenoort's *Frege and vagueness*, and "JSL LX 684(8)" refers to the eighth item on page 684 of volume 60 of the JOURNAL, that is, to Tarski's *Truth and proof*.

References such as 49***5*** or 280***1*** are to entries so numbered in *A bibliography of symbolic logic* (the JOURNAL, vol. 1, pp. 121–218).

MARY TILES. ***The philosophy of set theory, an historical introduction to Cantor's paradise.*** Courier Dover Publications, 2004, xiii + 239 pp.

In the preface to this book, the author reveals that she was motivated by the experience of attempting to teach undergraduate courses and seminars in philosophy of mathematics to audiences containing both philosophers and mathematicians. She found that there was a lack of material which was both not too philosophically technical for the typical mathematician (with a little exposure to philosophy) and not too mathematically technical for the typical philosopher (with a little exposure to mathematics).

In the introduction, she tells us that the main question she will investigate is the question as to whether Cantor "discovered" or "invented" the world of transfinite set theory. This is a good question since it leads directly to obviously interesting questions about the nature of mathematical objects in general and about the role of the actual infinite in mathematics. She suggests in particular that a careful consideration of Cantor's Continuum Hypothesis can serve as a focus for such an investigation.

The reviewer, when first approaching this book, had just taught an undergraduate special topics course in philosophy of mathematics, which he had managed by preparing lecture notes just ahead of the students. The issues of mathematical ontology and of actual infinity had been central in this course, and the Continuum Hypothesis had even made an appearance. The reviewer read this book with the idea that it might serve as a textbook for future versions of this special topics course.


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The author (in the introduction) outlines three possible views of the world of transfinite set theory. The first is the view that Cantor had discovered the realm of transfinite sets, a realm of actual mathematical objects, and the Continuum Hypothesis, being a clearly stated question about the actual realm of transfinite sets, must have a determinate answer (whether or not we can actually find a proof of the correctness of that answer). The second is the view that Cantor had invented the world of transfinite sets (much as an author invents the characters and situations in a work of fiction) and that in such an invented environment it is not necessarily the case that every question about the invented objects has a determinate answer (we do not have any way of answering questions about fictional characters to which the author has not given an answer in the text, and it would seem odd to insist that such questions must nevertheless have definite answers). The third is the typical premodern view that talk of the actual infinite makes no sense (or that there is simply no actual infinity) and so that Cantor's transfinite set theory must be false or nonsensical.

Of course these views admit variations. The last view can be subdivided (as the author does) between modern strict finitist views (which do not allow even arbitrarily large finite collections) and a classical Aristotelean view which has no difficulty with arbitrarily large finite sets and admits a notion of "potential infinity". The reviewer notes that the view that Cantor's transfinite set theory does not completely determine the facts of the matter about the Continuum Hypothesis is not incompatible with strong realist views about the objects of set theory: one could take the position (which the reviewer and others have sometimes contemplated) that the axioms of set theory do not sufficiently define what is meant by "set", while the notion of "constructible set" might be regarded as perfectly determinate (and of course for constructible sets the Continuum Hypothesis has a determinate truth value): someone taking this view would say that the Continuum Hypothesis does not have a determinate truth value, not because there is not a definite world of mathematical objects including actually infinite sets, but because the terms used in the statement of the Continuum Hypothesis have not been adequately defined. The author in analyzing this second position (as a fictionalist position) suggests that it should motivate investigation of all the alternative "possible worlds" in which CH and other independent statements are resolved in different ways because some of these different versions may have advantages over others in the original mathematical applications that motivated set theory in the first place: exactly the same course of action suggests itself to a strong realist who thinks that the terms of set theory are not sufficiently defined, for essentially the same practical reasons.

The author opens (as the reviewer did in his philosophy course) by trying to explain the classical finitist position, usually associated with Aristotle. She notes the existence of the strict finitist view under which even unsurveyably large finite sets are excluded, but does not pursue it in detail. The classical finitist view with its notion of "potential infinity" presents enough difficulty for the modern reader. The treatment of quantities "infinite by addition" (such as the sequence of natural numbers) as merely potential infinities is much easier than the treatment of quantities "infinite by division" (continuous quantities such as geometrical lines) as potentially infinite quantities. The reviewer notes that Spinoza remarked in the *Ethics* that it is "obvious" (from his classical standpoint) that a line is not a collection of points, while to the modern student of even elementary school mathematics it may be entirely unclear what else it could possibly be.

Continuing the discussion of classical finitism, the author includes an interesting discussion of the logic of classes from an Aristotelean standpoint. It is possible to formalize the Aristotelean syllogistic logic in terms of classes, as long as we suppose that in discussing a class which has a (potentially) infinite extent, we are discussing its defining concept (intension) rather than its extension (which we cannot consider as a bare collection of objects, which would have to be actually infinite). The author notes that a nominalist cannot take this view of classes, which relies on some kind of reality for universals: she suggests that a nominalist must either take a strict finitist view or admit the existence of actually infinite collections.

We pass over a brief discussion of (certainly interesting) topics in probability theory, and look at her first informal discussion of Cantor's proof that the power set $P(A)$ of a set $A$ (the set of all subsets of $A$) must be larger than $A$, in the sense that there can be no one-to-one correspondence between the two collections. There is nothing to say about the proof, which is presented in a quite standard way; what is interesting is the discussion which follows of a possible finitist response to the claim that the set $N$ of natural numbers is smaller than the set $P(N)$ of sets of natural numbers. She proposes a picture of $P(N)$ as a potential infinity, using the complete binary tree of height $\omega$. She suggests that a finitist might accept this as a picture of $P(N)$ as a potential infinity without acknowledging that it is a "larger" collection than $N$: the finitist would observe a more fundamental difference between $N$ and $P(N)$ in the fact that single elements of $N$ can be completely given (unlike the whole set $N$) whereas it is impossible to give even a single element of $P(N)$ (an infinite branch of the binary tree) as a completed totality. The finitist might accept Cantor's argument that there can be no one-to-one correspondence between $N$ and $P(N)$ but would not draw the conclusion that $P(N)$ was a "larger" set.

The author then discusses the development of calculus and analysis, asserting that a break with classical finitism occurs with the development of pathological functions such as the nowhere differentiable function first described by Weierstrass. The initial difficulty is that pathological functions might have such peculiar features as having infinitely many changes of direction in a finite interval (and there is a temptation to regard all these infinitely many features of the pathological graph as existing at once). The existence of pathological objects whose properties defied geometric intuition made it clear that analysis must be made fully algebraic. This led to the formal definition of real numbers as either Dedekind cuts or equivalence classes of Cauchy sequences: in either case, an individual real number (or geometrical point) is seen to be an actually infinite object. It is possible (as the author notes) to continue to take the classical finitist view, viewing individual real numbers, given by Cauchy sequences, as being potentially infinite objects which cannot be completely given "all at once". But this leads to the intuitionistic theory of the continuum, which is mathematically coherent but far more complicated than the classical theory.

There follows a quite standard brief introduction of Cantor's informal set theory and the axiomatic set theory *ZFC*. The reviewer notes that there is some question as to whether Cantor's distinction between the transfinite and the Absolute Infinite (p. 116) was actually a response to the paradoxes (it may have been a feature of his thought from the outset) and that it is not accepted by all readers that Cantor accepted the unrestricted axiom of comprehension (which Frege did adopt, mod technicalities). Cantor has an unfair advantage over Frege here as he did not adopt any formal axiomatization, so it is difficult to unequivocally convict him of contradiction. The author correctly notes the philosophically interesting issues about the individual *ZFC* axioms: there is no indication of the method of construction of arbitrary elements of power sets, and of course the axiom of choice is nonconstructive.

There is an error in the discussion of Morse-Kelley set theory on pp. 129–130: no essential strength is added by allowing quantification over classes in just the class comprehension axiom: it is also necessary to allow quantifiers over classes in instances of the axioms of separation and replacement to get Morse-Kelley set theory. Further, while the addition of an inaccessible cardinal does allow interpretation of Morse-Kelley in ZFC, it is worth noting that ZFC with an inaccessible is a stronger theory than Morse-Kelley set theory.

Finally, it is noted that the axiomatization of set theory made it possible to state Cantor's Continuum Hypothesis in precise formal terms, and also made it possible to obtain the results of Gödel and Cohen: the continuum hypothesis is consistent with the axioms of ZFC, because it is true of the constructible sets, and it is independent of the axioms of ZFC, because it is possible to construct models of ZFC using the method of forcing in which the continuum hypothesis does not hold.

There is a discussion of the contradictory theory of Frege, and of Russell's correction of Frege's theory using the theory of types. The reviewer does not choose to go into the complexities of the ramified theory of types in the limited space of this review. We do note that we do not understand the distinction between two forms of the axiom of reducibility which the author attempts to draw on pp. 171–3. No matter what interpretation of the axiom of reducibility is adopted on a philosophical level, the mathematical consequences are the same, and do include the existence of nondenumerable sets. Some philosophical interpretations of the axiom of reducibility allow one to continue to maintain that there are not really any nondenumerable sets from an external standpoint; the axioms of ZFC equally admit such interpretations (any first-order theory has countable models).

There is a discussion of the consistency and independence results in some detail. This discussion is remarkably good in that it both communicates a certain amount of detail and arguably is not *too* technically demanding for a nonmathematician. But the reviewer wonders whether even this much detail can be presented to nonmathematical undergraduates. The reviewer believes that it is misleading to say (p. 187) that all forcing conditions are finite sets of sentences $n \in a_\delta$ where $n$ is some set and $a_\delta$ is a new set being adjoined by the forcing (though this may be a reasonable way to describe certain specific examples). Forcing conditions are generally presented as elements of quite arbitrary partial orders.

In the final chapter, the author returns to the original question. Is the Continuum Hypothesis true or false? Does it even make sense to claim that there is a definite answer to the question as to whether CH is true? She lists different kinds of answer. Some have appealed to mathematical intuition: unfortunately some (such as Cantor) have intuition which tells them that CH is true and others (Cohen and perhaps Gödel) have intuition which tells them that CH is false. A second approach appeals not to intuition but to the mathematical consequences of making one assumption rather than another: a common approach is to consider the consequences in descriptive set theory of CH or other independent propositions in set theory (or the consequences of "reasonable" assumptions in descriptive set theory for CH or other independent propositions). The reviewer notes, for example, arguments current nowadays for the proposition that the cardinality of the continuum "ought to be" $\aleph_2$, based on rather esoteric considerations in descriptive set theory. Finally, the author attempts to outline a solution of her own, which the reviewer does not entirely understand, which seems to involve a view formally similar to classical finitism but admitting the existence of some actual infinities.

The reviewer hopes that the extended discussion above will give some impression of the detailed contents of the book. The reviewer thinks that the book could be used as an undergraduate text for a philosophy of mathematics course, though some supplementary discussion of the more familiar concepts of number and Euclidean space would be useful (even the mathematics undergraduate, to say nothing of the nonmathematical philosophy undergraduate, has more familiarity with number and space, for which there are very similar philosophical issues, than with sets). Some of the material seems to be more appropriate for graduate students. It must also be observed that a professional philosopher or mathematician could find the book interesting reading, though either of these might want more detail: the reviewer finds that the book leaves out interesting mathematical points (understandably but regrettably) and suspects that the reader who is a philosopher rather than a mathematician might have similar feelings about some of the philosophical issues. The reviewer does remark that the mathematics, though sometimes sketchy, is usually correct, which cannot be taken for granted in a treatment of this kind (the few apparent errors we found are noted in the review). The book is pleasant to read and generally clearly written.

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***The Notre Dame Lectures***, edited by Peter Cholak, Lecture Notes in Logic, vol. 18. Association for Symbolic Logic, A K Peters, Ltd., Wellesley, Massachusetts, 2005, vii + 185 pp.

Fall of 2000 must have been an exciting semester for the logic community at the University of Notre Dame. It was a brilliant idea to invite four lecturers to give four short courses on recent advances in four areas of mathematical logic. As every reader of this Bulletin knows, it is not easy to keep up with rapid developments in the diverse spectrum of specialized fields of research, even if they are close to one's own area of interest. All four lecture notes give clear and lively written introductions and surveys of results that were recent in 2000. It seems that the time-frame was just right. It gave each lecturer enough room for comprehensive introductions as well as for detailed development of more advanced material. This works much better than a brief workshop, and is not as heavy as a full semester course. Even though some important later developments are missing, the volume is an excellent source of materials for short courses and independent reading for all, from graduate students to old hands. There is much here to study, there are many exercises and references for further reading. The material is very readable, so the book can also serve as a bed-time reading for those who just want to find out what is going on in the areas covered. There were four courses: on Borel equivalence relations, given by Greg Hjorth; on model theory of diffference fields by Zoé Chatzidakis; on computability and randomness by Rodney G. Downey, and on weak fragments of arithmetic, by Paola D'Aquino. Below I describe the content of each course.

Hjorth's lectures present a proof of two important results on classification of Borel equivalence relations. One is due to Becker and Kechris and one to Hjorth and Kechris, and independently to Becker, for which Hjorth gives a different proof. If $E$ and $F$ are Borel equivalence relations of Polish spaces $X$ and $Y$, then $E$ is Borel reducible to $F$, $E \leq_B F$, if there is a Borel function $f : X \to Y$ such that for all $x_1, x_2 \in X$, $x_1 E x_2$ iff $f(x_1) F f(x_2)$. Let $E_0$ be the equivalence relation on $2^\omega$ defined by $f_1 E_0 f_2$ iff there is $n < \omega$ such that for all $k > n$, $f_1(k) = f_2(k)$. A corollary of the Becker-Kechris theorem says that if $F \subseteq L_{\omega_1,\omega}$ is a countable fragment and $T \subseteq F$ is a complete theory, then either, $E_0$ is Borel reducible to the isomorphism relation for countable models of $T$, or $T$ has an $F$-atomic model. (A model $M$ is $F$-atomic if it realizes no nonprincipal types types over the $F$-theory of $M$.) This corollary is used in the proof of the main theorem of the lectures. It says that if $\sigma$ is a sentence of $L_{\omega_1,\omega}$, for a countable language $L$, then either $E_0$ is Borel reducible to the isomorphism relation for countable models of $\sigma$, or there is a function, which is $\Delta_1$ definable over the hereditarlily countable sets, which assigns countable sets of ordinals as complete invariants to models of $\sigma$. The exposition is almost self-contained. Hjorth gives a lucid introduction to the theory of Borel subsets of Polish spaces, Borel equivalence relations (including a complete proof that $E_0$ is not Borel reducible to the identity relation on $2^\omega$), and Scott's analysis of countable structures.

Even though Chatzidakis' contribution is only ten pages longer, it covers much more ground and is more challenging reading. This is a presentation of the model theory of difference fields and a survey of its applications, including Hrushovski's proof of a version of the Manin-Mumford conjecture and his strengthening of a theorem of Lang-Weil on the number of points of varieties defined over finite fields. The intended level of exposition is "fairly elementary," but, as is the case with all other presentations in model theory of algebraically closed fields, some knowledge of algebraic geometry in necessary and so is some exposition to stability theory. Some readers will certainly find it helpful that many classical results in stability have been reproduced here in the more concrete context of the theory of difference fields. There are many exercises, and specially marked more advanced comments for readers with working knowledge of stability theory.

A difference field is a field with a distinguished endomorphism $\sigma$. A difference field $K$ is generic, if every system of $\sigma$-equations over $K$ (i.e., the equations in the polynomial ring $K[X_1, X_2, \dots, X_1^\sigma, X_2^\sigma, \dots, X_1^{\sigma^2}, X_2^{\sigma^2}, \dots])$ which has a solution in some difference field extending $K$, has a solution in $K$. Chatzidakis gives a brief introduction to algebraic sets and varieties, followed by the complete proof that generic difference fields are an elementary class axiomatized by the theory ACFA consisting of: ACF, the statement that $\sigma$ is an automorphism, and an axiom schema involving projections of $V$ onto $U$ and $U^{\sigma}$, where $V$ and $U$ are varieties defined over $K$ and $V \subseteq U \times U^{\sigma}$. She proves that ACFA is model complete and classifies its completions: they are obtained by adjoining to ACFA a description of the isomorphism type of the difference field consisting of elements algebraic over the prime field. This is followed by an introduction to types, the $\sigma$-topology on $K^n$ (an analog of the Zariski topology), independence, the SU-rank, and all that is used in the proof that any completion of ACFA eliminates imaginaries. Then, section 5 is devoted to the proof that the fixed field of a model $K$ of ACFA, $\{x \in K : \sigma(x) = x\}$, is a pseudofinite field. Section 6 is an introduction to orthogonality, modularity, and canonical bases. In section 7 Chatzidakis discusses genericity in algebraic groups and gives a proof of the follwing theorem: Let $G$ be a group, quantifier-free definable in a model $K$ of ACFA which is modular and whose group operation and its inverse are piecewise definable by terms of the language $\{+, -, \cdot, ^{-1}, \sigma, \sigma^{-1}, e\}_{e \in E}$, where $E \subseteq K$ is $\sigma$-algebraically closed in $K$. If $X$ is a quantifier-free definable subset of $G$, then $X$ is a Boolean combination of quantifier-free definable subgroups of $G$. Furthermore, if $G$ is defined over $E$, then so are these groups. The last section of the lecture lists without proofs various results on models of ACFA and their applications. It includes a discussion of the idea of Hrushovski's proof of a version of the Manin-Mumford conjecture.

In a dramatic change of subject, the third part of the volume consists of Rodney Downey's lectures on computably enumerable reals and randomness. Some of the main questions here still concern the foundations of the subject. What is a computable real? What is the right notion of computable randomness? How should levels of randomness be compared? For the first question there are at least three choices based on the Cauchy definition. Note an important convention: in the lectures as well as in the discussion below, all reals are assumed to be irrational. A real $\alpha$ (=Dedekind cut) is computable if (i) $\alpha$ is a limit of a computable sequence of rationals, or (ii) $\alpha$ is the limit of a computable monotone increasing sequence of rationals, or (iii) $\alpha = \Sigma_{n \in A} 2^{-n}$, for some computable set $A$. These are not equivalent definitions. A real $\alpha$ is computably enumerable (c.e.) iff $L(\alpha)$, the set of rationals smaller than $\alpha$, is computably enumerable. The lectures begin with a theorem of Calude, Khoussainov, Hertling, Wang, and Soare, giving several characterizations of c.e. reals. In particular, $\alpha$ is c.e. iff there is a computable increasing sequence of rationals with limit $\alpha$ iff there is an infinite c.e. prefix-free set $A$ with $\alpha = \Sigma_{n \in A} 2^{-|n|}$, where $A \subseteq 2^{<\omega}$, $|n|$ is the length of $n$, and $A$ is prefix-free if for all $\sigma \in A$, if $\sigma$ is an initial segment of $\tau \in 2^{<\omega}$, then $\tau \notin A$. The proof of the second equivalence uses a theorem of Kraft: If $A$ is prefix-free, then $\Sigma_{n \in A} 2^{-|n|} \leq 1$. Moreover, if $d_1, d_2, \ldots$ is a collection of lengths, possibly with repetitions, then $\Sigma 2^{-d_i} \leq 1$ iff there is a prefix-free set $A$ with members $\sigma_i$, with $|\sigma_i| = d_i$. Furthermore, from the sequence $d_1, d_2, \ldots$ we can effectively compute the set $A$. The proof and its discussion finish section 2 of the lectures.

The class of c.e. reals is not closed under subtraction. However, as shown by Ambos-Spies, Weihrauch and Zheng, the class d.c.e of differences of c.e. reals is a field. This is discussed in section 3. Section 4 and 5 are devoted to degrees of representations. A c.e. sequence of rationals whose monotonic limit is $\alpha$ is called a representation of $\alpha$. Downey discusses a theorem of Calude, Coles, Hertling, and Khoussainov saying that each $\alpha$ has a representation of degree $L(\alpha)$, and a theorem of Soare that if $B = \{q_i : i < \omega\}$ is a representation of $\alpha$, then $B \leq_T L(\alpha)$. There is also a discussion of other results concerning truth table reducibility, weak truth table reducibility, splittings, and $m$-degrees. In section 5 similar questions are discussed for degrees of prefix-free presentations, including a sketch of a proof of a theorem of Downey and LaForte saying that there is a c.e. real $\alpha$ which is not computable, but such that if $A$ is a prefix-free presentation of $\alpha$, then $A$ is computable.

Sections 6 and 7 contain an introduction to Kolmogorov complexity $C(x)$ and prefix-free complexity $K(x)$, for $x \in 2^{<\omega}$. The prefix-free complexity is defined like Kolmogorov complexity, except the Turing machine involved operates on a prefix-free domain. Two identities relating $C(x)$ to $K(x)$, due to Solovay, are given. This is a preparation for a discussion of randomness of reals. Four notions are compared in section 8: Chaitin-Levin, Kolmogorov, Martin-Löf, and Solovay randomness. For example, a real $\Sigma_{n\in A}2^{-n}$ is Chaitin-Levin random if there is an $\mathcal{O}(1)$ such that for all $n$, $K(A|_n) \geq n - \mathcal{O}(1)$. (Here $A|_n$ most likely is $\Sigma_{n\in A\cap[0,n]}2^{-n}$. This and some other notational conventions are not defined in the text. Unfortunately, the restriction sign and a few other TeX symbols did not show up in the printed version. This causes some difficulties.) Only the Kolmogorov complexity is a weaker notion, all others turn out to be equivalent by theorems of Solovay and Schnorr.

For reals $\alpha$ and $\beta$, $\alpha \leq_E \beta$ if for all $n$, $E(\alpha|_n) \leq E(\beta|_n) + \mathcal{O}(1)$, where $E$ is either $K$ or $C$. This and other reducibilities are discussed in section 9. Solovay's reducibility is defined and its characterizations are given. It is shown that Chaitin's $\Omega$ is complete with respect to $\leq_S$ among the c.e. reals, i.e., $\alpha \leq_S \Omega$ for all c.e. reals, and for any real, if $\Omega \leq_S \alpha$, then $\alpha$ is random. The section ends with a proof of a theorem of Kučera and Slaman generalizing this: if $\alpha$ is random and c.e., then for all c.e. reals $\beta$, $\beta \leq_S \alpha$. The Solovay degrees of c.e. reals form a complex dense upper semilattice whose first order theory is undecidable. Most of what is known about this structure, due to Downey, Hirschfeldt, LaForte, and Nies, is presented in section 10. Section 11 is devoted to other measures of relative randomness and some very interesting results due to Chaitin, Solovay and others. Much material here comes from an unpublished manuscript of Solovay on Chaitin's work. Some new, much simpler proofs due to Downey, Hirschfeldt and Nies, are given. In particular, it is shown that there is a c.e. noncomputable set $A$ such that, for all $n$, $K(A|_n) \leq K(n) + \mathcal{O}(1)$.

The last course by D'Aquino is on model theory of weak fragments of arithmetic. It starts with a discussion of IOpen, the weakest fragment in which the induction schema is restricted to quantifier-free formulas. D'Aquino outlines Sheperdson's construction of a computable nonstandard model of IOpen and briefly describes an extension of it due to Berarducci and Otero. For the rest of the lectures the basic fragment is bounded induction $\mathrm{I}\Delta_0$. The first topic is Parikh's theorem on $\Sigma_1$ functions which are provably total in $\mathrm{I}\Delta_0$, and its consequences. Some basic number theory is developed in $\mathrm{I}\Delta_0$ culminating in a proof of a version of the Chinese Remainder Theorem. The next section is devoted to $\Delta_0$-representations of the graph of the exponential function, giving an invariant meaning to $x^y = z$ in all models of $\mathrm{I}\Delta_0$. Three representations are given: Gödel's (this one is $\Sigma_1$ and requires $\mathrm{I}\Sigma_1$), Paris', and Pudlak's. In section 4, D'Aquino presents a proof of McAloon's theorem that every nonstandard model of $\mathrm{I}\Delta_0$ has a nonstandard initial segment which is a model of PA, and she discusses her extension of this result charaterizing those elements $a \in M \models \mathrm{I}\Delta_0$ for which there is an initial segment $J$ of $M$ such that $J \models$PA and $a \in J$. The main part of the lectures consists of an extensive survey of problems and results concerning provability of various number-theoretic facts in $\mathrm{I}\Delta_0$ and its extensions $\mathrm{I}\Delta_0 + \exp$ and $\mathrm{I}\Delta_0 + \Omega_1$, where $\Omega_1$ is the axiom $\forall x, y \exists z (x^{[\log_2 y]} = z)$. Here is a list of topics: The role of the $\Delta_0$-Pigeonhole Principle and Macintyre's question whether it is provable in $\mathrm{I}\Delta_0$; work of D'Aquino on a local version of Chebyshev's theorem (in $\mathrm{I}\Delta_0$); the question of provability of the MRDP-theorem in $\mathrm{I}\Delta_0$, and related results of D'Aquino on solutions of Pell's equations in models of $\mathrm{I}\Delta_0$; finally, residue fields $F_p$, for prime nonstandard elements $p$ in models of $\mathrm{I}\Delta_0$. The last section gives an outline of a theorem of D'Aquino and Macintyre saying that in a model of $\mathrm{I}\Delta_0 + \Omega_1$ each $F_p$ has a unique extension of each finite degree $n$.

As I already mentioned, all four lecture courses cover attractive areas of mathematical logic in a lively and highly readable manner. A very successful volume.

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William Tait. ***The provenance of pure reason: essays in the philosophy of mathematics and its history.*** Oxford University Press, Oxford, 2005, x + 332 pp.

Plato has a bad reputation these days: the word "Platonism" is usually accompanied by the word "naive," and the notion that mathematical discourse has meaning in virtue of reference to nonspacial, atemporal objects is judged to be convenient but ultimately untenable. Moreover, the views of mathematics that currently dominate the analytic tradition are variants of Quinean empiricism, a position that is more closely linked to that of Plato's rival, Aristotle. On such views, mathematical claims depend on experience for justification in an indirect but continuous manner.

The problem with this picture is that it fails to account for the distinct character of mathematics vis-à-vis the empirical sciences. Quine's arguments that there is no sharp distinction between analytic and synthetic knowledge does not absolve us of the philosophical task of making sense of methodological differences that are glaringly salient in practice. It is therefore encouraging to read a collection of essays that is based on a conception of mathematics that is more faithful to the subject.

At the risk of oversimplifying, Tait's central views can be summarized as follows. First, the only viable foundation for mathematics is that afforded by the axiomatic conception. Whatever basis mathematics has in intuition and experience, it only becomes mathematics *per se* when we pass from these to first principles, that is, the primitive concepts, definitions, and axioms that form the basis for mathematical reasoning.

Second, this axiomatic conception makes it possible to distinguish between questions that are internal to mathematics, and those that are external. From an internal point of view, the question "do numbers exist?" is trivially answered in the affirmative. From an external point of view, the question is close to nonsensical; at least, it presupposes that there are extra-mathematical (Tait calls them "superrealist") grounds that could serve to provide an answer, and such grounds are far from apparent. Similarly, uses of the notion of "truth" within mathematics are invariably benign and easily understood in a deflationary sense (excluding, of course, the model-theoretic notion of truth, which is simply a formal mathematical notion). In contrast, Tait seems to feel that philosophers' external notions of truth have had little to add to our understanding of mathematics other than a good deal of unnecessary confusion.

Finally, the axiomatic conception also makes it possible to distinguish between internal and external evaluations of a theory:

> . . . even in its application to empirical science, the value of mathematics lies precisely in its separation from empirical matters, in its being entirely a creature of reason. For only with this separation can we distinguish between the question of the adequacy and coherence of the mathematical theory, in itself, in terms of which we wish to understand the phenomena and the question, given its coherence, of whether or not the rational structure it describes is the right one for the phenomena at issue. (page 5)

The phrase "given its coherence" is indicative of Tait's skepticism that there is a substantial philosophical project in the latter. He takes this to support a hands-off attitude towards development of mathematics, as advocated by Cantor in his oft-quoted passage on mathematics and freedom.

The views just described are laid out most clearly in Chapter 3, *Truth and Proof*: *The Platonism of Mathematics*, which is one of Tait's most important works; and in a sequel, *Beyond the Axioms*: *The Question of Objectivity in Mathematics*, which appears as Chapter 4. But they are the driving force behind all the essays, and serve to unify the diverse set of topics they treat. The first six essays explicate various aspects of mathematics including finitism, the axiom of choice, the law of the excluded middle, and set-theoretic reflection principles. The last six essays interpret the views of a number of important historical figures in the philosophy of mathematics, including Plato, Cantor, Frege, Hilbert, Wittgenstein, and Gödel. For the most part, these interpretations are seen to support the conception of mathematics described above. As one would expect from a logician of Tait's stature, all the essays are informed by a sound formal understanding.

The centrality of proof and axiomatic reasoning endears Tait's account to proof theorists; his position is, in large part, an articulation of the understanding of mathematics that forms the basis for proof-theoretic research in the foundations of mathematics. It is therefore inevitable that some will label the conception a brand of formalism, in a pejorative sense. Tait is sensitive to this, and has taken pains to argue, throughout his work, that his characterization is not only philosophically coherent and faithful to mathematical practice, but, further, does not deny the essential open-endedness of mathematics or its role with respect to the empirical sciences. Indeed, he sees Wittgenstein as having solved the problem of reconciling the axiomatic conception with a reasoned empiricism:

> Only empirical explanation is possible for why we have come to accept the basic principles that we do and why we apply them as we do—for why we have mathematics and why it is at it is. But it is only within the framework of mathematics as determined by this practice that we can speak of mathematical necessity. In this sense, which I believe Wittgenstein was first to fully grasp, mathematical necessity rides on the back of empirical contingency. (page 116)

Tait essentially takes Wittgenstein's process of "hardening to a rule" to describe the dialectic process that leads to axiomatic first principles; and he takes Wittgenstein's characterization of mathematics as a "form of life" to be life within an axiomatic framework. The fact that the excerpt just quoted appears almost as an aside in an essay on the axiom of choice is characteristic of the tenor of these essays. So, too, is the conclusion that he draws from the observations:

> Notice that this view of things leaves no room for so-called epistemological foundations of mathematics or for "foundations of mathematics" in the sense of attempting to show that mathematics is "true" as opposed to showing that, in mathematics, a particular proposition is true. (*ibid.*)

Tait's essays are a pleasure to read. They are crisp, intelligent, compelling, and engagingly cantankerous. Space does not allow me to discuss them in detail, but they all merit serious consideration, including his analysis of the relationship between the axiom of choice and the law of the excluded middle; his analysis of reflection principles in set theory; his favorable interpretation of Plato's theory of forms (which encourages us to dismiss common contemporary readings that "attribute to him views that would have been as foolish or unintelligible in his time as they are in ours"); his readings of Cantor, Frege, and Wittgenstein; and his discussion of the unpublished writings collected in the third volume of Gödel's *Collected Works*. Instead, I will say only a few words about two of Tait's most longstanding interests, finitism and type theory.

Hilbert originally hoped to prove the consistency of infinitary mathematics using only finitary methods. Tait took up the latter notion in a 1981 essay, *Finitism*, which is reproduced in Chapter 1, and a 2002 follow-up, *Remarks on Finitism*, which is reproduced in Chapter 2. His goal was to provide an explication of finitism that is, first, independently interesting and defensible, and, second, consistent with the remarks in Hilbert and Bernays's two-volume *Grundlagen der Mathematik*. Tait has famously defended the formal system known as primitive recursive arithmetic, $PRA$, as the only analysis that meets both standards. Of the two goals, the second is the more nebulous; Hilbert and Bernays never articulated a clear position, and they had to adapt their views to Gödel's discovery of the incompleteness theorems between the writing of the two volumes. To date, however, there has been no conceptual analysis as compelling as Tait's, and although an appendix to Chapters 1 and 2 revises some of his claims as to Hilbert and Bernays's historical views, his commitment to *PRA* as the best conceptual explication of finitism has remained firm.

I will refer the reader to Tait's essays for the full analysis, and simply comment on an interesting issue that arises in passing. Hilbert's goal was to reduce modern mathematics to an epistemologically privileged fragment; in particular, he took the finitary objects to be "surveyable" in all their parts, thereby making it possible to ground finitary mathematics in a faculty of intuition. Tait, interestingly, challenges this assessment. He does, of course, allow that the concept of number is fundamental to most branches of mathematics, and this certainly accords elementary number theory a special status. But his argument casts doubt on whether there is an independent epistemic standard under which the concept of number is so privileged, and rightly emphasizes that stock appeals to a faculty of intuition fall short.

A number of essays in the book employ the "type-theoretic" characterization of mathematics, of which Tait himself was an early proponent, along with William Howard and Per Martin-Löf. On this view, mathematical objects always bear appropriate "types," and these, in turn, are characterized by rules that tell one how objects of that type are to be constructed and used in mathematical discourse. A further component of this view is that any mathematical proposition itself can be viewed as a type, namely, the type of objects that constitute a proof of the proposition. Both formal and philosophical considerations encourage one to view such types as datatype specifications, and the corresponding objects as computational data. As a result, the type-theoretic "propositions-as-types" framework is held by many to explicate constructive mathematics.

Tait is unusual in using type theory to explicate a *classical* mathematical standpoint, and he suggests the term "construction-theoretic" to characterize such a broader notion of constructivity. In some ways, the resulting characterization of classical mathematics is awkward. For example, in Chapter 5, Tait invests a good deal of effort in explaining why, on a construction-theoretic point of view, it is reasonable to postulate objects of type $\sim\sim A \to A$ for every proposition $A$. Another difficulty (and one which, in my view, Tait does not sufficiently address) has to do with extensionality and dependent types. Roughly, the question as to whether a (presentation of) an object has a certain type becomes undecidable unless one is careful as to the types of reasoning about equality that can play a role in type judgments; the necessary restrictions, while reasonable from a computational/constructive standpoint, seem artificial from a classical perspective. In short, the set-theoretic formulation of classical mathematics is in some ways more natural, and it is not clear what advantages the type-theoretic formulation has to offer.

Both the conceptual and historical analyses in this collection contain a wealth of insight. Tait's dismissals of alternative views is, however, sometimes too glib. For example, while skeptical that an external notion of truth has any role to play in the philosophy of mathematics, in selecting Reuben Hersch as a prototypical realist he has chosen an easy target; contemporary versions of naturalism at least purport to provide more robust notion of truths against which mathematical axioms can be measured. He similarly has little to say about the internal theoretical virtues that a mathematician might appeal to. One gets the sense that, on Tait's view, the proper role of the contemporary philosopher of mathematics is to clear away the metaphysical cobwebs and junk we have accumulated since Plato's time, and then step aside to let mathematics flourish. But viewing mathematics as a self-sufficient "form of life" does not inherently give us the conceptual wherewithal to engage in the practice reflectively, to evaluate our choice of axioms, or to cope with methodological and foundational disagreements when they arise. Moreover, as of late, some have expressed hope that it is possible to make sense of evaluatory claims in mathematics that go beyond judgments of correctness; for example, one might hope to understand what it is that makes a mathematical concept fruitful, powerful, or natural. An overly narrow focus on the axiomatic characterization of mathematics is not conducive to such a program.

Ultimately, however, where Tait is skeptical as to the role that philosophy can play, his skepticism is justified. His challenges to the philosophical community are therefore sharp and to the point. What this collection of essays provides is a lean, no-nonsense view of mathematics that can form the basis for substantive inquiry. The resulting conception of the subject is one that no serious philosopher of mathematics can ignore.

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***From sets and types to topology and analysis—towards practicable foundations for constructive mathematics***, edited by Laura Crosilla and Peter Schuster, Oxford Logic Guides, vol. 48. Clarendon Press, 2005, xix + 450 pp.

This book reminded me of another collection of papers, also co-edited by Peter Schuster: ***Reuniting the antipodes—constructive and nonstandard views on the continuum***, Kluwer, 2001. It brought together various non-classical approaches to Analysis. When I reviewed this book for ***Nieuw Archief voor Wiskunde*** I naturally raised the question: have the antipodes been reunited? Of course the answer was *no*: with few exceptions (most notably Erik Palmgren's contribution), the authors stayed firmly within their specialism.

A similar reunification project has led to the present book: like the ***Reuniting*** volume, it is also the proceedings of a workshop in Venice, this one from 2003. And in this case the 'antipodes' are the students of formal foundational systems (and their semantics) on the one hand, and the 'practicioners', the researchers doing 'constructive mathematics' on the other. Accordingly, the papers have been arranged in two parts: Part I—Foundations and Part II—Practice; an Introduction is meant to provide a link.

Indeed, the relationship between the people who study formal systems (whether from the point of view of proof theory, model theory or category theory) and the 'practical' constructivists has been delicate for decades.

I think the problem has two sides. On the one hand, constructivists of various schools (Brouwer, Bishop, different types of computable analysis) were loath to commit themselves to any well-specified theory of sets to work in, following Brouwer's aversion to Logic in general. In the Introduction, the editors mention this point of view, albeit (in my opinion) with rather more respect than it deserves. Can't we, at last, after 100 years agree that Brouwer's quarrels with formal logic (a subject then in its infancy), much similar to Kronecker's hostility towards Cantorian set theory, did not represent anything revolutionary but were *rearguard actions*, the conservative stance of an otherwise brilliant mathematician? In fact, it is hard to disagree with Kreisel's scathing review of the reprinted Brouwer ***Cambridge Lectures on Intuitionism*** from 1946–51 ("This sad little book ... ", ***Canadian Philosophical Review***, vol. 2 (1982), pp. 249–251) in which the great Intuitionist completely ignores the work of Kleene, Heyting and Kolmogorov but finds time to spend an entire paragraph to a silly theorem as 'absurdity of absurdity of absurdity is equivalent to absurdity'.

The hostility towards formalism, until recently quite widespread among construcivists, is the more curious if one observes that the majority of them are logicians.

On the other hand however, people who were concerned with the semantics of intuitionistic systems, often topos-theorists, showed a very similar reluctance to back up vague and pretentious claims about category theory providing new 'foundations' and toposes as 'worlds of sets' with precise analyses. Recently, I had occasion to ask a number of reputed topos-theorists the same question: 'is $N$ projective in the free topos?' (This question is answered in the affirmative in Lambek and Scott's ***Introduction to higher order categorical logic***, but for a proof they refer to an unpublished paper by Makkai and work by Friedman and Scedrov, of which it is not clear that it means the same thing) All of them said yes, this was well-known, and easy; on further prodding though, some proved to be mistaken as to what the logical meaning of it was, and none of them could even outline a proof. For such a fundamental question!

So, again, have the antipodes been reunited? Maybe surprisingly, my answer now would be: to some extent, *yes*. Researchers from previously disjoint schools have found connections between each other's work and became interested in other fields. Precise work on models of intuitionistic set theory in toposes, and predicative versions, pioneered by Joyal, Moerdijk and Palmgren, has led to a better understanding of these theories, as well as to a lot of variations on them. Models of IZF, CZF and variations can be compared, embedded into each other, axiomatized. Workers in locale theory and formal topology collaborate. A prominent researcher in Algebraic set theory as Alex Simpson takes an active interest in computable analysis. Bas Spitters, raised in Brouwer's tradition, is now looking at topos-theoretic models. The relatively new program of Intuitionistic Reverse Mathematics, advocated by Ishihara, forces the study of concrete formal systems.

A review such as this one is hardly suitable for discussing individual contributions. Instead, for such a discussion the reader is referred to the Introduction by the editors, who do an admirable job in 'talking them together'.

This Introduction is slightly less commendable where it tries to sketch the history of constructive mathematics through the 20th century. Two developments from which the subject has benefited enormously have been completely left out. The first is topos theory (for all its shortcomings); the second is the rise of Computer Science, in particular the abstract semantics of programming languages, but also the development of proof assistants. I find the failure to mention Computer Science at all very strange, given that two of the contributed papers (Hancock-Setzer and Berger-Seisenberger) are explicitly about programs.

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