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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).

KIT FINE. ***The limits of abstraction.*** Clarendon Press, Oxford, 2002, x + 203 pp.

A second-order abstraction principle is a principle of the form

$$\S x(\phi x) = \S x(\psi x) \leftrightarrow \mathrm{R}(\phi, \psi)$$

where § is a term-forming variable-binding operator which forms singular terms from open sentences and R is an equivalence relation over properties (or whatever is in the domain of the second-order variables). In the ***Grundlagen***, Frege had proposed basing arithmetic on the following abstraction principle:

$$\forall F \forall G(\mathrm{n}xFx = \mathrm{n}xGx \leftrightarrow F \text{ 1-1 } G)$$

where $\mathrm{n}xFx$ reads 'the number of Fs' and with F 1-1 G the second-order sentence which expresses the existence of a one-one correspondence between the F's and the G's. I will refer to this as the 'Bijection Principle' or 'BP'. However the 'Caesar problem', the inability of BP to decide 'mixed' identities such as 'Julius Caesar is the number five', led him instead to reduce numbers to classes (fruitlessly, since the same problem arises for them). When he came to formalise his informal proofs of ***Grundlagen***, in volume I of the ***Grundgesetze***, Frege added to his formal system the abstraction principle Basic Law V:

$$\forall F \forall G\big(\{x : Fx\} = \{x : Gx\} \leftrightarrow \forall x(Fx \leftrightarrow Gx)\big)$$

(actually a more general form of the above) and derived an axiomatic basis for second-order arithmetic from it. But just as he was about to send to press Volume II, extending his

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treatment to analysis, Russell played the party pooper by pointing out that Basic Law V is inconsistent in Frege's underlying second-order classical logic.

However Frege used Basic Law V essentially only to derive BP. The Peano-Dedekind axioms can be derived from the latter alone, as sketched by Crispin Wright in his ***Frege's conception of numbers as objects*** in 1983. Wright, and others such as Bob Hale and, in a different, more constructivist, fashion, Neil Tennant, have inaugurated a 'neo-Fregean' or 'neo-logicist' revival of the idea of founding mathematics on consistent abstraction principles, maintaining that by so doing one can avoid the standard epistemological problems which plague mathematics, platonistically construed.

But many philosophers feel that Basic Law V casts a shadow over all abstraction principles. What the neo-logicist needs is a criterion which separates out a consistent class of abstraction principles from the others, a criterion which we can reasonably believe generates only mathematical truths; this consistent class, moreover, must be strong enough to serve as a foundation for mathematics.

Kit Fine's ***The limits of abstraction*** is a book-length reworking of a long article of the same name (in ***The philosophy of mathematics today*** ed. M. Schirn, (Oxford: Oxford University Press, 1998) pp. 503–629) which addresses in considerable detail the philosophical and technical issues which arise when one attempts to found mathematics on abstraction principles, including those of the second-order form given above. It is in four parts, the first two broadly philosophical- an introduction to the main themes and a discussion of the Context Principle (a wholescale addition to the material from the earlier article)—with the second two parts concerned with the technical details. Part III is an analysis of 'acceptability' for abstractions, the final part setting out Fine's 'general theory of abstractions' ('GA').

What, then, makes for the acceptability of an abstraction? We want them to be 'non-inflationary': that is the equivalence relation featuring in the right-hand-side (the 'identity criterion' for the abstraction, pp. 103–4) should partition up the domain of individuals into no more equivalence classes than there are members of the domain. But this criterion will not single out a consistent set of principles since pairwise incompatible abstractions are non-inflationary with respect to different sizes of domain. Fine resolves this problem within a third-order setting (though he does also present schematic second-order simulacra of his GA theories). One generalises from abstraction operator constants $\S_1$, $\S_2$ and so on, to a three-place relation: x is an abstract, with respect to relation R, of property P—$x\ \mathrm{Abst}_{\mathrm{R}}\ \mathrm{P}$. One then lays down as an 'existence axiom' that every non-inflationary equivalence relation is 'applicable', that is, there is an abstract with respect to the relation for every property. The idea is a sort of plenitude principle: we admit as many abstractions as we can so that, as a first approximation, any model will contain all principles which are non-inflationary for a domain of the size of that model.

Now our primitive abstraction predicate ($x\ \mathrm{Abst}_{\mathrm{R}}\ \mathrm{P}$) has no very definite meaning without an identity condition on abstracts: Fine's basic Identity Axiom (p. 166), though he also treats of some variants, is:

$$x\ \mathrm{Abst}_{\mathrm{R}}\, F \ \ \&\ \ y\ \mathrm{Abst}_{\mathrm{S}}\, G \rightarrow \big(x = y \leftrightarrow \big(\mathrm{R}(F, G)\ \ \&\ \ \forall H\big(\mathrm{R}(F, H) \leftrightarrow \mathrm{S}(F, H)\big)\big)\big)$$

One effect of this principle is that the abstract of property P under R cannot be the same as that of property Q under S unless the equivalence class of the first property under R is the same as the equivalence class of the second under S. But it then follows that the non-inflation condition is insufficient to block paradox—a 'hyperversion' (p. 167) of Russell's paradox can be derived. The problem is that even when each of a set P of principles partitions up the properties of a domain of size κ into no more than κ equivalence classes, if the total number of equivalence classes featuring in one or other principle in P is greater than κ we have 'hyperinflation' and they cannot all be true together.

Fine therefore adds to the non-inflationary constraint in the existence axiom the condition that an equivalence relation be invariant, if it is to be applicable. A relation R (over a fixed domain, here I focus on what Fine calls invariance for a 'local' relation p. 111) is invariant if whenever $\mathrm{R}(\mathrm{C}, \mathrm{D})$, for subsets C, D of the domain then $\mathrm{R}(f(\mathrm{C}), f(\mathrm{D}))$ holds, for any permutation f. Thus bi-equinumerosity is a congruence for invariant relations, where two properties are bi-equinumerous iff there is a one:one map from one onto the other and from the complement of one onto the complement of the other. Such a level of abstraction from the particularities of the individual members of the domain perhaps gives the invariant relations the flavour of logical relations.

Fine refines the notion further: for example a predominantly invariant relation is invariant across a permutation which is an identity on an exponentially small subset of the domain; cardinal λ is exponentially small with respect to κ just in case $\kappa^{\lambda} \leq \kappa$. Modifications of the axioms which substitute such variant notions of the domain can be shown to have models too—Part IV Section 2. Fine takes the discussion of abstraction principles, in fact, to a level of generality and comprehensiveness well beyond the existing state of the art.

Consideration of the underlying model theory of the structures which satisfy his basic principles of identity and existence lead Fine to a very interesting new type of cardinal, the unsurpassable cardinals. These are cardinals κ for which $2^{\mathrm{cp}(\kappa)} \leq \kappa$, where $\mathrm{cp}(\kappa)$ is the number of cardinals $\lambda \leq \kappa$. He then shows (p. 158) that in any unsurpassable domain we can create a model in which the Identity Axiom holds yet all the non-inflationary, invariant equivalence relations are true together and that for a given set of urelements of size κ meeting certain fairly weak assumptions, there are models of GA and its variants at the first unsurpassable cardinal $\geq \kappa$. The unsurpassables thus play a similar sort of role in Fine's GA theory as inaccessibles do in ZF set theory, but a conflicting one: no inaccessible is unsurpassable. As to the relations between the two standpoints, it is consistent with ZFC plus the existence of a suitable supercompact cardinal that there are no unsurpassables. It is also consistent with ZFC that there are unsurpassables since ZFC + GCH entails that only fixed points in the aleph series (and $\aleph_0$) are surpassable (Stewart Shapiro has pointed out that Fine incorrectly states, p. 158, that ZFC + GCH entails that all singular cardinals are unsurpassable.)

Fine then ingeniously turns the problem of hyperinflation to his advantage. In the generalised theory of abstractions, he works with 'divisor' principles which divide the domain up into two exclusive and exhaustive equivalence classes. We need only assume in the background logic the existence of two individuals to guarantee, using his existence axiom, the applicability of each divisor abstraction, that is, the existence of an abstract governed by the divisor abstraction for each property of the domain. The theory ensures the existence of sufficiently many distinct equivalence classes, with each of the divisor abstractions generating the equivalence relations, to provide for the ontology of analysis. (Fine gives an interesting variant in which we take abstracts to be sets of properties; we then get a form of set theory with invariance constraining the comprehension axiom scheme and can generate analysis without need of the assumption of at least two individuals).

The technical material in the second half of the book is extremely well-done and impressive. There are, as far as I can see, next to no serious mistakes. John Burgess has pointed out that Theorem 5 on p. 159 is incorrect; the correction requires us to consider a set K which is smaller than a set smaller than the cardinality of the model (and adjust dependent results accordingly). There are, however, a number of typographical mistakes in these two technical parts. I noticed the following:

p. 63, 'III.2.6' should be 'III.6', (line -6);

p. 106, in the gloss on the definition of 'separated', ll. -11 to -9, it should read 'are identified if and only if their associated equivalence classes are the same';

p. 109, l. 8, the fifth occurrence of 'C' should be 'D';

p. 124, l. 3, '$f^+(C)$' should be '$f^+[C]$';
p. 125, l. 15, '$f^*[D]$': the 'D' should be 'C';
p. 129, l. -7, 'to care of' should be 'to take care of';
p. 138 ll. -8–7: 'bicard(C) = bicard(C)', should be 'bicard(C) = bicard(D)';
p. 140, l. -8, 'card(A) = **g**': '**g**' should be '**d**';
p. 143, in the definition of exponentially large, '$\leq$' should be '$>$';
p. 144, l. 2, the subscripting has gone wrong and it should read:

$$\aleph_1^{\aleph^0} = \aleph_1 \text{ whereas } \aleph_\omega^{\aleph^0} = \aleph_{\omega+1};$$

p. 155, the last line has 'criteia' instead of 'criteria';
p. 157, l. -11, the '$\leq$' has been wrongly superscripted along with cp(**c**);
p. 166 l. -8 the capitalised 'Y' should be 'y';
p. 167, the order in the second paragraph is wrong: where it describes 'Noninfl' as the second conjunct in the formula it is the third, 'Inv' is not the third conjunct but the second;
same page, l. -10, 'abstract' should be 'abstraction';
p. 177, l. 8: there is a redundant repetition of '$2^{q.k}$';
p. 189 l. -16–13. 'The right to left form of the identity axiom is used.': this should be 'left to right';
p. 190, l. 2 a right bracket after the last 'eq C' is missing.

The first two philosophical sections are hard to summarise snappily because Fine, though clearly sympathetic to neo-Fregeanism, is not a defender of the doctrine; in fact he criticises many important theses advanced by neo-Fregeans such as Wright and Hale. In section I.3 for example, he criticises the use they attempt to make of Frege's idea of 'reconceptualisation' in *Grundlagen* sections 63–64 and part II on the context principle is fairly negative as regards the possibility of according abstraction principles the status of implicit definitions without the usual epistemological cost of giving an independent justification of existential commitments.

It is hard to see, in fact, how Fine's general theory of abstractions can serve neo-Fregean purposes. These require, if there is to be some epistemological advantage over more standard forms of mathematical platonism, that we can come to know, or at least reasonably believe in, the truth of the basic abstraction principles purely by grasping their meaning, or their status as implicit definitions, or some such. But, arguably, it is question-begging for the neo-Fregean to assume that we can grasp the truth of the identity and existence axioms of Fine's systems in such a fashion, or indeed that we can grasp the truth of the existential claims in the axiom scheme of comprehension of second-order logic. A large chunk, particularly of part I, however, is positive, an informal discussion of the formal results to follow plus an assessment of their philosophical implications, one which is always interesting. No systematic position in philosophy of mathematics is set out however, nor does Fine claim to be doing so, though he mentions a future project, developing a theory of 'procedural postulationism' (pp. *v*, 100).

As to the overall value of the book, the publisher describes it as 'essential reading for all those working on the philosophy of mathematics'. This might bring to mind the blurbs one sometimes see attaching to the nth logic textbook or n^kth introduction to the philosophy of mind describing them as indispensable, essential reading for all students of these areas. In the case of this book, however, there is no exaggeration. At any rate, for anyone interested in abstraction principles as a foundation for mathematics, the book is indeed absolutely essential reading, taking, at it does, the discussion to new levels of philosophical comprehensiveness and technical sophistication.

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Kurt Gödel collected works IV–V: correspondence, edited by Solomon Feferman, John W. Dawson, Jr., Warren Goldfarb, Charles Parsons, and Wilfried Sieg, Oxford University Press, Oxford, 2003, xix + 646 pp. and xvii + 664 pp.

These two volumes bring to completion the publication of Gödel's ***Collected works***. Volume I, with the published papers of 1929–1936, appeared already eighteen years ago (1986), volume II with the publications of 1938–1974 in 1990, and volume III, the highly interesting collection of previously unpublished work, in 1995. The new volumes IV and V contain selected correspondence with fifty persons, chosen, as stated (twice) in the Preface, on the basis of "intrinsic scientific, philosophical, or historical interest," or to "illuminate Gödel's thoughts or his personal relationships with others." Letters in German are given with a facing English translation. Moreover, drafts of letters and passages from such are included, either because Gödel had not sent a letter, usually a missing answer, or because a draft contained crossed-out passages or the like that could prove informative. Typesetting has been done within the editorial project, which has made it possible to make precision-use of various typographical devices. One thing they do is to reveal Gödel's enormously hesitant way of composing sentences.

Two persons, namely Paul Cohen and Georg Kreisel, did not permit the publication of their letters in the correspondence. In the case of Cohen, Solomon Feferman's introduction covers the gaps to a sufficient extent. In the case of Kreisel, there was just one letter by Gödel. The reader can only wonder what panorama of the situation of logical research the letters from Kreisel to Gödel could paint, for the time span of 1955 to 1972.

The editing, overall preparation, and appearance of the volumes make an excellent impression. Misprints seem to be mainly trivial one-letter cases on the left (German) side. For each of the fifty correspondents, an essay, usually by some of the editors, explains the circumstances of the exchange as well as possible highlights of the letters. In this respect, the volumes are similar to the first three in the series. The obvious models for the editorial introductions are those in van Heijenoort's collection ***From Frege to Gödel***. Amusingly, the correspondence with van Heijenoort in these Gödel-volumes, altogether eighteen letters, is exclusively on matters concerning the translations and editorial introductions of Gödel's papers in that collection. Even this correspondence called for an introduction!

The profoundness of the editorial introductions seeks its equal in publications of scientific correspondence. They are extremely useful and time-saving for scholarly work in the recent history of logic. A logician reading the correspondence casually for enjoyment might instead do better to look at the introductions afterwards, with the pleasure of finding things out on one's own. Gödel's letter of 20 March 1955 to von Neumann offers a good example in this respect

The number of co-workers for these volumes is considerable and brings variation to the modes of presentation of the introductions, as well as a few incongruities. For example, the introduction to the correspondence with Popper states that Gödel and Popper came to know each other in 1935; a date that could stem from the impression created by Popper's autobiography of 1974. Popper mentions there (p. 80 in the Popper volume of the ***Library of living philosophers***) a lecture at Karl Menger's colloquium with Gödel in the audience. In a letter of August 1932, Gödel writes to Menger: "Recently I became acquainted with a Mr. Popper (a philosopher), who has written an infinitely long paper in which, so he says, all philosophical problems are solved. He endeavored to awaken my interest in it, and also appealed to the fact that you know him and that his plan to remove the problem of "meaning" from the world has pleased you very much. Do you think anything of him?"

It would not do justice to these volumes to just indicate their contents. On the other hand, a critical review of the whole could easily run to dozens of pages in length. I shall therefore consider in detail just two topics, namely Gödel's completeness and incompleteness theorems, with the hope of giving at least an idea of how these volumes can be used and what sorts of novelties can be found in them.

Gödel's recollection of his achievements in the early 1930s is known to have failed, as is well documented in the correspondence. The Herbrand-Gödel definition of recursive functions was given in Gödel's lectures at the Institute for Advanced Study in 1934. Gödel had written to van Heijenoort that Herbrand's letter of 1931 with the definition had disappeared, but that the definition "was formulated *exactly* as on page 26 of my lecture notes," adding a bit later that his "recollection is very distinct and was still very fresh in 1934." Well, the editors found Herbrand's letter and Wilfried Sieg in his editorial introduction had to conclude (vol. V, p. 12) that "on the evidence of Herbrand's letter it is clear that Gödel had misremembered." Another incident is connected with the decision problem. Gödel showed in his paper of 1933 that the quantifier prefix class $\forall\forall\exists \dots \exists$ is decidable for satisfiability, and stated without proof that this extends to predicate logic with equality. Burton Dreben was unable to work out the latter claim, as shown by his letter of 24 May 1966. Gödel, in his answer, gave the main idea of the proof and recollected that "no difficulty arose in carrying this through." A more detailed sketch by Gödel is given through a letter written four years later, on Gödel's behalf, by Dana Scott (vol. V, p. 568). In the end, it was found out that "Gödel was completely mistaken," as Warren Goldfarb puts it in his editorial introduction (vol. IV, p. 390): The claimed result about decidability is wrong, as was shown by Goldfarb himself in 1984. It may be that Gödel had made some trivial mistake by which he believed the matter to be easy, and not in need of a detailed treatment, as perhaps indicated by the quotation above.

Gödel's first result was the proof of completeness of the predicate calculus that he found in 1929. Already the first volume of the ***Collected works*** revealed a surprise: Gödel had submitted his proof as a doctoral thesis to the University of Vienna in 1929, and the publication of the original thesis text in 1986 brought to light Gödel's insight that a constructive proof of completeness would amount to a decision method for predicate logic, an insight of which there is no trace in the published paper of 1930. Apparently, one is to conclude that the decidability of predicate logic was unlikely in Gödel's mind, contrary to, say, Jacques Herbrand who ended his thesis of 1930 with the statement that a positive solution of the *Entscheidungsproblem* of predicate logic would "yield a general method in mathematics and would enable mathematical logic to play with respect to classical mathematics the role that analytic geometry plays with respect to ordinary geometry." The question of intuitionistic completeness proofs lay dormant and surfaced again some thirty years later, through remarks Gödel had made to Kreisel, which led to new research work well into the 1970s.

Gödel reported on completeness in the famous Königsberg conference in September 1930. But he also revealed there, in some seemingly off-handed remarks, that formal systems cannot be made complete once they contain arithmetic. From that point on, the correspondence is centered on incompleteness. It is only very many years later, namely in connection with the van Heijenoort volume, that we find interesting material on completeness. An especially intriguing issue is what, exactly, Skolem had proved in his paper published in 1923. Van Heijenoort (p. 510) had reported some relevant passages from his editorial correspondence with Gödel, some more in the editorial introduction to Gödel's thesis and completeness paper in volume I of the ***Collected works***, and now we have the full letters. The first mentioning of Skolem's 1923 paper is in a letter of Gödel's of 4 October 1963: "I think I first read it about the time when I published my completeness paper. That I did not quote it must be due to the fact that either the quotations were taken over from my dissertation or that I did not see the paper before the publication of my work." Gödel's interpretation of Skolem, from a letter of 14 August 1964, is that Skolem "implicitly proved: "Either A is provable or $\overline{A}$ is satisfiable" ("provable" taken in an informal sense). However, since he did not clearly formulate this result (nor, apparently, had made it clear to himself), it seems to have remained completely unknown." On August 27, Gödel writes, more sharply, that "Skolem advances some syntactic considerations, but they do not establish (nor, I think, are intended to establish): "If A is not provable, A is satisfiable"." (The unnegated second A is a slip of Gödel's or a misprint.) On September 18, Gödel wrote that Skolem "had no clear ideas in these matters. However, anyone who has can, without any further *mathematical* inferences, immediately draw the conclusion I stated in my previous letter." Less and less is credited to Skolem in this *crescendo* of sharpening statements; still, it became important for Gödel to stress that he had not seen Skolem's paper. In the last item of the van Heijenoort letters, of December 1965, Gödel indicates that it should be "stated somewhere that I had not read the passage in question in Skolem's writings prior to completing my dissertation," but van Heijenoort seems to have ignored this request. Perhaps he had reread Gödel's first letter on the issue that had given as a "fact" a disjunctive reconstruction of the circumstances around Skolem's paper, had left open the possibility that he had seen the paper prior to submitting his own paper for publication, and had stated only that Gödel was "practically sure" he had not seen the paper when he wrote his dissertation.

The question of Skolem's role in the completeness proof is further clarified in the correspondence with Hao Wang who in the late 1960s was preparing an introduction to Skolem's ***Selected works in logic***. Wang sent the introduction to Gödel who responded in December 1967, writing that Wang says, in effect, "that the completeness theorem is attributed to me only because of my attractive treatment," with which he obviously disagreed. The editorial introduction to Wang suggests here (vol. V, p. 381) that Skolem's (supposed) failure to comprehend that he had very nearly proved the completeness of predicate logic was caused by his approaching logic from the tradition of Schröder that "did not have a clear concept of formal proof." (This view is expressed by Wang himself in his introduction to Skolem's papers, p. 22.) Gödel writes to Wang that whereas he had not read Skolem's paper of 1923 for his dissertation work, he had instead read Skolem's paper of 1920. If one reads the *whole* of that earlier paper, not just the first section that deals with the Löwenheim-Skolem theorem and is translated in van Heijenoort's volume, one finds that Skolem was absolutely clear about the notion of formal derivation. Even the title of his paper, "Logico-combinatorial investigations into the satisfiability or provability of mathematical sentences," hints at the right direction. After the satisfiability-part in the first section, there come two sections that deal with formal derivability in lattice theory and plane projective geometry. He puts down axioms, conceived as rules of proof for the manipulation of formal expressions, and states that "*here we have a purely combinatorial conception of deduction on which I would like to put emphasis, because it proves to be especially useful in logical investigations.*" (Incidentally, Stanley Burris found out some seventy-odd years later that the second section contained a polynomial time decision procedure for formal derivability with the axioms of lattice theory, a result believed to have stemmed from 1988.) Thus, Skolem's problem, unlike perhaps that of others who followed Schröder's algebraic approach to logic, was not the lack of a "clear concept of formal proof." In Gödel's understanding, from the letter of December 1967 to Wang, Skolem's viewpoint in logic and mathematics was that "meaning is attributed solely to propositions which speak of *concrete and finite objects*, such as combinations of symbols," and that this viewpoint prevented him from taking the necessary nonfinitary step needed for a full statement of the completeness theorem. Gödel's view is in accordance with what Skolem's paper of 1920 suggests. It is further confirmed by Skolem's other paper of 1923, on primitive recursive arithmetic. Its writing was prompted, as Skolem repeatedly emphasized, by his reading of the ***Principia mathematica*** in 1919. Each instance of a free-variable derivation has a finitistic sense, whereas quantification over an infinite domain remains problematic. Thus, this work displays precisely the finitistic, combinatorial conception of logic and mathematics Gödel took to be characteristic of Skolem's work. One aspect that seems not to have been mentioned in discussions of Gödel's completeness proof is that the undecidability of predicate logic was established only in 1936. Before that date, no one was in a position to claim that a nonfinitary step in proofs of completeness is unavoidable. From this point of view, Gödel's charge, in the December 1967 letter to Wang, that Skolem had demonstrated surprising "blindness (or prejudice, or whatever you may call it)," carries with it a touch of hindsight.

As a second sample topic, I discuss incompleteness. Gödel has recollected the reception of the incompleteness result in discussions with Hao Wang. The present volumes contain correspondence with Johann von Neumann that brings new light to the circumstances of Gödel's greatest discovery. The former became enthusiastic right at the Königsberg meeting and started to work on incompleteness, arriving at the unprovability of the consistency of arithmetic. He explained this to Gödel in a letter of November 1931, but wanted to wait with publication until Gödel himself was finished. The latter wrote, in response, that he, too, had discovered what is now called Gödel's second incompleteness theorem. Von Neumann drew back his plans to publish but continued to work on the topic. A letter of January 1932 shows that he had arrived at the idea of provability logic, with the specific result that a propositional logic to which a provability operator has been added, is decidable. He also offered to "write it up" if Gödel is interested or "has not thought of it himself." Apparently, Gödel did not catch on this at the moment (or had he thought of it?), and consequently we do not have any von Neumann version of provability logic, unless, of course, some forgotten notes pop up from some source. We find Gödel writing to von Neumann on March 14, 1933, announcing "an interpretation of Heyting's calculus for sentential logic by means of the concept "provable"," with, quite surprisingly, no reference to von Neumann's suggestion.

Gödel published in the early 1930s short reviews, altogether over thirty in number. One, reprinted in vol. I of the collected works, is on a paper of Skolem's published in 1931. Skolem's view was that the Löwenheim-Skolem theorem leads to a "general relativism" in the foundations of mathematics, even in arithmetic. Therefore "the possibility of a complete characterization of the sequence of natural numbers is dubious" (see Skolem's ***Selected works***, p. 285). Strangely enough, Gödel does not comment on this view in his review, but there is instead a letter from Gödel to Skolem in which the former notes Skolem's remark and the relevance of the incompleteness theorem. According to the editorial introduction, it is not known if Skolem received the letter, and no further correspondence is reported. A few years later Gödel reviewed Skolem's paper on non-standard models of arithmetic. The latter never mentions Gödel in his two papers on the topic; Gödel, in turn, states (vol. I, p. 379) that Skolem's result "follows without difficulty from the investigations of the reviewer," thereby, in the words of Feferman (vol. I, p. 25), "misprizing" Skolem's achievement. Poor Skolem, who had written in 1929 that "it would be an interesting task to show that each collection of first order formulas on the natural numbers remains valid when certain changes in the meaning of a 'number' are made" (see ***Selected works***, p. 269).

Next in line on incompleteness is the role of Gentzen. Von Neumann, as shown by his letter of November 1931, had declared the consistency of classical mathematics to be unprovable in an absolute sense. This "von Neumann conjecture" fell with the Gödel-Gentzen translations of classical arithmetic to intuitionistic arithmetic. They clearly showed the consistency problem to be within the reach of intuitionistic arguments. Heyting, for example drew this conclusion immediately after having read Gentzen's manuscript, and wrote to Gödel on 15 April 1933 about it. Gödel's reply is somewhat puzzling. He wrote back on 16 May that he knew the translation and that "it should have been known in Göttingen since at least June 1932." As a reason for the latter, Gödel writes that he had "reported on it in the Menger colloquium, to wit, in the presence of O. Veblen who shortly afterwards went to Göttingen." We know from Menger's later recollections, based on minutes of the meetings, that Veblen got very enthusiastic about the lecture, and invited Gödel to visit the Institute for Advanced Study. However, to conclude that people in Göttingen can be presumed to have known this result of Gödel's sounds rather like an indication of an excited state of mind. There is no reason to think that Gentzen would have known of Gödel's discovery. It seems that at least four persons had the idea, the first and most original one being Kolmogorov in 1925. The others may have taken their inspiration from Glivenko's theorem and therefore, indirectly, from Kolmogorov, as Gödel did (see Troelstra's introduction to Gödel's paper in vol. I, p. 282). Gentzen, after having found his version of the "G-G" translation, possibly early in 1932, had set as his goal an intuitionistic proof of the consistency of analysis. He had cleared pure logic by his sequent calculus in 1933, and had a proof of the consistency of first-order arithmetic by December 1934. The latter is witnessed in a letter of 5 December 1934 to Hellmuth Kneser, reproduced in Menzler-Trott's Gentzen biography ***Gentzens Problem*** of 2001 (p. 48). Incidentally, as concerns sequent calculus, Gödel remarked in his Notre Dame lectures on logic that it offers an approach to logic more satisfactory than the axiomatic approach of Russell and Hilbert, as reported in Dawson's Gödel biography ***Logical dilemmas*** of 1997 (p. 136).

With the manuscript of Gentzen's long paper on the consistency of arithmetic in his briefcase, Bernays set on to sail along with Gödel to New York on 20 September 1935. Dawson's biography (p. 109) mentions that Gödel explained to Bernays, during the voyage and later in Princeton, the details of the second incompleteness theorem which found their way to the second volume of Hilbert and Bernays' ***Grundlagen der Mathematik***. However, Gentzen's consistency proof was certainly of much greater interest to both. In fact, from the correspondence between Bernays and Gentzen it can be seen that Gödel had made a suggestion of his own for clarifying Gentzen's proof, which latter had been considered problematic by Bernays. Gentzen writes on 11 December 1935 that "he had known the possibility of modification suggested by Gödel, but it cannot be used from a finitary point of view, due to its impredicativity" (in Menzler–Trott, p. 58). Gödel's correspondence contains also a letter by Bernays (7 January 1970) in which the latter recollects his talks with Gödel about Gentzen's original proof.

In his 1938 "Zilsel" lecture (see volume III of Gödel's works), Gödel was wrestling with Gentzen's proof. By 1941, he had found his alternative consistency proof, as is shown by his paper "In what sense is intuitionistic logic constructive?", published in volume III. The proof became known only in 1958 through an article in the journal ***Dialectica***. As a reason for the delay, Gödel later gave his "shifting interests."

Later correspondence with Bernays, in connection with the planned English version of the Dialectica-paper, witnesses of a tormented, ageing, and sick Gödel who refines and refines details that often seem very minor. The paper arrived at the stage of proofs, but Gödel did not let them go off his hands into print.

With Gentzen's paper on the provable initial segments of transfinite induction, a complete clarification of the second incompleteness theorem was achieved. The paper was finished in 1940 and, incredibly enough, published in Germany in 1943 and reviewed in the US in 1944 (Bernays in ***JSL***, vol. 9, pp. 70–72). The closest Gödel came to an appreciation of Gentzen's determination of the proof-theoretical strength of Peano arithmetic, doubtless the most important step ahead after his own paper of 1931, seems to be a passage in a letter of March 1966 to Constance Reid who at the time was preparing her Hilbert-biography. He first states that Hilbert had tried to prove the consistency of classical mathematics "on the basis of concrete evidence" and continues that "viewing the situation from a purely *mathematical* point of view, consistency proofs on the basis of suitably chosen stronger metamathematical presuppositions (as they have been given by Gentzen and others) are just as interesting, and they lead to highly important insights into the proof theoretic structure of mathematics."

The volumes contain also correspondence that sheds new light on Gödel's work in philosophy. The most extensive such correspondence is with Gotthard Günther, a less-known figure who had started his academic work with a book on Hegel's logic. Gödel kept reading Günther's work and answering to the latter's lengthy letters until the late 1950s. Other extensive philosophical exchanges are contained among the correspondence with Bernays.

Here figures prominently the philosopher Leonard Nelson who had had a strong influence on Bernays. Of Gödel's work in physics, there is no professional correspondence, but instead a letter of Gödel's to the well-known Einstein-biographer Carl Seelig. Gödel wrote about his involvement with general relativity in a relaxed, pleasant German, the high point being his admission: "The fact that here I, as a newcomer to the field of relativity theory, could immediately obtain essentially new results seems to me sufficient to demonstrate the unfinished state of the theory."

Reading these two volumes, one comes to think of the nature of Gödel's contacts. It was not usually he who opened a correspondence on a theme. Gaps in correspondences witness of long delays in answering, and drafts of answers of the great pains that correspondences could cause on him. One also understands that many contacts were by telephone and, sometimes, by personal visits to Princeton.

Volume V ends with a close to one hundred pages long "finding aid" to the Kurt Gödel Papers held at the Princeton University Library, available on microfilm for those who wish to read more. Among the many items of interest are Gödel's notes for his lecture series. Menger had urged already in the early 1940s for a publication of Gödel's Notre Dame lectures on logic, and there should be at least another such near-finished set of lectures, that on intuitionistic logic given in Princeton in 1941. The introduction by Anne Troelstra to Gödel's 1941 paper on intuitionistic logic in volume III, pp. 186–189, certainly arouses one's curiosity as to their contents, but for some reason or other, the lecture notes are not included in these ***Collected works***.

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PETER SMITH. ***An introduction to formal logic.*** Cambridge University Press, Cambridge, 2003, viii + 357 pp.

This book will need to jostle for room on shelves already crowded with introductory logic texts. I suspect it will prove most popular with teachers in philosophy departments with degree requirements ensuring a captive audience of students in a first formal logic course, a good percentage of whom are math-phobic.

The book covers the standard material for such first courses in many philosophy departments—classical propositional logic, classical first order logic without identity, then with identity. It also includes brief discussions of some further topics. The closing chapters are devoted to definite descriptions and existence, and the introduction of functions into first order logic. Briefer discussions of philosophical issues related to the formal material then under consideration—extensionality, necessity and contingency, the meaning of conditionals, etc.—occur at various points, as do brief indications of what students might expect to encounter if they take further logic courses, such as free logic or modal logic. Trees are used throughout as the proof method. This is probably a wise choice, as trees are also used in many of the best recent texts suitable for second courses in formal logic for philosophy students. However, instructors who feel that experience with natural deduction proofs improves students' work in other areas of philosophy will prefer texts such as Bergmann et al. ***The logic book***, which cover both natural deduction and trees at a similarly introductory level.

The tone throughout is informal, almost chatty, which I think a benefit in a book pitched at this level. On the few occasions where the author tackles a relatively elaborate proof—e.g., of the expressive completeness of his preferred set of connectives, or of completeness for propositional logic and of predicate logic without identity—the author talks the reader through the proofs. The result is pleasingly easy to digest. This is indicative of the overall feel of the book. The author is clearly an experienced teacher of logic at this level, and in the text he seems intent on forestalling any of the possible elementary misunderstandings his students have stumbled into, as well as answering the question behind the blank undergraduate stares one regularly encounters in required courses: Why should I care?

The book will strike some instructors as pretty slow going. Other than those already mentioned, the author essentially proves no metatheoretical results. In comparison with, for instance, Richard Jeffrey's ***Formal logic: its scope and limits***, it seems fair enough to say that in twice the space Smith covers less than half as much. While many find Jeffrey's book far too compressed for successful teaching in a first course, to spend 200 pages, as Smith does, on classical propositional logic is moving rather far in the opposite direction. Much of that space is spent introducing trees twice, first in a signed then in an unsigned version, and with working through many examples of both truth tables and trees in detail. The examples are well chosen, but students might well learn as much by doing some exercises instead of reading them—not many students manage to be stumped by truth tables or trees for classical propositional logic, in my experience. Moreover, instructors will need to flag the spots where students need to resume paying close attention as they read. For instance, a brief but clear discussion of some crucial differences between propositional and quantificational trees is tucked away on p. 240, on the next to last page of Chapter 25, just before the exercises.

The philosophical interludes are generally well done, providing enough detail to make clear what is at issue and the connection between the philosophical discussion and the formal material at hand. Of course, such brief discussions inevitably will leave some disagreeing with what is included and how the discussion is slanted—the useful discussion of the differences between the vernacular and material conditionals concludes with a sympathetic description of Frank Jackson's "robust conditionals" proposal, which will gratify those who agree with Jackson but aggravate his critics; bright students will be puzzled when apparent exceptions to the "exceptionless principle" of the idiscernibility of identicals are dogmatically dismissed out of hand on p. 306, especially when such respectful attention is paid to "quirks of the vernacular" earlier in the book; there is essentially no discussion of the role of the principle of bivalence; and so on. But it is no bad thing that the book opens the way for discussion of these matters.

More irksome is the author's habit of inserting remarks casually disparaging standard practice in logic texts—not including a "therefore" sign in the object language for propositional logic; using the expression "valid wff," which the author deems a "pretty perverse" bit of nomenclature because logicians loudly insist elsewhere that validity is a property of inferences, not sentences; the use of "interpretation" to refer to truth value assignments and interpretations of first order languages, when, really, an *interpretation* should give a *meaning* to expressions, not merely an extension; among others. In most cases there are good reasons for the common practice, so I confess some satisfaction that the author has, in effect, to acknowledge as much when he needs to explain why we consider all possible "valuations" when answering questions about validity, even though some richer, official notion of "interpretation" idles in the background (pp. 91–92). More problematically, Smith complains: "Oddly, quite a few logic books officially define arguments as one step affairs." The problem is supposed to be that with such a definition a "reasoner" might get from initial premises to a conclusion which "follows" by "two quite terrible moves: it would therefore be extremely odd to dignify his supposed 'proof' as deductively cogent" (p. 48). Here it seems to me Smith is inviting the sort of conflation of validity and provability that it should be part of a first course in logic to warn students away from—of course there can be many incorrect proofs even in the case of a valid argument. By regarding arguments as multi-step affairs, as is common in "the vernacular" and in critical thinking texts, the author risks losing an important lesson.

But there will be quibbles with any textbook. There are a reasonable number of well chosen exercises at the end of nearly all of the 36 chapters, with solutions gradually being added to a website for the book. The site also includes a corrections page for the small number of errors, mostly typographical, that found their way into the book. The book is a worthy choice for instructors who find its leisurely pace an asset.

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Jonathan Bennett. ***A philosophical guide to conditionals.*** Clarendon Press, Oxford, 2003, viii + 388 pp.

An encyclopedic guide to the philosophical side of the literature on conditionals is now offered us by a long-time contributor, Jonathan Bennett. This survey will be indispensable for years to come, both as a map of a sprawling field, and as a treasury of valuable examples. There is, however, no pretense of covering the more technical literature, so what the logician should look for here is not information about formal systems of conditional logic, but motivating considerations.

Indicative and subjunctives. Chapter 1 divides conditionals into two classes. Like others, Bennett prefers a division based on tangible differences in sentences rather than intuited differences in propositions expressed. He draws the line between conditionals expressible in English by sentences that have "would" in the consequent, which following tradition he labels "subjunctive", and others, labeled "indicative". He notes that the labels are appropriate for German, inappropriate for French, debatable for English. With the indicative it is typically being taken to be an open question whether the antecedent holds, while with the subjunctive the antecedent is typically being taken to be contrary to fact. Hence the alternate, more theoretically loaded, terminologies "open-question" *vs* "counterfactual" or "epistemic" *vs* "ontic", which Bennett deprecates. He writes $A \rightarrow C$ for the indicative, $A > C$ for the subjunctive.

Implicature. In Chapters 2 and 3, Bennett introduces some notions from philosophy of language. The ordinary-language school conflated meaning with use, semantics with pragmatics, truth with assertability, until a dissident, H. P. Grice, developed his famous theory of *implicature*. Here P *implicates* Q if the fact that P is being asserted suggests that Q. Truth and assertability come apart when a true P implicates a false Q. Grice emphasized one case. In ordinary conversation, cooperative speakers observe certain maxims which, among other things, favor more over less informative assertions, and more over less well-grounded ones. If the fact that P is being asserted, together with the supposition that the maxims are being observed, implies that Q, then P *conversationally* implicates Q. Thus a disjunction conversationally implicates that the speaker does not know which disjunct holds, since a speaker observing the maxims who knew which would assert that disjunct rather than the less informative disjunction. In contexts where the assumption of cooperativeness lapses, as with a disjunctive hint in a guessing game, the implicature lapses also.

Another class of implicatures is recognized by many theorists, though it seems rare, since they all cite the same few examples. Saying (1) "She is poor but honest" rather than (2) "She is poor and honest" suggests that there is a contrast between poverty and honesty. Nonetheless, a contrast is not *entailed* by (1). If it were, (1) would be equivalent to (3) "She is poor, and she is honest, and that is a contrast", and the question (4) "Is she poor but honest?" would be equivalent to the question (5) "Is it the case that she is poor, she is honest, and that is a contrast?". But where (5) merely asks, (4) already *suggests* that there is a contrast, so the two questions are not equivalent. The suggestion of contrast is equally present in affirmative assertions like (1) and negative ones like "She is not the poor but honest creature she claims to be", in questions like (3) and in commands like "Find someone poor but honest to hire for the job". If the contrast were a presupposition, as "France has a king" is of "Is the King of France bald?" and either answer, (1) would have no truth value if no contrast exists. Many theorists, however, report an intuition that (1) has the same truth-value as (2), even if the suggestion conveyed by "but" is wrong. Such theorists call this suggestion a *conventional* implicature, not an entailment, presupposition, or conversational implicature. Frank Jackson explains that signaling something not part of our message alongside that message can help us get our message across by preventing our hearers from being distracted by the thought that we are overlooking something. Saying "He's a politician but honest" is like saying "He's is a politician and honest — and no, that's not a slip of the tongue: I really do mean 'honest' and not 'dishonest'. I'm aware that politicians as a rule are dishonest; I'm saying he's an exception".

Assertibility. Many writers use "assert*i*bility" for assert*a*bility modulo implicatures. In the same chapters where Bennett introduces Grice's and Jackson's views on conversational and conventional implicature, he criticizes their views on when $A \to C$ is true. Disagreement on this topic now takes place against a background of agreement on when $A \to C$ is assertible, so let me review assertibility first. It is generally agreed that P is assertible by a given speaker if that speaker believes P strongly enough. How much is enough may be vague, with one standard for casual conversation, another for court testimony, and so on. On the highly idealized assumption that strengths of beliefs can be assigned precise numerical measures, and that these so cohere as to obey the Kolmogorov axioms, the received view may be restated in terms of the speaker's subjective probability function π, thus: (*) P is assertible iff $\pi(\mathrm{P})$ is high enough. Bennett devotes Chapter 4 to the views of Ernest Adams on the assertibility of indicative conditionals. This involves the notion of *conditional* probability $\pi(\mathrm{C/A})$. If $\pi(\mathrm{A}) \neq 0$, then $\pi(\mathrm{C/A}) = \pi(\mathrm{A}\ \ \&\ \ \mathrm{C}) \div \pi(\mathrm{A})$. (In the degenerate case where $\pi(\mathrm{A}) = 0$, many set $\pi(\mathrm{C/A}) = 1$ by convention, though some make a great to-do about rejecting this convention, unimportant though degenerate cases are, except as potential exceptions to otherwise general rules.) The Adams criterion reads: (**) $A \to C$ is assertible iff $\pi(\mathrm{C/A})$ is high enough. This would be wrong for assert*a*bility. For one thing, $\pi(\mathrm{A/A}) = 1$, yet $A \to A$ is only assertable in very special cases (see Esther 4:16 or II Kings 7:4). For another, conditionals where $\pi(\mathrm{A}) = 0$ or 1 and $\pi(\mathrm{C}) = 0$ or 1 are as a general rule unassertable, even when $\pi(\mathrm{C/A}) = 1$. Gricean theory, however, can explain why.

The simplest explanation for *why* (**) holds would be that it is a special case of (*), that $\to$ is an operation that applied to A and C produces a proposition satisfying $\pi(A \to C) = \pi(\mathrm{C/A})$. Bennett devotes chapter 5 to the surprising result of David Lewis that no operation can satisfy this equation (unless all relevant probabilities are either 0 or 1, in which case the material conditional $\sim A \vee C$ does so). Now in some areas we speak and unreflectively think as if something were true (or absolute, or objective) when it is not, whether or not many or most of us recognize upon reflection that it is not. Thus talk of the sun rising has survived through all the phases of the conversion of first scientific, then lay opinion from geocentrism to heliocentrism. There are some indications that much of our speaking and unreflective thinking about probability treats subjective credences as if they were objective chances. The one theory of the conditional Bennett does not seriously consider is the "error" or "fictionalist" theory that we speak and unreflectively think of $A \to C$ as if it were a proposition of the kind the Lewis result shows not to exist. The three main views he does seriously consider I will call *materialism*, *idealism*, *nihilism*. (My labels, not his. He calls nihilism NTV for "No Truth Value".)

Materialism holds $A \to C$ to have an objective truth-value, the same truth-value as $\sim A \vee C$. The main problem is to explain why assertibility conforms to (**) rather than (*) and goes by $\pi(\mathrm{C/A})$ rather than $\pi(\sim A \vee C)$. Idealism holds $A \to C$ to have only a subjective truth-value, being true *for a speaker* iff $A \to C$ is assertible *for that speaker*. Since $A \to C$ is equated with "$\pi(\mathrm{C/A})$ is high", assuming the speaker knows what his or her own subjective probabilities are, we have $\pi(A \to C) = 1$ iff $\pi(\mathrm{C/A})$ is high, and $\pi(A \to C) = 0$ iff $\pi(\mathrm{C/A})$ is not high. So $\pi(A \to C)$ is high iff $\pi(\mathrm{C/A})$ is high, making (**) a special case of (*), even though we do *not* have $\pi(A \to C) = \pi(C/A)$, which would violate the Lewis result. Nihilism holds $A \to C$ to have no truth-value. The Lewis result may then be interpreted as telling us that assertively uttering a conditional sentence is not performing an ordinary act, assertion, applied to a special kind of proposition, a conditional proposition, but rather is performing a special kind of act, assertion of one proposition conditional on another, applied to two ordinary propositions. The rule (**) is then not an *instance* of the rule (*) for ordinary assertion, but the *analogue* of it for conditional assertion.

Materialism. Going back to chapters 2 and 3 now, the parts I omitted to discuss above contain Bennett's criticisms of Grice's and Jackson's attempt to defend materialism by appeal to implicature theory. Ordinary-language philosophers maintained that $A \to C$ cannot mean $\sim A \vee C$ because the former is often unassertable even though the latter is true, giving examples where the only grounds for the disjunction are grounds for one of the disjuncts. But as we have seen, Gricean theory can explain why generally a disjunction $\sim A \vee C$ will not be assertable if either disjunct $\sim A$ or C is. Bennett presses different examples, where Gricean theory seems to imply that $A \to C$ should *not* be assertable, though intuitively it is. His two main types of examples are illustrated by (1) "It won't rain, and if it does, we'll hold the party indoors" and (2) "Even if it rains, the party will be a success", the latter type being called *non-interference* conditionals.

Now Gricean maxims cannot be inviolable commandments, since different ones sometimes point in different directions. If we have just good enough grounds to assert a disjunct, but further and overall better grounds to assert a disjunction, the maxim favoring more informative assertions will point towards asserting a disjunct, and that favoring better-grounded assertions towards asserting the disjunction. Nothing in Gricean theory forbids us to assert both, at least not if we signal that one is better-grounded than the other: There is no false conversational implicature in "$\sim A \vee C$, most likely the former". Thus examples of type (1), which surely would sound better with "most likely" before the first conjunct, may not be decisive. As for examples of type (2), the "even" before the antecedent seems obligatory, while inserting "then" before the consequent is not an option, as it is with other indicative conditionals. So a Gricean theorist might claim that these are not standard indicative conditionals, but some related special construction (though we would then be owed an account of that construction). Thus even examples of type (2) may not be decisive.

Be that as it may, it is now widely agreed that materialism cannot be reconciled with (**) by appeal to Gricean theory alone. Jackson proposes a different defense, hypothesizing that $A \to C$ differs from $\sim A \vee C$ by carrying a conventional implicature such that asserting $A \to C$ suggests $\pi(C/A)$ is high for the speaker. This hypothesis trivially yields (**), but Bennett convincingly criticizes it has having almost nothing to do with the general theory of conventional implicature. Most glaringly, the Jacksonian hypothesis applies only to "if" in assertions, not denials, questions, commands, and so on.

Idealism. Now (**) pertains to *subjective* probabilities. In Chapter 6 Bennett illustrates (even somewhat belabors) the point that assertibity of conditionals varies from speaker to speaker, and begins the argument, concluded in Chapter 7, against idealism, and for nihilism. Bennett argues that if one asserts $A \to C$ and someone asks "Are you sure?", one doesn't take this to be a question about one's subjective psychological state. This is not entirely compelling, since equally if one asserts "I believe that P" and someone asks "Are you sure?", one doesn't take the question to be about one's psychological state (except, perhaps, is the questioner is a psychoanalyst), though one's original assertion surely was. Worse are two abstract arguments against idealism in §41, each resting on a principle clearly dubious for epistemic propositions. (Thus one assumes that if P can be true while Q is not, then P can be perfectly certain while Q is not — obviously questionable for Q = "It is perfectly certain that P".) A better argument appeals to the intuition that $A \to C$ is false if A is true and C false. This intuition goes against idealism, since $\pi(C/A)$ may be high regardless of the truth-values of A and C, but obviously also goes against full nihilism. Bennett does, in Chapter 8, acknowledge the intuition, and adopts a three-quarters nihilism, allowing $A \to C$ to have a truth-value in just this case. He does not endorse, but shows sympathy towards, Dorothy Edgington's semi-nihilism, which takes $A \to C$ to have the same truth-value as C when A is true, and none when A is false. Her view involves a comparison of conditional assertions with conditional commands (which cannot be said to have been obeyed or disobeyed if the condition fails) and conditional promises (which cannot be said to have been kept or broken if the condition fails), and unlike the three-quarters view offers a *principled* reason why truth-values are present in some cases and not others.

One area where we might look for evidence for or against versions of nihilism is the behavior of conditionals embedded in more complex constructions. Materialism and idealism seem to imply that conditionals should be freely embeddable, and nihilism that they should not be embeddable at all, but in fact they admit some kinds of embeddings and not others. There is considerable debate surrounding almost every kind apart from $A \to (B \to C)$, which is widely held to amount to $(A \ \& \ B) \to C$. No 'ism emerges covered with glory from these debates, or from the examination of various special uses of conditional sentences, which Bennett discusses in an even-handed manner.

Nihilism. Let me not be so even-handed myself, and mention a couple of arguments *against* nihilism that Bennett neglects. First, consider conditional wagers where the condition of the bet is itself conditional. Suppose I think no one has a chance of beating the incumbent Kay in the up-coming election except Ella, and that even she won't have a chance if her rival Emma also jumps into the race, splitting the opposition vote. I may then say, "I bet Kay wins, provided that if Ella runs, so does Emma." Now in general if I bet that C provided that A, the bet is off if A turns out false and on if A is turns out true. And in this particular example, the natural interpretation of my bet is that it is off just in the one case where the relevant material conditional is false, where Ella runs and Emma doesn't. Second, consider guessing games (which Bennett himself mentions briefly, though he does not attach as much importance to them as others might), where many conversational implicatures are canceled. If I know the facts and give you the hint, "If it was done by Colonel Mustard, then it was done with the lead pipe," this is surely an honest hint whether it was done by Colonel Mustard with the lead pipe, or by Mrs. Peacock with the lead pipe, or by Mrs. Peacock with the rope. Such examples point to the principle that when I know all the relevant facts — in the extreme idealization, when I am omniscient — then a conditional $A \to C$ is assertible by me if and only if $\sim A \vee C$ is true. As a general rule, what is true coincides with what would be assertible by me if I were omniscient. If the general rule applies to the case of $A \to C$, then its truth-conditions are those of $\sim A \vee C$, as *per* materialism. But the general rule admits exceptions for epistemic examples, beginning with "I am not omniscient". So the principle suggested by the examples may not be incompatible with idealism. But the examples seem hard to reconcile with nihilism.

There is another fact, of special importance to mathematical logicians, also hard to reconcile in a principled way with nihilism. The technical result that the formula CH is not derivable in the formal system ZFC if the latter is consistent is applied to conclude that mathematicians would be wasting their time if they tried to solve Hilbert's first problem by ordinary mathematical means. This application, about which there is no serious doubt among experts, presupposes that indicative conditionals in mathematics are adequately represented by material conditionals. Materialism by no means follows, since mathematics has many special features. But it would seem to be a constraint on any acceptable theory of indicative conditionals that its complex account of them must simplify to a materialist account in contexts with the special features of mathematics.

Bennett recognizes no such constraint. He distinguishes "independent" conditionals, which follow from general laws alone, mathematical or causal or whatever, and "dependent" conditionals, even those that follow from general laws plus particular facts. He does not claim that independent conditionals are idiomatic. There are some unquestionably idiomatic uses of conditionals — see "unction" in the *Devil's Dictionary* — but Bennett recognizes that "if" means the same in conditionals asserted on grounds of general law as in conditionals asserted on other grounds. He recognizes that a bright person may find a route from antecedent to consequent using general laws alone while a dim one finds only a route using particular facts, and that when this happens and the two assertively utter the same conditional sentence, they are *agreeing with* and not *talking past* each other. But though he acknowledges that "if" means the same in both kinds of conditionals, and though he acknowledges that much of his theory fails for "independent" conditionals, still he makes little serious, sustained effort to seek a unified theory. The difference between what I think is called for and what he is content to rest with is like the difference between Einstein seeking to account for the aspects of the motion of Mercury that Newtonian theory gets wrong by devising a new theory that would get Mercury right but yield Newtonian theory as a limiting case, and an anti-Copernican simply adding another epicycle as a patch every time a hole is discovered in his planetary theory. Bennett is unembarrassed about all this. Of his restricted version of nihilism or NTV, which makes an exception for independent conditionals, he writes (p. 118): "Critics may find it suspect because it has a funny shape, resulting from ad hoc trimming back of unrestricted NTV. I reply that the trimmings are indeed ad hoc: each responds to a threat of falsity in the way sensible people *do* meet such threats, by not saying the thing that was going to be false." In view of his statement, it is not surprising to find Bennett later in the book coming out with friendly words about Ptolemaic astronomy.

Conditional logics. In Chapter 9 Bennett gives an informal account of the elegant theory of Adams about which inferences preserve assertibility of conditionals. He discusses the failure of *antecedent-strengthening* (inference from $\mathrm{A} \to \mathrm{C}$ to $(\mathrm{A}\ \&\ B) \to \mathrm{C})$ and *contraposition* (from $\mathrm{A} \to \mathrm{C}$ to $\sim\mathrm{C} \to \sim\mathrm{A}$), among others, illustrating with Euler diagrams (which he follows a number of earlier writers in mistakenly calling "Venn diagrams").

In Chapter 10 he turns to subjunctive conditionals. The received view is that these do have truth-values and that their assertibility is governed by (*). Moreover, following Robert Stalnaker and David Lewis, it is generally agreed that, roughly speaking, $\mathrm{A} > \mathrm{C}$ is true iff $\mathrm{A}\ \&\ \sim\mathrm{C}$ is a remote possibility compared to $\mathrm{A}\ \&\ \mathrm{C}$, or less roughly speaking, iff some possible state of the world in which $\mathrm{A}\ \&\ \mathrm{C}$ holds is a less remote possibility than any in which $\mathrm{A}\ \&\ \sim\mathrm{C}$ holds. Here the relevant possible states are *metaphysical*, ways the world (is or isn't but) could potentially have been, and "remote compared to" is "less similar to the way the world actually is than". However, "similarity" cannot be understood as simple overall similarity here, but rather is a weighted sum of similarities in different respects (sameness of causal laws, sameness of particular facts in the past, and so on). In the resulting logic, antecedent-strengthening, contraposition, and related inferences fail.

Bennett attempts an informal explanation of the similarity of indicative and subjunctive conditional logics. A more systematic and satisfying explanation would require drawing on more technical results of Adams. Suffice it to say here that (**) can be restated as saying that, roughly speaking, $\mathrm{A} \to \mathrm{C}$ is assertible iff $\mathrm{A}\ \&\ \sim\mathrm{C}$ is a remote possibility compared to $\mathrm{A}\ \&\ \mathrm{C}$. But here the relevant possibilities are *epistemic*, ways that, for all the speaker knows, the world may actually be, and "remote compared to" is explained in terms of probabilities. (Hint: Take $-\log(\pi(\mathrm{P}))$ as a measure of how far from certainty P is.)

There are disagreements among theorists over the formal properties attributed to the comparative similarity relation S, partly explained in Chapter 11. Suppose I could have flipped a coin but didn't. If you hold that one or the other of "If I'd flipped it, it would have come up heads" and " … it would have come up tails" must be true, and more generally uphold the law of *conditional excluded middle*, $(A > C) \vee (A > \sim C)$, then you must take S to be antisymmetric. Suppose we each flipped a coin and mine came up tails and yours heads. If you hold "If mine had come up heads, they would have matched" to be true (thus assuming that even if mine had come up different, yours would have come up the same), you will be led to deny that S is connected, which also makes a difference to the logic, though the simplest example is much more complicated that conditional excluded middle. (Adams shows that indicative conditional logic corresponds to *Lewis's* version of subjunctive logic, with connectivity and without antisymmetry.)

There are also many disagreements over the substance rather than the form of S, over the comparative weight to be given to sameness of causal laws, sameness of the past, and so on. These issues about "law" and "cause" are of great interest to philosophers, even though most make no difference to the formal logic, and Bennett devotes about half his book — the bulk of chapters 12–21 — to them. Since I am writing for a readership of logicians, and have limited space, I will skip over all this philosophically valuable material.

Indicative and subjunctives again. Though mainly intended as a guide to existing literature, Bennett's book does include some novelties, most importantly in Chapter 22, the bulk of which is new. There Bennett turns to a proposal to reclassify future indicatives like (1) "If Hillary runs, she will win," with subjunctives. The proposal is based on the claim that any grounds that would at present warrant something like (1) would in future warrant asserting a corresponding subjunctive like (2) "If Hillary had run, she would have won," should the antecedent turn out false. Bennett distinguishes carefully different types of grounds there might be for asserting an indicative and argues convincingly that the claim of the reclassifiers is based on overlooking all but one of the types. Similar considerations would argue against reclassifying future subjunctives like (3) "If Hillary were to run, she would win," with indicatives.

Similar considerations are invoked in the concluding Chapter 23 against several attempts to unify the indicative and subjunctive theories. If there is an overarching thesis to the book, it is that proposals for unification are premature until more work has been done on indicatives and subjunctives separately. Given the complexity of the maze of issues through which Bennett skillfully guides us, it is hard to disagree.

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WARREN GOLDFARB. ***Deductive logic.*** Hackett Publishing Company, Indianapolis, 2003, xv + 292 pp.

It is rare to be able to say of a text published for the first time that it and its exercises have been tested in the classroom for more than twenty years, but this and more is what we finally have in Goldfarb's *Deductive Logic*. No book can be all things to all audiences, but this book is remarkable for the wide range of classroom goals to which it is well-suited: fully rigorous preparation for advanced logic, fully precise discussion of topics of philosophical interest, and-wonder of wonders-improvement of a student's reasoning in ordinary language. As such, the text is perfectly designed for the education of a philosopher, undergraduate or graduate, but its rigor and succinctness make it excellent preparatory reading for the mathematics student who is turning to logic and foundations, and it has the resources to be popular with the generic undergraduate fulfilling a quantitative reasoning requirement. The author, the reviewer, and several others have used a manuscript version of this text for the first two purposes for many years, and in the last five years it has been used with success for courses fulfilling general quantitative reasoning requirements. Since its publication last year the text has been adopted at a diverse group of colleges and universities.

The bulk of the text, which covers first-order logic, through completeness, is a three-by-three matrix divided into sections on truth-functional logic, monadic quantification theory and polyadic quantification theory, each of which progresses through sub-sections titled “Analysis”, “Logical Assessment”, and “Reflection”. The separation of monadic and polyadic quantification allows the student to become accustomed to the potentially alien notion of quantification in its easiest form before attempting to grapple with statements and formulas of arbitrary complexity. This separation also allows Goldfarb to present a (clever and elementary) decision procedure for validity of monadic formulas, which serves as a warm-up for the later completeness proof for first-order logic since the two proofs use the same technique. The “Analysis” subsections teach the student the logical language appropriate to each section, and discuss, with exceptional explicitness and thoroughness, how and why any English sentence is paraphrased into the logical language in the way or ways that it standardly is. The “Logical Assessment” subsections cover the notions of interpretation, validity, implication, equivalence, and satisfiability, and how to test for all of the latter; for truth-functional logic by truth tables, for monadic quantification theory by the decision method just mentioned, and for polyadic quantification theory by a natural deduction system. The subsections titled “Reflection” are devoted to metalogic: they present and show how to prove general laws, such as the transitivity of implication and that unsatisfiable schemata are equivalent to each other and to no others, discuss important properties such as expressive adequacy of a set of connectives, introduce useful techniques such as disjunctive normal form and prenex form, prove important theorems such as the soundness and completeness of the deduction system, and discuss other important results such as the undecidability of validity. Students find this progression natural (if also challenging), perhaps because within a section of the text each subsection performs a step of what we might call “logical ascent”.

Deductive Logic is, in the reviewer’s opinion, the text of choice for any teacher who wants to cover the completeness and undecidability results for first order logic without presupposing any prior mathematics background. Completeness is proved using little more than elementary operations on formulas; the proof, which follows that of Quine in *Methods of Logic* and is based on Gödel’s original idea, and to which Goldfarb has added a distinctive accessibility of presentation, is simplicity itself in both detail and overall structure. Completeness follows from one lemma which requires for its proof the proof of one theorem. (The theorem is the compactness of truth-functional logic, proved with memorable clarity via properties of a binary tree and vividly intuitive terms for them). The lemma asserts the existence for every unsatisfiable formula of a finite list of quantifier-free formulas (all instances of the first formula) whose conjunction is truth-functionally unsatisfiable. From this it is easy to show how to construct a deduction of any valid formula: negate it, find that list of truth-functionally unsatisfiable formulas, and use them to construct a deduction of the original formula by reductio ad absurdum. That list of truth-functionally unsatisfiable formulas is found by putting a formula in prenex form and applying the universal and existential instantiation rules until the needed instances appear. Though the proof is not constructive, it does isolate the one non-constructive step, which occurs in the proof of the infinite case of the compactness theorem, and the constructive remainder gives the student a good feel for why the theorem is true. Bergmann, et al.’s proof of completeness in *The Logic Book* (1998), by contrast, is so long and full of intermediate lemmas about subtly different forms of consistency as to lose the intuitive thread of the result. Barwise and Etchemendy’s proof of completeness in *Language, Proof, and Logic* (2003) is elegant at some sophisticated level for using the Henkin method, but requires the wheeling in of so much machinery, including set theory, that it puts the result out of reach for the majority of students.

If one’s goal is not teaching beginners to prove completeness, but teaching beginners to be better reasoners, this text is also outstanding. The approach of its sections on paraphrase is not the common listing of rules of thumb for getting from one language to a new language introduced arbitrarily. Rather, from Goldfarb's uniquely extensive, explicit, and transparent discussion the student learns through one patiently described example after another that the justification of a paraphrase of an English sentence by a particular logical formula comes from what the intended meaning of a statement of English imposes on the structure of the truth conditions of that statement. That is, the student learns not merely a translation manual from her ordinary language to an arbitrary second language, but also that the second language represents the logical structure of her everyday language. (This, the reviewer opines, is the missing ingredient to explain statistics suggesting that students who take symbolic logic courses end up no better at reasoning in real life.) Students who go away from a course with this text not remembering a single theorem nevertheless retain an improved awareness of the logical structure, and hence the commitments entailed by, statements of English.

The same holds for validity of arguments of English, due to a habit throughout the book and its exercises of never leaving English behind. That is, in the final subsection of logical assessment, that for polyadic quantification theory where deductions can be somewhat elaborate, one can ease the burden on the student by isolating the exercise to one of showing merely that a certain deductive relation holds between two formulas. However, many exercises at this stage in the book still begin with arguments in English, requiring the student to go all the way from a realistic English argument through its paraphrase and formulas, and to a deduction to check for validity. Thus the student is never allowed to forget that the method is a method for evaluating real deductive arguments. The text has this in common with Bergmann et al. (1998) and Klenk, *Understanding Symbolic Logic* (1989), but this is lacking in the approach of Barwise and Etchemendy (2003). Twenty years of fine-tuning produced in *Deductive Logic* a deduction system that is natural and free of clumsiness, and in which complicated deductions can be done perspicuously. The deduction exercises are slightly more challenging than those of other books, which has the benefit of making exercises involving realistic arguments in English possible. But though the text explains examples of simple deductions to prepare the student, it would be helpful in future editions to have a few more illustrations and exercises of a level in between the two. Also, some teachers will lament the fact that the system does not contain inference rules concerning truth-functional logic, but simply takes all truth-functionally valid formulas as axioms. (*N.B.* The text had relatively few typos even in the first printing, all but a few of which were corrected in the second. An error that will be corrected in the third printing occurs on page 190, in the official statement of Rule EII of the deduction system. It says that an instance may be written provided that the instantial variable has not occurred free at any line up to and including the line of the existential schema, line (m). Instead, the rule should read "up to line (n)" where (n) is the line on which the existential schema is instantiated. Only one later reference to EII, p. 203, line 9, contains this mistake.)

For philosophy instructors *Deductive Logic* has an especially apt set of resources. In addition to the virtues so far listed one gets, at the appropriate moments, thorough and pointed discussions of use and mention, the material conditional and its relation to ordinary language, and, in a final section at the end of the book, identity, names, and definite descriptions, and deductions involving them. In addition the author introduces a formal system (axiom system) for truth-functional logic, in order to illustrate what such a thing is, what a formal proof looks like, and what the advantages and disadvantages of such a system are. I know of no other contemporary logic book that treats all of these topics with philosophical precision.

This text is remarkably short ($\sim$ 290 pages) for covering so much material with such rigor, especially considering the strikingly large typeface and wide margins that make it visually approachable for the neophyte. The shortness of the text also assists students in keeping the overall structure of the project in mind as they struggle with details. The book can be so short because it bucks the trend of exposition by successive approximation and repetition.

Mirroring the elegance of logic itself, the text says in most cases exactly once all and only the things a student needs to understand. As a consequence, it is important for any class, but particularly for less motivated or prepared students who are fulfilling a requirement, that an instructor emphasize the need for students to read, study, re-read and re-study every assigned portion of the text until they understand it thoroughly. This makes not for more work, but for a different kind of work, which students come to enjoy once they grasp what is required, provided the material is taken at a pace appropriate to the audience. The sleekness of the text is balanced by the liveliness of its examples which are taken from a variety of areas, from European history and opera to relations of respect, admiration, love and desert. Hackett is to be commended for keeping the price of this book so low, at $32.50, hardcover. Goldfarb is owed our gratitude for making his underground classic available to the general public.

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JODY AZZOUNI. ***Deflating existential consequence: a case for nominalism.*** Oxford University Press, Oxford, 2004, viii + 342 pp.

Several years ago Gideon Rosen and I were invited to write a retrospective on nominalism as it developed during the 1980's and 1990's. We ended by suggesting that nominalists are in the grip of a certain picture, and that until that grip is broken, the most that objections to particular versions of nominalism can accomplish will be to inspire the development of other versions. The book under review confirms our prediction.

Nominalism. The problems that older versions of nominalism face can be illustrated by considering the relationship between (1) "There exist numbers greater than 10^{10} that are prime", something a mathematician might say, and (2) "Numbers exist", something only a philosopher would assert or deny. Nominalism has heretofore been understood as involving the denial of (2), leaving nominalists with a choice among the following views: (a) mathematicians who utter (1) are making a false assertion; (b) mathematicians who utter (1) are performing some other speech act other than making an assertion; (c) (1) does not imply (2). As to (a), for a philosopher to take this line is to suggest that philosophers are in a position to correct mathematicians *about mathematical questions*. In view of the comparative historical records of success and failure of mathematics and philosophy, this suggestion is, as the late David Lewis said, "comically immodest", as even many nominalists recognize.

As to (b), it is indeed sometimes hard to tell whether an utterance is an assertion, advancing a statement as a literal truth, or is some other kind of speech act, advancing it as an apt metaphor, a useful fiction, or something else. As to (c), it is indeed sometimes hard to tell whether one statement implies another. Moreover, sometimes one can recognize that an utterance is not intended as an assertion without being able to articulate just what it is intended as; and sometimes one can recognize that a statement does not have the logical implications, hence cannot have the logical form, it seems to have, without being able to articulate what logical form it *does* have.

For example, suppose Dick Cheney says, "Iraqi weapons of mass destruction exist only where U. N. inspectors have not looked," and Maureen Dowd replies, "Iraqi weapons of mass destruction exist only in the mind of Dick Cheney." We appear to have here an assertion and a counter-assertion of the same logical form, but everyone will recognize that Cheney is committed to the truth of "Iraqi weapons of mass destruction exist", while Dowd is committed to the opposite. Is this because Dowd's utterance was not an assertion but some other speech act, or is it because that statement has a logical form different from the one it seems to have? It's hard to say, but we don't need to be able to say in order to recognize that she is not committed to what Cheney is. Discourse is not embarrassed by the difficulties of linguistic analysis, and such examples abound.

But while there is no *general* requirement to provide an analysis, the mathematical case is special. To return to (b), if this line is taken, it is not just purely mathematical utterances that will have to be declared non-assertions, but also mixed mathematical utterances, which are actually ubiquitous and practically indispensable in scientific theorizing, and widespread in everyday conversation also, and include many utterances that people have heretofore unhesitatingly *called* assertions. This is so much so that one must wonder what is left of the concept of "asssertion" if all these examples are disallowed. It is for *this* reason, and not on account of any *general* requirement, that if line (b) is taken, some positive account of what mathematicians are doing, if not making assertions, is urgently needed.

To return to (c), the conclusion that (1) implies (2) is immediate upon applying mathematical logicians' standard methods for representing the logical forms of mathematical statements, and these methods have, in the case of statements that (like (1) and unlike (2)) mathematicians actually consider, proved to be highly reliable, in the sense of agreeing with mathematicians' intuitive judgments. This is so much so that one must wonder what is left of the concept of "implication" if these methods are declared incorrect. *That* is why mathematics is special, and if line (c) is taken, some positive account of what the logical form of mathematical statements is, if not what mathematical logicians say it is, and some explanation of why standard methods have proved to so highly reliable, if they are incorrect, is urgently needed.

Commitment. The sonorous phrase "ontological commitment" had no use or meaning before Quine gave it one through a stipulative definition. His definition in principle equated it with "existential consequence", making it idle; in practice, it was given some work to do by some slight modifications of usage. Thus "ontological" was used more narrowly than "existential", to apply to those existential statements that (like (2) and unlike (1)) are so very general that no one but a philosopher would assert or deny them; and "commitment" was used more broadly than "consequence", in that only theories are said to have consequences, while both theories and the theorists who assert them may be said to be "committed". In practice, talk of "commitments" rather than "assertions" was useful mainly in connection with the assertions of non-philosophers, who do not address very general ontological questions, and of certain philosophers who make what appear to be existential assertions about, say, propositions, only to turn around and say, "but such things as propositions do not exist".

As the Cheney-Dowd example and others remind us, it is sometimes difficult to determine whether an utterance is an assertion, and sometimes difficult to determine what the logical form of a statement is. As a result, ontological commitments are sometimes difficult to determine. Quine notoriously declared himself unwilling to assess ontological commitments except for theorists writing and theories written in the kind of highly stylized or regimented way that is characteristic of mathematical prose, which was historically the body of prose mathematical logic was created to analyze and is perhaps the only large body of prose that admits routine formalization. Critics have complained, for instance, that since mathematical prose makes no use of distinctions of grammatical mood, regimentation virtually forces us to treat modality the way it is treated in mathematical statistics, where possibilities are represented by points in a space of possible states of the world. As a result something as simple as "I could have stayed home today" becomes something like "There exists a possible state of the world in which I am home today", introducing an apparent but arguably spurious commitment to reification of possible states of the world or "possible worlds". The author of the work under review also criticizes Quine on ontological commitment, but on quite different grounds.

The thesis of the present book is, in brief, that the burden on nominalists of producing a positive account of the kind that is called for—not because of any *general* requirement, but because of specific features of the mathematical case—if option (b) or (c) is chosen, can be shirked. A philosopher, it is suggested, can (literally) assert (2), and/or (equivalently) assert that (2) is (literally) true, and still not be "ontologically committed" to numbers. *A fortiori*, the philosopher can (literally) assert (1), and (equivalently) assert that (1) is (literally) true, and on top of that acknowledge that (2) is an "existential consequence" of what has just been asserted, and still not be "ontologically committed" to numbers. The author speaks of the "separation thesis" about the gap between existential consequence and ontological commitment, though it could equally be called an "ineffability thesis", to the effect that there is an intuitive notion of "ontological commitment" expressible neither by "exists" or "there exists" nor by *any* word or phrase of ordinary or scientific language. (The author does sometimes try to eff the ineffable by saying things like "Numbers do not *really* exist", as if there were a contrast between *real* existence and mere *existence*. But *real* existence is not existence *plus something more*; it is existence as *contrasted with* something less, mere allegation or appearance or whatever of existence.)

There is a genre of philosophical humor called "Proofs that p", in which the characteristic style of argument of various prominent philosophers is parodied. In the case of one famous nominalist, the proof that p attributed to him reads as follows: "[Another philosopher] has insinuated that my thesis that p is false, on the basis of alleged counterexamples. But these so-called 'counterexamples' depend on construing my thesis that p in a way that it was obviously not intended—for I intended my thesis to have no counterexamples. Therefore, p." The thesis of the present book is in effect that a philosopher can say *anything*, including "It is literally true that there exist such things as Fs", and when someone says that makes him "ontologically committed" to Fs, just reply "This so-called 'ontological commitment' depends on construing my assertion that it is literally true that Fs exist in a way that it was obviously not intended—for I intended my assertion to involve no ontological commitment."

I do not find the ineffability thesis intelligible, because Quine's coinage "ontological commitment" has no meaning apart from Quine's stipulative definitions. Moreover, the arguments presented by the author for the ineffability thesis consists mainly in pressing examples like the Cheney-Dowd example above; I have already said what such examples show; and it isn't that there is an intuitive notion of "ontological commitment" unrelated to existential consequence (or of "*real* existence" distinct from mere *existence*). In any case, the anti-nominalist reply is obvious: "The question we have been concerned with is whether numbers exist. If you are prepared to make a (literal) assertion of (2) and/or an assertion that (2) is (literally) true, that's the end of the question so far as we are concerned. We have no use for the phrase 'ontological commitment', which seems to have done nothing but sow confusion since the day it was coined, and if you want it you may have it to use as you please." Indeed, Rosen and I have said this already.

Ontology. Part of the reason we have no use for "ontology" is that we differ with Quine over what he called the "ethics of terminology". He thought it was permissible to take a term for an old enterprise considered discredited, and apply it to a new one considered more creditable. The old enterprise was the pre-Kantian one of attempting to catalogue what kinds of objects there are in reality "as it is in itself"—reality not as we apprehend it, but as God apprehends it. This enterprise was discredited by Kant's observation that the very idea of reality consisting of objects with properties is a merely projection of a feature of our understanding, its operating with judgments in subject-predicate form—or as a philosopher after the linguistic turn would prefer to say, a feature of our language, its operating with sentences in noun-verb form. (This objection to ontology in the pre-Kantian sense is reflected in the epigraph Quine chose for his *Word and Object*.)

The new enterprise of "ontology" in the post-Quinean sense is simply a glorified taxonomy, an attempt to catalogue what kinds of objects there are in reality as apprehended by us through our everyday and technical language, our commonsense and scientific theories. The danger posed by Quine's transferring "ontology" from the old enterprise to the new—rather than coining contrasting labels, perhaps pre-Kantian "ontosophy" *vs* post-Quinean "ontics"—is that some may be led to confuse the two homonymous enterprises. And indeed, there is evidence suggesting that some nominalists have done so. (There is such evidence in the fact that they misrepresent their *opponents* as being engaged in the pre-Kantian enterprise, insofar as they misrepresent their opponents as followers of Plato, the pre-Kantian philosopher *par excellence*: Though Quine was anything but a Platonist, nominalists with annoying persistence call him one.)

As Lichtenberg said, with most people unbelief in one thing springs from blind belief in another. If it is "ontology" in the pre-Kantian sense that nominalists are engaged in, then the post-Quinean objection is not to their skepticism about the abstract, but rather to their credulity about the concrete. Above all, it is to their imagining, two centuries after Kant, that an object-property distinction is a feature of reality "as it is in itself". In actual fact, it is not certain even that all human languages have a noun-verb distinction, and if we cannot be sure that the Nootka speak to each other in a language with this feature, how much less can we be sure that God speaks to Himself in such a language.

Fiction. About midway through the first half of the book the author turns from discussion of "ontological commitment" to a tortuous dialectic that ultimately issues in the conclusion that *according to commonsense standards* only concrete and not abstract objects "*really* exist". (Hartry Field's well-known epistemological argument for nominalism forms just one stretch towards the middle of the long, winding road.)

The starting point is a certain view about fictional and mythical characters. Let us suppose for the sake of example that Æsop was the first to tell stories about talking foxes. Suppose we hear him tell such a story. One might think that, being already in possession of the concepts of "fox" and "talking", and already in possession of evidence that none of the former do the latter, we conclude that such characters as the story is about do not exist and *for that reason* conclude they and the story about them are just made up. The author seems to think that the process goes the other way around: that we first conclude the characters and the story about them are just made up, and *for that reason* conclude that the talking fox does not *really* exist. He wants us to view the situation this way because his next step will be to try to get us to view mathematical objects as being just made up, and he wants us to conclude for that reason that they do not *really* exist.

The author's conclusion that numbers are "just made up" seems from the anti-nominalist point of view a kind of use-mention confusion. Let us suppose for the sake of example that Pythagoras was the first to speak of numbers. What he did was to introduce new locutions, with rules as to what statements involving these locutions are to count as true in what circumstances, which rules of use constitute the meaning of "number", the concept of number. You may hesitate to accept the concept until it is shown to have some practical utility, but if you do not accept it, then the question "Is the number of chairs in this room a prime number?" has for you no meaning. Once you do accept it, the rules constituting the concept guarantee that "The number of chairs in this room is a prime number" counts as true if there is a chair in this room, and another, and another, and no more. The rules make no provision for ascribing temporal properties to numbers. So before *and* after accepting the language and concept of number the question, "When did the first prime number come into existence?" makes no sense, and neither does the answer "When people made up the language of numbers." The statement that numbers are "just made up" is not even false, but rather is meaningless, if taken literally.

In short, the analogy the author wants us to see is between the role of Æsop in making up or introducing something new, statements about talking foxes, and Pythagoras making up or introducing something new, statements about numbers. The disanalogy he wants us to ignore is between our previously possessing the concepts of "fox" and "talk" and the knowledge that foxes don't talk, and our not previously possessing even the concept of "number" and therefore needing rules of use to give the word meaning. For one's attention to be selective in this curious way is a large part of what it is to be in the grip of the nominalist picture.

Physics. I am slighting, as of less direct interest to logicians, the second half of the book, where the author turns from the philosophy of mathematics to that of physics. Of this second half Hartry Field is quoted by the publisher on the back cover of the book as saying that "the inclusion of substantial material on the applications of mathematics is a strength." This positive evaluation is the more significant since the material is largely critical of Field's own views. Though formulated in terminology from the first half of the book, and presented as an application of the doctrines enunciated therein, I believe this material could be reformulated in Field's own terminology, though I will attempt no such reformulation here.

The publisher also quotes Michael Resnik as saying "The discussion of truth and existence is superb." It is evident from the above that I do not agree with Resnik's lavish praise. Let me close by saying I *do* agree with the more guarded praise in the part of Field's statement not yet quoted: The book "offers a quite distinctive viewpoint" and "will provoke considerable discussion".

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DEBORAH J. BENNETT. ***Logic made easy: How to know when language deceives you,*** W. W. Norton & Company, 2004, 256 pp.

There had been a great tradition of logic in the Middle Ages. The formal logic of Aristotle and the Stoics had been extended to cover reasoning with modalities and tenses, and deep solutions to the liar paradox had been proposed. By about 1500, however, that tradition was pretty much played out. People were mostly recapitulating earlier work, especially that of the logicians from the 14th Century. The tradition became less and less used, as it was difficult to master and no longer very exciting.

In 1662, Antoine Arnauld and Pierre Nicole wrote *La logique ou l'art de penser* as a summary of logic, which included many of their new ideas. That became the new standard. Unfortunately, it was a pretty bad standard. Little of the formal logic tradition remained beyond memorizing the study of Aristotelian syllogisms, and, as the title suggests, logic was assimilated with what we now call psychology.

That standard persisted until the beginning of the 19th Century, when there began a new interest in Aristotelian logic; the best-known worker in that vein was Richard Whately. At the same time, Boole and others began what we now know as modern logic. The Aristotelian tradition limped on, improved, but still not much more than the syllogisms plus some rhetoric. That continued to be taught until the beginning of the 20th Century, and even later in some universities, but as research and serious study it was hopelessly bad and overwhelmed by the work in modern mathematical logic.

In 1843 John Stuart Mill wrote *A System of Logic*, which had a much more ample conception of logic. Logic, as a guide to reasoning well, included not just the study of valid deductions, as in Aristotle's *Prior Analytics*, but also what Aristotle presented in his *Posterior Analytics*: how to generalize, how to reason about cause and effect, the nature of explanations, all collected by Mill under the rubric "induction." That book dominated the study of non-formal logic for more than a century.

Though Mill's work was influential among philosophers, logic as practiced by those who called themselves "logicians" became more and more the study of only formal logics of valid inferences. Eventually, almost all teaching of logic became the teaching of formal logic.

The result was that logic as a prescriptive science of how to reason well was ignored, except for remnants of the Aristotelian syllogisms and lists of fallacies presented in courses on rhetoric or occasionally in a course in philosophy. Even in mathematics, where modern formal logic was meant to serve as a standard, there was nothing offered to non-specialists beyond a little about how to write proofs offered in a first course in abstract algebra.

In the 1950s and early 1960s a few professors in the United States began writing texts that would help students reason well. Some texts were simply the Aristotelian tradition of syllogisms revived, some were a mix of Aristotelian logic and rhetoric, and some had all that plus a bit of formal logic. All the books were notable, however, for being remarkably badly written from a pedagogical point of view, and often not correct in their logical analyses. Nonetheless, hungry for any course to teach students how to reason, most universities adopted courses on "critical thinking" based on these and later texts.

As a logician, I became painfully aware of how badly prepared I was for teaching how to reason when I was asked to teach a course on critical thinking in 1995. Though I considered myself an expert in logic, I was stymied when presented with a newspaper editorial. Formal logic was of little use, and the material in the critical thinking texts I consulted on how to reason with strong rather than valid inferences was either trivial or wrong. Hence—to make a disclaimer—I wrote an undergraduate text on critical thinking and then a research text on the nature of inference, trying to bring together all of logic once again. For it's not that there is formal logic and it's poor cousin informal logic, but simply logic, the art of reasoning well, parts of which have been formalized and studied mathematically.

Deborah Bennett has written a book that seems to be in the tradition of critical thinking texts, but the focus of her book is unclear. Designed for the general public, with no exercises, it focuses almost entirely on the Greek tradition of logic. Much of it is devoted to the history of logic, but there is nothing on the Middle Ages, and it stops with one sentence about predicate logic, with no description of what that is. Many pages of her book are on the psychology of reasoning, describing experiments that show how people reason incorrectly. But the experiments are described so badly that often I couldn't figure out their point; one which crucially depends on colors is illustrated with a gray and white diagram. And as with many works on why people reason badly, an example of reasoning is labeled "a mistake" while no explicit standard of what constitutes good reasoning is first exposited. There is also much in the book on formal logic, but her exposition shows an elementary confusion of logic and metaphysics. For example, she writes on p. 31, "Closely related to the law of the excluded middle is the law of noncontradiction. The law of noncontradiction requires that a thing cannot both be and not be at the same time. ... The Stoics, who developed further rules of logic in the third century B.C., acknowledged the law of the excluded middle and the law of noncontradiction in a single rule, 'Either the first or not the first'—meaning always one or the other but not both." Laws of logic are about propositions—whether conceived as linguistic, as thoughts, or as abstract objects—and not about any other "things" in the world; "the first" was meant to be replaced by a proposition in the Stoic dictum. Bennett also confuses language and talking about language when she says on p. 130, "Two classic fallacies of inference involve the conditional syllogism. 'If p (is true) then q (is true). q (is true).' Conclusion? There is none." This is also wrong because we can make many conclusions. It's just that most of them do not follow validly, though some can, for example, not-not-q. She also confuses logic with psychology when on p. 29, after saying "The guiding principle of rational behavior is consistency," she describes consistency entirely in terms of belief and behavior. Her work is also notable for not giving definitions of the key terms it uses, "valid deduction" and "inference", and the word "argument" does not appear in the index. This book will not teach formal logicians anything of interest about the nature of reasoning well, and will surely confuse newcomers to logic.

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Handbook of the history of logic, edited by Dov M. Gabbay and John Woods, Volume 1: Greek, Indian and Arabic logic. Elsevier, Amsterdam, 2004, viii + 618 pp.—therein:

Julius Moravcsik. *Logic before Aristotle: development or birth?* Pp. 1–26

John Woods and Andrew Irvine. *Aristotle's early logic*. Pp. 27–100

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Fred Johnson. *Aristotle's modal syllogisms*. Pp. 247–308

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This is the first book in a series of several large volumes on the history of logic. This series adresses " ... members of the research communities in logic, history of logic and philosophy of logic, as well as those in kindred areas such as computer science, artificial intelligence, cognitive psychology, argumentation theory and history of ideas ... The ***Handbook of the history of logic*** aims at being a definitive research work for any member of the relevant research communities" (from the Preface). — The first volume has, in accordance with its title, three main areas: Greek Logic (Aristotelian logic, Stoic logic — five articles, 437 pages), Arabic logic (two articles, 83 pages), Indian logic (one article of 85 pages). Thus this ***Handbook*** continues with a tradition, which was begun by *Bocheński*, of including a certain amount of discussion on non-European logic. Indian and Arabian logic are very different in this respect: Whilst Arabian logic is a direct offspring of Greek logic and, since the Middle ages, has entered the main stream of European logic once again, Indian logic is considered to have grown up independently of the Greek tradition. Let us remark that this volume of the ***Handbook of the history of logic*** does not contain any information on the only logic which is based on a non-Indo-European language: Chinese logic. We will return to this point later on.

There are inherent obstacles impeding our understanding of the achievements of our logical ancestors. The work of many a great logician suffered from serious interpretative faults of posterity. Aristotelian Logic has been the main target for misleading interpretational attacks for more than 2000 years, and Stoic logic has undergone a continuous maltreatment almost up to the present day. Concerning the Stoics, O'Toole and Jennings, in their masterful chapter in the book under review, point to the problematic role of historians of logic like Prantl and Zeller who had not enough formal background and interest to enable them to differentiate between the Aristotelian logic of terms and the Stoic logic of propositions. Today, in the age of mathematical logic, we frequently meet another kind of misinterpretation of ancient logics, committed even by outstanding logicians (and historians!) like Łukasiewicz and Bocheński: It is not that we know too little of formal and symbolic systems, but, on the contrary, " ... that there is a danger that the historian of logic possessing this requisite of mathematical logic may allow his or her familiarity with the discipline to obscure, or even distort, the historical enterprise" (p. 398). Thus, O'Toole and Jennings try to " ... attempt an interpretation of Stoic logic less coloured by a reverence for modern formal systems, and more in harmony with what the texts seem to indicate as being the place of logic in the Stoic system as a whole" (p. 400). This statement could serve as a motto for the whole handbook series!

The first chapter by J. Moravcsik, "Logic before Aristotle: Development or Birth?" gives some answers to the question "was there logic before Aristotle in Western Culture?" The author traces the development of what was a necessary background for logic as a formal discipline, relying on the work of Bruno Snell, under the general headline "from myth to logic". The paper concentrates on a discussion of concepts and vocabulary presupposed by logic (Chapter 1), gives an account of the links between logic and definition (Chapter 2) and, in the last chapter, elaborates on the differences between Plato's Method of Division and

Aristotle's logical deduction. The summary of the paper contains the following sentences: "Our main point is that the rise of logic is both a matter of development and the matter of instantaneous creation. The required vocabulary must have a historical process preceding it. Once that is in place, the possibility of constructing a logic is there" (p. 21).

In the second chapter, "Aristotle's Early Logic", John Woods and Andrew Irvine focus on Aristotle's works ***Topics*** and ***On Sophistical refutations***. They assert " ... that the logical theory of the ***Analytica Priora*** presupposes Aristotle's theory of syllogisms ... These theories appear implicitly in the ***Topics*** and ***On Sophistical refutation***" (p. 34). The authors consider the works within the ***Organon***, the ***Categories*** and ***On interpretation***, not to be about logic at all, so it is only consequent that they exclude them from discussion within their paper on Aristotle's Early Logic. Let me just remark that only few experts in the field would ascribe to this point of view; however, we cannot step into the details here. (See the review of David Hitchcock, which is to appear in ***Argumentation*** vol. 18 (2004), regarding John Woods book ***Aristotle's earlier logic***, Oxford: Hermes Science, 2001, from which significiant parts of the chapter are drawn).

The third chapter by George Boger, "Aristotle's Underlying Logic", is the central one on Aristotle's logic within this volume. Boger says "Our concern here is to present Aristotle's system of logic while also revealing the mathematical sophistication of his logical investigations" (p. 113) which shows that his work belongs to the line of research begun by Łukasiewicz and continued by Smiley and Corcoran. Boger neither overemphasizes the value of symbolic representation of his findings nor restricts himself too much to a pure philological inquiry into Aristotle's works. — The chapter begins with a presentation of Aristotle's early work, the ***Categories***, ***On interpretation***, and ***Metaphysics*** which are the very basis for everything Aristotle writes on the subject of logic later on in his ***Prior analytics***. The basic concepts comprise the two types of declarative sentence, namely, *kataphasis* and *apophasis*, their connection to the four categorial sentence patterns (*problêmata*) , and the notions of contradiction and contraries (*hai antiphaseis* and *ta enantia*). Let us note that Boger's presentation does not completely solve the problem of the exact relation of Aristotle's ***Categories*** to the ***Prior analytics***: Whilst, in the ***Categories***, the roles of subject and predicates in a proposition are by no means symmetrical, the whole formalism of the syllogistic relies on the interchangeability of the subject and predicate of a proposition. Thus, there is no "smooth transition" from the early works of Aristotle to the formal system of the ***Analytica Priora***, and this certainly requires further research efforts. — The next subsection deals with the semantics of Aristotelian logic, and the following main part of the chapter is concerned with Aristotle's formal system of deduction, comprising — in addition to the elements already mentioned — three conversion rules, 14 syllogism rules, and two kinds (direct and indirect) of deduction. Boger presents his investigations into Aristotle's system within a kind of a system of natural deduction, thus following the "deductionist" way of interpretation, begun by Smiley and Corcoran in the 1970's. This interpretation accords much more to the text of the ***Prior analytics*** than Łukasiewicz' "axiomatic" formalism, published in his groundbreaking book from 1950. — Boger then describes the processes of completion (*teleiousthai*), which Aristotle utilizes in order to show that 10 of the 14 categorical syllogistic rules are — in modern terms — *derived rules* in the sense that they can be shown to be valid by deducing them from the four "basic" syllogisms of the first figure. This set of four basic syllogisms shrinks further (Pr. An. A 7) by Aristotle's proof that two of the syllogism rules of the first figure, *Darii* and *Ferio*, can also be completed — indirectly (by *reductio ad absurdum*) — to *Barbara* and *Celarent*. Boger emphasises the differences between the processes of completion, reduction, and analysis, however, his lines of distinction would deserve a detailled discussion which cannot be given here. This holds especially for his relatively short description of the process of "analysis", a concept which we believe can only be fully understood by diving deeper into Book B of the ***Prior analytics***. The chapter also contains a detailed section on

Aristotle's method of showing the inconcludence of "patterns not resulting in a syllogism". Boger's tables on this subject (pp. 198, 199, 200) will probably become the standard reference to this part of Aristotle's logic. – The whole chapter will be of interest to a wide audience, and its careful style of examination may be considered as a prototype for a thorough research on an ancient logic and as a solid basis for further research.

Fred Johnson's chapter on "Aristotle's Modal Syllogisms" is the continuation of a line of formal interpretation of Aristotle's logic which was begun by Łukasiewicz. Johnson starts by presenting Łukasiewicz's formal system for assertoric syllogistics, and he then explains his theory of rejection. The main part of the chapter is concerned with the syntax and semantics of modern modal systems which are more or less capable of reproducing Aristotle's results on modal syllogisms (Johnson introduces an "index of Aristotelicity" of a formal system). Whilst this chapter — in particular the extensive occupation with semantics — is of eminent value for researchers in the field of mathematical methods in modal logic, historians of Aristotle's logic, however, will probably not feel satisfied. Johnson's approach shares all the disadvantages of Łukasiewicz' method, which does not aim to conform as much as possible to the classical text but to invent an axiomatic system which produces as many classical theorems as possible. Thus Johnson's interesting chapter is more a contribution to the history and theory of modern formal interpretation of Aristotle's modal system than an article on Aristotle's modal system itself.

Chapter five contains the sole article on non-European logic within this volume, and, as it seems, within the whole series. J. Ganeri's chapter, entitled "Indian Logic" thus covers an immense amount of material, ranging from ancient Nyāya logic of some centuries B.C. until the Navya Nyāya of the middle ages (about 1300 AD). This is a project which, from the very concept, could not, and, in fact, did not succeed. Indian logic is not just an appendix to European logic, and it is not adequate to add an Indian fig leaf to a western-oriented "History of Logic". One should be aware of the fact that the Indian rooted logic provides us with formal logic systems which are not based on the Greek tradition, and this should be considered as an opportunity to critically reflect on our own tradition and the self-conception of our logic. However, this would require a careful discussion of the historical and philosophical background of Indian logic. In contrast to this, Ganeri's chapter "Indian Logic" is an attempt to provide a Western reader with a set of highlights of Indian logic and to show how these topics can be framed into certain systems of formal symbolic logical environments. Section 1 is on argumentation and debate, containing the "five-membered Indian syllogism", Section 2 on the Buddhist *trairūpya* theory and Diṅnāga's 'wheel of reasons' (*hetucakra*), Section 3 on Jaina logic, and Section 4 on logic in Navya–Nyāya. Within each section, Ganeri utilises a different system of symbolic logic in order to make the Indian ideas understandable to Western readers: He uses standard predicate logic in Section 1, the theory of classes in Section 2, a kind of "assertion logic" in Section 3, and a graph-theoretical formalism in Section 4. While this appears to be helpful for a reader who is experiencing his first contact with Indian logic, one should always be aware of the danger of imputing ideas from our own modern logic into an ancient system. — It is certainly not possible to do justice to Ganeri's chapter on Indian logic within this overall review. Nevertheless, I will add three critical remarks regarding the content of the paper. The first one is on Ganeri's formalisation of the "five-step schema" of the Nyāyasūtra. Quite in the tradition of St. Schayer, Ganeri speaks of the schema as "five-step proof" (p. 324), thus taking for granted that the Naiyāyikas disposed of a concept of a deductive system like we have today. In addition, Ganeri makes no mention about the discrepancy between his own interpretation on pp. 324–6 and Schayer's interpretation on p. 327, which is radically different with respect to the meaning of the fourth step (application, *upanaya*). — The second remark concerns subsection 2.1. Here the author employs a notational system which is not precise: He does not make a distinction between symbols for classes (or sets) and properties which define these classes. On p. 343, he writes about a "property S", and some lines below he writes "A reason property for S is any member of the class $\{X : X\cap \neq \emptyset \quad \& \quad X \cap \text{non}S = \emptyset\}$." Thus, we have a property, a class, and members of this class, but there is no precise notational differentiation between these types. The third remark concerns Diṅnāga's *hetucakra* in Section 2. Here Ganeri does not utilise symbolic methods which would have been of some help. The *hetucakra* is one of the few examples in Indian logic where there is a very clear formal structure which cries out for a symbolic representation. However, I do not blame Ganeri for not having employed symbolic tools, but for having given an incomplete representation of the content of the *hetucakra*: In his Figure 3 on p. 346, a complete column is missing. This column should contain, in rows 4–9, Diṅnāga's six examples for which h (*hetu*) is missing, but s (*sādhya*) is present: space – space – space – space – lightning – atom (see Chatterji's translation of the *hetucakra*, cited in the bibliography of the article under review).

"The Megarians and the Stoics" by Robert R. O'Toole and Raymond E. Jennings is one of the central chapters of this handbook. To be precise, the essay is concerned mainly with the logical doctrines of the Early Stoa (Zeno, Kleanthes, and Chrysippus). The authors begin with comments on the history of misunderstandings and misconceptions of Stoic logic by historians like Prantl and Zeller as well as by Łukasiewicz, Mates, Bocheński, the Kneales and others. "The obstacle for both the later and earlier writers, it seems to us, is that they have allowed their preconceptions to obscure their understanding" (p. 399). One of the main assertions of the authors is that " ... it is surely false that the logical connectives which appear in the Stoic syllogisms were in general defined as binary connectives" (p. 398). O'Toole and Jennings show meticulously how the edifice of Stoic logic is founded on Stoic ethics, epistemology, ontology and their understanding of semantics and inference. The authors go deep into a discussion of the role of *lekta, pragma* and *axiōmata* as a prerequisite for the understanding of the building blocks of Stoic formal logic. It is impossible, at this point, to review this chapter of the handbook — it certainly deserves a separate review. This article will probably become the standard reference for anyone interested in Stoic logic.

The last two chapters of the Handbook are devoted to Arabic logic. Tony Street's chapter "Arabic Logic" should, in the words of the author (p. 526), more precisely be titled as "Peripatetic logical writings in Arabic produced in the realms of Islam between 750 and 1350, with special reference to the syllogistic." The chapter contains sections on "The Translation of the Organon", "Alfarabi and Avicenna", "Logic and the Islamic Disciplines" and "Logic after Avicenna". Besides an extensive Biibliography, there are appendices A: "Avicenna's Modals", B: "Later Modal Logic", and C: "Bibliographical Notes". This chapter is an excellent introduction into the subject and a starting point for further research.

Charles Burnett's short (10 pages) interesting chapter titled "The Translation of Arabic Works on Logic into Latin in the Middle Ages and Renaissance" gives answers to the question: Why was there any need to translate the texts of the ***Organon*** from Arabic into Latin during the twelfth and thirteenth century, given that Latin scholars " ... already had good translations of at least the first half of the ***Organon*** ... , made by Boethius in the early sixth century. And, when they wished to complete the ***Organon***, they were able to do so by translating the texts directly from the Greek" (p. 597)?

Let me add some remarks concerning the concept and realization of this Volume 1 of the "Handbook". In their short Preface, the editors write about the freedom given to the authors " ... to develop their own interpretation of things." It goes without saying that such a liberal approach has not only advantages. Concerning the subject of "Indian Logic", this approach was not successful, as we tried to show in our review of the corresponding chapter. Indian logic, together with its offsprings based on Buddhist logic in Tibet and China, is such a vast subject that it would deserve a volume of its own. Alternatively, an article comparable in scope and style to the one on Stoic logic would have been an appropriate choice. — Considering the fact that "Arabic Logic", in this Volume of the Handbook, has as the subject "Aristotelian syllogistic in Arabic writings between 750 and 1350", it is not clear why "Arabic Logic" was included in the title of the book or why the articles on this subject are not published in their appropriate historical context. Was this just an attempt to "sex up" the title?

In addition to my remark on the absence of a chapter on China at the beginning of this review, I want to make a short comment on the importance of Chinese logic. It is well known that, long before Buddhist logic came from India, there existed also an indigenous Chinese logic of the later Mohists around the 3rd century B.C. An introduction into this subject as well as into the later Chinese Buddhist logic is given by Joseph Needham and Christoph Harbsmeier, ***Science and civilisation in China, volume 7***, Part I, Language and Logic in Traditional China. Cambridge University Press: Cambridge, 1998. We agree with Harbsmeier, who writes on p. xxi of that book: "The history of logic reflection in China is therefore [because of its being based on a non-Indo-European language; K.G.] of extraordinary interest for any global history of logic and hence for any global history of the foundations of science." The absence of a chapter on Chinese logic in the ***Handbook on the history of logic*** must be considered as a lost opportunity which is not due to a lack of literature on this subject (see also A. C. Graham, ***Later Mohist logic, ethics and science***. The Chinese University Press, Hong Kong, 1978).

The volume under review has an Index which is of limited value because references to the chapters on Indian Logic, Stoic logic and on the last chapter are missing. Some chapters (e.g., Boger's article) are troubled by typesetting errors which are too numerous to report here. On p. 372, items 1–4 of a list are missing. An entertaining type of error occurs repeatedly in the chapter on Stoic logic: "$\alpha\nu\delta\ \alpha\nu o\tau\eta\varepsilon\rho\ \tau\iota\mu\varepsilon\ \alpha\gamma\alpha\iota\nu\sigma\tau\ \iota\tau\ \ldots$ " (p. 413, see also pp. 411, 413, 415, and others), is not Greek, but English: "and another time against it ... ".

Notwithstanding these critical remarks, this Volume 1 of the "Handbook of the History of Logic" is of value due to the excellent chapters by George Boger on Aristotle's syllogistic logic, by Robert R. O'Toole and Raymond E. Jennings on Stoic logic, and because of the two chapters on Arabic Logic by Tony Street and Charles Burnett.

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ERNEST SCHIMMERLING. *Covering properties of core models*. ***Sets and proofs.*** (***Leeds, 1997***)**,** London Mathematical Society Lecture Note Series 258. Cambridge University Press, Cambridge, 1999, pp. 281–299.

PETER KOEPKE. *An introduction to extenders and core models for extender sequences*. ***Logic Colloquium '87*** (***Granada, 1987***)**,** Studies in Logic and the Foundations of Mathematics 129. North-Holland, Amsterdam, 1989, pp. 137–182.

WILLIAM J. MITCHELL. *The core model up to a Woodin cardinal*. ***Logic, methodology and philosophy of science, IX*** (***Uppsala, 1991***)**,** Studies in Logic and the Foundations of Mathematics 134, North-Holland, Amsterdam, 1994, pp. 157–175.

BENEDIKT LÖWE and JOHN R. STEEL. *An introduction to core model theory*. ***Sets and proofs*** (***Leeds, 1997***)**,** London Mathematical Society Lecture Note Series 258, Cambridge University Press, Cambridge, 1999, pp. 103–157.

JOHN R. STEEL. *Inner models with many Woodin cardinals*. ***Annals of Pure and Applied Logic***, vol. 65 no. 2 (1993), pp. 185–209.

ERNEST SCHIMMERLING. *Combinatorial principles in the core model for one Woodin cardinal*. ***Annals of Pure and Applied Logic***, vol. 74 no. 2 (1995), pp. 153–201.

PHILIP D. WELCH. *Some remarks on the maximality of inner models*. ***Logic Colloquium '98*** (***Prague, 1998***)**,**, Lecture Notes in Logic 13, Association of Symbolic Logic, Urbana, Illinois, 2000, pp. 516–540.

The investigations in inner model theory trace back to Gödel's proof of the relative consistency of the Axiom of Choice and the Generalized Continuum Hypothesis in 1938. After Gödel's discovery of the constructible universe **L**, however, the topic remained dormant for a couple of decades. Although revived by the dramatic developments in set theory in the mid sixties, and despite Jensen's elegant analysis of its internal structure and rich combinatorics, the constructible universe, being just one particular model of set theory, was perhaps overshadowed by the tremendous possibilities that the method of forcing offered at that time. Thus, it is fair to say that the real birth of the inner model program only came in 1974 with the advent of Jensen's covering theorem. It was only at this point that people realized that one very concrete model is able to approximate faithfully most of the global structure of the set theoretic universe **V**, and that the only factor determining whether such an approximation is possible is the large cardinal structure of **V**.

Building inner models beyond **L** involves, among many other issues, bringing together two highly antagonistic aspects—the uniformity of the internal structure of **L** on the one hand, and the reflection properties given by large cardinals on the other hand. It is therefore not surprising that some time was needed to find a construction of the core model **K** that would be sufficiently general to produce models with measures of high Mitchell order, not to mention models with overlapping extenders. The sustained effort was finally crowned by the Mitchell-Steel construction of a fine structural model containing a Woodin cardinal. The ideas used in their construction were soon developed to produce the true core model **K** below a Woodin cardinal, and also various kinds of models with many Woodin cardinals. These developments constitute a significant step towards the most ambitious challenges facing inner model theory—models for superstrong cardinals.

A significant number of applications of inner models builds on their covering properties. Also, as was pointed out above, covering was one of the main sources of motivation for the inner model program. It is therefore a good strategy to understand the covering properties of **L** before focusing on higher models. Schimmerling's paper *Covering properties of core models* [***Sets and proofs*** (***Leeds, 1997***), pp. 281–299, London Mathematical Society Lecture Note Series 258, Cambridge University Press, Cambridge, 1999] gives an outline of Jensen's proof from a modern perspective. The author isolates certain key elements of the proof which became important both within and outside of inner model theory. He also includes an intuitive discussion what inner models are and what kind of difficulties should be expected when trying to generalize Jensen's proof to those models. The proof is divided into three steps. The first step is a discussion of the simplest non-trivial instance of the covering theorem—the so-called weak covering at countably closed cardinals. Focusing on this special case makes it possible to explain all essential ideas while maintaining the technicalities at the lowest possible level of difficulty. The countable closure makes it easy to guarantee that certain ultrapowers formed in the course of the proof are well-founded. In the general case where we don't have the countable closure, the well foundedness is not always guaranteed. However, it is still the case that all relevant ultrapowers are well-founded "very often", in the sense made precise. The proof of this fact, or in Jensen's terminology, the "frequent extension argument", is presented as the second step towards the complete proof. The third step is then straightforward—it involves putting the two previous steps together.

Koepke's paper *An introduction to extenders and core models for extender sequences* [***Logic Colloquium '87*** (***Granada, 1987***), pp. 137–182, Studies in Logic and the Foundations of Mathematics 129, North-Holland, Amsterdam, 1989] is one of very few papers which are directed to a broader audience and which appeared in print before Mitchell-Steel's book. It contains a compact and self-contained overview of the core model theory up to one strong cardinal, as well as a sample application: If the existence of a successor Jónnson cardinal is consistent with ZFC, then so is the existence of a strong cardinal. The first few sections of the paper are devoted to a detailed discussion of extenders and their relationship to elementary embeddings, and constitute a suitable introductory text to the subject. In the next two sections, the notions of iterability and iterable structure are introduced. The text here follows the older apporach of Dodd and Jensen, in that the extenders on the extender sequences are all *total*. The author then presents proofs of the basic results concerning iterable structures, such as the absoluteness of iterability, the comparison lemma, a version of the Dodd-Jensen lemma and the bicephalus lemma. With the theory of iterable structures at hand, he devotes two subsequent sections to a brief discussion of *the* core model **K** itself, listing its all fundamental properties. From today's point of view, the most important of these properties, namely the maximality theorem, the rigidity theorem and the weak covering theorem are valid in a much more general context (when suitably reformulated) and do not depend on the actual approach to the construction of the core model. It is also worth noting that these properties of the core model can be formulated without any direct reference to fine structure theory. This gives the author sufficient tools to present a full proof of his result on Jónnson cardinals.

An excellent non-technical introduction to inner model theory aimed at a broader audience can be found in Mitchell's paper *The core model up to a Woodin cardinal* [***Logic, methodology and philosophy of science, IX*** (***Uppsala, 1991***), pp. 157–175, Studies in Logic and the Foundations of Mathematics 134, North-Holland, Amsterdam, 1994]. Due to strong emphasis on motivation, it is particularly suitable as a first reading in the subject before proceeding to technical texts. In four sections, the author gives an intuitive idea what "the true core model" should be, explains the importance of Woodin cardinals, outlines the construction of the model, and finally concludes with four conjectures illustrating the up-to-date understanding of the core model. To explain the idea of the core model, he describes classical examples like Gödel's **L**, the model $\mathbf{L}[\mu]$ and the Dodd-Jensen core model, and also gives the essential criteria for the core model, such as the method of construction, the condensation principle, rigidity, the covering lemma and correctness. In connection with the influence of Woodin cardinals, he discusses the links with the axiom of determinancy and questions concerning Δ^1_3 well-ordering of reals. Furthermore, he introduces the reader to Woodin's two famous forcings—the stationary tower forcing and the extender algebra, and shows how the former can be used to violate weak covering. In the section discussing the actual construction of the model, he briefly introduces extenders and then focuses on the two key ideas of the modern inner model theory: *iteration trees*, which are implemented to avoid the problem of moving the generators, and the use of *partial* extenders on extender sequences of premice, which guarantee a good fine structure theory. Out of the four conjectures collected in the last section, the first one regarding Σ^1_3-correctness was solved by Steel for the true core model below one Woodin cardinal under an extra large cardinal assumption. The remaining three questions, concerning the exact consistency strength of ω_2-saturated, countably complete ideal on ω_1, the relationship between the failure of the weak covering lemma for **K** and the stationary tower forcing, and the consistency strength necessary for certain special situations in which successors of singular cardinals become collapsed in generic extensions, remain still open.

A reader interested in an informal exposition at a higher technical level than that in Mitchell's paper might consult *An introduction to core model theory* by Löwe and Steel [***Sets and proofs*** (***Leeds, 1997***), pp. 103–157, London Mathematical Society Lecture Note Series 258, Cambridge University Press, Cambridge, 1999]. The authors here reveal enough technical details that are needed to present most of the crucial ideas and methods of modern core model theory, and also give a proof of the result, due to Steel and Woodin, concerning the lower bound for the consistency strength of generic absoluteness of the theory of $\mathbf{L}_{\omega_1}(\mathbb{R})$. The paper contains two main sections which focus on the general theory of inner models and on the theory of the true core model, respectively. The section on the general theory begins with a discussion of extenders and coherent extender sequences which are needed to define the notion of a *premouse*. Next, the authors introduce iteration trees and iteration strategies, prove the basic results on premice such as the comparison lemma and the Dodd-Jensen lemma, and present the theorem on existence of cores for sufficiently iterable premice, whose sophisticated proof goes beyond the framework of the paper. All of this allows to describe Steel's $\mathbf{K}^c$-construction, explain the branch existence theorem and give the proof of the branch uniqueness theorem for 1-small premice, which is based on a remarkable argument of Martin and Steel. At the end of the section, results on iterability of the model $\mathbf{K}^c$ and its initial segments are discussed. The section on the true core model opens with Steel's cheap covering lemma, one of the basic ingredients in the construction of the true core model **K** and with an example which shows that $\mathbf{K}^c$ is not absolute relative to set-generic extensions. Next the notions of hull and definability property are introduced; these are crucial for the definition of **K**. After introducing **K**, the authors prove both its existence and the cheap covering lemma for **K**, and list several deep results concernig **K**, such as the weak covering lemma, a general version of the rigidity theorem and Steel's result concerning the Σ^1_3-correctness. The end of the section is devoted to a discussion of thick classes which leads to the proof of the set-generic absoluteness of **K**, and to the inductive definition of **K** which builds on the notion of an α-strong premouse. The latter is immediately applied to obtain a Woodin cardinal from generic absoluteness of the theory of $\mathbf{L}_{\omega_1}(\mathbb{R})$. In the concluding section, the authors briefly discuss the situations beyond one Woodin cardinal; in particular, they describe two phenomena illustrating new kind of obstacles the theory faces at this level: the impossibility of constructing **K** "locally" and the existence of iterable core models that are not internally iterable.

The results in Steel's paper *Inner models with many Woodin cardinals* [***Annals of Pure and Applied Logic*** vol. 65 (1993), no. 2, pp. 185–209] document early progress on extending inner model theory beyond one Woodin cardinal. They also include applications concerning $\mathbf{L}(\mathbb{R})$-absoluteness, the complexity of the natural well-ordering of reals in the model M_ω and **HOD**-calculations, obtained independently by the author and Woodin. The first section focuses on the generalization of the fully background certified construction for tame premice, that is, premice that are allowed to contain many Woodin cardinals, but never contain a Woodin cardinal overlapped by an extender. The author introduces the weak iteration game where the "good" player is allowed to play *maximal*, but not necessarily cofinal well-founded branches through iteration trees, and the "bad" player is limited to produce iteration trees on premice that appear at the end of such branches. It is then proved that all relevant premice are iterable in this weaker sense. This amount of iterability is sufficient to obtain the comparison lemma for tame premice, but still not enough to develop the full fine structure theory for the model. Such a theory is developed in the second and third sections under a more restrictive assumption that all premice which appear in the construction are meek; roughly speaking, such premice cannot have a measurable limit of Woodin cardinals. The main point here is to establish the uniqueness of cofinal realizable branches on countable iteration trees on realizable premice, which is obtained by applying a comparison argument to the models corresponding to possible distinct branches. The fourth chapter discusses the applications of the theory developed so far combined with Woodin's genericity iterations. Let $\mathbb{R}^*$ be the reals of the symmetric collapse of M_ω below the supremum of its Woodin cardinals. The first absoluteness result asserts that the theory of $\mathbf{L}(\mathbb{R}^*)$ agrees with that of $\mathbf{L}(\mathbb{R})$ even if we allow parameters from $\mathbb{R}^*$. The proof of this fact is then adjusted to conclude that the boldface theory of $\mathbf{L}(\mathbb{R})$ cannot be changed by small forcing, as well as to obtain a Shoenfield-type absoluteness theorem: Σ_1-statements that hold in $\mathbf{L}(\mathbb{R})$ are true in $\mathbf{L}(\mathbb{R})^{M_\omega}$. The last result is a precursor of the later work of Steel and Woodin on the calculation of **HOD** in $\mathbf{L}(\mathbb{R})$—the author shows that the reals of M_ω are exactly those which are ordinal definable in $\mathbf{L}(\mathbb{R})$, and even a bit more. The key point here is the characterization of the levels of ω-small premice that project to ω. As this can be done in terms of weak iterability with respect to the game of ω rounds, the set of all such levels, as well as the natural well-ordering on the reals of M_ω, are both $\Sigma_1(\mathbf{L}(\mathbb{R}))$, and this reflects to $\mathbf{L}(\mathbb{R})^{M_\omega}$.

Studying combinatorial properties of core models constitutes a relatively large program within inner model theory. Schimmerling's paper *Combinatorial principles in the core model for one Woodin cardinal* [***Annals of Pure and Applied Logic*** vol. 74 (1995), no. 2, pp. 153–201] is the first contribution to this program at the level of Woodin cardinals. The author establishes several **L**-like principles in core models of Mitchell-Steel type and applies his results to obtain a lower bound for the consistency strength of the Proper Forcing Axiom. In the first section, the author briefly reviews the Mitchell-Steel fine structure theory and gives a characterization of Jensen's principle $\diamondsuit^+_\kappa$; such a characterization is based on a standard version of the condensation lemma. The second section is devoted to the first attempt at Jensen's $\square_\kappa$. Here he needs to formulate and prove a finer version of the condensation lemma, which is applicable to the so-called almost Σ_1-preserving embeddings, and which enables him to imitate Jensen's original **L**-construction in an extender model to obtain $\square_\kappa$ below the first cardinal λ whose Mitchell order is λ^{++}. Now he encounters a serious limitation on the method. As he points out at the end of the section, almost Σ_1-elementary substructures of premice need not necessarily collapse to premice once we are beyond such λ. To overcome this obstacle, which turns out to be unavoidable in further generalizations of the construction of square sequences, he has to consider collapsing structures other than premice and establish their relationship to the collapsing levels of the model. This relies on a detailed analysis of Dodd parameters, and is carried out in certain relevant situations in the third section. An entirely general analysis of Dodd parameters in Mitchell-Steel models was later done by Steel. The fourth section contains some additional fine structural means needed for the main construction. This construction is carried out in the fifth section, where the author introduces the hierarchy of square principles $\square_{\kappa,\lambda}$ and proves $\square_{\kappa,\mathrm{cf}(\kappa)}$ in **V** whenever his extender model correctly computes the cardinal successor of κ. He also establishes $\square_{\kappa,<\kappa}$ and $\square_{\kappa,1+\mu}$ in any extender model; here μ is the number of all ordinals below κ whose Mitchell order in the extender model is at least κ^+. Roughly speaking, his construction still resembles Jensen's original construction in **L**, but requires splitting ordinals between κ and κ^+ into three disjoint sets and arguing in each case separately, and, as was already mentioned above, dealing with collapsing structures other than levels of the extender model. The last chapter is devoted to applications—the author obtains a Woodin cardinal from either the Proper Forcing Axiom or the Stationary Reflection Principle at κ.

Welch's paper *Some remarks on the maximality of inner models* [***Logic Colloquium '98*** (***Prague, 1998***), pp. 516–540, Lecture Notes in Logic 13, Association of Symbolic Logic, Urbana, Illinois, 2000] discusses three topics related to inner models—the maximality properties of inner models, questions concerning elementary embeddings and various connections through the concept of Jónsson cardinal. The first section is expository and focuses on the discussion of three questions. The first of them is that of Kunen on the consistency strength of an elementary embedding $j\colon \mathbf{M} \to \mathbf{V}$; in this context it is always assumed that $\langle \mathbf{V}, j\rangle$ is a ZFC-model. The author states a joint result with Vickers which shows that a model with such an embedding can be obtained from a Ramsey cardinal, thus giving a negative answer to Kunen. A related question addresses the consistency strength of such an embedding with some additional requirements on its fixpoints and the author lists some partial results on the lower bounds, again joint with Vickers. The last question (in the paper listed as Question 2) concerns the Jónssonness of $\aleph_\omega$; a result due to Donder and Koepke shows that the consistency strength of such a situation is at least that of a proper class of measurable cardinals. The main part of the paper is the second section which focuses on the computations of successor cardinals, in particular successors of Jónsson cardinals. By a result of Mitchell and Welch, $\mathbf{V} \neq \mathbf{L}[A]$ whenever A is a subset of a regular Jónsson cardinal. The author strengthens this by showing that either every such set A has a sharp or there is a sharp for an inner model with a strong cardinal. The next result he obtains is the weak covering lemma for Jónsson cardinals: Under the assumption that the sharp for an inner model with a strong cardinal does not exist he shows that the true core model **K** computes successors of regular Jónsson cardinals correctly and that the situation reflects on a stationary subsets of such cardinals. What makes this result even more interesting is that it gives a correct **K**-calculation of successors of certain *weak* inaccessibles, as well as the fact that the reflection is guaranteed without any "easy" reflection property on the background. Under the additional assumption that there is a measurable cardinal, he is able to extend his arguments to Steel's core model below a Woodin cardinal.

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ANDREAS WEIERMANN. *Complexity bounds for some finite forms of Kruskal's Theorem*. ***Journal of Symbolic Computation***, vol. 18 (1994), pp. 463–448.

ANDREAS WEIERMANN. *Termination proofs for term rewriting systems with lexicographic path ordering imply multiply recursive derivation lengths*. ***Theoretical Computer Science***, vol. 139 (1995), pp. 355–362.

ANDREAS WEIERMANN. *Bounding derivation lengths with functions from the slow growing hierarchy*. ***Archive of Mathematical Logic***, vol. 37 (1998), pp. 427–441.

Hofbauer's paper, *Termination proofs by multiset path orderings imply primitive recursive derivation lengths*, ***Theoretical Computer Science***, vol. 105 (1992), and independently Cichon's (erroneous) paper *Termination orderings and complexity characterisations* in ***Proof Theory***, P. Aczel and H. Simmons and S. S. Wainer (editor), 1992, triggered the interest of the proof theory community in the worst-case complexity analysis of (finite) term rewriting systems.

In the three papers under review Weiermann establishes smooth applications of proof-theoretic methods to term rewriting theory, and also clarifies the connection between two distinct uniform measures of termination orders, introduced in the term rewriting literature. Dershowitz and Okada argue in their paper *Proof-theoretic techniques for term rewriting theory*, ***Logic in Computer Science***, 1998, that a suitable uniform measure of a termination order $\prec$ ought to rest on the order type of $\prec$. Complementary Hofbauer and Lautemann stressed in their ***Rewriting Techniques and Applications*** paper *Termination proofs and the length of derivations*, 1989, that the strength of a termination order $\prec$ should be measured through the derivation length functions of rewrite systems R, shown to be terminating via $\prec$.

It turns out that the difference of the proposed uniform measures is not so deep as it may seem. Cichon (in the above cited paper) was the first to claim that for specific instances of termination orders the *slowgrowing hierarchy* provides the correct tool to link the order type of a termination order $\prec$ with the derivation length function of a rewrite system R, shown to be terminating by $\prec$. This conjecture is sometimes called *Cichon's principle*. It ought to be stressed that this principle is false in general. Let Dl_R denote the derivation length function of a rewrite system R. I.e.,

$$\mathrm{Dl}_R(m) := \max\{n\colon \exists(t_1, \dots, t_n)[t_1 \to_R \cdots \to_R t_n] \wedge |t_1| \le m\},$$

where $|t|$ measures the syntactic complexity of t suitably. According to Cichon's principle, for any rewrite system R, whose termination follows by an application of Kurskal's Theorem, it should hold that Dl_R is majorized by G_α—G_α denotes the α^{th} element of the slowgrowing hierarchy—for some α less then the small Veblen ordinal, denoted as $\vartheta\Omega^\omega$. In the spirit of Girard's Hierarchy Comparison Theorem this can be reformulated: Dl_R ought to be majorized by a multiple-recursive function. In her paper *Encoding the Hydra battle as a rewrite system*, ***Mathematical Foundations of Computer Science***, 1998, Touzet introduced a rewrite system R coding a restrained version of the (standard) Hydra Battle such that no multiple-recursive function can majorize Dl_R. However, termination of R can be shown via Kruskal's Theorem, i.e., R is simply terminating. Thus the principle fails even for simply terminating rewrite systems. Complementary results have been achieved by Lepper, *Simply terminating rewrite systems with long derivations*, ***Archive for Mathematical Logic***, vol. 43 (2004).

The first article under review renders an analysis of a restricted form of Kruskal's Theorem. From the viewpoint of term rewriting the paper can be conceived as a proof-theoretic study of the well-foundedness proofs of simplification orders. Let $\langle X_i \colon i < n\rangle$ be a finite sequence of partial orders. We write $T(\langle X_i \colon i < n\rangle)$ to denote the partial order on the set of terms over $\mathrm{dom}(X_0) \cup \cdots \cup \mathrm{dom}(X_{n-1})$ ordered by the (homeomorphic) embedding relation. Kruskal's Theorem becomes: If $\langle X_i \colon i < n\rangle$ is a finite sequence of well-partial orders, then $T(\langle X_i \colon i < n\rangle)$ is a well-partial order. Let us write T instead of $T(\langle X_i \colon i < n\rangle)$. Suppose a finite bad sequence (wrt. the embedding relation) of terms $\langle t_1, \dots, t_m\rangle$ in T is given. Furthermore assume this sequence is given effectively, i.e., the number of symbols in t_i is bounded in the number of symbols in t_1. A smooth mapping f, called *reification*, of the well-founded tree of bad sequences into the ordinals below the small Veblen ordinal (aka Ackermann ordinal) is defined via an ordinal mapping o from (the names of) partial orders into $\vartheta\Omega^\omega$. The definition of f is arranged carefully enough such that the syntactic complexity of $f(\langle t_1, \dots, t_{i+1}\rangle)$ is elementarily bounded in the syntactic complexity of $f(\langle t_1, \dots, t_i\rangle)$. The latter provides the basis to employ a norm-based Hardy hierarchy $\{\mathrm{H}_\alpha \colon \alpha < \vartheta\Omega^\omega\}$ to establish an effective bound on the length of the bad sequence $\langle t_1, \dots, t_m\rangle$.

By definition a termination order is a simplification order if it extends the embedding relation. The analyzed form of Kruskal's Theorem is sufficient to yield the well-foundedness of simplification orders. I.e., the given reification (re)establishes the well-known result that the order type of any simplification order is $< \vartheta\Omega^\omega$. Furthermore, suppose R denotes an arbitrary finite and simply terminating rewrite system, defined over T. Then as a corollary to the above one obtains an effective majorization of $\mathrm{Dl}_R(m)$ by $\mathrm{H}_{\mathrm{o}(T)}(c)$, where c is elementary in m and the syntactic complexity of $\mathrm{o}(T)$. The optimality of this upper bound follows by work of Lepper (cited above). Hence in the case of an arbitrary simply terminating R, the Hardy hierarchy provides the correct tool to effectively link the worst-case complexity of R with the order type of its termination order. The open problem posed at the end of the paper has meanwhile been solved negatively by Touzet (in the above cited paper).

The second article yields a derivation length analysis of rewrite systems R, whose rewrite relations are contained in a lexicographic path order (LPO). The central result is that the derivation length function Dl_R is majorized by a multiple-recursive function. This result is optimal as for any multiple-recursive function f, one can define a corresponding rewrite system R terminating under LPO such that the derivation length of R majorizes f. We fix a rewrite system R, whose rewrite relation is contained in an LPO. The paper defines a mapping ψ from the ground term algebra into the natural numbers. The main body of the paper is dedicated to show that the mapping ψ is a monotone interpretation that normalizes any rule $(l, r) \in R$. Thus $\psi(t_1)$ provides an upper bound for the maximal possible rewrite sequence (in R) starting with t_1. Together with some auxiliary results this suffices to establish the main result. Complementary results have been reported by Arai in *Some results on cut-elimination, provable well-orderings, induction, and reflection*, ***Annals of Pure and Applied Logic***, vol. 95 (1998).

I have already mentioned that the supremum of the order types of simplification orders is the small Veblen ordinal. The same holds for LPOs. An application of Girard's Hierarchy Comparison Theorem yields that the multiple-recursive functions are captured by the slowgrowing hierarchy (in its standard definition) at the stage $\vartheta\Omega^\omega$. Thus the second paper establishes the correctness of Cichon's principle for rewrite systems R, whose rewrite relation is contained in some LPO.

Cichon's principle is directly addressed in the third paper under review. Let $\prec$ denote either an instance of the recursive path order (RPO) or an LPO. When considering a finite rewrite system R, then the termination proof (via $\prec$) for the induced rewrite relation does not make use of the full power of $\prec$, instead R is already normalizing under a restrained approximation of $\prec$. This has been observed and employed by Buchholz in his paper *Proof-theoretical analysis of termination proofs*, ***Annals of Pure and Applied Logic***, vol. 75 (1995), to define directed approximations $\prec_k$ of $\prec$. Informally $s \prec_k t$ holds if $s \prec t$ and the numbers of symbols in s is bounded effectively in the number of symbols in t. Furthermore this constraint holds hereditarily also for all subterms of s and t.

The third paper establishes an extension of this idea to the segment of ordinals below the first subrecursive inaccessible ordinal, i.e., the first ordinal where the slow- and fastgrowing hierarchy catch up. The gist of the work amounts to the fact that $\alpha <_k \beta$ implies $C_k(\alpha) < C_k(\beta)$, for a suitable chosen slowgrowing function C_k. This result allows the definition of a class of (finite) rewrite systems such that Cichon's principle holds for this class. Whenever we are able to find (effectively) a $k < \omega$ such that a given rewrite relation $\to_R$ is contained in $\prec_k$ (under an interpretation of the ground term algebra into α), then $C_k(\alpha)$ yields an upper-bound for the derivation length of R.

As a corollary one obtains a uniform method to effectively majorize the derivation length function Dl_R if R is normalizing either by an LPO or RPO. Contrary to Buchholz proof of the latter, the presented proof method does not employ proof-theoretic arguments, but rests on elementary calculations within subrecursive hierarchies. Related and overlapping results can be found in Arai, *Variations on a theme by Weiermann*, ***The Journal of Symbolic Logic***, vol. 63 (1998).

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HEINZ–DIETER EBBINGHAUS. *Zermelo: definiteness and the universe of definable sets*. ***History and philosophy of logic***, vol. 24 (2003), pp. 197–219.

HEINZ–DIETER EBBINGHAUS. *Zermelo in the mirror of the Baer correspondence, 1930–1931*. ***Historia mathematica***, vol. 31 (2004), pp. 76–86.

As formulated in his 1908 paper presenting a first axiomatization of Cantorian set theory, Zermelo's Axiom of Separation (*Aussonderungsaxiom*) presupposes a notion of *definiteness*: "If for each element of a set M, the propositional function (*Klassenaussage*) $\mathfrak{E}(x)$ is definite, then M possesses a subset $M_{\mathfrak{E}}$ that contains all and only those elements x of M for which $\mathfrak{E}(x)$ is true." This has usually been construed to mean that it is properties that are classified as either definite or not definite. Zermelo provides the following elucidation: "A question or proposition (*Aussage*) is termed *definite* if its validity is determined without arbitrariness by the basic relations of the domain by means of the axioms and the universally valid laws of logic." Zermelo envisions a thought-experiment whereby the user of his axioms is justified in applying *Aussonderung* to set M and property $\mathfrak{E}$ provided that, for arbitrary $\mathfrak{a} \in M$, it is apparent, as the result of carrying out the experiment, that proposition $\mathfrak{E}(\mathfrak{a})$ has a determinate truth value. (During the experiment, the user may appeal to the definitions of power set, sum set, and so forth and to the classical understandings of propositional connectives "not," "and," and "or" and quantifiers "all" and "there exists.") Years later, Zermelo suggested that his intention in 1908 was only to ban non-germane properties such as "green-painted." However that may be, it is hardly surprising that those working in foundations came to regard Zermelo's own formulation of *Aussonderung* as unacceptable. (Russell pronounced it "so vague as to be useless.") Zermelo was aware of the problem and, in an article published in 1929, attempted to remedy it. That solution, based on finitary second-order logic, got little attention, and Zermelo himself lost interest in it. In the first of the two articles under review, Ebbinghaus describes Zermelo's later, final efforts at rehabilitating definiteness. Ebbinghaus draws heavily on previously unpublished documents from the *Nachlaß*, among them lecture notes for a course on set theory given in Freiburg during 1933–34. These documents appear in the German original in an appendix. Of special interest here is Zermelo's attempt to define sets as domains of categorically characterizable structures.

As is well known, Skolem in the 1920s effectively recasts definiteness, as a classification of properties of sets, in terms of the notion of well-formed formula, with one free variable, say, of the language of first-order set theory. This works from the point of view of foundations but has nothing whatever to do with Zermelo, as Ebbinghaus' report makes abundantly clear. Along the way, by comparing Zermelo's 1930 *Grenzzahlen und Mengenbereiche*, as published, with an unpublished first draft, Ebbinghaus manages to pinpoint Zermelo's transition to advocacy of thoroughly infinitary formulations of mathematical theories generally. (Up to this point, Zermelo used finitary second-order logic.)

It is in his 1929 Warsaw lectures that Zermelo first fielded the suggestion that sets be defined as "domains of categorical systems." He acknowledged difficulties in establishing that *Aussonderung* is satisfied in such a structure: In other words, how is it clear that an arbitrary subdomain of a categorically characterizable domain is itself categorically characterizable? After all, if the language in question is finitary, then cardinality considerations alone indicate that there will not in general be enough categorical characterizations to go around. But, in Zermelo's case, the language issue is ever indeterminate, as Ebbinghaus shows. One possibility, not explicitly mentioned by Ebbinghaus, is that Zermelo's categorical characterizations are as abstract as, and no smaller than, the mathematical structures being characterized. What Ebbinghaus does describe is a situation whereby, by the early 1930s, Zermelo's thinking about sets had come to be dominated by the issue of definability. Ultimately, Zermelo tried, unsuccessfully, to show that categorically characterizable domains constitute an inner model of the Zermelo–Fraenkel axioms. Ebbinghaus speculates that Zermelo may have had in mind something like Gödel's constructible hierarchy.

The second of Ebbinghaus' articles amounts to a commentary on seven letters to Zermelo from algebraist Reinhold Baer (1902–79) concerning the foundations of mathematics. Extensive quotations from the letters are presented in English translation, and the relevant passages appear in the original German as an appendix. Although Zermelo's half of their 1930–31 correspondence has not survived, Baer's detailed responses to Zermelo's lost letters enable Ebbinghaus to give a convincing reconstruction of the latter's very negative reactions to Skolem's proof of the Downward Löwenheim–Skolem Theorem and Gödel's recent proof of his First Incompleteness Theorem as they apply to axiomatic set theory in particular. Zermelo is clearly more exercised by Skolem's result, and Gödel's Second Theorem never comes up. What is somewhat surprising here is Baer's own clear assessments of the ideas of Skolem and Gödel and his astute questioning of Zermelo's counterproposals. Regarding Zermelo's apparent difficulties in reasoning within models of set theory, the reader will find Ebbinghaus' diagnoses instructive.

Ebbinghaus suggests that Zermelo did not, in fact, bother to check the details of Skolem's 1923 proof, and he looks to Kreisel for support. However that may be, in the end Zermelo came to regard those details as irrelevant: The likes of the so-called Skolem Paradox function, by Zermelo's lights, as a reductio of the finitary techniques figuring in that proof. Having said that, we note that Baer, in his letter of June 2, 1930, is reacting to Zermelo's flawed efforts, using just such techniques, to prove the invariance of cardinalities across models. Apparently Zermelo's later theorizing concerning foundations involved, as Ebbinghaus puts it, several "mutually exclusive approaches." It seems just possible to see in Zermelo's late work, published and unpublished, a foreshadowing of a latter-day "many logics" point of view. For one senses in his writings a conviction that logic is not *one thing*. And whereas some among his contempories—Carnap comes to mind—may have shared this view, they, in contrast to Zermelo, likely held that only foundational investigations based on finitary logic are going to pay off.

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G. Klima. *Consequences of a closed, token-based semantics: the case of John Buridan*. ***History and Philosophy of Logic***, vol. 25 no. 2 (2004), pp. 95–110.

Over the past few decades, logicians and philosophers of logic and language have been especially interested in issues such as (Liar) paradoxes, the notion of truth (in connection with the famous Tarskian T-condition), the notion of (logical) consequence (in particular since J. Etchemendy's 1990 ***The Concept of Logical Consequence***), and the behavior of proposition-tokens insofar as it is partly determined by pragmatic elements (cf. two-dimensional semantics). So, it is perhaps a pleasant surprise to the modern reader to discover that medieval philosophers and logicians were just as interested in such issues, and that their discussions are often remarkably akin to modern discussions. The paper under review focuses on how John Buridan—who lived in the first half of the fourteenth century and is one of the finest medieval logicians—tackles such issues. One of the distinctive traits of Buridan's logic and semantics is his staunch commitment to proposition-tokens—i.e., actually formed propositions—as truth-value bearers. (It must be remarked though that this view was not exclusive to Buridan, other medieval logicians such as Ockham held it too. But Buridan probably offered the most thorough analysis of its consequences). Klima's aims are more ambitious than mere historical analysis; he uses Buridan's work to make two claims, namely that "(1) Natural languages are essentially semantically closed, yet they do not have to be on that account inconsistent. (2) Natural language semantics has to be token-based, as a matter of principle" (p. 95). In order to substantiate these claims, Klima provides careful reconstructions of Buridan's theory of consequence, of his treatment of the 'Reciprocal Liar', and of his discussion of the conditions for the correct assertion of the predicate 'true' of a proposition. Given Buridan's aforementioned 'tokenality', it is evident why the analysis of his doctrines is a privileged vantage point for the defense of these claims.

Buridan's concept of consequence is in many senses remarkably similar to the current model-theoretic, modal concept of (logical) consequence: a proposition **B** is a consequence of a proposition **A** iff it is impossible for **A** to be true while **B** is false. But given his commitment to tokens, he must refine this definition in order to accommodate cases such as 'No proposition is negative': every time it is uttered, this proposition falsifies itself, and therefore vacuously satisfies the definition of antecedent for any consequent in a valid consequence, while considerations concerning the contrapositive of such a consequence show that it is absurd to claim that from 'No proposition is negative' any proposition should follow. Buridan's solution to this problem is to distinguish the context *in* which a proposition is true from the context *of* which the proposition is true (see A. Prior, *The possibly-true and the possible*, ***Mind***, vol. 78 (312) (1969), pp. 481–492), very much in the spirit of recent developments in two-dimensional semantics (cf. the entire issue of ***Philosophical Studies***, vol. 118 (1–2)). The phenomenon at stake with the puzzle of the 'Reciprocal Liar' (Plato says: 'Socrates says something true'; Socrates says: 'Plato says something false'; Robert says: 'Plato says something false' (p. 101)) is of a different nature, though, even if pragmatic elements are also involved (i.e., who is the utterer of the token 'Plato says something false'). Two-dimensional semantics cannot handle this puzzle, and in terms of modern apparatuses, Buridan's solution to this puzzle seems to resemble the most Gaifman's pointer semantics (see H. Gaifman, *Pointers to truth*, ***Journal of Philosophy*** vol. 89 (1992), pp. 223–261). Finally, Buridan's discussion of the notion of truth spins out from his treatment of liar-type paradoxes, but concerns all propositions. While the standard Tarskian analysis of the notion of truth is based on the idea that a proposition **A** is equivalent to the proposition '**A** is true', Buridan holds that every proposition *virtually implies* its own truth. Klima carefully spells out the consequences of this 'virtual implication' and its differences from the Tarskian biconditional. One of the perhaps unexpected conclusions offered by Klima is that Buridan's logic is a logic without truth, what we could call a correspondentist logic, as his revised definition of consequence (which is not based on the truth of the propositions involved) shows.

Thus, in the paper under review, Klima addresses three rather complicated aspects of Buridan's logic, while each of them on its own deserves an in-depth analysis. One might say perhaps that this paper presents only the tip of the iceberg, and that a lot more work must be done on Buridan's logic and on token-based semantics in general (a claim with which Klima would certainly wholeheartedly agree—in fact he is currently preparing a continuation to this paper, in the context of the debate with S. Read on Buridan's logic without truth—cf. footnote 22). Its ideological aim is to give compelling evidence for the thesis that natural language semantics must be semantically closed and token-based, not only for (nominalistic) metaphysical reasons, but also for purely semantic reasons. Based on Buridan's system, the author also sketches some of the logical features that a closed, token-based semantics should have, and thus the paper provides inspiration and motivation for further work on the topic. However, one must bear in mind that each of the three arguments presented actually pertain to distinct (albeit related) issues, and a careless reading of the paper might give the wrong impression that this is not the case. Finally, perhaps the most important achievement of this paper is to show (again) that the study of the history of logic can greatly contribute to modern debates, since the issues that were dealt with in different historical periods are more closely related to our own concerns than we might expect, and the solutions given by the old masters are often impressively clever and insightful.

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