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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.

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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).

PAUL HOWARD AND JEAN E. RUBIN. ***Consequences of the axiom of choice,*** Mathematical Surveys and Monographs, vol. 59. American Mathematical Society, Providence, RI, 1998, viii + 432 pp.

When a mathematician who is not a set theorist asks me a question about set theory, there is a good chance that the question will involve the axiom of choice (AC). Is AC used in such and such a proof? Can one avoid AC in this proof? Is the result actually equivalent to AC? Sometimes, such questions are quite easy to answer, but when they aren't, the book under review is the place to look. The authors have successfully completed two huge tasks: (1) They have collected information about all sorts of consequences of AC, 382 pairwise inequivalent (or at least not known to be equivalent) statements, not counting AC itself and not counting statements that turned out to be provable without AC. For many of these statements, numerous equivalent forms are given. In addition, the authors have collected the proofs used to establish implications between these statements and the models used to establish non-implications. (2) They have organized all this information in a usable format.

Among the consequences of AC that are treated in this book, there are two extreme cases—those equivalent to AC and those provable without AC. Of course, the latter class could include all theorems of vast areas of mathematics where AC plays no role. It seems that the authors have included those statements for which there was once a serious question whether AC is needed, and for which the answer turned out to be negative. A similar criterion seems to have been used for the class of statements equivalent to AC; only those statements are included

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for which there was once a genuine question whether the statement is equivalent to AC. Thus, for example, Zorn's Lemma is not listed here. For more extensive information about statements provably equivalent to AC, the appropriate source is ***Equivalents of the Axiom of Choice, II*** by Herman Rubin and Jean E. Rubin (Studies in Logic and the Foundations of Mathematics, vol. 116, North-Holland, 1985).

The main body of the book under review is in five parts. Part I lists the consequences of the axiom of choice, here called "forms," organized into equivalence classes under the relation "proved to be equivalent" (so future research might merge some of these classes). The classes are numbered in the order in which the authors encountered them, so the numbering has no mathematical significance (except that the "provable without AC" and the "equivalent to AC" classes occupy positions 0 and 1, respectively). Nevertheless, this part of the book is useful because it groups equivalent statements together. Thus, for the Boolean prime ideal theorem (form 14) one finds, on pages 21 to 27, 90 equivalent statements, drawn from logic, topology, lattice theory, Banach space theory, etc. Of course, most of the equivalence classes of statements are smaller; many are singletons. But for the more important consequences, the familiar "weak axioms of choice," one finds in Part I a useful compendium of their equivalent forms.

The implications and non-implications between the various forms are tabulated near the end of the book, in Part V, with pointers to the relevant literature and to the intervening parts of the book. This information is also available, along with search programs, in software that comes with the book.

Part II lists the same statements as Part I did, but organized according to topics. It is subdivided into the following major topics: algebra, analysis, topology, logic, choice principles, cardinality, orderings, maximal principles, measures, graphs, fixed points, and miscellaneous. Several of these are subdivided further, so that ultimately each topic occupies only a few pages and the reader can efficiently look for a specific statement. If the statement has been considered in the literature, it's almost certain to be here. If, however, you are interested in a statement that hasn't been explicitly considered before, then you may have to be alert for possible reformulations. For example, a few years ago, I wanted information about the statement "Any non-zero vector in any vector space has a non-zero image under some linear map to the field of scalars." The book contains about three pages of forms concerned with vector spaces, but this statement isn't there. In the section on fields, however, there is form 284, "A system of linear equations over a field F has a solution in F if and only if every finite sub-system has a solution in F," and one can check that this is equivalent to the statement I wanted.

Part III gives the models of set theory used to establish non-implications between the various forms. This part of the book presupposes familiarity with the standard methods for producing models of set theory without AC, namely the method of permutation models (due to Fraenkel and Mostowski) and the forcing method (due to Cohen); the necessary background is covered in, for example, ***The Axiom of Choice*** by Thomas Jech (Studies in Logic and the Foundations of Mathematics, vol. 75, North-Holland, 1973). The authors list 47 forcing models and 54 permutation models, some with several variations. For each of these, they describe the model, summarize the relevant literature, give (by numbers from Part I) a minimal list of forms known to be true in the model (i.e., enough such forms to provably imply all the others), give a full list of all the forms known to be true in the model, and similarly with "false" in place of "true."

Part IV, titled "Notes," contains information of many different sorts. The first note is concerned with open problems about implications that don't fit into the book's tables because they ask "does the conjunction of two (or more) forms imply another?" The second note lists known non-implications of this sort (with references), and Note 120 does the same for provable implications. Note 3 lists implications that hold in all permutation models (but not in all forcing models). Some of the 145 other notes give background material, particularly definitions, needed for understanding many of the forms. Since the forms come from extremely varied areas of mathematics, it is useful to have the necessary definitions here. Other notes give proofs that are not (easily) available in the literature. Others give additional references to the literature. Some are quite extensive surveys of particular topics; for example Note 15 summarizes what is known about implications between principles of choice for families of finite sets. Note 103 summarizes the results of Jech, Sochor, and Pincus about converting permutation models into forcing models (an important technique, because permutation models are generally easier to construct but forcing models give stronger independence results). The ordering of the notes follows no discernible pattern, so the way to find specific information in Part IV is to be led there by pointers from the other parts. On the other hand, the reviewer enjoyed browsing in Part IV without having any specific target in mind.

The book ends with an extensive list of references (28 pages) and subject and author indexes.

In a work of this magnitude, there are bound to be some errors; the non-trivial ones that the reviewer noticed are as follows. In form 14L, the canonical map from a space to its Stone-Čech compactification is not a surjection; it only has dense range. In the definition of "collectionwise Hausdorff" on page 268, the sets in the family J should be pairwise disjoint. In Note 20, "strong limit cardinal" is not a synonym for "inaccessible cardinal"; the former does not require the cardinal to be regular. Also, in the same note, the definition of κ-additive measure should involve unions of strictly fewer than κ sets, so that the subsequent definition of "measurable" means what it should. Two pages later, still in the same note, result 2, quoted from Apter, should not have all uncountable cardinals singular but only those $> \aleph_1$. In the table of implications in Part V, tiny errors become more important because there is no context to correct them, but they are also harder to find and correct; my only contribution in this direction is that, on page 324, the entry saying that form 7 implies itself, though true, should presumably say that form 1 implies form 7. Finally, although minor slips in a bibliography rarely make much difference, the importance of Zermelo's 1904 paper introducing AC motivates me to correct its title (on page 418); it should be *Beweis, daß jede Menge wohlgeordnet werden kann*.

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PETER PHILIPP. ***Logisch-philosophische Untersuchungen***, edited by Ingolf Max and Richard Raatsch, Perspectives in Analytical Philosophy, vol. 20. de Gruyter, Berlin, New York, 1998, xvii + 433 pp.

The editors present a collection of papers (mostly in German) of their late colleague and teacher that is divided into a larger part on deontic logic and a smaller one on Wittgenstein's philosophy.

About 30 years after the publication of G. H. von Wright's classical article "Deontic Logic" in 1951, Peter Philipp can be situated not only amongst the various subsequent approaches to the field of deontic logic, but considered as a representative of a novel conception of formal normative philosophy.

Commencing with a discussion on the ontological status of norms, Peter Philipp does not understand norms as independently existing abstract entities, but defines normative concepts as established and abolished by correctly performed acts of promulgation and elimination, expressed by speech acts.

One of Peter Philipp's main concerns in deontic logic is based on this philosophical foundation: it is to find the utmost realistic representation of the usage of normative language. He clearly favors a descriptive approach for his deontic systems over the traditional approach of Standard Deontic Logic that is constructed analogously to alethic modal logic. He argues that this prescriptive approach results in a loss of expressive capacity and in a restriction of deontic concepts to solely rational terms by which the deontic concepts are interdefinable, authorized by the axioms of the logic respectively. This procedure cannot succeed in modelling the genuine usage of normative language and therefore the logics do not entirely depict the properties of normative concepts and reasoning.

Philipp instead proposes that relations between norms are not a function or capacity of logic itself. They are introduced by a kind of normative act of promulgation by deontic authorities that are categorized into groups and levels of logical capacity. The basis for these acts of promulgation and elimination lies in the concept of explicit assertion as the definite announcement of certain facts.

This concept is adopted from Werner Stelzner's epistemic logic of assertion (cf. W. Stelzner, ***Epistemische Logik. Zur logischen Analyse von Akzeptationsformen***, Akademie Verlag, Berlin 1984). A 4-place epistemic predicate $A(x, p, t_i^j, y)$ is introduced that is interpreted as "x commits the act of explicit external assertion of p towards y in the interval t_i^j". Accordingly, it is central to Peter Philipp's systems that it is the deontic authority that assigns a normative status to a certain action.

Peter Philipp introduces groups of axioms according to the strength of the deontic authorities' logic (elementary logical, consistent, weakly consequent, strongly consequent) and continues with derivation rules that are again modified versions of the standard ones. The logics are extendable to cover the concepts of implicit and explicit, derived and derivable sets of norms of the authority's system, where the process of derivation licences the connection between the sets of norms. The language is extended to incorporate concepts of singular, general and conditional norms and the treatment of actions. The relationship between these different concepts is characterized in form of theorems that again are categorized by the logical strength of the deontic authorities.

Thus, a formal frame of meta-rules that is applicable to various concepts of derivation and normative understandings in deontic reasoning is established. It is a remarkable feature of his basic system that it is applicable to, for example $\mathcal{SDL}$, and there produces a distinction between explicit, implicit and derived norms, even under the condition that his own analysis of explicit and implicit norms is not assumed.

Naturally, he decides for a specific definition of these concepts for his own applications that are formalized in analogy to Anderson's reductive terms of deontic logic. Thus, in Peter Philipp's system we are dealing with expressions such as $P^e(x^m, K, y^m, t^k, t^l) =_{\text{def}} B^e(x^m, M(K \wedge I(y^m)), t^k, t^l)$, where x^m is the deontic authority, y^m is the addressee and K is the object that is explicitly allowed, which is defined by the explicit assertion of the possibility of K without the addressee's punishment I in the interval defined by t^k and t^l.

Finally, one aspect of Philipp's systems shall be mentioned that is exceptional in the literature on deontic logic. His hierarchy of logical agents allows to model different kinds of inconsistencies in their range of reasoning—although the question whether there are agents of the respective types or not remains open. The system itself is an open system. Consequently, incompleteness and inconsistency of normative reasoning can be described. This provides a realistic model for practical application of the actually inconsistently and incompletely experienced normative systems. Yet, depending on the degree of logical strength of the deontic authority, the relations that are proposed between the normative terms of obligation, prohibition and permission and that are added to the logical base of the specific system, can eventually be logically interdependent again (under the condition of the strongest logic which converges with the rational approaches) and the system closed.

Normative systems may themselves be inconsistent and incomplete within deontic agents or larger structures—this is notably considered in the language of Peter Philipp's refined deontic meta-system, in which the setting and elimination of norms and their specific relations are depended on the deontic authorities' basic logical strength, and can itself be given a consistent description.

The Wittgenstein part contains an extended introduction of Philipp's 1990 edition of Wittgenstein's *Tractatus* and *Untersuchungen* (L. Wittgenstein, ***Tractatus logico-philosophicus. Philosophische Untersuchungen***, edited by Peter Philipp, Reclam, Leipzig 1990). One of its main goals consists in defending Wittgenstein against criticisms based on attacks against conceptions trying to monopolize his philosophy. In a more specialized paper, Philipp critically examines the Hintikkas' interpretation of the *Private Language Argument* (Hintikka, M. B. and J., ***Investigating Wittgenstein***, Basil Blackwell, Oxford 1986). According to Philipp, this position sums up to "although it is true that every person knows just his own sensations, another person is able to establish the referential relation between the expressions this person uses and the sensations of this person, and, therefore, understands this language"—and this is wrong. Unfortunately, he had no opportunity to discuss a positive argument for his own position.

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Peter T. Johnstone. ***Sketches of an elephant: a topos theory compendium.*** Oxford Logic Guides, vols. 43, 44. Oxford University Press, Oxford, 2002, xxii + 1160 pp.

The whimsically modest title "*Sketches of an Elephant*" (you know, the four blind men who think the elephant is like a snake, a tree trunk, etc.) fails to do justice to the ambitious scope of this project or its masterful execution. It could easily have been called "*Principia Toposophica*," perhaps reflecting its role in the logic of the new century.

Roughly speaking, the notion of a topos is to logic in general what a boolean algebra is to propositional logic, but this needs to be qualified in several ways. The original conception, due to A. Grothendieck, was mainly to be a vehicle for cohomology (see Allyn Jackson, *Comme appelé du néant—As if summoned from the void: The life of Alexandre Grothendieck*, ***Notices of the American Mathematical Society***, vol. 51, 2004). Logically, such a "Grothendieck topos" is something like a universe of continuously variable sets. Before long, however, F. W. Lawvere and M. Tierney provided an elementary axiomatization more closely related to higher-order logic. These two aspects continue to be present, among several others referred to in the "elephant" image. Compared to traditional algebraic logic, however, the "algebraic" treatment of logic pursued in topos theory is closer in spirit to that found in "algebraic" topology, where the use of functorial methods is taken for granted. In topos theory, moreover, not only are such methods brought to bear on logic, but conversely, the topos approach permits the application of logic in a range of situations in topology, geometry and algebra — extending Grothendieck's original conception from cohomology.

This scope of application, deriving from the generality of its methods, is what makes topos theory "foundational," in a sense that is neither the deductive-axiomatic one of Frege and Russell, nor entirely the descriptive-axiomatic one of Eilenberg & Steenrod. Be that as it may, it is clear that the book fits into the Oxford Logic Guides, and should be of great interest to readers of this Bulletin. The purpose of this review, then, is to provide a field guide to this strange new territory, which may in places seem rather forbidding to the newcomer.

First, as to the nature of the beast, this pachyderm is foremost a compendium of the main results of research in the field since its Bourbakian beginnings in the 1960s, refined, related and organized into a body of theory that displays the maturity of the discipline as it now stands. No such corpus would have been possible even twenty years ago (before e.g., the work of A. Joyal and M. Tierney, *An extension of the Galois theory of Grothendieck*, ***Memoirs of the American Mathematical Society***, vol. 309, 1984), and as useful as was the author's early textbook ***Topos theory*** (Academic Press, 1977), the difference between it and this book displays how much the field has ripened (but it also reflects the fact that the *Elephant* is not intended as a textbook). Although it includes some results not previously published (or hard to find), as well as many improved proofs of known results, the strength and value of the work really lies in its encyclopedic character and remarkable success in organizing and interrelating this amount of quite technical material into a coherent whole, with uniform notation, terminology, prerequisites and depth of rigor. Moreover, the high degree of deductive coherence, shared definitions and lemmas, etc., that are possible in such a book make it far more useful (and readable) than the sum of the individual research papers from which its results are mainly drawn.

The actual content is not restricted to topos theory proper, but also covers a range of results and methods from category theory that are related to toposes, and are employed in topos theory to one degree or another. This course of action not only serves to make the book largely self-contained, but contributes to the uniformity of notation and terminology and systematic organization as well. It also makes the book valuable as a resource for those related fields, such as general categorical logic; indexed, fibered, and internal category theory; allegories; locale theory; and several others.

The two volumes are divided into four main parts, two in each, corresponding roughly to different aspects of the topos concept, as follows:

A. Toposes as categories
B. 2-categorical aspects of topos theory
C. Toposes as spaces
D. Toposes as theories

The contents of the projected third volume are indicated to be:

E. Homotopy and cohomology
F. Toposes as mathematical universes.

Each of these parts falls into several chapters and sections, the general contents of which we will now survey individually.

A. Toposes as categories. A topos is by definition a cartesian closed category with a subobject classifier. To the logician, cartesian closure essentially means that the category is a model of the lambda-calculus, while the subobject classifier adds in a type of "propositions" or "truth-values". In the presence of equality relations for each type X (provided by the diagonal arrow $X \rightarrowtail X \times X$), this combination of resources permits not only the definition of the usual logical operations like negation, conjunction, and quantification, but also the formation of terms of all higher (finite) types, such as functionals and relational quantifiers. The notion of a topos thus corresponds logically to that of a system of higher-order predicate logic; but let us immediately ward off a possible misconception: topos theory is not chiefly concerned with the use of such systems of logic, in some form or other, to deduce fragments of mathematics, the way the logician might expect. Rather, it is occupied more with the study of the great variety of such toposes as they occur in nature, as it were, and their interrelations. Thus the perspective is closer to the way that group theory is about groups than the way analysis is about the real numbers.

For instance, the category $\mathcal{S}$ of all sets and functions is a topos, as is the category $\mathcal{S}_f$ of finite sets, but so also are the product category $\mathcal{S} \times \mathcal{S}_f$ and the functor category $\mathcal{S}^{\mathcal{S}_f}$. Indeed, the essentially algebraic character of the topos concept makes it closed under many important operations for constructing new objects out of old ones. An important class of examples is provided by the categories $\mathrm{Sh}(X)$ of all sheaves on a topological space X. From a logical point of view, such a topos can be regarded as consisting of sets varying continuously over the space X. Another kind of topos is the category $\mathcal{S}^G$ of sets A equipped with an action $A \times G \to A$ by a group G, and all equivariant functions; these can be seen as sets that in some sense "respect" the group structure. There are many more kinds of toposes, including ones constructed from various deductive logical systems, and ones determined by different notions of computability. Part A provides the basic definitions and examples, together with some of the most important general facts about the concepts involved. There is also a useful survey of P. Freyd's theory of "allegories", an abstraction of categories of relations which sheds light on the topos concept from a different angle.

The mappings of greatest interest in topos theory are the "geometric morphisms" between toposes, which arise quite naturally in a range of circumstances. If X and Y are topological spaces, for instance, each continuous function $X \to Y$ induces an associated geometric morphism $\mathrm{Sh}(X) \to \mathrm{Sh}(Y)$; similarly, a homomorphism of groups $G \to H$ gives rise to a geometric morphism of toposes $\mathcal{S}^G \to \mathcal{S}^H$. But the general notion of a topos then also makes possible geometric morphisms of the form $\mathrm{Sh}(X) \to \mathcal{S}^G$, relating a space X and a group G, along with many others relating logical systems, computation, topology, and algebra. In logic, set-theoretic forcing, boolean-valued and permutation models, and Kripke semantics can also all be described, related, and combined using toposes and geometric morphisms. The basic theory of geometric morphisms including some fundamental factorization theorems (akin to the Noether homomorphism theorem for groups) concludes Part A.

B. 2-categorical aspects of topos theory. This is the most technical part of the book, and will likely be the most difficult for the logical reader to appreciate. It consists of results on indexed, fibered, and internal categories that are motivated mainly by their use elsewhere in the theory. This is not to say that the content is of no logical interest, but only that its interest will be more apparent in its applications. The important notion of an internal category in a category, for instance, is related to the idea that a logical system can be formalized as an axiomatic theory in a (meta-)language, i.e., another logical system. This makes it possible to describe the interpretations of a "theory" represented by an internal category $\mathbb{C}$ in the "meta-language" category (perhaps a topos) $\mathcal{S}$ as functors of the form $\mathbb{C} \to \mathcal{S}$, so that the category of all models of the "theory" is a certain subcategory of the functor category $\mathcal{S}^{\mathbb{C}}$ (then also a topos). Such "functorial semantics" (developed in Part D) requires some of the notions developed here. Accordingly, the general reader is advised to refer to results in this part of the book as required for the comprehension of other parts.

The specialist, on the other hand, will welcome the systematic and authoritative presentation of these tools, the development of which was scattered across the journals. In this presentation, for instance, the theory of the 2-category $\mathfrak{BTop}/\mathcal{S}$ of toposes bounded over an arbitrary base topos $\mathcal{S}$ now seems to be quite mature and satisfactory.

C. Toposes as spaces. After the spade-work of the previous two Parts, this volume begins to develop the real fruits of the theory. According to the original conception of Grothendieck and his followers, a topos is a generalized topological space, and the category of toposes and geometric morphisms can replace spaces and continuous mappings for many purposes, thereby extending topological and geometric ideas to algebraic and other situations that give rise to toposes. Accordingly, we first consider the relation between classical topological concepts and toposes.

In passing from a space X to the topos $\mathrm{Sh}(X)$ of all sheaves of sets on X, one needs only the poset $\mathcal{O}(X)$ of open sets of X, not the point set. Specifically, a sheaf F on X is a contravariant functor on $\mathcal{O}(X)$, thus taking each pair of open subsets $U \subseteq V$ to a function between sets $FV \to FU$, satisfying the expected condition on compositions for $V \subseteq W$. To be a sheaf, such a functor must satisfy a "patching condition" for open covers $U = \bigcup_i U_i$ that essentially says it is "continuous," in the sense that FU is determined by the values FU_i. A classical example is the sheaf of continuous, real-valued functions $FU = \mathcal{C}(U, \mathbb{R})$, for which $U \subseteq V$ acts on any $g\colon V \to \mathbb{R}$ just by restriction to U. Here it is clear that given an open cover $U = \bigcup_i U_i$, there is a unique continuous function $f\colon U \to \mathbb{R}$ for every family of such $f_i\colon U_i \to \mathbb{R}$, provided only that all f_i and f_j agree on every $U_i \cap U_j$. What is required here is just the lattice structure of $\mathcal{O}(X)$; thus one also has toposes built from complete Heyting algebras or "frames", which have (many of) the same lattice-theoretic properties as such posets $\mathcal{O}(X)$. A frame can be regarded as a "pointless space," and they share many properties with classical spaces, while encompassing also many other kinds of posets like boolean algebras, Scott domains and Lindenbaum algebras of logical theories (suitably completed). There is an intimate connection between toposes and such "pointless spaces"; logically, boolean-valued models of set theory arise as toposes of this kind.

In this scheme of "pointless topology" a space is determined by a system of open subspace inclusions instead of by a set of points. One can further generalize to "spaces" determined by "covering systems" of maps in a category. There is an associated natural generalization of the notion of a sheaf for such generalized covering families, and the topos of all such sheaves then plays the role of the "space" determined by the system of covering families. An early example of this idea was in algebraic geometry, where covering families of étale maps of schemes were considered, but Cohen forcing provides another example, as do Beth models of intuitionistic logic.

Regarded thus as generalized spaces, toposes and their mappings enjoy many familiar properties of classical spaces. Open maps, connectedness, compactness, and similar concepts apply, and behave in ways analogous to their classical counterparts. In some cases, classical theorems for spaces follow from, and are subsumed under, much more general ones about toposes, which reveal aspects that were not evident in the topological setting. This is the case for the theory of open and proper maps. The appearance of classifying toposes in the treatment of exponentiability reveals connections between logic and topology that are also hinted at in the relation between exponentiability, compactness and definability.

The descent theorems and groupoid representations are a bold extension of Galois theory to toposes, with connections to logical definability and invariance. Beth's definability theorem from elementary model theory is actually a miniature version of some of the results developed here (in this connection see M. Makkai, *Duality and definability in first-order logic*, ***Memoirs of the American Mathematical Society***, vol. 503, 1993).

D. Toposes as theories. This final part (to date) is of course the most overtly "logical" of the four, but also connects the logical aspect to the geometrical and algebraic ones.

The basic propositional operations of conjunction, disjunction, negation, etc. of course have an order-theoretic, algebraic meaning, which has long been known and studied, but it was not until the pioneering work of F. W. Lawvere in the 1960s (see, e.g., *Adjointness in foundations*, ***Dialectica***, vol. 23, 1969, pp. 281–296) that it was known how to extend this treatment to full first-order logic. Lawvere's discovery that *all* of the logical operations are instances of adjointness made it possible for the first time to give a truly algebraic treatment of logic, since adjoint functors are "algebraic" in a precise sense. The following two-way rule of inference for the existential quantifier, for instance,

$$\frac{\varphi(x, y) \vdash \psi(y)}{\exists x.\varphi(x, y) \vdash \psi(y)}$$

says that the existential quantification of x is left adjoint to adding a dummy-variable x, in a calculus of entailments $\vartheta(x, \dots, y) \vdash \vartheta'(x, \dots, y)$ between formulas with a common "context of variables" (a list $(x, \dots, y)$ of which variables may occur freely). Semantically, this is just the fact that taking direct images $f(U)$ of subsets $U \subseteq X$ along a function $f : X \to Y$ is left adjoint to taking inverse images $f^{-1}(V)$ of subsets $V \subseteq Y$,

$$U \subseteq f^{-1}(V) \quad \text{iff} \quad f(U) \subseteq V.$$

Given the resulting description of a first-order logical theory $\mathcal{T}$ as an algebra of a certain kind (a suitably structured category), a model M of $\mathcal{T}$ in another such algebra $\mathcal{S}$ then becomes simply a homomorphism $M : \mathcal{T} \to \mathcal{S}$. If $\mathcal{S}$ is e.g., the category of all sets, then the category

$\mathrm{Hom}_{\mathrm{Alg}}(\mathcal{T}, \mathcal{S})$ of all such algebra homomorphisms is essentially the same as the category of all classical $\mathcal{T}$-models (the notion of natural transformation of functors usually gives the right definition of a homomomorphism *between* $\mathcal{T}$-models). In this framework of "functorial semantics," the sorts of theories usually studied in logic occur as the "free algebras", and more generally, the algebras presented by generators and relations corresponding to the basic non-logical constants of the language and the axioms of the theory.

As in the theory of classifying spaces in topology, every logical theory has a "classifying topos", the geometric morphisms into which correspond uniquely (up to isomorphism) to models of the theory. This remarkable fact establishes a fundamental connection between functorial semantics, classifying spaces in topology, and the theory of algebraic extensions, that cuts three ways, as it were. It makes it possible to import logical methods into algebra and topology, and conversely, in ways that are only beginning to be exploited (see, e.g., I. Moerdijk, ***Classifying spaces and classifying topoi***, Lecture Notes in Mathematics, vol. 1616, Springer-Verlag, 1995).

One issue that deserves mention here is the appearance of intuitionistic logic in connection with categorical semantics and toposes. It appears, not for any philosophical reasons, but for practical, mathematical ones, related to the effect of continuous variation and the "internalization" of logic. That intuitionism should thus reappear is a surprising and fascinating mathematical development which should be of especially great interest to logicians, who can thereby relate their methods and results to naturally occurring phenomena in other branches of mathematics.

The higher-order and type-theoretic structure of toposes results, as already mentioned, from cartesian closure, which is the algebraic basis underlying the so-called "Curry-Howard Isomorphism" familiar to logicians. Toposes provide convenient models for systems of higher-order logic, as well as various type theories like the typed lambda-calculus and the dependent type-theory of Martin-Löf. Moreover, these theories are actually *deductively complete* with respect to topos models (Gödel-incompleteness not withstanding!).

But perhaps even more importantly, the usual systems of type theory and higher-order logic are, of course, also *sound* with respect to topos semantics, permitting the use of such theories to reason about objects and structures in any toposes. It can thus be of real practical benefit to, say, a geometer using toposes for some geometric purpose to have at his disposal the logical calculus of dependent type theory or higher-order logic. This sort of approach has been pursued in some parts of algebraic topology and geometry, and more systematically in what is called "synthetic" differential geometry. Some of these topics will presumably be addressed in Volume Three, which should be of particular interest to readers of this BULLETIN.

Throughout, the exposition is rigorous without being formal; it is also respectful and pleasantly literary. The style overall is more Halmos than Lang, and it meets the standard of clarity set by the author's lucid ***Stone spaces*** (Cambridge University Press, 1982). Definitions and proofs are transparent, and diagrams are used effectively, if not profusely, in the contemporary style (as opposed to the more diagrammatic fashion evident in some older category theory). The mathematical prerequisites are moderately high (this is no textbook), but appropriate to the intended audience of research logicians and mathematicians who are not specialists in category theory; Mac Lane's ***Categories for the working mathematician*** (Springer-Verlag, 1971) surely suffices. While not intended to document the historical developments or priority in results, the suggestions for further reading provided in each section form a useful guide to the literature, and the bibliography is the most comprehensive one currently available in the field.

With the completion of this mammoth undertaking, Professor Johnstone will have performed an invaluable service to the topos theory community by bringing in the harvest of their collective labors over the last four decades. May Logic reap some of the fruits.

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Relevant logics and their rivals, Volume II, A continuation of the work of Richard Sylvan, Robert Meyer, Val Plumwood and Ross Brady, edited by Ross Brady, with contributions by Martin Bunder, André Fuhrmann, Andréa Loparić, Edwin Mares, Chris Mortensen and Alasdair Urquhart. Western Philosophy Series, vol. 59. Aldershot, Ashgate, 2003, xiv + 425 pp.

When volume 1 of ***Relevant Logics and their Rivals*** (RLR1) appeared in 1982, it was confidently expected that volume 2 would not be far behind: much of the material intended for the second volume was already written and the decision to publish in two volumes was made only on account of the bulk of the manuscript. (A third volume on quantified relevant logics was sometimes mooted, but this was always a doubtful part of the project since very little material for it had actually been written.) It is not exactly clear what prevented the quick appearance of the second volume. Sylvan, who was the main driving force behind the book, was always working on several large projects at the same time, and inevitably each task took time away from all the others. After a few years' delay, of course, the task of bringing the material up-to-date had become substantial, and grew steadily greater with time. As Ross Brady reports in his Prologue to volume 2, at various times Sylvan attempted to restart the project, most notably in 1988–9 when he started to revise the work in line with more recent thinking, typically not by rewriting the extant text, but by writing a series of new short pieces which, as Brady says, were hard to connect with the existing material. Sylvan's sudden death in 1996 meant that, if the second volume was ever to appear, another driving force would have to be found. That turned out to be Brady, one of the four coauthors of RLR1.

Brady was faced with a difficult dilemma: either to try and complete the text as originally intended or to bring it up-to-date. Wisely, he chose the second option, producing a work which, while it incorporates a good deal of the ground-breaking work of the 1970s and early 1980s, includes much new material as well, with new contributions by Martin Bunder, Ed Mares, André Fuhrmann, Chris Mortensen, Alasdair Urquhart, and Brady himself. Brady is the primary author of the last three chapters: two on recent developments primarily in sentential relevant logic, and the third on quantified relevant logic, thus going some way to making good on the promise of a third volume devoted to this topic. These chapters make up over 150 pages of the text. They begin (pp. 192–4) with a very useful list of axioms, rules and meta-rules in terms of which he defines the main systems discussed in the new material. Although the nomenclature in these three chapters differs somewhat from that used in the earlier material (and in RLR1), readers, who are otherwise likely to be overwhelmed by the sheer variety of systems discussed, will do well to familiarize themselves with the list at the outset to keep their bearings, especially if they don't have RLR1 to hand. The present volume, for example, opens by developing variants of the unstated 'system B' (for 'basic'). Readers may remember this as the 'minimal logic' from vol. 2 of Anderson and Belnap's ***Entailment*** (pp. 173, 213ff), but otherwise will have to find it in RLR1, pp. 287–8 if they have not looked at Brady's list. In addition to writing most of the final three chapters, Brady has silently updated and corrected the earlier material, as well as performing the usual chores of an editor. This is a very large labour, admirably carried out, and the relevant logic community has every reason to be grateful to him.

In RLR1, the first two chapters set out, with some degree of polemic, (some of) the competing relevant and non-relevant treatments of implication and deducibility, together with criteria for deciding between them. The first half of the third chapter sets up the syntax and semantics of the relatively straight-forward first degree theory, FD, and then moves on, around the middle of the book, to consider higher degree systems. The final two chapters of the first volume deal with the worlds semantics of relevant conditional logics with affixing, that is where the conditional satisfies the rule $A \to B, C \to D \Rightarrow B \to C \to .A \to D$. Alternative semantics for affixing systems are developed in two central chapters of the present volume: Chapter 8 (by Sylvan and Plumwood) on operational semantics, and Chapter 9 (by Sylvan, Meyer, Brady, Mortensen, and Plumwood) on algebraic semantics.

RLR1 was already notably general in its approach to conditionals, but the thrust of much of RLR2 is to generalize still further. For example, non-affixing systems are discussed in Chapter 10, *The More General Semantical Theory of Implication and Conditionality* (by Sylvan, Loparić, and Plumwood). Here the authors revert to the (to this author at least) more familiar worlds semantics. The chapter also includes a short but very useful discussion on relevant theories of counterfactual conditionals by Mares and Fuhrmann. The second volume generalizes also in the types of operators and connectives employed in conjunction with the conditional. Through the first volume, for example, the emphasis is on systems with normal negation, that is with negations which satisfy a full set of contraposition principles as well as the full double negation principle, $A \leftrightarrow \sim\sim A$. Volume 2, on the other hand, begins with a chapter by Sylvan and Plumwood (Chapter 6, notably short by the standards of these authors) on relevant systems with non-normal negations. The approach, as in volume 1, is semantical: To begin with, the rule for evaluating negations

$$I(\sim A, a) = T \text{ iff } I(A, a^*) = F$$

is kept but the conditions on the Routley star are relaxed; after that, the evaluation rule itself is changed. As happens frequently in this volume, the changes are rung on the basic system B, and in this case the results are ranges of relevant minimal- and intuitionist-style logics. These results are presented only in the most general terms, and as Sylvan and Plumwood note, 'Most of the galaxy of systems semantically comprehended [here] are unknown or little studied' (p. 12). In Chapter 7 (by Sylvan, Meyer, and Plumwood) a second type of negation is added to the system, supplementing the Routley star in the semantics with a Routley dagger. The purpose here is to capture in relevant logic a form of 'internal negation', in which both A and its negation can be true, which Routley had already found useful in his investigations of Meinong's theory of objects. Chapter 7 also considers the addition of different types of intensional functors, yielding relevant deontic, alethic, tense, and imperative logics. The chapter ends with a presentation, compressed into little under a page, of *A General Relevant Semantical Theory for n-place Connectives* (p. 36).

Like the range of minimal and intuitionistic systems, the galaxy of systems addressed in the two volumes of RLR is daunting, to put it mildly. In this respect the transition from the first-degree system, FD, to higher degree systems in the middle of the first volume is something of a turning point in the book as a whole. For, according to RLR1, FD is 'without significant relevant rivals', whereas at higher degrees we face 'an embarrassment of relevant options' (p. 170). However, as Brady points out in Chapter 12, even this unanimity at the first degree fails if we start, as is done in RLR2, from the basic system B. For while FD contains classical logic, B contains no classical tautologies and thus has a first degree fragment, which Brady calls L1, which is weaker than FD (pp. 288–302). This illustrates a general difference of approach between the two volumes. RLR1 begins classically and considers modifications to deal with failures of relevance, arriving thus at FD and, shortly after, the Anderson-Belnap systems, E and R. These systems result from the attempt to save as much modal logic as possible, 'to maximize power in the classical mode' (RLR1, p. 231), consistent with avoiding the paradoxes of strict implication. This approach is criticized in RLR1 for mistaking the symptom, the paradoxes of strict implication, for the underlying disease, which is the principle of suppression which allows logically true premisses to be suppressed in an argument. In RLR2 the general approach is quite the other way around. It is to start with the weak system B and to build up power by adding principles to it. This results in a huge explosion in the number of systems which come up for consideration.

That there are more relevant systems to consider than modal systems is an unavoidable fact of life, resulting from the fact that relevant logics recognize distinctions that modal logics obliterate. Moreover, as his pluralism developed, Sylvan's propensity for being embarrassed by choices weakened over time. Nonetheless, the sheer range of systems which appear in these pages is likely to prove bewildering to the reader; a clear classification system would be helpful. A number of important distinctions are made at various places in the two volumes. One is the distinction already mentioned between affixing and non-affixing systems, another is the distinction (RLR1, pp. 224–5; RLR2, pp. 151–5) between replacement systems (which allow substitutivity of co-implicants) and non-replacement systems; and there are several others. It is difficult to know which of these distinctions is more fundamental than the others or how they intersect to produce cross-classifications. One distinction that Sylvan set much store by is the distinction between deep relevant logics and the others. This distinction is mentioned in RLR1 (p. 126; the word 'deep' being apparently a late addition to the text), but it is left to Brady in RLR2 (pp. 195–8) to develop the notion properly. The standard relevance condition, met by the Anderson-Belnap logics, requires that if $A \rightarrow B$ is a theorem, A and B share a sentential variable. The depth relevance condition, met by deep relevant logics, requires that if $A \rightarrow B$ is a theorem A and B share a sentential variable at the same depth, where the depth of a variable occurring in a formula depends upon the number of nested $\rightarrow$'s between it at the whole formula (RLR2, p. 195).

Brady's concluding three chapters, which make up a substantial portion of the whole volume, do an admirable job in bringing the volume up-to-date by covering, if only sometimes in a summary manner, the major development since the bulk of the manuscript was originally written. In addition to the chapter on quantified relevant logic already mentioned, Brady covers the Gentzenization of relevant logics, content semantics, Routley and Priest's simplified version of the Routley-Meyer semantics, semantic decision procedures, rules and the Deduction Theorem in relevant logics, and several other topics. In addition, there are sections by Mortensen on closed set logic, Bunder on combinatory logic, and Urquhart on (un)decidability results and the interpolation problem. As an added bonus, the book includes as an Appendix the now-legendary Meyer-Routley monograph, *Extensional Reduction II*, written in 1975 and previously unpublished. (Part I was published in ***The Monist*** (1970), vol. 60, pp. 355–69). Sad to say, however, this work is still not available in its entirety, for space limitations required the excision of several sections.

RLR1 and RLR2 must surely constitute the most exhaustive study of implication undertaken to date. Together they form the most comprehensive survey available of the past three decades' work on relevant logic. The range and complexity of the material is breathtaking. As one who has worked with Sylvan's ***Nachlass***, I can testify to the difficulty of the task Brady undertook. He has carried it out with exemplary skill and the relevant logic community (and many others) will long be in his debt.

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The Cambridge companion to Bertrand Russell, edited by Nicholas Griffin, Cambridge University Press, Cambridge, UK and New York, USA, 2003, xvii + 550 pp.—therein:

IVOR GRATTAN-GUINNESS. *Mathematics in and behind Russell's logicism, and its reception*. Pp. 51–83.

RICHARD CARTWRIGHT. *Russell and Moore: 1895–1905*. Pp. 108–127.

PETER HYLTON. *The theory of descriptions*. Pp. 202–240.

MARTIN GODWYN AND ANDREW D. IRVINE. *Bertrand Russell's logicism*. Pp. 171–201.

MICHAEL BEANEY. *Russell and Frege*. Pp. 128–170.

GREGORY LANDINI. *Russell's substitutional theory*. Pp. 241–285.

ALASDAIR URQUHART. *The theory of types*. Pp. 286–309.

The above papers have been selected from ***The Cambridge companion to Bertrand Russell*** as those likely to be of greatest interest to readers of this journal. Together they cover the logically most productive period of Russell's life, which may be taken to run from 1900 to 1910. The major events of this period include: the discovery of the paradox (May 1901), the publication of ***Principles of mathematics*** (May 1903), the discovery of the theory of descriptions (June 1905), the discovery of the ramified theory of types (1907), and the completion (with Whitehead) of ***Principia mathematica*** (1910). There is a wealth of material from this period that remained unpublished at the time but is becoming available in ***The collected papers of Bertrand Russell***, the relevant volumes being ***Toward the "Principles of mathematics", 1900–02***, edited by Gregory H. Moore, London: Routledge, 1993, ***Foundations of logic, 1903–05***, edited by Alasdair Urquhart with the assistance of Albert C. Lewis, London: Routledge, 1994, and ***Toward "Principia mathematica", 1906–08***, which has yet to be published. The papers under review provide a useful guide to this important chapter of Russell's career, in many cases making use of formerly unpublished material to shed light on Russell's logical development. The papers may be grouped as follows: The first sets the historical stage and provides an overview of Russell's work in logic while the remaining pairs deal respectively with the theory of denoting, the nature of logicism, and the resolution of the antinomies.

Historical. Grattan-Guinness locates the work of Whitehead and Russell at the end of a long tradition of rigorization and reduction in mathematics. This tradition began with a rigorous account of the notions of limit, continuity, and derivative (by Bolzano, Cauchy, Weierstrass and others) and ultimately led to the reduction (via informal set theory) of the reals to the rationals and the rationals to the natural numbers (by Dedekind, Cantor and others). Dedekind and Peano then gave a rigorous account of the natural numbers and Cantor both developed set theory into an independent discipline and extended it into the transfinite. Whitehead and Russell brought all of this material into a single general framework, isolating several axioms of set theory (for example, infinity and choice) and pressed the reduction further with the aim of showing that ultimately even the arithmetical notions could be reduced to set theoretic notions and that these in turn could be reduced to logical notions.

The theory of denoting. Russell's conception of logic is richer than the modern conception both in terms of the principles it embraces and the kinds of entities these principles involve. During the time of the ***Principles*** logic involved the extensional notion of class and the intensional notions of proposition, propositional function and denoting concept. These four notions play a central role in what follows and given that Russell's conception of the three intensional notions is not widely known it will be useful to start with a brief account, following the excellent exposition of Cartwright.

Consider the proposition *Socrates is human*, the propositional function *x is human* and the denoting concept *the master of Plato*. The proposition is taken to be a structured entity that contains as constituents both Socrates and the concept *human*. The occurrence of these constituents, however, differs in that the proposition is *about* the former but not the latter. For this reason Socrates is said to *occur as a term* in the proposition. A *term* simpliciter is then defined to be anything that can occur as a term in some proposition. The propositional function *x is human* may be described as what remains of the proposition when one removes Socrates or as the common component of *Socrates is human*, *Plato is human*, and all such variants. The denoting concept *the master of Plato* is a structured entity which has the characteristic feature that when one substitutes it for Socrates in the above proposition the resulting proposition is not about the concept but rather about what the concept *denotes*, namely, Socrates. In addition to such *definite* denoting concepts (that is, denoting concepts that unambiguously denote a single entity) there are *indefinite* denoting concepts such as *a man* that denote one of many entities. The most general denoting concept, *any*, is referred to by Russell as 'the variable'. (Note that Russell is using both 'term' and 'variable' in a non-linguistic sense.)

There is a certain tension in the ***Principles*** concerning terms and denoting concepts. In a famous passage (quoted by both Cartwright and Hylton), Russell argues that if A is a term then the proposition *A is not* must be either false or meaningless: "For if A were nothing, it could not be said not to be; "A is not" implies that there is a term A whose being is denied, and hence A is" (***Principles*** p. 449). If one takes as A the denoting concept *the present king of France* then this argument seems to show that the present king of France has being. Although Russell does not discuss the present king of France in the ***Principles*** he does discuss chimeras and, in the continuation of the above quotation, he does indeed conclude that chimeras have being. Cartwright and Hylton (and others) claim that in the ***Principles*** Russell did not see that his theory of denoting concepts enabled him to escape this conclusion. This, however, is questionable. The ***Principles*** is a patchwork of various papers and drafts written during different periods and the above passage is a relic from a paper written by 21 June 1900, long before Russell came to investigate the notion of denoting. One should therefore turn to more recent passages for a fuller picture. Once the theory of denoting enters, the above argument disappears and in §47 (written May 1901) Russell retreats to the innocuous claim that anything that can be *mentioned* has being. In §73 (written May 1902) Russell discusses denoting concepts that do not denote. He rejects the proposition *chimeras are animals* on the ground that if legitimate it would be about nothing (since the denoting concept *chimeras* does not denote) but he retains the proposition *for all x, if x is a chimera then x is an animal*. On similar grounds it would seem that he would reject the proposition *the present king of France is not* while retaining *there is not an x such that x is the present king of France*. In this way one can trace the emerging influence of the theory of denoting in the ***Principles*** and in the end see that the machinery for escaping the alleged conclusion is fairly explicitly at work.

The theory of denoting was further developed by Russell during the years 1903–1905 and culminated in "On Denoting", ***Mind***, vol. 14 (1905), pp. 479–93, one of the most famous papers of twentieth-century philosophy. This is the paper in which Russell contextually defines away descriptive phrases, thereby eliminating all denoting concepts (with the exception of the variable) from among his intensional primitives. This technique is now standard in logic and is nicely explained in Hylton's paper. But "On Denoting" has remained enigmatic in other respects and commentators have debated such questions as: Is Frege the true target of Russell's criticisms? How is one to make sense of the famous Gray's Elegy passage? What is the true reason for the new theory? What is the alleged connection with the resolution of the antinomies? Fortunately, with the appearance of ***Foundations of logic, 1903–1905***, we now have much more information about Russell's views at the time. Here one finds well over 100 pages of material—much of it first-rate—in which Russell develops the theory of denoting up to and including the point where he discovers the new theory. It is unfortunate that Hylton does not discuss this material since it sheds a great deal of light on the above and other questions. For example, consider the question of the reasons for the new theory. In the unpublished manuscripts one sees that Russell's old theory of denoting had something to say about each of the three puzzles listed in "On Denoting" as test cases for any theory of denoting. This leads to the question of why the new theory was adopted in place of the old. The manuscripts tell the story. A note on the first leaf of the manuscript "On Fundamentals" reads "Pp. 18ff. contain the reasons for the new theory of denoting". Here one finds that in developing the theory (in light of the puzzles) Russell is forced to distinguish various modes of occurrence and the theory becomes overly complicated. He then gives an argument—an illuminating version of the Gray's Elegy argument—to the effect that by the lights of the theory it is impossible to express the theory. Since the difficulties have to do with making statements *about* denoting concepts, Russell takes the course of eliminating denoting concepts while preserving the function they were introduced to perform. He is thereby led to the new theory. The new theory is then applied to give a clean solution to one of the puzzles and to eliminate classes. Thus, on the face of it, the reasons in favour of the new theory appear to be (i) that it is simpler, (ii) that it provides a cleaner treatment of the puzzles, (iii) that it avoids what Russell saw as the internal collapse of the old theory, and (iv) that it has fruitful consequences, in particular, the elimination of the classes, which were suspect because of the paradox. More, of course, needs to be said on the subject. It is to be regretted that although this material has been available now in published form for over ten years it has barely entered the literature.

The nature of logicism. The paper of Beaney contains a useful comparison of the logicism of Frege with that of Russell. The emphasis, however, is on Frege. Godwyn and Irvine concentrate on Russell's logicism but rely heavily on later, more popular expositions. It would have been nice to see a more mathematically informed treatment of logicism and its limitations. For example, both papers give an oversimplified presentation of Frege's theory of number and neither paper mentions Myhill's result that if the controversial axiom of reducibility is removed from the system of ***Principia*** then the resulting system cannot define the natural numbers in such a way that all instances of induction are provable. The paper of Godwyn and Irvine also contains a number of errors. For example, in the course of discussing the distinction between a law of thought in the psychological and the logical sense the authors say that "Frege seems willing to admit that 'it is impossible to effect any sharp separation between the two' ". This is a striking attribution since Frege is well-known to maintain precisely such a sharp separation. When one looks up the cited attribution one finds that by "the two" Frege is referring not to *psychology* and logic but to *mathematics* and logic, as one should expect of a logicist. The paper does, however, have the virtue of correcting a common misconception regarding Russell's logicism by discussing in detail his regressive method for discovering the axioms and the associated view according to which, just as in the natural sciences, the laws of logic derive their justification in part through their consequences.

The resolution of the antinomies. In May 1901 Russell discovered that his conception of classes and propositional functions led to antinomies. His first attempt at a solution was to stratify classes and deny that propositional functions are terms. In an appendix to the ***Principles*** he set forth a theory of types which included, in addition to a hierarchy of types of classes, a separate type for propositions and a type including all types. Both features were designed to preserve the universality of logic, the latter by preserving the meaningfulness of quantification over all objects. However, Russell shows that the system leads to an antinomy concerning propositions (involving classes). He considers the natural course of stratifying propositions but rejects it as "harsh and highly artificial", concluding that "the totality of all logical objects, or of all propositions, involves, it would seem, a fundamental logical difficulty" (p. 528 and cf. Urquhart p. 289).

For the next four years Russell developed a surprising variety of type-free theories. His goal was to discover an intrinsically motivated theory that gave a unified solution to all of the antinomies. The first hope of such a solution came with the theory of descriptions since it enabled Russell to reduce classes to propositional functions. Indeed, as noted earlier, in "On Fundamentals", immediately upon discovering the theory of descriptions Russell uses it to effect such a reduction. This reduction to a minimal framework held the hope of a simple, unified solution to the antinomies, the idea being that natural considerations in the minimal framework would bring (through the reduction) order to the higher reaches. The more minimal the framework the greater the hope. For this reason (and because propositional functions were also problematic) Russell turned to the ultra-minimal framework of the substitution theory where propositional functions were reduced to a system concerning propositions, the variable and a single operation of substitution. The original theory and its descendants are carefully reconstructed in Landini's paper by making extensive use of unpublished manuscripts.

The expression '$p/a;b!q$' is taken to mean that q is the result of substituting b for a in p. For example, if p is the proposition *Socrates is human* and a is Socrates and b is Plato, then q is the proposition *Plato is human*. This primitive is governed by a number of axioms which Landini isolates. The expression 'p/a' is treated as an incomplete symbol for a class, namely, the class of b such that the q such that $p/a;b!q$ is true. In this way, a class is proxied by a pair consisting of a proposition and a constituent. For example, the class of humans is proxied by p/a, where p and a are as above. One can then derive the comprehension principle and show that the resulting classes inherit a simple type structure. The system is sufficiently strong to generate arithmetic. Unfortunately, it is too strong. For, as Russell discovered by April 1906, a clever choice of substitution yields a new antinomy. Russell attempted to remedy the situation by restricting the formulas in the substitution axioms to quantifier-free formulas. But the resulting system is too weak to generate arithmetic and when one adds the necessary comprehension axioms inconsistency returns.

Eventually Russell abandoned the type-free approach and stratified propositions and propositional functions according to the kind of generality they involve. (Landini claims that despite appearances Russell never gave up on the unrestricted variable. This is a difficult thesis to sustain in light of Russell's many apparent statements to the contrary. The reader is referred to Landini's book (JSL LXIV 1370) for his defense of this claim.) The resulting theory—the ramified theory of types—was developed in 1907. This period is covered in Urquhart's excellent paper, a very much needed contribution to the literature. Urquhart gives a rigorous version of the theory based on an earlier exposition by Church. The heuristic principle motivating the theory is the so-called vicious circle principle according to which "Whatever involves an apparent variable must not be among the possible values of that variable". The true motivation behind this principle and the ramified hierarchy has remained something of a mystery. It may be that just as the substitution theory leads to a simple theory of types so too Russell's views at the time on the nature of propositions, propositional functions, and the variable, lead to a ramified theory of types. This is defended by Goldfarb in *Russell's reasons for ramification*, ***Rereading Russell: essays in Bertrand Russell's metaphysics and epistemology***, edited by C. Wade Savage and C. Anthony Anderson, Minneapolis: University of Minnesota Press, 1989. Another possibility is that the theory was adopted because of its fruitful consequences. For Whitehead and Russell say of the theory that "it suggested itself to us in the first instance by its ability to solve certain contradictions", though they add that the theory is "not wholly dependent on this indirect recommendation: it has also a certain consonance with common sense which makes it inherently credible" (***Principia mathematica***, p. 37). Furthermore, it is precisely in 1907 that Russell began underscoring his view that axioms are largely justified by their consequences. It is hoped that this question will be illuminated by the appearance of ***Toward "Principia mathematica", 1906–08***, volume 5 of the ***Collected Papers***.

The main fruitful consequence of the ramified theory is that it provides a unified approach to the resolution of the antinomies. The manner in which it handles the set theoretic antinomies is familiar. That it also handles the semantic antinomies was first shown in detail by Church (JSL XLI 747) and Myhill (*A refutation of an unjustified attack on the axiom of reducibility*, ***Bertrand Russell memorial volume***, edited by George W. Roberts, London: Allen & Unwin, 1979): The system of ***Principia mathematica*** is intended as a general framework to which one can add predicates and principles pertaining to any given special science. When one adds the predicates of an intensional semantics—that is, the predicates expressing when a given open formula expresses a given propositional function—and the natural principles governing them, one can define the extensional notions of truth and satisfaction with the consequence that these are precisely the Tarskian notions. The ramified theory of types thus implicitly contains both the standard approach to the set theoretic antinomies and the standard approach to the semantic antinomies. Unfortunately, as noted earlier, without the axiom of reducibility the system is too weak to generate arithmetic. Thus, in his search for an intrinsic solution to the antinomies Russell was led once again to a system too weak for the purposes of logicism.

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D. M. GABBAY, A. KURUCZ, F. WOLTER, and M. ZAKHARYASCHEV. ***Many-dimensional modal logics: theory and applications.*** Studies in Logic and the Foundations of Mathematics, vol. 148. Elsevier, Amsterdam, xiv + 747 pp.

There are many applications of modal logic across philosophy, computer science, artificial intelligence and linguistics. Species of modal logics investigated and used widely include temporal, epistemic, dynamic and description logics. There is also wide interest in studying the mathematical, logical and computational aspects of modal logics. Two recent major concerns of modal logicians, particularly over the last decade have been attempts in two directions to extend the usefulness of the formalisms. One concern has been the often poor computational properties of many of these logics. This has initially been addressed by more careful study of the decision problems and the complexity of decision procedures if they exist.

The other realisation is that real world applications usually involve a complex combination of different sorts of modal logic. For example, reasoning about systems of interacting agents involves reasoning about combinations of time and knowledge. Hence we have had many investigations into specific combinations of two or modal logics and more recently into more general results and techniques for combining modal logics.

Many-dimensional, or multi-dimensional, modal logics are combinations of modal logics such that the method of combination exhibits a multi-dimensional aspect in the sense of vector geometry. Consider for simplicity a combination of two unimodal logics, each with a single modal box. We suppose that we have a semantical presentation of each modal logic separately, so that they are defined on Kripke frames like (W_i, R_i) for $i = 1$ and $i = 2$ respectively. The product frame is $(W_1 \times W_2, R_1', R_2')$ with $(a, b)R_1'(c, d)$ iff aR_1c and $b = d$ and $(a, b)R_2'(c, d)$ iff $a = c$ and bR_2d. Endowing such product structures with interpretations for the atoms clearly gives us a semantic structure for a language with wffs $\Box_1\alpha$ and $\Box_2\alpha$ for each wff α. This leads us to the product logic, the two-dimensional product of the two modal logics. Generalisations to higher dimensional products and multimodal logics are straightforward.

Other ways of combining modal logics exist. Some are just variations on this product construction but others are very different. In existing applications of specific combinations of modal logics, the product construction and its closest variants are perhaps the most common.

The stated aim of this book is to look at the computational aspects of many-dimensional combinations of modal logic and it concentrates upon decidability (of satisfiability), complexity of the decision problem, and axiomatizability. It is admitted in the preface that these are not the most directly practical of concerns for automated reasoning with the logics but a careful argument there makes a convincing case that these are essential studies in order to gain deeper insights into these logics and any possibly better behaved variations.

The book is a solid research monograph with in-depth as well as wide coverage of the latest theoretical advances in a very mathematical part of theoretical computer science. So the "and applications" part of its title must be understood in that context. It assumes a high level of competence in mathematical logic of its reader but such a reader is rewarded with careful and rigorous development of many interesting techniques and results. Furthermore, substantial effort is put into discussing the importance, interest and use of the results and also to the organisation of their presentation.

The book begins with an unusually informative preface before its sixteen chapters which are organised into four parts.

Part 1 is an introduction to modal logic and a survey of "applications" of modal logic, i.e., of the basic ideas and an up-to-date overview of temporal logic, epistemic logic, description logic e.t.c. and some many-dimensional combinations (of these) which already have their own established bodies of work.

Part 2 contains the meat of the book. It starts with *fusions*, a basic but not really multi-dimensional way of combining logics, and it ends with some variations on products but the chapters here are mainly devoted to systematically presenting a range of general results about the computational behaviour of products of modal logics and demonstrating fully the main techniques for establishing these results. Of the techniques lined up here, special mention must be made of the new method of *quasimodels* for proving decidability. Quasimodels are (very roughly) surrogates for two-dimensional product structures which are built from a collection of slices along one dimension, each slice being a compacted finite representation of the projection of the product. Incidentally, there seem to be connections between quasimodels and the *mosaic* technique which also has been used to analyse the computational behaviour of modal logics, and the possibility of such connections are briefly mentioned in a few places in the book (but there is no entry for mosaic in the index). The older method of *tiling* to prove undecidability is also illustrated well within these chapters.

Part 3 is a shorter part on first-order modal logics: earlier chapters have established that because of a long-understood close connection between first-order quantification and the modal logic S5, we can regard first-order modal logics as modal products. Results from the 1960s showed that many such logics are highly undecidable and so they have been relatively neglected. However, some recent work has identified some reasonably useful fragments which have much better computational behaviour. This includes the "monodic" fragment which only allows formulas whose temporal subformulas $\alpha U\beta$ or $\alpha S\beta$ have at most one free variable. By also restricting the non-modal part of the language, some decidable first-order modal and temporal languages can be defined. Many of the new results in this vein are presented carefully in these chapters.

Part 4 is entitled "Applications to knowledge representation" and comprises chapters on temporal epistemic, modal description, and spatio-temporal logics. The chapters use the techniques of earlier in the book to examine the computational behaviour of some interesting combined logics. Amongst many examples there are new decidability and complexity results for some temporal-epistemic logics with past-time operators. The next two chapters concentrate on modal description logics: one contains an example using the method of quasimodels to give a decidability result and the next presents tableau systems for modal description reasoning tasks. The final chapter contains a whole raft of interesting sometimes undecidability and sometimes complexity results for various spatio-temporal logics including variations on the well-known BRCC8 combined with the temporal logic of until and since over the usual linear orders.

The 16 chapters are followed by a helpful epilogue giving an overview of what has been a most intricate account of a complex area. Particularly impressive here are the several wide-ranging "rules of thumb" setting out some very general summaries of particular features of the component logics which lead to certain computational behaviour. There are also hints of some future paths for the subject. Then comes a very comprehensive and useable series of lists and indices. There is a list of tables, a list of logics and languages, an index of symbols, a subject index and a forty page bibliography of the works cited in the book.

The highlights of this book include its wide coverage, the useful overviews presented in many tables, diagrams and rules of thumb, the lists of clearly stated important open problems and the full proofs of most results with some of the latest techniques in action, explained fully. Minor quibbles might include the fact that applications are not so well covered and that it is sometimes difficult to find the correct attribution of a result to its original author. I suppose that one could also quibble about the book's title: anything from "Multi-dimensional modal logics" to "The theory of combining modal logics" might have been slightly more accurate. After all, much of the book is about two-dimensional products (which are generally the most interesting) and, as noted above, the theory here outshines the applications somewhat.

To summarise, this is a *must have* reference for the modal logic researcher, a great introduction (and more) for the suitably qualified newcomer and a most rewarding read.

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CHARLES CHIHARA. ***A structural account of mathematics.*** Oxford University Press, Oxford, 2004, xiv + 380 pp.

The close of 1899 saw Hilbert and Frege locked in spirited debate. Frege had maintained that geometrical theories should be understood as they had always been understood—as theories of a definite and privileged subject matter (points and lines) given to us in intuition. But Hilbert had insisted upon a quite different and altogether revolutionary conception. On the 29th December 1899 Hilbert wrote:

> "[I]t is surely obvious that every theory is only a scaffolding or schema of concepts together with their necessary relations to one another, and that the basic elements can be thought of in any way one likes ... any theory can always be applied to infinitely many systems of basic elements" (See G. Frege, ***Philosophical and mathematical correspondence***, Oxford: Blackwell, 1980.)

Responding to Frege in this way, Hilbert struck upon a conception of mathematics that was destined to predominate in the 20th century. According to this conception, an axiom system is not to be regarded as a statement about some privileged subject matter given in some way or other, but rather as a system of conditions describing a relational structure. This way of thinking about axiom systems became entrenched in mathematical practice. By 1950 the mathematical collective Bourbaki was able to famously declare: "from the axiomatic point of view, mathematics appears thus as a storehouse of abstract forms—the mathematical structures". (See N. Bourbaki, *The architecture of mathematics*, ***American Mathematical Monthly***, vol. 57 (1950), pp. 221–232.)

For most of the century subsequent to the Frege–Hilbert debate philosophers of mathematics have been too much preoccupied with other foundational endeavours—with logicism, formalism and intuitionism—to take too much notice of this novel aspect of mathematical practice—the emphasis upon structure. But recent years have seen a great burgeoning of interest in what mathematicians have to say about structure. Indeed there have been a number of high profile attempts to make the notion of structure key to a philosophical understanding of what mathematicians are about. These different attempts to account for mathematical practice are distinguished by the different interpretations they impose upon the notion of structure. Some philosophers (Shapiro, Resnik) have interpreted the structures (or the positions in the structures) that mathematicians study in a realistic spirit, treating them as a range of extraordinary abstract objects. Others have sought to interpret structural talk in a nominalistic vein. Hellman, for example, has suggested that talk about the structure a mathematical theory describes should be construed as shorthand for talk about the possibility of there being objects—ordinary or otherwise—that collectively satisfy the axioms of the theory in question.

In ***A Structural Account of Mathematical Objects*** Charles Chihara seeks to illuminate mathematical practice by developing his own distinctive take on "the structural content" of mathematical theories, an interpretation that he situates with respect to the alternatives offered by Shapiro, Resnik and Hellman (amongst others). Chihara's account of structural content draws upon the "constructability theory" advanced in his earlier work ***Constructability and Mathematical Existence*** (Oxford: Oxford University Press, 1990).

Before elaborating upon this theory let me explain the underlying motivation that Chihara provides for developing a structural account of mathematics. Chihara conceives of his structural account as a solution to three independent puzzles. The first puzzle concerns the fact that whereas Euclid's ***Elements*** offers postulates governing the *possible* construction of geometrical figures, the axioms of Hilbert's ***Foundations of Geometry*** are existential in force, positing *actual* points and lines. Chihara deems it vital that an adequate philosophy of mathematics offer an analysis and explanation of this dramatic shift of focus—after so many centuries—in the study of geometry.

The second puzzle concerns the peculiar fact that mathematicians appear to equate mathematical existence with consistency. The fact is peculiar because nowhere outside of mathematics would we consider the mere consistency of a concept—for example, the concept *God*—to suffice for the existence of something falling under that concept.

The third puzzle (due to van Inwagen) is based upon metaphysical considerations and concerns our grasp of the fundamental primitive of set theory. Lewis has shown that set theory may be reduced to a mereological theory in which singletons are the only atoms. Since it treats singletons as atoms this theory provides no account of the character of the relation that obtains between a singleton and its only member. But, van Inwagen has argued, it appears not only to be the case that we do not have, but that *we could not* have an account of this relation. van Inwagen concluded, and Lewis agreed, that we have no idea of how we could possibly understand the notion of a singleton.

van Inwagen arrived at this surprising conclusion by reflecting upon the following schedule of properties and relations. According to van Inwagen, properties are either *intrinsic* or *extrinsic*. The intrinsic properties of a thing are those properties the thing has in virtue of the way that it is. The extrinsic properties are those properties the thing has in virtue of the relations it bears to other things. Once intrinsic properties are distinguished from extrinsic properties, *intrinsic* relations may be distinguished from *extrinsic* ones. Intrinsic relations are relations that supervene in some way or other upon the intrinsic properties of their relata or composites of their relata. There are two kinds of intrinsic relations: *internal* and *external*. Internal relations are relations that supervene upon the intrinsic character of their relata. By contrast, external relations supervene upon the intrinsic properties of the composite of the relata. Extrinsic relations are relations that fail to supervene in either of these ways.

The puzzle that van Inwagen identified may now be seen to arise in the following way. Neither on Lewis' theory nor any other do we have the slightest idea of what the intrinsic properties of a singleton or the composite of a singleton and its member might be. Consequently we have no idea of how the relation between a singleton and its member can supervene upon the intrinsic properties of its relata or their composite. This means that we cannot classify this relation as intrinsic (as internal or external). But nor, van Inwagen maintained, can we classify the relation as extrinsic. For we have no idea of what other things in the universe might be responsible for the obtaining of the relation between a singleton and its only member. So we are left in the deeply puzzling predicament of not being able to explain how we understand the relation between a singleton and its member.

It is by appeal to the notion of structure that Chihara seeks to resolve these three puzzles. The first puzzle is resolved, Chihara claims, because Hilbert's axioms, when properly understood, do not make any existential assertions. In fact, rather than being assertions they are akin to uninterpreted sentences, sentences that merely serve—in Hilbert's hands—to characterise "a kind of structure". The second puzzle is resolved because, Chihara continues, whilst the consistency of a theory or system of postulates cannot guarantee the existence of an object, this collective trait does ensure that "the postulates can be regarded as characterising a genuinely possible kind of structure" (p. 47). The third puzzle is resolved, Chihara also claims, once we see our way past a critical assumption infelicitously made. According to van Inwagen and Lewis set theory is "a theory about real objects" and, given this assumption, there must be a fact of the matter concerning whether the membership relation is some specific relation of one definite kind or another. But, Chihara maintains, this assumption is mistaken. Set theory merely characterises a kind of structure: "the axioms tell us what (so-called) 'sets' there must be in the domain of a model [of the theory] and also how the things in the domain must be related ... in order that we have a structure of the type in question" (p. 54). Consequently an understanding of set theory does not presuppose a grasp of some particular relation.

The resolutions that Chihara provides to these different puzzles leave one wondering: just what does Chihara means when he appeals to "a structure" or "a kind of structure"? Chihara answers this question by maintaining that the structural content of a sentence ϕ of a mathematical theory T is expressed by a sentence of the form (p. 66):

Any model of T will have to be a model of ϕ.

However this reply raises as many questions as it answers. In particular, it leaves one wondering about the status of the models (sets) over which statements of structural content appear to quantify. To understand Chihara's thinking on this matter we must turn to a consideration of his constructability theory.

This theory is best understood in the context of Chihara's treatment of Quine's indispensability argument for the existence of mathematical objects. According to Chihara's reading of this argument, Quine issued a challenge to the nomininalist to accommodate the application of mathematics in the natural sciences without incurring a commitment to mathematical objects. Since Quine believed that the nominalist could not meet this challenge he concluded that mathematical objects are indispensable to the natural sciences, and insofar as we accept the findings of natural science we cannot coherently foreswear the existence of mathematical objects. To undermine this argument Chihara has endeavoured to show that the nominalist *can* meet the challenge Quine raised. His constructability theory is an attempt to do so. Viewed from the most general perspective we may think of this theory as reducing mathematics to the simple theory of types. With this reduction in place talk of sets ("$\{x : x \text{ is F}\}$") is replaced by talk of open sentences ("'Fx'") defining them. An assertion that an open sentence exists is then construed as an assertion that it is possible to construct a token open sentence of this type. In this way, existential claims about mathematical objects give way to claims about the constructability of open sentence tokens. According to Chihara, the constructability theory may be used to account for the application of mathematics in the natural sciences without incurring a commitment to the existence of mathematical objects.

In ***A Structural Account of Mathematics*** Chihara employs the constructability theory to specify the structural contents of mathematical claims. In particular, the theory is employed to explain how talk of models (sets) may be used to articulate the structural information expressed by mathematical claims without generating a commitment to mathematical objects. In doing so, Chihara claims to resolve two further puzzles about mathematics. One of these concerns the fact that even practicing mathematicians cannot agree whether mathematical objects exist. The other concerns the fact that we have no way of explaining how we can know about or gain reference to mathematical objects. Since the constructability theory shows how mathematical discourse may be interpreted without incurring a commitment to mathematical objects both puzzles dissolve away (p. 295, p. 306).

It has been argued by Burgess that nominalistic reconstructions of the kind that Chihara advocates are undermined by a dilemma. (See J. Burgess, *Why I am not a nominalist*, ***Notre Dame Journal of Formal Logic***, vol. 24 (1983), pp. 93–105.) Either nominalist reconstructions are to be understood as *revolutionary* replacements for existing mathematics and science, or alternatively, as *hermeneutic* accounts of what mathematicians and scientist have really meant along. According to Burgess neither alternative is theoretically appealing. In the former case, the transition costs of replacing old science with a nominalistic alternative are enormous and there are no good scientific grounds for undertaking such a revolution. In the latter case, the prospects of finding sufficient evidence for such an implausible linguistic hypothesis are remote indeed. In the present work, Chihara seeks to evade this dilemma by denying that his account of mathematics is either hermeneutic or revolutionary. Instead its role is to "validate" what mathematicians and scientists say, "to facilitate the understanding of the logical correctness and rationality" of mathematical practice (p. 239). In emphasising this role Chihara claims to isolate a significant feature that distinguishes his own account of mathematics from those of his competitors—whether platonist, nominalist or structuralist—that offer analyses of what mathematicians really mean by what they say.

The foregoing discussion constitutes only a sketch of ***A Structural Account of Mathematics***. Nevertheless, enough has perhaps been said to provide a sense of what is distinctive and novel about Chihara's conception. In addition to providing a defence of this conception ***A Structural Account of Mathematics*** also includes extended discussions of alternative forms of structuralism, the flaws inherent to a variety of indispensability arguments, and the extent to which Chihara's account may be compared with Putnam's if-thenism and Field's fictionionalism. Along the way Chihara raises many important questions. Whilst many philosophers may disagree with his answers, most will find themes and issues in this work worthy of continued reflection.

For reasons of space I will confine the present review to a brief discussion of two of these issues. First, few (if any) philosophers of mathematics have found Burgess' dilemma fatal to their views. This is because most (if not all) philosophers of mathematics—whether platonist, structuralist or otherwise—conceive of their accounts as "validating" rather than replacing or analysing mathematical discourse. (See, for example, G. Hellman, *Maoist mathematics*, ***Philosophia Mathematica***, vol. 6 (1998), pp. 334–345.) However, what it is to the point is not so much whether a philosopher conceives of his or her account in such a manner but whether it is legitimate for them to do so. And it is far from evident that Chihara is entitled to conceive of his structural account in this way.

To succeed in evading Burgess' dilemma Chihara's account must be neither revolutionary nor hermeneutic. But Chihara encounters the following difficulty: his own use of this account appears to rely upon its being hermeneutic. Recall the first puzzle that Chihara employed to motivate the structural account:

> "How could Hilbert have felt justified in postulating axioms which assert that there exist an infinity of points and lines, when Euclidean plane geometry was developed and applied for a multitude of centuries without any such commitment to an apparent ontology of imperceptible objects?" (p. 44)

Chihara responds to this puzzle by claiming that "when properly understood" Hilbert's axioms "did not in fact make any such existential assertions". Hence Hilbert was able to feel justified in postulating these axioms even though—given their surface form—they appear to stand in stark contrast with the axioms of Euclidean geometry. But how are we to understand this resolution of the puzzle as resting upon anything other than a hermeneutic claim, a claim about what Hilbert really meant? For if Chihara has not lighted upon what Hilbert really meant then the question raised goes unanswered, a question about how *he* could have felt justified in positing an infinity of points and lines. Consider the second puzzle: how can mathematicians intelligibly equate consistency with existence when outside mathematics it is evident that consistency does not suffice for existence? Chihara resolves the puzzle by claiming that the best way to understand mathematicians—Poincaré is discussed in this context—is to attribute to them a non-standard conception of existence (where "existence" means "genuinely possible kind of structure") (p. 47). But again it is difficult to see how this resolution could be effective unless it is understood as a hermeneutic suggestion about what mathematicians really mean beneath the superficially perplexing guise of their remarks. Unless mathematicians mean something non-standard by "existence" it is unclear in what sense (if any) Chihara's account succeeds in displaying the underlying "rationality" of their linguistic performances.

Second, following Lewis' lead, Chihara sets great store by the third puzzle discussed—van Inwagen's argument. For both Lewis and Chihara this argument provides a powerful motivation for adopting a structuralist account of set theory. But why set such store by the argument in the first place? The argument presupposes that the obtaining of relations must depend upon the obtaining of intrinsic properties (whether it be upon the intrinsic properties of the relata, or a composite of the relata, or of something else). Russell's ***Principles of Mathematics*** taught analytic philosophers that it was wrong to expect that the obtaining of relations will invariably depend upon the intrinsic features of their relata (that relations will invariably be internal). Why should we anymore expect that the obtaining of relations will depend upon the intrinsic features of the composite of their relata, or the intrinsic features of anything else? van Inwagen's argument appears to rely upon a metaphysical prejudice in favour of the intrinsic. Once this prejudice is discarded, it is unclear what, if any, motivation his argument supplies for adopting structuralism. It is correspondingly unclear whether Chihara (or Lewis) have presented us with a convincing case in favour of a structuralist account of mathematics.

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A. W. F. EDWARDS. ***Cogwheels of the mind: the story of Venn diagrams.*** The Johns Hopkins University Press, Baltimore and London, 2004, xvi + 109 pp.

A Venn diagram is a familiar pictorial device used to express a number of logical propositions. These pictures are commonly used as a teaching device to logically represent the inclusion or exclusion of sets with certain properties. However, used as such, the diagrams are usually expressed as two or three circles (which are called 2-set and 3-set Venn diagrams, respectively) which can represent at most eight logical areas.

The main thrust of the book is to show how it is possible to increase the number of propositions which can be expressed by such a diagram *ad infinitum*.

Before this aim is reached, a rich history of logical diagrams is given. There is a detailed historical and biographical account of John Venn's work on these diagrams including how the Venn diagrams superseded the Euler diagram and Venn's idea for expansion into 4-set and 5-set diagrams (which can represent 16 and 32 logical areas, respectively). Many other personalities are introduced, including Samuel Dodgson (a.k.a. Lewis Carrol), whom the author explained, "lost his courage" after creating only a 5-set diagram.

In order to expand to arbitrary n-set diagrams, one must look beyond the traditional circles. Venn himself constructed the 4-set diagram with ellipses, but even these cannot be further extended. Carrol tried to extend the number of areas using rectangles, but some of the areas which should be intersecting are in fact disconnected. Venn and others attempted diagrams with irregular shapes and managed a 5-set diagram, but this also could not easily be extended. The problem hinged on finding the right shape to represent the regions.

The author's solution projects the diagram onto a sphere. The ball can be split on its perpendicular axes to give a 3-set Venn diagram and then mutually intersecting wavy lines crossing the equator complete the diagram. The 4-set diagram of this type looks like a tennis ball. This diagram, known as the Edwards-Venn diagram, can be easily projected onto two dimensions. This two dimensional image can be seen as intersecting "cogwheels" with increasing numbers of "teeth", hence the title of the book.

The two dimensional Edwards-Venn diagrams are topologically equivalent to diagrams previously (and unknown to the author at the time) discovered by Branko Grünbaum. Grünbaum used polygons with increasing number of sides to represent the logical areas. The author found these diagrams to be unsatisfactory due to the speed at which the polygons converge on each other.

Along the same lines of thinking as the author's solution, Smith created an extendable set of Venn diagrams using a family of sine curves. An n-set diagram of this type is given by the formula $y = \sin(2^i x)/2^i$ where $x \in [0, 2\pi]$ and $0 \leq i \leq n - 2$. Many curves in these diagrams will intersect in the same place unlike the Edwards-Venn diagrams, where only two curves can intersect at any one point.

Other algorithms for producing Edwards-Venn diagrams are also discussed, including those with rotational symmetry. One can enumerate all completely symmetrical n-set Venn diagrams using a computer program.

Applications of these new Venn diagrams to other areas of mathematics is the other major topic of the book. In connection with communications theory and combinatorics a Gray code interpretation of the Edwards-Venn diagrams is given. The Gray code is a sequence of binary numbers in which only one digit is changed in each successive pair. The areas of general Venn diagrams can be thought of as a binary numbers relating to the truth or falsity of the logical propositions. The Edwards-Venn diagram has the property that every boundary between logical areas also represents a change in only one digit in the binary representation.

In connection with graph theory, the two dimensional Edwards-Venn diagrams can be interpreted as planar representations of hypercubes. In particular, the dual graph of a Venn diagram is a maximal planar subgraph of a Boolean cube.

The book in general is geared towards non-mathematicians. The mathematics is very light and generally well-explained. There are many insightful and intriguing diagrams, including examples of Venn diagrams in national flags. In addition to the biographical history of the persons involved, the author adds his own story of his developing enthusiasm for the problem and how the solution was discovered. It can be interesting for scientists and non-scientists alike to witness how these discoveries are made.

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I. JANÉ. *Reflections on Skolem's relativity of set-theoretical concepts*. ***The Philosopher's Annual,*** edited by Patrick Grim, Peter Ludlow, and Gary Mar, vol. XXIV. CSLI Publications, Stanford, 2003, pp. 95–121

C. WRIGHT. *On being in a quandary: relativism, vagueness, logical revisionism*. ***The Philosopher's Annual,*** edited by Patrick Grim, Peter Ludlow, and Gary Mar, vol. XXIV. CSLI Publications, Stanford, 2003, pp. 273–325

Readers unfamiliar with this collection should know that it is a sort of "greatest-hits" compilation as judged by a reasonably distinguished set of nominating editors. In the reviewer's opinion most of the collected articles are well worth reading in spite of some unevenness in style.

Ignacio Jané performs a valuable service in his article on Skolem's work. The latter, like many other milestone contributions to logic and foundations, is something of which far too many logicians and philosophers have insufficient knowledge. An hour or two reading Jané's account will raise anybody, with sufficient good will, above the level of cocktail-party chit-chat.

Skolem held from about 1922 onwards that set-theoretical notions were relative, in a sense which Jané exposes. First, concerning the famous theorem which bears the name of both Löwenheim and Skolem (to the effect that every consistent set of sentences of a first order language has a countable model) Skolem says in a paper of 1941 (JSL VII 35):

> Its most important application is the critique of the the set-theoretical concepts and especially that of the higher infinite powers.

Possibly the least thoughtful interpretation of this is something along the lines of:

> Skolem complains that certain well-understood notions—that of an uncountable set especially, cannot be adequately characterized in a first-order way, and hence are suspect.

The author of this review confesses that he would probably have subscribed to this before reading Jané's article.

Jané argues that the *real* Skolem argument is more like:

1. All the evidence that has been given for the existence of uncountable sets is inconclusive.
2. Countable models are relevant because axiomatized theories are supposed to "secure" set-theory and that which sets actually exist is determined by the models of the theory.
3. In this context, emphasizing countable models is legitimate since no other models can be constructed without set-theory.

The T-shirt version: *The existence of uncountable sets requires more than mere postulation*. We might recall here Russell's famous remark about the advantages of postulation.

The not-so-dumb (nsd) response to Skolem might be phrased:

> Everybody believes in Cantor's diagonal proof and so we all believe that the existence of uncountable sets has been established.

The reviewer confesses that he would certainly have said this. However, Jané supplies a compelling answer to the nsd response.

> Look at the actual Cantor proof and not the one in your head. It establishes that no countable set of sets of integers can contain all sets of integers. To make this imply the existence of uncountable sets we must have recourse to the principle that there *is* such a thing as the set of all sets of integers. But isn't that simply assuming the existence of uncountable sets?

In the conclusion of his 1922 attack on Zermelo, Skolem writes: "The most important result above is that set-theoretical notions are relative." It is the nature of that relativity that forms the core of Jané's argument. An earlier influential commentary was provided by Benacerraf who regarded the relativity in question to be to some or other axiomatic presentation as opposed to some non-axiomatic version of set-theory.

Jané points out that this view collides with the text. Skolem does indeed talk about something he calls "the original set-theory" (which it is tempting to call *Volksmengenlehre*) which he remarks is inconsistent. So there is no there, there—at least no theory, properly speaking. In spite of this, Jané observes that Skolem can and does avail himself of certain "well-defined collections" which we may presume to be part of pre-set theory—rather than part of the "original."

That Skolem found the use of certain collections unproblematic is shown, says Jané, by the new proof of the Löwenheim-Skolem theorem which he offers in the 1922 paper. The reason for the new proof is to avoid two features of his 1920 proof: the formation of an intersection of an infinite family of sets, and his use of the axiom of choice. The elimination of these two in the new proof, Skolem tells us, shows that "general set-theoretical notions are unnecessary for the understanding of the content of these theorems." It seems that Skolem rejects non-axiomatic set theory but still feels free to help himself to the use of suitably defined sets and functions.

So for Skolem a *set* properly speaking is an element of the domain of a certain model M, which is the model for a set of axioms (for instance ZFC). In that model, there is a relation E defined over the domain, which is the formal membership relation. On the other hand there are also what Jané calls "intuitive collections" which are distinct from sets. It will be important therefore to distinguish E from $\in$ which is the relation holding between an object and a collection to which it belongs.

We may suppose that for every element a of the domain of M there is a certain (informal) collection a^* which consists of the (formal) members of a. Jané extends this idea also to the informal counterparts of (formal) functions and relations.

Jané puts the central argument for relativism like this: If the axioms of ZFC are consistent then by the Löwenheim-Skolem theorem the axioms have a countable model. This is to say then that the domain of the (formal) model M, could be countable. One of the theorems of ZFC asserts the existence of uncountable sets—the power set of ω for instance. So there is a set a in the domain of the model which, according to the model, is uncountable. However the corresponding collection A^* is countable since all its (informal) members belong to the domain of M which is countable.

The reason for this uncomfortable situation is, according to Skolem, that while every subset of a has a corresponding definable (informal) sub-collection of a^*, it is not true that every definable sub-collection of a^* corresponds to some subset of a, as a diagonal argument shows. Also, while for every (formal) bijection between two (formal) sets a and b there must correspond an (informal) bijection between a^* and b^* not every informal bijection between the latter two must correspond to some formal bijection in M between a and b. In just this fashion there is an informal bijection between ω^* and $(\mathcal{P}(\omega))^*$ but no corresponding formal bijection in M—that being what it means for $\mathcal{P}(\omega)$ to be uncountable.

This is Skolem's relativity, we have a set a being uncountable relative to the axiomatization or (which is to say the same thing) to the model M.

Jané's article goes on to discuss other forms of relativity which Skolem conjectured—the relativity of ω for example, and then takes on some of the standard "explanations". Perhaps the most interesting (due to von Neuman and others) is that Skolem's relativity is more a matter of the existence of non-isomorphic models of set theory (its lack of categoricity) than anything else. Jané produces a wonderful quote from a paper of Skolem published in 1929 which says flat out that lack of categoricity follows from relativity and not the other way about.

The remainder of Jané's paper is devoted to trying to find an interpretation of Skolem's various remarks on relativity that is coherent at least, and plausible at most. Whether or not this Herculean task has been accomplished is a judgement best left to the individual reader. The reviewer certainly feels improved by having read the paper.

Crispin Wright takes on three problems:

1. To give an account of vagueness which is not afflicted by the heartbreak of Sorites.
2. To provide a coherent account of the sort of relativism "germane to matters of taste, value, etc."
3. To provide a wrinkle-free case for the revision of classical logic of the sort usually sponsored by Intuitionists and their kin.

To the reviewer's knowledge there is no existing body of literature connecting these issues (unless the article under review be thought ponderous enough to constitute a body all by itself) so Wright's approach has the cachet of primacy at least.

To Wright's way of thinking the single thread which connects the three is the notion of *indeterminacy*. There are, of course, many different ways in which this idea might be cashed out, but according to Wright, if one takes what he calls a "broadly epistemic" conception of the nature of indeterminacy, a side effect will be the uniform treatment of what appears to be three distinct problems.

The original proposal for an epistemic conception of vagueness, roughly speaking that vagueness resides in the impossibility of our acquiring knowledge of the precise values of the predicates involved (or rather of the precise values of the characteristic functions of their extensions) seems to have been in the Richmond Campbell article *The Sorites Paradox*. ***Philosophical Studies***, vol. 26 (1974), pp. 175–91. In one of those odd twists of fate which make life worth living, the reviewer chanced to be on hand during those halcyon days of 1972–73 when Campbell wrote his paper and attempted to get it published. Seldom were such savage referees' reports seen, containing such phrases as "absolutely insane and without a vestige of merit." When the paper was finally accepted for publication Campbell, exhausted by the effort, never again took up the cudgels on behalf of this view, in print at least. Can this have had some role in Campbell's later conversion to feminism? The reader must judge for herself. Thus ended yet another sad chapter in the history of science.

Wright's own take is only "broadly" epistemic because, as he puts it

> ... the Epistemicists have hold of an insight which may be detached from the the extreme, and for many bizarre-seeming metaphysical realism which—with their own encouragement—is usually regarded as of the essence of their view." (p. 274)

What this means is that Campbell need not have said in his article that there actually *are* precise values for "Bald" and "Tall" etc. and still said that vagueness consists in our lack of knowledge.

The story begins with matters of taste. Suppose you would rather be sailing while I hold that one can have all the fun of sailing and none of the inconvenience by standing fully clothed in a cold shower, tearing up hundred dollar bills. It seems that we have a disagreement. But what is the nature of this disagreement? Wright canvasses four different views and comes down on the side of the one he calls "*True Relativism*." On the latter view "genuinely conflicting opinions about a truth-evaluable claim may each be unimprovable and may involve no misrepresentation of any further fact."

Wright first canvases a proposal made in his earlier **Truth and Objectivity** (1992) in which the notion of "minimally truth-apt" might be thought to correctly describe the contending positions in a dispute over tastes. The problem is that this earlier treatment doesn't seem to allow the possibility that neither participant in a dispute of this kind is making a mistake—this being a requirement of True Relativism.

In the run up to a final working out of this relativism, Wright considers the idea that "there is no fact of the matter" as a third alternative and rejects it though it would put both contending opinions on the same footing, since the footing in question would be falsehood. We then carry forward into a summary of the the Sorites "problem" and the case for revising classical logic.

Supposing F to be a vague predicate (of the kind which gives rise to the Sorites) and x' to represent the successor function, Wright refers to the formula $(\exists x)(Fx \wedge \neg Fx')$ as the "unpalatable existential". This, according to Wright is what we need to understand in order to deal with the Sorites:

1. How the falsity of $\neg(\exists x)(Fx \wedge \neg Fx')$ can be consistent with the vagueness of F; and
2. how and why it can be a principled response to refuse to let $\neg\neg(\exists x)(Fx \wedge \neg Fx')$ constitute a commitment to the unpalatable existential, and hence—apparently—to the precision of F. (p. 293).

In what Wright calls "the basic case for revisionism" we are to assume that the discourse at issue is one for which "we have no guarantee of decidability; we do not know that it is feasible, for each of its statements P, to come to know P or to come to know not-P" (p. 293).

This cloud of unknowing is supposed to be summarized in

$$(\mathrm{NKD}) \quad \neg K[(\forall P)(FeasK[P] \vee FeasK[\neg P])].$$

Of course NKD is incompatible with the so-called law of excluded middle (LEM). This seems intuitive enough but it turns out to be inconsistent with the following principles of Evidential Constraint and Agnosticism respectively (taken together).

(EC) $P \rightarrow FeasK[P]$
(AG) P should be regarded as unknown just in case there is some possibility Q such that if it obtained, it would ensure not-P, and such that we are (warranted in thinking that we are) in no position to exclude Q.

This would seem to constitute a wrinkle in the case for dropping LEM.

The key to providing the case in a permanently pressed way is, so Wright tells us, to find a way to get rid of AG. This is where quandaries come in. What Wright calls the Quandary view is to say that, at bottom, indeterminacy is fuelled by ignorance.

> . . . as a starting characterization of cases where we do not know, do not know how we might come to know, and can produce no reason for thinking that there is any way of coming to know what to say or think, or who has the better of a difference of opinion. (p. 299)

Some later worries about the possibility that we might be as ignorant as all that concerning P but still know that "either P is knowable or its negation is". This leads to a refinement of the definition of a quandary state which is later summarized as:

> A proposition P presents a quandary for a thinker T just when the following conditions are met:
> 1. T does not know whether or not P
> 2. T does not know any way of knowing whether or not P
> 3. T does not know there there is any way of knowing whether or not P
> 4. T does not know that it is (metaphysically) possible to know whether or not P.

The last condition is intended to rule out that we be entirely ignorant concerning P and yet still know that either P or its negation is knowable,

Now all of these conditions would be met if it were true that "T knows that it is impossible to know whether or not P" but that condition is excluded by Wright so that, as he puts it "a quandary is uncertain through and through".

The great thing about not knowing which way to turn (in this sense) as regards P, is that we may be in that situation while still being in a position, so we think, to reject not-P. Wright claims to have established this somewhat startling claim by considering predications of color on surfaces open to view in good conditions. In that test-case it seems that so long as we find EC compelling, we must reject AG. This leaves the way open to accept NKD and hence the case for rejecting LEM is saved.

"But what about the Sorites?" I hear you cry. In this case we can reject the major premise and still take F to be vague so long as we construe this vagueness epistemically in the Wright manner (which it is tempting to call "weakly epistemically") —to say that some of the predications of F on objects in the sequence are quandary-bound. The good part is, of course, that such denial need not lead to an acceptance of the unpalatable existential, or indeed any of what we might think of as assertions of precision, since all those should also be construed as quandary-bound. As Wright remarks

> Rather, the classical-logical moves which would impose such commitments are to be rejected precisely because they allow transitions from known premises to quandary-conclusions. (p. 313)

And finally we get back to True Relativism. We must deal with earlier arguments intended to show that in a dispute, even one involving minimally truth-apt claims, as in disputes over matters of taste, one side will always be right and the other wrong. However when examined closely, these arguments are committed to the principle of double negation elimination which is not supported where quandaries rear their puzzling heads.

The final lesson is that when there is a failure of the 'somebody must be wrong' principle ("cognitive command" in Wright's idiom) it is mistaken to think in terms of 'those who differ can both be right.' Better to say in these situations "we *have no warrant for* a certain claim." (p. 315) This emphatically does not mean that for all we know the negation of the claim is true. We may, and in the key cases *do* have deductions which show that the negation is not true.

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ANDREAS WEIERMANN. *An application of results by Hardy, Ramanujan and Karamata to Ackermannian functions*. ***Discrete Mathematics and Theoretical Computer Science***, vol. 6 no. 1 (2003), pp. 133–141.

This novel paper is an interesting application of deep parts of classical analytic number theory to the mathematical logic of Ackermann function theory. Recall that the Ackermann function is a recursive function that ultimately dominates any primitive recursive function and, thus, is not itself primitive recursive. More specifically, the author shows formal connections between important asymptotic results of Hardy and Ramanujan, Tauberian results of Karamata-Wiener, and computability theory. Particular emphasis is given to the local structure of Ackermann type descent recursion for ordinals represented by a Hardy–Ramanujan style coding for certain bounded-above ordinals. The reviewer believes the approach used above will result in the introduction of Ackermann functions into analytic number theory, where they are now absent, due to their super-fast growth. Indeed the axiomatic set theory of small ordinals could benefit from the introduction of a Hardy–Ramanujan style of coding ordinals.

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A Festschrift in honour of Professor Ivor Grattan-Guinness, edited by John Dawson, ***History and Philosophy of Logic***, vol. 24 no. 4 (2003), pp. 257, 259, 261–366 and vol. 25 no. 1 (2004), pp. 3–51.

The founding editor of the journal ***History and Philosophy of Logic***, Professor Ivor Grattan-Guinness, retired from the faculty of Middlesex University in September 2002. Grattan-Guinness (*1941) took his Ph.D. in 1969 and D.sc. in 1978, becoming professor of the history of mathematics and logic at Middlesex University in 1993. In 2003 the catalogue of his wide and versatile scientific production listed over two hundred publications. One of his most important works, ***The Search for Mathematical Roots 1870–1940***, was published in the year 2000 by Princeton University Press. Over the years, he has edited several journals, supervised a number of dissertations, and taken an active part in many scientific societies, associations, and commissions. All in all, it is easy to endorse every word in John Dawson's appraisal of Grattan-Guinness as a person who "has done more than any living person, perhaps more than any person, to make the combined field of history and philosophy of logic a recognized and lively area of research" (p. 257).

The Festschrift is divided into two issues of ***History and Philosophy of Logic***. It begins in the last issue of volume 24 with the editor's foreword (p. 257), an exposition of the highlights of Grattan-Guinness's career (p. 259), four articles (pp. 261–363), and Grattan-Guinness's bibliography on the history and philosophy of logic (pp. 356–366). The latter part consists of three more articles in the first issue of volume 25.

In the first article, John Corcoran provides the reader an insightful critical discussion on George Boole's attempts to extend and perfect Aristotle's classical system of logic (pp. 261–288). Corcoran focusses on Boole's mathematical theory of the laws of thought as presented in his second major monograph ***Laws of Thought*** (1854), and concludes that even though modern logic is, in a sense, much closer to Aristotle's logic than that of Boole, there can be no doubt about Boole having "widened logic and changed its nature to such an extent that he fully deserves to share with Aristotle the status of being a founding figure in logic" (p. 261). If Aristotle is seen as the founder of logic as formal epistemology, then Boole should be taken as the founder of logic as formal ontology. In any case, regardless of Leibniz's pioneering results in the field of the algebra of logic, it was Boole who constructed the first mathematical treatment of logic and thus "persuaded the world's logicians that logical reality is mathematical, or at least that logic should be pursued mathematically" (p. 286). But was it his first monograph ***Mathematical Analysis of Logic*** (1847), or his late masterpiece ***Laws of Thought*** (1854), that launched the continuous development of mathematical logic? Corcoran convincingly argues for the latter alternative. He draws us a picture of Boole as an important mediator between two logical epochs. Corcoran's article nicely complements his previous studies on Boole, such as an article written with Susan Wood, *Boole's Criteria for Validity and Invalidity* (**Notre Dame Journal of Formal Logic**, vol. 21 (1980); reprinted in J. Gasser, editor, ***A Boole Anthology***, Kluwer 2000), which offers a detailed comparative analysis of Aristotle's Organon and Boole's *Mathematical Analysis* of Logic.

The second contribution is Adrian Rice's detailed account of the overlap between Augustus De Morgan's early work in the fields of probability theory and logic (pp. 289–305). This overlap is displayed in De Morgan's use of mathematics to facilitate logical procedures. According to Rice, De Morgan strongly argued, in his early work, that the inclusion of probability as a part of logic makes certain deductions clearer (p. 294). However, the publication of his ***Formal Logic*** in 1847 was in many respects a turning point in De Morgan's career as a logician, and only a few years later he dealt with mathematical probability for the last time. Moreover, his correspondence with Boole shows that, already by 1851, it was Boole and not De Morgan who had a genuine interest in using probability in the field of logic (p. 303). Many of us have heard at least something about De Morgan's achievements, but only a few know anything about his applications of probability theory in his logical excavations. Hence, Rice's survey is a welcome contribution to an understanding of the development of logic during the mid-19th century.

Michael Scanlan's article discusses the American Postulate Theorist's influence on Alfred Tarski's foundational work around the turn of the 1930s (pp. 307–325). The American Postulate Theorists comprised a group of philosophers, mathematicians, and logicians (e.g., Veblen, Sheffer, Langford), who all shared an interest in extending the application of the axiomatic method to mathematical theories such as number theory, Boolean algebra, and Euclidean geometry. The group preferred to speak of "postulate sets" rather than "axiom sets." This preference was motivated by an intention to avoid confused historical associations of the term "axiom" (p. 308). Scanlan concentrates on Langford's contributions and concludes that, at least in his case, it is safe to say that work in the tradition of American Postulate Theory had "an influence of *a certain sort*" on Tarski (p. 315, emphasis by MS). According to Scanlan, Langford succeeded in "defining the nature of the problem Tarski would set out to solve with his semantic methods" (p. 323). Even if this conclusion is perhaps not altogether earth shattering, it is certainly worth noting. Moreover, Scanlan's carefully composed essay includes an interesting exposition of Langford's methods (pp. 318–322).

The last contribution to the first part of the Festschrift is Joseph Dauben's richly illustrated account of the introduction of modern mathematics to China (pp. 327–363). During the Cultural Revolution abstract pure mathematics was condemned in China for being irrelevant to the worker. However, in the 1970s, Chinese mathematicians found an opportunity to link

Marxist ideology and the foundations of mathematics with the help of Karl Marx's critique of the calculus, as presented in his ***Mathematical Manuscripts***, and Abraham Robinson's nonstandard analysis. According to Dauben, this application of Robinson's novelty "not only rehabilitated infinitesimals in a technical sense, but [. . .] could be used to justify and promote two new fields of study in China—model theory and nonstandard analysis" (p. 359). This development not only offered to Chinese mathematicians a route for rehabilitation, but also led to a general reform of mathematics in China (p. 337). Dauben's essay is an important reminder of the remarkable resilience of mathematics in China during the time of the Cultural Revolution.

The second part of the Festschrift is opened by Volker Peckhaus's historical re-evaluation of Jean van Heijenoort's famous distinction between logic as calculus and logic as language (pp. 3–14). Van Heijenoort introduced this distinction in 1967 by comparing Gottlob Frege's and Leopold Löwenheim's conceptions of logic in the light of the Leibnizian notions of *calculus ratiocinator* and *characteristica universalis*, thereby laying the foundations for what can be called the received view of the history of modern logic as a contest between the universalists, as represented by Frege and Russell, and the algebraists, as represented by Boole, Schröder, and Löwenheim. Van Heijenoort based his distinction on quantification theory, which he found missing in the algebraic tradition. Peckhaus shows that van Heijenoort was mistaken in this respect, and therefore quantification theory cannot be considered an adequate criterion for splitting the modern history of logic in two dominant traditions. What is more, he points out that both Frege and Schröder criticized the other's system as being a mere general calculus of reasoning, and on the other hand regarded their own systems as a better realization of the Leibnizian dream of a common language of thought (p. 10).

Peckhaus's contribution is followed by Albert C. Lewis's discussion on Hermann Günther Grassmann's theme of unifying logic, pedagogy, and the foundations of mathematics (pp. 15–36). Lewis pays special attention to Grassmann's life-long career as a high-school professor (*Oberlehrer*) and concludes that Grassmann's mathematical creativity must have been strongly motivated by his calling to teach mathematics (p. 34). Lewis's paper is an important contribution to a better historical understanding of Grassmann's thought.

Jan Woleński's survey of the reception of Frege's ideas in Poland during the early 20th century wraps up the Festschrift (pp. 37–51). Woleński shows that Frege had a much better and more profound reception in Poland than in most other countries. Moreover, he emphasizes that even though it was "Frege the logician" and not "Frege the philosopher" who made an impression on Polish logicians and philosophers, Frege's work was much more popular among philosophers than among mathematicians (pp. 47–48). In Woleński's words, "Frege was discovered by a philosopher (Russell) and welcomed by logicians with a solid philosophical background" (ibid.). Woleński's well-documented article fills in a detail in the history of Polish logic and corrects the mistaken idea about Frege's infinitesimal fame during the first decades of the 20th century.

The ***HPL*** Festschrift in honour of Grattan-Guinness is an enjoyable treasure trove for all of those who are interested in the history and philosophy of formal sciences.

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