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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.

The Reviews Section is edited by Alasdair Urquhart (Managing Editor), Lev Beklemishev, Mirna Džamonja, David M. Evans, Erich Grädel, Geoffrey P. Hellman, Denis Hirschfeldt, Julia Knight, Michael C. Laskowski, Roger Maddux, Volker Peckhaus, Wolfram Pohlers, and Sławomir Solecki. Authors and publishers are requested to send, for review, copies of books to *ASL, Box 742, Vassar College, 124 Raymond Avenue, Poughkeepsie, NY 12604, USA.*

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).

LUCA VIGANO. ***Labelled non-classical logics,*** With a foreword by Dov Gabbay, Kluwer Academic Publishers, 2000, 291 pp.

The book combines the approach of Natural Deduction with the notion of labelled formulas (in Gabbay's style) and applies it to some non-classical logics, namely those (two-valued) logics admitting some sort of Kripke semantics of possible worlds, notably modal logics and relevant logics. Possible worlds serve as labels: if w is a possible world and A a formula then $w : A$ is a labelled formula called left well formed formula; if w, w' are possible worlds and R denotes the (accessibility) relation of a Kripke model then xRw' is a right well formed formula. A system $N(K)$—normal deduction variant of the modal logic K is presented. Combined with any Horn theory $N(T)$ of R it gives a system $N(L) = N(K) + N(T)$ complete w.r.t. a set of deduction rules and a natural Kripke semantics. Similarly for logics stronger than K, namely D, T, B, $S4$, $S4.2$, $KD45$ and $S5$. Results on normalization and subformula property are proved. Corresponding labelled sequent systems are defined.

Then this is generalized to a Kripke model with several accesibility relations of arbitrary arity, each admitting a universal and existential interpretation. First a base system $N(B)$ is defined and soundness and completeness od each $N(B) + N(T)$ ($N(T)$ as above) is proved. As a prominent example, various kinds of relevance logic (with a ternary R) are studied. All the systems discussed till now are implemented in the theorem prover Isabelle.

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Part II is substructural and complexity analysis of modal sequent systems, with results on elimination or bounding contractions in K, R, $K4$ and $S4$ and showing that the proof search for those logics is in PSPACE.

The book is very interesting and presents many deep results. I have one critical remark and one suggestion.

In this foreword Gabbay expresses his opinion that the book pioneers the way modal logic is going to be studied in the future. I do not object; but if I imagine a reader who knows well modal logic and even has read some proof theory I must admit that such reader will have problems.

(a) The deduction rules sometimes contain formulas in square brackets; their meaning is commented on p. 19. But definition 2.1.11 (on p. 26) of a derivation of a formula is too sketchy (tree formed using the rules and depending only on $\Gamma \cup \Delta$). Here the reader will have to guess several things. My advice to such reader is: read the proof of soundness (p. 31), this will help.

(b) When introducing the relevance logic, the novice will wonder what is the intuitive meaning of the ternary relation. This can be (slowly) uncovered from the rules, but some comments would be very helpful.

I repeat that the book is interesting and deep; let me close with one question. Many-valued logics (fuzzy logics) are not mentioned; but they can be also seen as labelled logics, $w : A$ (where w is a truth value) being read "A is at least w-true". See e.g., G. Gerla, ***Fuzzy logic*** (BSL IX 510). How far can the methods of labelled logic be usefully applied to fuzzy logics? My suggestion is: try to clarify.

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GEORGE BOOLE. ***The mathematical analysis of logic. Being an essay towards a calculus of deductive reasoning by George Boole — Die mathematische Analyse der Logik. Der Versuch eines Kalküls des deduktiven Schließens von George Boole,*** Schriftenreihe zur Geistes- und Kulturgeschichte: Texte und Dokumente. Hallescher Verlag, Halle, Saale, 2001, 195 pp.

George Boole's mathematical and philosophical work has experienced a remarkable renaissance during the past two decades. Desmond MacHale's detailed biography of Boole was published in 1985. A comprehensive selection of Boole's manuscripts on logic and its philosophy came out in 1997, followed three years later by an important collection of both classical and recent studies on his life and thought (Ivor Grattan-Guinness & Gérard Bornet, eds., ***George Boole: Selected Manuscripts in Logic and Its Philosophy***, Birkhäuser, Basel 1997; James Gasser, ed., ***A Boole Anthology. Recent and Classical Studies in the Logic of George Boole***, Kluwer, Dordrecht 2000). In addition, his epoch-making ***The Mathematical Analysis of Logic*** has been republished several times during the past few years (1996, 1998, 2001). The 2001 edition of this work provides the reader not only with the original text, but also with the first complete German translation on parallel pages (pp. 8–161), followed by an epilogue by the translator Tilman Bergt (pp. 163–186), Boole's complete bibliography (pp. 190–194), a list of related literature (pp. 187–189), and a modest chronological table (p. 195).

Toward the end of the 17th century, Gottfried Leibniz advanced the idea that algebraic formulae might be used to express logical relations. However, he faced insuperable problems in using his idea as the basis of a logical calculus. Indeed, it took more than one hundred and fifty years before Boole finally achieved Leibniz's objective of systematically developing an algebra of logic with his first monograph ***The Mathematical Analysis of Logic*** (1847). In William and Martha Kneale's words, "with his genius for generalization [Boole] saw that an algebra could be developed as an abstract calculus capable of various interpretations"

(William & Martha Kneale, ***The Development of Logic***, The Clarendon Press, Oxford 1962, p. 405).

But why such a buzz about Boole's ***The Mathematical Analysis of Logic***, especially since it does not even represent Boole's final word on the subject? His later contributions contain significant changes to his system of 1847 (see, e.g., Ivor Grattan-Guinness, *On Boole's Algebraic Logic after The Mathematical Analysis of Logic*, in Gasser, ed., 2000, pp. 213–216). Even Boole himself emphasized the difference between his early and mature views. In 1851, in an article on probability theory, he even regretted the publication of his first monograph. However, in their important survey of Boole's criteria for validity and invalidity, John Corcoran and Susan Wood have cited three good reasons for paying close attention to ***The Mathematical Analysis of Logic*** (***Notre Dame Journal of Formal Logic***, vol. 21 (1980), pp. 609–638). First, they point out that in order to form a clear and coherent picture of the development of Boole's thought throughout his career, it is necessary to understand his thought at each stage. Secondly, there is no justification for assuming that every modification in Boole's theory was indisputably a change for the better. Thirdly, Corcoran and Wood want to remind those students of Boole's work whose interest is limited to his final views that these may de determined with the aid of comparisons with his earlier views, particularly as Boole himself never made it explicit what he took to be errors in ***The Mathematical Analysis of Logic***.

It is easy to subscribe to the observations made by Corcoran and Wood. Moreover, as Tilman Bergt correctly emphasizes (p. 163), ***The Mathematical Analysis of Logic*** is both interesting and illuminating also by the mere fact that it marked the beginning of a still on-going development of mathematical logic and paved the way for the rise of computer science. And last but not least, the present English-German edition is well justified simply because ***The Mathematical Analysis of Logic*** has been previously translated only into Polish, French, Italian and Spanish.

This bilingual edition of ***The Mathematical Analysis of Logic*** has more to offer than just George Boole's first monograph in two different languages. It also includes interesting bio- and bibliographical material, as well as an elaborate epilogue by Tilman Bergt. After a short sketch of Boole's life, Bergt discusses three themes which were central to Boole's pursuit of the algebra of logic: (1) the role of language from the viewpoint of the foundations of mathematics and logic, (2) the algebraization of mathematics, and (3) the formalization of logic (p. 164). With regard to the first theme, Bergt emphasizes that in Boole's view language should be seen as an instrument for accurate thinking. Moreover, he argues that even though Boole undeniably held that the laws of both natural and artificial languages mirror the logical laws of thinking, it is false to conclude, as it has been suggested, that Boole attempted to extract the laws of logic from the natural languages (pp. 165–169). Bergt also opposes the widespread psychologistic interpretation of Boole's philosophy. Bergt locates Boole's contributions to the algebraization of mathematics in an illuminating brief sketch of the history of mathematics from the late 17th century until the early 19th century. He correctly points out that Boole was influenced in an important way by Joseph Louis de Lagrange and by the members of the famous Analytical Society, George Peacock in particular (pp. 169–176). Before concluding his epilogue, Bergt provides the reader with an instructive discussion of the formal foundations of Boole's logical system as set forth in ***The Mathematical Analysis of Logic*** (pp. 179–185).

All in all, the new English-German edition of ***The Mathematical Analysis of Logic*** is a welcome, well-organized, and handy piece of work for all of those who are interested in excavating the antecedents of today's mathematical logic and computer science.

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SERGIO FAJARDO and H. JEROME KEISLER. ***Model theory of stochastic processes,*** Lecture Notes in Logic, vol. 14. Association for Symbolic Logic, A K Peters, Ltd., Natick, Massachusetts, 2002, xii + 136 pp.

Nonstandard analysis has found some of its most interesting and deepest applications in stochastic analysis. An early landmark is the 1976 paper of Robert Anderson, (JSL L 243), in which Brownian motion and the Itô stochastic integral are given a very appealing intuitive construction in terms of random walks with infinitesimal steps. This, and many other applications of nonstandard analysis use the fundamental construction of Peter Loeb (JSL L 243) that converts an internal measure into an external σ-additive measure. Subsequently, these advances were consolidated in a general theory of stochastic processes based on nonstandard analysis. Keisler was among the principal contributors; his monograph of 1984 (JSL LI 822) proves the existence of strong solutions of a very general class of stochastic differential equations in suitable hyperfinite adapted spaces. Subsequent developments include the work of Douglas Hoover on the model theory of stochastic processes (which forms one of the main topics of the book), and the development of adapted probability logic by Keisler, Hoover and Fajardo.

The present monograph is a general study of stochastic processes on adapted spaces using the notion of adapted distribution; it combines ideas from stochastic analysis, model theory and nonstandard analysis. The authors describe one of their goals as follows: "From the viewpoint of nonstandard analysis, our aim is to understand why there is a collection of results about stochastic processes which can only be proved by means of nonstandard analysis" (p. x). This remark may seem surprising to logicians with some acquaintance with nonstandard techniques, since it is a fundamental result that nonstandard analysis is a conservative extension of standard mathematics. The explanation for this apparent discrepancy is that the authors are working in adapted spaces derived from hyperfinite adapted spaces by the Loeb construction. These spaces are much richer than the spaces of conventional probability theory, and allow a much wider variety of constructions. For example, there are examples of stochastic differential equations that have no strong solution in the usual standard space of continuous functions from $[0, 1]$ into R with the Wiener measure (M. T. Barlow, ***Journal of the London Mathematical Society***, ser. 2 vol. 26 (1982), pp. 335–347), but have strong solutions in Loeb adapted spaces, by the results of Keisler.

It is essential, of course, that these solutions should be considered as legitimate as solutions in the standard spaces. The fact that this is so reflects a general attitude of stochastic analysts; they are not fussy about the underlying probability space, provided that the process in question has the appropriate distribution. In the basic case of random variables, we can say that two random variables are "alike" if they induce the same probability distribution on the Borel sets. Similarly, two real valued stochastic processes are "alike" if they have the same finite dimensional distributions.

However, the main focus of the present monograph is adapted spaces. In these models, the single probability space of the basic theory of stochastic processes is replaced by a time-indexed increasing family of σ-algebras (a filtration) representing the increase in information through time. This setting, basic to recent probabilistic work, allows the application of martingale techniques. Here, there is no generally agreed notion of what it means for two stochastic processes on adapted spaces to be "alike". The appropriate definition of this concept is one of the main themes addressed in the monograph.

Fajardo and Keisler's solution to the problem is given by the notion of adapted equivalence. They define the class of adapted functions relative to an adapted space inductively as those functions obtained by repeatedly acting on a random variable x by applying bounded continuous functions to the random variables x_t and taking conditional expectations with respect to algebras in the filtration. Then two stochastic processes x and y on adapted spaces are "alike" if for every adapted function f, the random variables $f(x)$ and $f(y)$ have the same expeced value. The relation $x \equiv y$ defined in this way is called "adapted equivalence". It is similar to, but stronger than, an earlier notion ("synonymy") defined in an unpublished 1981 paper of David Aldous. The authors also define a stronger notion of isomorphism between processes; two processes x and y on (possibly distinct) adapted spaces are said to *isomorphic*, $x \simeq y$, if there is an adapted isomorphism h between the spaces so that for all $t \in [0, 1]$, $x(\omega, t) = y(h(\omega), t)$ almost surely.

Fajardo and Keisler prove a number of results, all of which are intended to show that adapted equivalence is the "right" notion of equivalence. In Chapter 4, they also discuss a whole family of weaker and stronger notions of equivalence, including a stronger notion of game equivalence.

The other main topic of the monograph, in addition to the equivalence problem discussed above, is a more general project of understanding exactly why the Loeb construction is so rich and fertile in its consequences, frequently succeeding where classical constructions fail. The authors' explanation for this is found in three properties inspired by classical model theory. They show that hyperfinite adapted spaces are universal, homogeneous and saturated (saturation in fact is a consequence of the first two properties). Although these properties are inspired by their model-theoretic versions, they are defined here in probabilistic terms, using the notion of adapted equivalence.

An adapted space Ω is defined to be *universal* if given any Polish space M and M-valued stochastic process y on an arbitrary adapted space Γ, there is an M-valued stochastic process x on Ω such that $x \equiv y$. Ω is *homogeneous* if for every pair of stochastic processes x and y defined on Ω, $x \equiv y$ implies $x \simeq y$. Ω is *saturated* if for every pair of stochastic processes, x, y on an arbitrary adapted space Λ and every stochastic process x' on Ω such that $x' \equiv x$, there exists a stochastic process y' on Ω such that $(x', y') \equiv (x, y)$ (where this last relation represents equivalence of M^2-valued processes, assuming that x, y, x', y' take values in a Polish space M).

The fact that hyperfinite adapted spaces have these three powerful and fecund properties lies at the core of their success in stochastic analysis. In contrast, the classical spaces can be shown to fail here. The authors define an *ordinary probability space* to be a probability space $(\Omega, \mathcal{F}, \mu)$ where Ω is a Polish space and μ is the completion of a probability measure on the family of Borel sets in Ω. An *ordinary adapted space* is an adapted space that is ordinary as a probability space. The authors show that no ordinary adapted space can possess any of the three properties; in fact, they show even stronger negative results. Although an ordinary probability space can be universal (since a probability space is universal if and only if it is atomless), no ordinary probability space can be saturated or homogeneous.

In the later chapters of the monograph, the authors develop the theory of rich adapted spaces and neometric adapted spaces. The object of these theories is to show how some of the benefits of nonstandard analysis can be obtained without going through the normal logical apparatus. In rich adapted spaces, for example, the usual "lifting procedure" familiar to nonstandard analysts can be replaced by a short cut, based on a countable compactness principle. The authors remark that: "It makes it possible to enjoy many of the benefits of nonstandard methods without paying the normal entry fee" (p. 100).

This last quoted remark shows the authors' desire to persuade mathematicians to see the advantages of nonstandard analysis. They remark: "It has been very hard to convince the mathematical community of the attractive characteristics of nonstandard analysis and its possible uses as a mathematical tool" (p. 23). Let us hope that the authors succeed in their goal of spreading the word about the great potential of the theory of infinitesimals to the wider mathematical community.

Although many of the concepts in this monograph were inspired by model theory, logical formalism is completely avoided here. The concepts of universality, homogeneity and saturation are derived from the corresponding concepts in adapted probability logic. The monograph might have been easier for logicians to read if the authors had indicated briefly the logical versions of their basic notions. However, this is a minor quibble. This is a masterly monograph by two leading researchers that deserves to be read by all mathematicians interested in the foundations of probability theory and stochastic analysis.

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STEPHEN WOLFRAM. ***A new kind of science,*** Wolfram Media, Inc., Champaign, IL, 2002, xiv + 1197 pp.

Wolfram proclaims the investigation of A New Kind of Science. The study of computer generated mathematical phenomena is surely a kind of science which arose in the last 20 years. While investigations in e.g., complex dynamical systems have pursued the 'old' scientific methods of using (computer-generated) examples to motivate precise mathematical definitions and prove theorems, Wolfram takes a more descriptive approach. On the basis of examining literally billions of configurations he proclaims certain uniformities. There is no attempt in the book to either define or prove in the usual mathematical sense of the term. Indeed, identifying the perspective from which he writes poses a major difficulty for understanding. 'Randomness' is a central concept of the book. But it is only on pages 512–516 that one learns that Wolfram means by random a rather loose notion of pseudorandom: meets common statistical tests. Rather than guessing his meanings I use words as they are commonly used by logicians. Accordingly, I will describe as 'conjectures' certain statements which Wolfram characterizes as 'proven' or just asserts are true. References to some of the best of the many reviews of this book appear in *A mathematician looks at Wolfram's new kind of science* by L. Gray in ***Notices of the American Mathematical Society***, vol. 50 (2003), pp. 200–211; I confine myself to issues raised that have a specifically logical tinge as they concern the foundations of computation. In particular, I am avoiding any issues in the philosophy of science.

In general a cellular automaton acts on a k-dimensional space (Z^k) with each cell labeled by one element of a finite vocabulary V, by simultaneously updating each cell in accord with a rule that uses only the contents of the cells within radius r. More particularly, the basic objects of most of Wolfram's analysis are *pictures* generated as follows. A two state one-dimensional radius 1 cellular automaton acts on an (in practice finite) string of black and white cells. At each stage each cell is updated by a rule which depends on the previous value of itself and its immediate neighbors. Each iteration represents one row in a two dimensional picture. Wolfram developed a useful scheme of numbering these automata. For example, Rule 110 preserves the color of a cell unless the cell is white and one or both of its neighbors is black.

We consider six 'conjectures', which are discussed in the book.

CONJECTURE 1. *There is a two state, one dimensional, radius one cellular automaton* (*rule* 110) *which is capable of universal computation.*

The truth of this assertion depends on the Church's thesis like problem of defining 'universal computation'. The ability to simulate a universal Turing machine is a weaker demand than the ability to simulate a universal cellular automaton (which has infinite input). Matthew Cook has provided a mathematical argument that rule 110 can simulate all Turing machines. Even this is an extraordinary result. It says that one can simultaneously minimize all three parameters of cellular automata complexity (vocabulary, dimension, radius) and still have a universal machine for computing recursive functions. Wolfram provides a sketch of the argument, which is due to Cook. (Read Gray *op. cit.* for an account of the fascinating controversy concerning ownership of this result.) In contrast to earlier constructions of Turing machines designed to be universal, Cook takes a given simple machine and argues that it is universal.

But there are further logical questions about the meaning of universal, which are raised, for example by B. Durand and Zs. Rókas *The game of life: universality revisited*, in Delorme and Mazoyer, editors, ***Cellular Automata: A parallel model***, vol. 460 of Mathematics and Its applications, Kluwer Academic, 1998. The essential problem is whether the encoding and decoding might do all the work leaving a 'universal machine' which is a fraud. This particular encoding and decoding is carefully described by Cook, though not in Wolfram's book. The preprocessor is given a tape with a code for a Turing machine program and an input. Successively encoding this information by a tag system and a cyclic tag system, we arrive at the input for an infinite tape. It consists of a finite segment and cyclic information at each end. The finite segment and the cycle depend on *both* the program and the specific input. This type of coding appears to be accepted in the 'small universal Turing machine' community. But three issues arise. First, give technical arguments that in a precise sense the coding and decoding do not add too much information. (Martin Davis did this for Turing machine computability in *A note on universal Turing machines*, in C. E. Shannon and J. McCarthy, editors, ***Automata Studies***, vol. 34 of Annals of Mathematical Studies, pp. 167–176, Princeton University Press, 1956.) Second this simulation requires an exponential slow down (cf. B. Durand and Zs. Rókas *op. cit.*). Finally, even if the coding and decoding are considered technically benign, the supposed simplicity of rule 110 is impaired by the recognition that it uses extensive problem-dependent pre- and post-processing.

CONJECTURE 2. *Rule* 30 *produces pseudorandom sequences.*

Since this is the tool for 'random generation' in Mathematica, this must be true in the 'meets statistical tests' sense of Wolfram. But it is unclear whether any formal argument for computational indistinguishability in the sense of e.g., O. Goldreich, (***Modern Cryptography, Probabilistic Proofs, and Pseudorandomness***, Springer-Verlag, Berlin-Heidelberg-New York, 1999) has been given.

Wolfram's consistent generalization to 'all cellular automata' of properties discussed only for low dimensional automata amounts to:

CONJECTURE 3. *The behavior of* 1 *and* 2 *dimensional cellular automata of small radius and vocabulary is typical of the behavior of all cellular automata.*

I almost write assumption rather than conjecture for this item; the only hint at an argument for it is that such complexity is found in these cases that surely they must represent all possibilities.

CONJECTURE 4. *All cellular automata can be classified into* 4 *categories.*

This conjecture has two parts.

CONJECTURE 4a. *There are four types of pictures.*

The types are described vaguely and perhaps inconsistently at various points in the book. In short the pattern dies out, is periodic, random, or 'exhibits complicated structure'. The inconsistency arises as in the first chapter 'self-similar' patterns are identified. These appear highly determined but seem to be later classed as 'random'. (Does this inconsistency disappear with the Wolfram meaning of 'random'?) In Wolfram's earlier ***Cellular Automata and Complexity***, Addison Wesley, 1994, several more precise attempts at a definition are presented; this precision has disappeared in this book, which is intended for a popular audience. Gray *op. cit.* suggests at least 6 or 7 different types of pictures.

CONJECTURE 4b. *There are four types of cellular automata.*

A picture depends on a cellular automata (rule) and an input. Culik and Yu *Undecidability of ca classification schemes*, ***Complex Systems***, vol. 2 (1988), pp. 177, translated the classification of pictures to a classification of automata by a 'worst case' analysis; Baldwin and Shelah *On the classification of cellular automata*, ***Theoretical Computer Science***, vol. 230 (2000), pp. 117–129, showed that a classification of the form, "rule x is class y if with probability z it has class y behavior" is not possible. Wolfram takes a decidedly biological view of the classification. 'And in a sense the situation was similar to what is seen, say, with the classification of materials into solids, liquids, or gases, or of living organisms into plants and animals. At first a classification is made on the basis of general appearance. But later, when more detailed properties become known, these properties turn out to be correlated with the classes that have already been identified. Often it is possible to use such detailed properties to make more precise definitions of the original classes. And typically all reasonable definitions will then assign any particular system to the same class.' (p. 235). Unfortunately, there is little evidence in the book for the last sentence.

CONJECTURE 5. *The above classification of automata determines their computational power.*

Class 4 automata are asserted to be 'capable of universal computation'. There are serious problems with this conjecture. As noted above one has to make both the classification and the notion of universal computation precise. Easy operations on cellular automata preserve universality (in some sense) but change the classification. Wolfram makes no distinction between the ability to *simulate* an arbitrary Turing machine (cellular automata) and the ability to *compute* every recursive function. Wolfram freely uses the term universal computation when (I believe) universal simulation is meant. On page 717 of the book, we find:

CONJECTURE 6. *The principle of computational equivalence*: *'almost all processes that are not obviously simple can be viewed as computations of equivalent sophistication'*

Process here is an open-ended term including physical processes. But it seems quite strong enough for logical purposes if we restrict ourselves to cellular automata. In particular this should imply a precise conjecture that most (all) class 4 automata are 'computationally equivalent'. More stunning to a logician is that, as noted by H. Cohn in *Review of Wolfram: A New Kind of Science*, ***MAA online***, 2002, this leaves no room for the intermediate degrees. Perhaps, Wolfram's intent is that the machines which compute intermediate degrees are 'unnatural'. Or perhaps this is an effect of the fundamental divergence between the meaning of 'compute' for, say, Turing and for Wolfram. A Turing machine computes a function. For Wolfram, a cellular automaton just computes. He raises a fundamental question of comparing two computing processes, not some classical function that they compute.

In summary, Wolfram provides a number of fascinating insights and speculations into the behavior of cellular automata. Formalizing and determining the truth of these suggestions could be a rewarding task for logicians. In particular, (i) give a satisfying formalization of the notion of a *universal* cellular automata, (ii) find, if possible, formal connections between the dynamic behavior of computation by a machine (roughly Wolfram's classification) and the complexity of computations it can do. Cellular automata may become a paradigm for parallel computing complexity as Turing machines are for serial computing complexity. But to explore that avenue, look not at Wolfram but at complexity theory work as in the volume edited by Delorme and Mazoyer, ***Cellular Automata: A parallel model***, vol. 460 of Mathematics and Its Applications, Kluwer Academic, 1998.

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Automata, logics, and infinite games: A guide to current research, edited by Erich Grädel, Wolfgang Thomas, and Thomas Wilke, Lecture Notes in Computer Science, vol. 2500 (Tutorial). Springer-Verlag, Berlin Heidelberg, 2002, viii + 385 pp.

In the 60's, Büchi and Rabin proved the decidability of monadic second order logic (MSO) on infinite words and infinite trees respectively. These two deep results of mathematical logic were obtained by means of automata theoretic characterizations of the expressive power of MSO on these structures. In the late 70's, it was realised that infinite words or trees can be used as models of the behavior of computer systems. Consequently, the results of Büchi and Rabin, with the underlying theory of automata on infinite structures, became of direct interest to computer scientists. Since then, a considerable amount of research effort has been devoted to the development of the theory towards its application in computer science. This effort has produced a large number of deep and mathematically appealing results that form, altogether, *the mathematical basis for the modeling, analysis and synthesis of discrete non-terminating systems*.

The book ***Automata, Logics and Infinite Games***, edited by Erich Grädel, Wolfgang Thomas and Thomas Wilke, provides, in a progressive and didactic order, a tutorial in this field.

It is, at first sight, an advanced, and quite complete, course on automata theory on infinite structures and its connections with mathematical logic. Most results in this field, which were previously scattered among dozens of research papers, are now presented in the nineteen chapters of this single volume. The notes following each chapter not only give references to original papers but also survey the related results that may have been omitted in the text.

The book is however not limited to this. The deep connections that exist between automata and fixedpoint formulas are also addressed in detail. Major results about Kozen's mu-calculus are carefully stated and proved. Recent extensions of the field such as mu-calculus on tree-like structures and guarded logics are also presented in the last chapters of the book.

An important feature of this tutorial is also that infinite state-based two player games are presented and used, across all chapters, for modeling the interaction between automata (resp. fixedpoint formulas) and their inputs (resp. models). This allows a fairly uniform description of automata semantics and results in this (quite new) part of games theory simplify many proofs.

As intended by the editors, the book *does* provide a theoretical basis for the construction and analysis of reactive programs.

Major algorithmic and complexity issues related to automata determinization, complementation or emptiness are carefully detailed in this book. Special attention is paid to the various definitions of infinite acceptance conditions (describing how a *finite* automaton may "accept" its infinite input) and the way these definitions interplay with the expressive power and the complexity of the associated automata. These aspects of the theory are crucial for building efficient software tools for verifying or synthesizing computer systems.

The extension of the whole theory to tree-like structures and guarded fixedpoint logics also widens the scope of application. It allows more elaborate models of behaviors than just words or trees.

Although more hypothetically, the rich game-theoretic framework presented in this book is also promising for the design and analysis of computer systems. In fact, infinite turn-based games are simple and elegant models of the ongoing interaction between programs and their environments.

No doubt this book is an invaluable source for (self-) training and will soon become a major reference. Many open problems are mentioned in the text. Following the editors' own preface, we can only share the wish that this book will even guide the reader to the solution of these problems.

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John Woods. ***Paradox and paraconsistency: Conflict resolution in the abstract sciences,*** Cambridge University Press, Cambridge, New York, 2003, xviii + 362 pp.

The following proof, attributed to Lewis and Langford, purports to show that anything follows from a contradiction:

(1)	$\varphi \wedge \neg\varphi$	Hypothesis
(2)	φ	(1), Simplification
(3)	$\varphi \vee \psi$	(2), Addition
(4)	$\neg\varphi$	(1), Simplification
(5)	ψ	(3), (4), Disjunctive Syllogism

Classical logicians accept the proof, and conclude that *ex falso* is a legitimate rule of inference; relevance logicians oppose the validity of Disjunctive Syllogism, and do not accept *ex falso*. Here we have a classic case of a *conflict* in logic: and it is such conflicts that are considered in Woods's book.

For Woods, the *abstract sciences*, such as logic, mathematics, and set theory, are sciences that have no empirical checks to help resolve conflicts: no experiments seem useful in resolving the dispute over *ex falso*. Woods fears that, without any other conflict resolution strategies, abstract theories are in danger of losing their grip on *objectivity*, and laments a number of developments that acquiesce to antirealism in the abstract sciences: *logical pluralism*, which sees distinct logics as equally acceptable; the *received view* of the significance of the set- and truth-theoretic paradoxes, according to which the paradoxes show that there *is no concept* of a set (or of truth); and *stipulationism*, which attempts to recover from the last conclusion by *stipulating* that such-and-such claims will be true of our new successor notion of 'set' or 'truth'. He sees stipulationism as the height of postmodernism in the philosophy of logic and mathematics, with its condition on acceptable stipulations in mathematics: "A stipulation is acceptable to the extent that the right people are disposed to believe it." (p. xiv)

One possible recovery for realism is *naturalism*, which rejigs the concept of a set not so as to preserve as much of the old concept as possible, but rather "with a view to facilitating the broader aims of mathematics, broadly indispensable in turn, to science." (p. xv) But Woods fears that, once up and running, "our best scientific accounts of how beings like us know the world show that we do so in ways that fulfill the canons of idealism." (p. xvi) Thus naturalism leads to *anomalous realism*: we retain the realist stance even when it delivers the goods for idealism. And this Woods decries as "*mauvaise fois*" (*sic*).

Woods's approach is to assume the realist stance, and to try to develop resolution strategies that do not allow the realist stance to regress into the bad faith of anomalous realism. His book mixes general discussions of resolution strategies and various dialectic positions a theorist might take with specific case studies—in particular disputes in modal logic, disputes concerning *ex falso*, disputes about sets (that arise in response to Russell's paradox), and disputes about truth (that arise in response to the Liar's paradox). Woods identifies a number of resolution strategies, but two main strategies stand out: the method of analytic intuitions, and the method of costs and benefits.

Much of the book is a sustained attack on the method of analytic intuitions. This method, which just seems to be traditional conceptual analysis, is primarily a method of theory development: We identify certain of our beliefs about sets or about implication as constitutive of the concept, and develop our theory around those beliefs. Two problems can emerge. (1) In the case of competing logics (or even competing post-Russell-paradox set theories), different theorists take different intuitions to be 'analytic'. A conflict arises when theorist X's 'analytic' intuitions entail something which contradicts theorist Y's 'analytic' intuitions. X's surprising (or unsurprising) result is Y's counterexample, and X and Y find themselves in a position of question-begging stalemate: The method of analytic intuitions is an impediment rather than an aid to conflict resolution, since neither X nor Y can see how to give up concept-constituting beliefs. The method *can* be sustained when supplemented by a *reconciliation* strategy, for example, one that disambiguates, and offers logic X as a theory of implication, and logic Y as a theory of inference. (This might include a distinction between implication, as a truth-function, and inference, as a function from belief-stocks to belief-stocks.) But too often such disambiguation is impossible, and we are left with stalemate. (2) In the face of a contradiction arising from a single theory (a kind of conflict of the theory with itself), a conceptual analyst is apt to turn to *pessimism*: to conclude that there was no concept there to begin with, no concept of 'set' for example. This invites the stipulationism that Woods finds too post-modern for comfort. Another response to such a contradiction is to embrace the contradiction, but to soften that position with a contradiction-tolerant paraconsistent logic. We return to this in the next paragraph.

Woods emphasizes the importance of intuitions, but suggests a more modest approach to them. None should be taken as sacrosanct: rather than beliefs with an epistemic privilege of a distinctive concept-constituting *kind*, they are merely very strong beliefs that we should take especially, though defeasibly, seriously. And in sorting through these often-conflicting intuitions, he seems to plumb for his other method, the method of costs and benefits. Oddly, though this method is first introduced on page 6, the idea of "following the course with the least *costs*" is first mentioned (as such) on page 71, and the first thing that looks like a clear weighing of costs and benefits of rival abstract theories occurs on pp. 167–170. Woods's first serious characterization of this method, beyond its name, is on page 18: "At the heart of the cost-benefit approach to conflict resolution is Locke's *argumentum ad hominem*." *Ad hominem* is not a *fallacy* but rather a set of strategies for bringing out tensions in an opponent's position. This seems very much like the give and take of any philosophical argumentation, and it is unclear what it has to do specifically with costs and benefits. Woods first introduces specific costs and benefits when discussing paraconsistent set-theory as a solution to Russell's paradox (pp. 167–170). The main cost of ZF is its putative unnaturalness and ad-hocness. The main cost of paraconsistent set-theory is that it crimps Disjunctive Syllogism. A potential benefit of paraconsistent set-theory is a more faithful account of paradoxicality, and its faithfulness to the idea that logic ought to be "the handmaiden of all sciences, including mathematics" (p. 170). Also, as far as we now know, it might also turn out to be "fruitful" (p. 169) and "easier to work with and fully equipped to discharge its standing obligations to number theory and the other mathematical sciences" (p. 170). Elsewhere, Woods mentions the complexity of the decision-problem for non-classical logics as a potential cost.

My worry about all of this is that the method of costs and benefits is no more clearly a friend to non-anomalous realism than is the method of analytic intuitions. Suppose that Theory X is easier to work with than Theory Y and that the logic underlying X has a simpler decision problem. Suppose that the weight of costs and benefits favours X. What do these anthropocentric issues have to do with *truth*? While we might have inductive or other evidence that simpler *physical* theories tend to be better approximations of the truth than more complex ones—we might even have physical explanations for this—I am more pessimistic about the claim that low-cost *abstract* theories tend to be better approximations of the truth than high cost ones, *especially* when the costs and benefits are themselves so anthropocentric; and when the notion of *simplicity* is quite unlike the notion as it applies to physical theories. (How much should we care about the "simplicity" of the decision problem?) There is a related problem that Woods does touch on: Even in a cost-benefit analysis of conflicting intuitions, we have to contend with conflicting intuitions about what counts as a *cost* or a *benefit*, i.e., with conflicting values. So the problem of conflict-resolution is in danger of simply being moved to a different level.

While the book's project is an interesting one, I should point out its significant flaws. First, it is full of passages that appear to be straightforward blunders in argument or exposition. Consider, for example, the beginning of the exposition offered of one of Quine's attacks on modal logic (p. 48):

"*Objection One* (*Quine, 1953a*)
(1) $\exists x\, N(x > 7)$
is accessible to the Existential Instantiation rule. Here are two applications of it:
(2) N$(9 < 7)$
and
(3) N(The number of planets > 7)."

Unfortunately, no such Existential Instantiation rule is relevant, one way or the other, to Quine's objections to modal logic: As both Quine and modal logicians know, even classical logic allows no valid Existential Instantiation rule of the kind that Woods's exposition seems to need—i.e., no rule according to which you could infer Fc from $\exists x\ Fx$, for an arbitrary name c. (Incidentally, there is nothing under "Quine 1953a" in the bibliography.)

The book is peppered with similar infelicities—they are often subtler than the one just cited, or would require too much stage setting to bring to light here. And there are many passages that would require much effort to discern whether the passage contains a genuine argument or point, or is yet another infelicity. When such passages occur in the same book as the passage just cited, a cost-benefit analysis suggests that the effort would not be repaid. (For an example of the effort being made and confusions being exposed, see J. C. Beall's and David Ripley's sympathetic attempt to make sense of Woods's discussion of the Liar, in the ***Notre Dame Philosophical Reviews***, online at `http://ndpr.icaap.org/content/archives/2003/6/ripley-woods.html`.) The book is also marred by long-windedness, by the introduction of technical machinery of no clear use, by lengthy asides of no clear point, and by a very large number of typos, especially when the going gets technical.

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N. C. A. da Costa and F. A. Doria. *Consequences of an exotic definition for $P = NP$*. ***Applied Mathematics and Computation***, vol. 145 (2003), pp. 655–665.

To any first order theory T which contains a reasonable fragment of Peano Arithmetic we may associate computable functions $f : \mathbb{N} \to \mathbb{N}$ such that f is total in reality but T cannot prove that f is total. For instance, we may let $f_T(n)$ be $\sigma + 1$ where σ is the maximum of all values $\{e\}(k)$ such that $k \leq n$ and T proves that $\{e\}$ is a total function from $\mathbb{N}$ to $\mathbb{N}$ via a proof of length $\leq n$. The paper under review provides a failed attempt to exploit this phenomenon to show that $P = NP$ is consistent with the system *ZFC* of set theory.

An earlier such attempt was "On the consistency of $P = NP$ with fragments of *ZFC* whose own consistency strength can be measured by an ordinal assignment" by the same authors (available at `xxx.lanl.gov/abs/math.LO/0006079`). However, this attempt failed as well; a criticism may be found in "A note on an alleged proof of the relative consistency of $P = NP$ with *PA*" by the present reviewer (available at `xxx.lanl.gov/abs/math.LO/0007025`).

What the paper currently under review shares with the above-cited older one is that it only makes use of pretty elementary methods and that nevertheless crucial steps in the argument are not spelled out at all; in both cases, one such step just appears to be a gap for which no filling is in sight.

Let T be the theory *ZFC*, and let $f = \{e\}$ be a computable function, where e is a concrete integer. The authors define a Σ^0_2 sentence of the language of *ZFC* which they denote by $[P = NP]^f$ and which says that either $\{e\}$ is not total or else there is some integer m and a Turing machine with running time $n \mapsto O(n^{\{e\}(m)})$ (sic!) which decides a given NP-complete problem (the satisfiability problem for formulas of sentential logic, say). If id denotes the identity then $[P = NP]^{\mathrm{id}}$, abbreviated by $[P = NP]$, is a fair formal version of the statement that P equals NP.

If $f = f_T = f_{ZFC}$ is the function defined in the first paragraph above then it is less clear if $[P = NP]^f$ should count as a fair version of the statement that $P = NP$, but anyway if a strengthening of ZFC proves that f is total then it also proves that $[P = NP] \leftrightarrow [P = NP]^f$.

The alleged key results of the paper are supposed to be proven on pages 662 and 663 culminating in Corollary 4.6 which asserts that $[P = NP]$ is relatively consistent with ZFC. By what we have discussed so far it is clear that in order to verify Corollary 4.6 it would be enough to prove that for some $f = \{e\}$, $ZFC + [P = NP]^f +$ "f is total" is consistent, assuming the consistency of ZFC; to try and prove this is an ambitious challenge. Nevertheless, this is exactly what the authors pretend to have achieved by their "informal proof" of Proposition 4.4; it is here (on p. 662 lines 5 to 3 from bottom) where the crucial gap of the paper appears.

It is a short "proof" indeed for such a strong statement. If ZFC is consistent then so is $ZFC + [P = NP]^f$, just because ZFC can not prove that f is total (cf. the proof of Proposition 4.1). An appeal to the possibility of further adding a reflection principle to $ZFC + [P = NP]^f$ without introducing an inconsistency is then used for getting the desired result. This appeal remains a complete mystery. It is true that there are reflection principles which can be added to ZFC alone without introducing an inconsistency, but it is entirely dubious why this should also be possible for $ZFC + [P = NP]^f$. In fact, if ZFC were to prove $\sim[P = NP]$ (a widely held belief) then such a possibility would be ruled out.

In the author's model of $ZFC + [P = NP]^f$, a statement which they denote by [*ZFC is* Σ_1 *sound*] *fails* by their Lemma 4.3; [*ZFC is* Σ_1 *sound*] is supposed to follow from a reflection principle; therefore, that reflection principle also has to *fail* in their model of $ZFC + [P = NP]^f$. The conclusion should therefore really have been: the *negation* of said reflection principle is relatively consistent with $ZFC + [P = NP]^f$.

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