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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).

Games, logic, and constructive sets, edited by G. Mints and R. Muskens, CSLI Lecture Notes, vol. 161. CSLI Publications, Stanford, CA, 2003, xii + 128 pp.—therein:

Johan van Benthem. *Logic and game theory: Close encounters of the third kind*. Pp. 3–22.

Gabriel Sandu and Ahti Pietarinen. *Informationally independent connectives*. Pp. 23–42.

Hans van Ditmarsch, Wiebe van der Hoek, and Barteld Kooi. *Descriptions of game states*. Pp. 43–58.

Andreas Blass. *Resource consciousness in classical logic*. Pp. 61–74.

Grigori Mints. *Quick cut-elimination for monotone cuts*. Pp. 75–84.

Michael Rathjen. *The anti-foundation axiom in constructive set theories*. Pp. 87–108.

Sergei Tupailo. *On non-wellfounded constructive set theory: Construction of non-wellfounded sets in explicit mathematics*. Pp. 109–125.

This book is a mixed bag of seven short papers (about 10 to 20 pages each) arising from the 9th CSLI workshop on logic, language, and computation held in Stanford University in 2000. The book has three parts, roughly as indicated by its title. I mean only to be factual, not critical, when I say that the papers are mostly quite specialised and sometimes very technical, and their connection with papers in the other parts is (sometimes vanishingly) slight. The three parts can certainly be read independently: in fact I would guess that many readers will read only the papers in "their" part. The book seems more appropriate for specialists than

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as an introduction to the subjects of its title. There is an index and a short foreword on the circumstances that brought the book into being, but no overall introduction.

The book is therefore hard to review as a coherent whole, and hard for a single reviewer to tackle.

Part I, "Logic and games", has 55 pages and three papers. Van Benthem's paper is unusual in the book in that it is in survey style. It stands as a useful warm-up to Part I of the book, but is interesting as an agenda-setting paper in its own right. It contends that game theory and modal logic have a lot in common. Already, the same discoveries have been made independently in both fields, and interaction between them is currently quite active. The paper could be used as a compendium of current research problems in this area—often even the right formulation of questions is open as well.

Van Benthem lays out ways of treating games of perfect and imperfect information with logical (mainly modal) machinery. Modal logic of actions can describe how the players can move, and for games of imperfect information, modal logic of knowledge can express what the players know. Bisimulations provide a matching semantic viewpoint, and algebraic game constructors can also be used and axiomatised. These methods are already used to analyse processes in computer science, and van Benthem considers games to be analogous: "Game equivalences are like process equivalences, and issues of appropriate language design are similar, too." [page 13].

One of the unifying questions is indeed game equivalence—*when are two games the same?* One answer is by bisimulation, a very fine notion of equivalence. A coarser one is obtained by considering the "enforceable" sets of outcomes—one player has a strategy to guarantee an outcome in such a set, however the other player moves. Games with the same enforceable sets can be considered "the same" for certain purposes. A modal language for enforceable sets is introduced, and a notion of bisimulation for it, capturing the coarser notion of game equivalence. A third notion of equivalence considers enforceable intermediate conditions too, defined again by a modal logic. Some other kinds of equivalence are mentioned, the reader being referred to van Benthem's on-line notes for more details. There follow brief sections on game algebra (constructing complex games from simpler ones, e.g., by choice or concatenation), infinite games, games with imperfect information, and a discussion of preferences and rational behaviour.

In quantifiers like $\forall x\ \exists y/x\ \varphi$ of Henkin (*Some remarks on infinitely long formulas*, in: ***Infinitistic methods, Proc. Symp. Foundat. Math., Warsaw, Panstwowe***, Pergamon Press, NY, 1961, pp. 167–183), the y/x means that y is *independent of* x. They can be given semantics using, e.g., games. Sandu and Pietarinen's methodological paper aims to extend this independence to *propositional connectives*—as for example in (i) $(\alpha \vee\!/\!\wedge\, \beta) \wedge (\gamma \vee\!/\!\wedge\, \delta)$, where $\vee\!/\!\wedge$ means that the $\vee$ is independent of the central $\wedge$ in some sense. Setting this up is not easy, and a few "death traps", some pointed out by Hodges, *Compositional semantics for a language of imperfect information*, ***Logic Journal of the IGPL***, vol. 5 (1997), pp. 539–563, are discussed. How, for example, can the choice of disjunct be independent of the earlier choice of conjunct ($\alpha \vee \beta$ or $\gamma \vee \delta$), since the very formulas presented for choice immediately signal which conjunct was chosen? And what about (ii) $(\alpha \vee\!/\!\wedge\, \beta) \wedge p$ for atomic p? The authors analyse the problem using games and epistemic logic. They produce some propositional fragments that exclude (ii), and explicate $\vee\!/\!\wedge$ as a uniform choice (in a game of imperfect information) of *left* or *right* disjunct rather than of a formula. They worry that players will not then know which formula they have chosen. On the one hand, this seems inevitable, but on the other it is indeed strange for a semantic evaluation game. So they propose an alternative approach using Henkin's quantifiers; but I could not see how this helped. I felt that a discussion of what these logics might be used for might have helped to motivate the definitions. There are a few minor inaccuracies in the text.

The third paper, by van Ditmarsch et al., fits quite well with van Benthem's. It discusses a logic for describing states of knowledge in a game—here, at the start of a card game. Each player is dealt a finite number of cards. What they know and don't know is described in modal logic, both syntactically and semantically. Various syntactic descriptions are given and compared. There is a notion of a particular "standard" model; the syntactic descriptions determine this model up to bisimulation. The paper is pretty self-contained, but it only considers the starting position of games, and would be best read in the wider context of van Ditmarsch's Ph.D. thesis "Knowledge games" (2000).

Part II, "Classical logic", has 22 pages and contains two papers. Blass's rightly comes first, since it is linked to Part I by involving some games. Resource limitations are usually imposed proof-theoretically, for example by saying that assumptions may only be used once in a proof. In contrast, Blass uses semantic methods, compactness, and simple classical equivalences. He begins by giving what may be a new and simple semantic proof of Herbrand's 1930 theorem that a first-order formula ϕ is valid if and only if a *finite disjunction* of closed instances of a "Herbrandization" ϕ_H of ϕ is a propositional tautology (here, we regard atomic subformulas as propositional atoms). The key 'resource' idea now is to count the disjuncts. Blass defines ϕ to be *simply Herbrand valid* if a *single* closed instance of ϕ_H is a tautology. This notion is not well-behaved—e.g., it is not preserved by modus ponens. But if we say that a *propositional* formula is *universally simply Herbrand valid* (usHv) if every instance of it obtained by replacing propositional atoms by first-order formulas is simply Herbrand valid, then we do get a nice notion—for example, the cut rule preserves usHv formulas. Blass characterises usHv formulas as the substitution instances of *binary* propositional tautologies—i.e., ones in which each atom occurs at most twice. In a final section, he mentions earlier work on binary tautologies (Jaśkowski, *Über Tautologien, in welchen keine Variable mehr als zweimal vorkommt*, ***Zeitschrift für Mathematische Logik und Grundlagen der Mathematik***, vol. 9, (1963), pp. 219–228) and tentative links with game semantics for fragments of Girard's affine logic.

I found this paper extremely attractive. The proofs are direct, and the mathematical writing is some of the best I can remember, with super-lucid explanations of procedures and proofs using very little notation, and strikingly pertinent comments and examples.

Mints's short note "Quick cut-elimination for monotone cuts" gives a simple cut-elimination procedure for sequents of the form $H_1, \ldots, H_n \Rightarrow M$ where the H_i are universal Horn and M is monotone. Proofs are by induction on proof structure, using pruning transformations of derivations. The proof length increases by a factor of at most $\prod_i ax_i$, where ax_i is the maximum of 1 and the number of axioms traceable to the antecedent of the ith cut in the original proof. This increase is much less than in the general case, and better than a more complex and slightly more general algorithm of Baaz and Leitsch, *Cut normal forms and proof complexity*, ***Annals of Pure and Applied Logic***, vol. 97 (1999), pp. 127–177.

Part III, "Constructive set theory", is 38 pages long and contains two papers on related material. Both are technical and aimed largely at specialists (this reviewer is not one). Rathjen's concerns the proof-theoretic strength of intuitionistic set theory with Aczel's anti-foundation axiom (AFA), and some fragments of it. The paper interprets it in Martin-Löf type theory and then into a fixed point extension of Peano arithmetic, and so obtains an upper bound on its proof-theoretic strength. Lower bounds are obtained by interpreting intuitionistic theories $\mathbf{RA}^i_\alpha$ of the ramified hierarchy. It goes on to consider the strength of the system with additional axioms for existence of large sets: in particular, that every set extends to a "regular" set that is a model of constructive set theory without $\in$-induction. Although this system is proof-theoretically weak, a great deal of category theory can be developed in it. There is some introductory text, but familiarity with recent work of Crosilla and Rathjen is needed in the later parts.

The final paper, by Tupailo, again concerns the proof-theoretic strength of constructive set theory without foundation, this time with the natural numbers as urelements. An upper bound of $\varphi(\varepsilon, 0)$ is established by coding the system into Explicit Mathematics. The paper explains the coding in detail, but some definitions and proofs are omitted—the author's paper in Annals of Pure and Applied Logic (2003) is needed for a full understanding.

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BERTRAND RUSSELL. ***Correspondance sur la philosophie, la logique et la politique avec Louis Couturat (1897–1913).*** Édition et commentaire par Anne-Françoise Schmid. Transcription et notes sur la langue internationale par Tazio Carlevaro. Éditions Kimé, Paris, 2001. 737 pp.

Russell's exchange of letters with Louis Couturat, extending over sixteen years that include his most fruitful period in logic and philosophy, is among the most important of his correspondences. Couturat was not an original thinker like Russell, so that in some ways the correspondence between the two is less important than those with Frege, Whitehead and Moore. On the other hand, Russell's correspondence with these latter figures is either incomplete or limited in time. In the case of Couturat, virtually all of the letters on both sides survive, so that they form an extraordinary record of the evolution of Russell's thinking.

The surviving letters, almost complete, were rediscovered in the 1980s, and subsequently donated to the Centre de Documentation et d'Études sur les Langues Internationales, part of the Municipal Library of La Chaux-de-Fonds in Switzerland. The originals of some of the letters remain in the Russell Archives in Hamilton, but the bulk of the letters, from both correspondents, is to be found in Switzerland.

The correspondence covers a very large range of topics, including not only logic and the foundations of mathematics, but also the history of philosophy (especially the philosophy of Leibniz), philosophy of science, epistemology and current political events (particularly the Boer war).

The trajectories of Couturat's and Russell's career show a strong convergence, but then an increasing divergence and estrangement as the correspondence petered out in the years following 1907. Couturat himself, born four years before Russell in 1868, began his career in a brilliant fashion, achieving top honours in mathematics and philosophy; Henri Bergson considered him as a possible successor at the Collège de France. However, Couturat, rather than making original contributions in philosophy and logic, confined himself to popularizing and publicizing other people's ideas.

Couturat and Russell were brought together in the first place by a review by Russell of the former's *De l'Infini mathématique* in *Mind*. In the succeeding years, Couturat became a kind of disciple of Russell, popularising mathematical logic in France, together with the new philosophy linked to it. He arranged for a translation of *An Essay on the Foundations of Geometry* to be published by Gauthier-Villars in 1901, and in 1905 published *Les Principes des mathématiques*, based in part on Russell's own *Principles of Mathematics*, and containing a violent attack on Kant's philosophy of mathematics, a deliberate provocation towards the reigning neo-Kantian orthodoxy in French philosophy.

Couturat entered into polemical debates with gusto, and in 1904 incited a well known exchange between Russell and Pierre Boutroux, son of the well known philosopher Émile Boutroux, who was married to Henri Poincaré's sister. Russell later came to regret the polemics occasioned by Couturat's pugnacity, and referred in print to the misunderstandings caused by certain "indiscreet advocates" of symbolic logic, undoubtedly a hit at his former disciple. In the correspondence, though, Russell does not exhibit as much caution as his later remarks suggest. Couturat's book of 1905 was based on a series of articles that aroused the wrath of Poincaré. Russell responded in his letters to Couturat's sometimes importunate demands for material to refute neo-Kantian philosophy, as well as material for his series of lectures at the Collège de France, in which he had the express intention of attacking contemporary philosophical fashions, and "making a lot of people hopping mad" (p. 556).

The International Congress of Philosophy of 1900 in Paris was a turning-point for both Couturat and Russell, though for somewhat different reasons. It was in Paris that Russell encountered for the first time the mathematical logic of Peano and his disciples, while on the other hand, Couturat began his involvement with the movement for an international language. These events would strongly determine the future intellectual evolution of the two men, and their subsequent divergence was closely linked to their different intellectual loyalties.

In the letters of 1900–05, however, this divergence is not clearly apparent. To a contemporary reader, the close kinship between the early development of symbolic logic and the movement for an international language comes as a surprise. The two streams of thought, ultimately quite different, had a good deal in common initially. Peano, one of the founders of modern symbolic logic, was a strong proponent of various forms of international languages, including his own invention, *Latino sine flexione*, Latin without inflexions. Couturat himself, originally an adherent of Esperanto, became a devotee of Ido, a fact that led Russell to an irresistible pun. On 24 March 1912, he wrote in a letter to Lady Ottoline Morrell:

> I enclose 2 letters (which please return), one from Peano & one from Couturat, both occupied with the international language (or rather languages). First there was Vollapük, then Esperanto, then an improvement on Esperanto called Ido (its proficients are called Idiots), then Peano's "Latin without Inflections." These various sects hate each other like poison, but Esperantists & Idiots hate each other most because they are nearest akin. Couturat is an Idiot. I am ashamed to confess that he was my earliest disciple.

These sarcastic and disdainful remarks, though, date from the end of the friendship between the two men. In the correspondence from the early years of the century, Russell exhibits much more sympathy for Couturat's new-born enthusiasm. The fact that Russell at this time considered Esperanto and symbolic logic as rivals reminds us of the closeness of their origins. A key feature distinguishing the symbolic logic of Peano, Frege, Whitehead and Russell from the earlier work of algebraic logicians like Boole, Venn and Schröder is the attempt to parallel the linguistic structures of ordinary language rather than mathematical equations. Hence, it is not surprising to find Couturat objecting to symbolic logic that it cannot be used to order dinner in a hotel or find one's way in a railway station (p. 217). Couturat remained faithful to the methods of the algebraic tradition in logic, and argued for the dissociation of "logical symbolism, incorporating an algorithm and involving the analysis of thoughts, on the one hand, and on the other hand, a universal language" (p. 217). Modern readers may well sympathise with Couturat's arguments here. Perhaps the only remaining contemporary relic of Russell's vision of logic as a universal language is the touching devotion of analytic philosophers to first-order logic.

For historians of logic and philosophy, this extraordinary correspondence is an indispensable source of information on the evolution of Russell's ideas. During the years of active correspondence, Russell kept Couturat informed of the development of his ideas on the foundations of logic and mathematics, even including detailed technical developments in his letters. Russell's letters of 8 December 1900 and 17 January 1901 are particularly interesting and significant, since they represent his thinking just before the emergence of the Contradiction. Russell maintains that Cantor's diagonal argument is in error, because it does not apply to the universal class. Shortly afterwards, he reworked Cantor's proof into a paradox, and the logical crisis emerged that was not resolved until the final version of the theory of types.

When Couturat announced his intention in 1904 of writing an introductory monograph on mathematical logic (which never appeared), Russell sent him a summary of his current thinking on logical fundamentals, titled *Outlines of Symbolic Logic* (***Collected Papers of Bertrand Russell Vol. 4***, Routledge 1994, pp. 77–84). However, Couturat was ultimately a disappointment to Russell as a logical interlocutor. The *Outlines*, with their new ideas, were of no use to him. He complained of the subtleties and complications of the later developments of mathematical logic, as compared with the algorithmic simplicity of algebraic logic and expressed his fatigue and disgust with technical labours (p. 494). It is not surprising that as Couturat turned away from logical technique, while Russell engaged with the increasing subtleties and complications of the theory of types, the correspondence rapidly declined from 1907 onwards.

The correspondence is entirely in French. Russell's writing is fluent and idiomatic; the reader can verify the accuracy of his French because all of his mistakes are meticulously pointed out in the notes to the letters. On the other hand, there are frequent mistakes in the English passages printed here.

This is a very fine edition. The editor has provided detailed and informative annotations to the letters, explaining allusions and references and explicating some of the more difficult material. Tazio Carlevaro, who performed the tedious work of transcription, has provided the notes on the international language. In addition to the annotations, there is a useful introduction, a table of logical notation, a bibliographic index, an index of names, a subject index, and finally a detailed bibliography.

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Michael Wolff. ***Abhandlung über die Prinzipien der Logik.*** V. Klostermann, Frankfurt, 2004, xvi + 318 pp.

This book is divided into two parts: "Analysis of logical languages" and "The construction [*Aufbau*] of the logical functional calculus out of the elements of the syllogistic", followed by a conclusion and three appendices: "On the completeness of a syllogistic without logical conjunction", "Mathematical induction", "Additional remarks on the question of the reducibility of truth-functional expressions to non-truth-functional expressions". Various indices, a five-page bibliography, and an overview of the rules complete the volume.

The preface as well as the introduction announce revolutionary moves: "deductive logic" has a "superior" (*übergeordnet*) part: formal logic, and a subordinated (*untergeordnet*) part: mathematical logic.

The "principles of logic" are twofold: the "formal-logical principles" and the "logical postulates", or briefly "postulates" (p. XIV). An example of the former is the principle of bivalence, according to which "not true" means the same as "false"; an example of a postulate is the law of the excluded middle: "of two sentences such that one is the negation of the other, exactly one is true" (p. 25). The formal-logical principles enjoy absolute validity, the postulates do not; for example, the law of excluded middle fails for false sentences whose negation is also false, which happens when non-referential singular terms occur (pp. 24–25) .

The author presents, successively, three "languages": of syllogistic, of classes, and of predicate logic. The "language of syllogistic" is the core of formal logic, and starts with the assertion of the existence of concept -pyramids, that is, of concepts structured in ways that are expressed by the familiar phrases: "all P are Q", "some P is Q", "no P are Q", "some P are Q". To these phrases the author adds individual terms of the form "the P about which we are talking [*das in Rede stehende* ...]", conditionals "if ... then ... ", disjunctions "either ... or ... ", three alethic modalities: "it is true that ... ", "it is possible that ... ", and "it is necessary that ... ". With regard to the language of the calculus of classes, the author rejects the claim made in Hilbert-Ackermann's famous textbook, namely that the translation of the syllogistic into the language of the calculus of classes allows for a "more systematic treatment" of the former-precisely the opposite is argued by Wolff (pp. 67–68). Analogous claims are made for predicate logic: the author rejects the view (ascribed by him to Frege) that the syllogistic can be entirely expressed within the framework of modern predicate logic; on the contrary, the predicate logic invented by Frege fits within the syllogistic language, mainly with the extra help of three postulates: excluded middle, sufficient condition ($A, B \Rightarrow A$), and the existence of elements in the universe of discourse (p. 150). (It should be added that the syllogistic envisaged by the author admits predicates with more than one argument-place, cf. pp. 73–75, 256)

Generally, this book is an attack on Frege in the name of Kant and ancient logic. Perhaps Wolff's deepest cut into Fregean doctrines is the rejection of the view that the falling of an object under a concept is the logical fundamental relation. Such a view is "untenable" (p. 138). Instead, the relations among concepts are logically fundamental (p. 78). Now, to the extent that the falling of an object under a concept is the converse of predication, Wolff's approach amounts to removing—from logic and philosophy—the primacy of predication (of genuine Fregean predication that is, not of the pseudo-predication of the Aristotelian tradition, according to which, for example, "living being" is said of "human").

Wolff thinks that Frege's predicate logic cannot be characterized as "pure logic" (p. 142), but his own theory seems to fall short of this ideal, at least for two reasons. First, the existence of the concept-pyramids should be explicitly postulated rather than just taken for granted. As Saccheri would say, there is nothing wrong with the concept-pyramids, but since their existence "cannot be proved, at least by logic, it must be postulated, in order to proceed scientifically" (***Logica demonstrativa***, Olms, Hildesheim, 1980, I, 4). Thus, at the very foundation of the author's reconstruction of logic there is a postulate, that is a principle of the second kind, on which the allegedly pure and absolutely valid formal-logical principles of the first kind become dependent. Another source of contamination of the author's pure syllogistic may be seen in his claim that sentences with non-referential singular terms are false rather than meaningless; this claim is based on sheer arbitrariness ("one can use words as one wishes", p. 25), which does not seem to be a guarantee of purity.

In this reviewer's opinion, the principles are two: (i) every sentence has at least one of the two truth-values, (ii) every sentence has at most one of the two; "at least" plus "at most" yields "exactly", which is the standard view. I would refer to (i) as the principle of bivalence, and to (ii) as the principle of non-contradiction. I find it difficult to combine this conception of the principles with the author's (above described) version of bivalence and excluded middle.

Disagreements and objections aside, the reading of this book is, for lovers of philosophy, logic, and their history, an invigorating and challenging experience. Specifically, it is refreshing to have a chance of wrestling with criticisms of Frege inspired not by a shallow linguisticism (for example, when Frege is blamed for insisting on the variety of meanings of "is") but by Kant and such classical authors.

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Frege's lectures on logic: Carnap's student notes, 1910–1914. Translated and edited by Erich H. Reck and Steve Awodey. Open Court, Chicago, 2004, xiv + 170 pp.

Except for five semesters studying at Göttingen, Gottlob Frege (1848–1925) spent his entire academic career at the University of Jena, where he lectured on mathematics and logic from 1874 until he retired in 1918. In general, his lectures were poorly attended. One course of lectures that he gave regularly (in 1879 and then almost every year from 1883 to

1918, student numbers and his own health permitting) was entitled 'Über Begriffsschrift'. This provided an introduction to the logical system that he had first expounded in his book, *Begriffsschrift* (published in 1879), and then revised in the first volume of his *Grundgesetze* (published in 1893). These lectures had very few students, and sometimes none at all. But one student who did attend in the winter semester of 1910–11 was Rudolf Carnap, who made shorthand notes on them. Carnap subsequently attended a second course, 'Begriffsschrift II', in the summer semester of 1913, and made notes on these lectures too. These notes were found in Carnap's papers after his death, and were transcribed, edited and published, with an introduction by Gottfried Gabriel, in German, in ***History and Philosophy of Logic*** in 1996. The present book contains an English translation of these notes and (a slightly revised version of) Gabriel's introduction.

Carnap attended another course given by Frege, entitled 'Logic in Mathematics', in 1914. There is some mystery about this course. It is not on the list of courses officially recorded as having been offered by Frege at Jena (reprinted in Lothar Kreiser, *Gottlob Frege*, Felix Meiner, Hamburg, 2001, 280–4). Nor is a course under that title recorded as offered at any other time. With the exception of his lectures on Begriffsschrift, all of Frege's other lectures were on mathematics rather than logic, although he also held discussion classes on the fundamental concepts of mathematics, and in 1915 to 1917 offered a course on the foundations of arithmetic. Nevertheless, a course on 'Logic in Mathematics' was certainly given, and Frege's own lecture notes, fully written up, were published in his *Nachgelassene Schriften* in 1969 (translated as *Posthumous Writings* in 1979). Carnap's notes on these lectures have also survived, and are published for the first time in the present book, transcribed and translated into English.

As well as translating Gabriel's original introduction to Carnap's notes on Frege's lectures on Begriffsschrift, the editors, Erich Reck and Steve Awodey, also provide their own introduction (pp. 17–42). They assemble various reports on Frege's lectures, most notably by Carnap himself, explain Frege's notation and outline the content of the lectures, and indicate their influence. As far as Frege's logical system is concerned, what we find presented in his lectures at the time that Carnap attended them is what Reck and Awodey call the 'inferential' part of Frege's logic without the 'constitutive or constructive' part, i.e., without the part involved in his theory of logical objects (p. 34). Axioms V and VI of the *Grundgesetze*, in particular, in other words, are simply not discussed. That is significant, since it clearly shows that Frege had doubts about his conception of numbers as logical objects in the aftermath of his recognition of Russell's paradox, but was still convinced about the value of his logical system (as a formal theory).

As far as the influence of the lectures is concerned, Reck and Awodey make four points. First, they argue, Frege may have influenced—through Carnap—the general adoption of higher-order logic with simple rather than ramified types in the late 1920s and early 1930s (p. 39). Secondly, they indicate—uncontroversially—the effect that Frege's semantic concerns with sense and reference, names and descriptions, concepts and objects, and so on, had on Carnap's 1947 book, *Meaning and Necessity* (pp. 38–9). Thirdly, they suggest, Frege may have encouraged Carnap's 'scientific temperament' (p. 39). And finally, they argue that Carnap's work on axiomatic systems in the late 1920s, in attempting to combine Fregean logicism with Hilbertian formalism, had an influence on Gödel, and hence reveals a further path from Frege to later developments in modern logic (pp. 40–2). To pick up here just on the third point, though, Reck and Awodey do not mention the most obvious exemplification of Carnap's scientific temperament—in Carnap's concern with 'rational reconstruction', as he called it in his *Aufbau* of 1928, or 'explication' as he later called it (from 1945). This is surprising, since explication—though not under that name—is precisely what Frege discusses in his lectures on 'Logic in Mathematics'. Frege's set-theoretic reconstruction of the natural numbers was the model for Carnap's own work, and this methodological influence was surely the most crucial of all. (For details of this influence, see Michael Beaney, ***Carnap's Conception of Explication: From Frege to Husserl?***, in S. Awodey and C. Klein, eds., *Carnap Brought Home*, Open Court, Chicago, 2004, 117–50.) In fact, Reck and Awodey say nothing in their introduction about Frege's lectures on 'Logic in Mathematics', concentrating—like Gabriel—on the lectures on Begriffsschrift. Since there is a certain amount of overlap between their introduction and Gabriel's introduction, something might have been said instead about Frege's 'Logic in Mathematics' lectures.

What about Carnap's notes themselves? Here my overall view is that they shed little genuinely new light on either Frege's or Carnap's work. Frege's views are clearly recognisable—in their familiar form—in Carnap's notes, but Carnap adds little comment of his own. He occasionally notes when there was laughter at Frege's sardonic remarks, and at one point, when Frege talks of the different definitions of number that people have given as "[lying] beside each other, like the animals in paradise", comments: "The old man waxes poetic!" (p. 144). But there is nothing to suggest any moments of intellectual inspiration or conversion. If Carnap's philosophical path in life was influenced by Frege, it did not come immediately from Frege's lectures. Nevertheless, for the historical record, it is good to have Carnap's notes in the public domain, and all those involved in the production of the book have done a marvellous job—transcribing Carnap's shorthand, editing and translating the material, and type-setting Frege's two-dimensional notation. Both Gabriel's and the editors' introductions give a useful account of Frege's lectures on Begriffsschrift, and above all, the superb clarity which Frege's lectures famously exemplified comes out beautifully.

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Manuel Bremer. ***An introduction to paraconsistent logics.*** Peter Lang, Frankfurt, 2005, 249 pp.

In recent decades, paraconsistent logic has become a major subfield of both mathematical and philosophical logic. While there are numerous articles offering general introductions to paraconsistency and paraconsistent logic (e.g., Bryson Brown, *On paraconsistency*, in Dale Jacquette, ed., ***A companion to philosophical logic***, Blackwell, 2002; Graham Priest, *Paraconsistent logic*, in D. M. Gabbay and F. Guenthner, eds., ***Handbook of philosophical logic***, 2nd ed., vol. 6, Kluwer, 2002), until now there has been no *book*—at least in English—fully dedicated to providing such an introduction. (Bremer's ***Wahre Widersprüche; Einführung in die parakonsistente Logik***, Academia Verlag, 1998, is of course written in German. Priest's ***In Contradiction; A Study of the Transconsistent***, Martinus Nojhoff, 1987, discusses paraconsistent logic at length, but is primarily an extended philosophical defense of *dialethism*—the view that some contradictions are, or at least can be considered, true—which defense involves the formal development of a fairly specific approach to paraconsistent logic.) ***An introduction to paraconsistent logics*** aims to provide a panoramic overview of paraconsistent logic—its definition, history, motivation, formal embodiments, applications, and philosophical implications. We look at each of these aspects of the book in turn.

Definition. Bremer defines paraconsistency in essentially the standard way: "A logic is *paraconsistent* iff a set of statements can be simply or explicitly inconsistent without being trivial with respect to the concepts of $\vdash$ and $\vDash$ defined by that logic. The decisive property is: $(\exists A)(A \in \Gamma \wedge {\sim}A \in \Gamma) \wedge (\exists B)\Gamma \nvdash B$" (p. 14). (The existential quantification over Γ is left implicit.) So far, so good. However, Bremer goes on to draw a potentially misleading distinction between *weak paraconsistency* and *strong paraconsistency*. The former is the view that "we need a paraconsistent logic to deal with inconsistencies, especially with inconsistent theories, although weak paraconsistency sees inconsistencies still as something bad that should be *avoided* and has to be avoided in the end" (p. 15). The latter is the view that "(some) contradictions are unavoidable. Some contradictions can even be proven. Some contradictions, therefore, have to be considered as being *true*!" (p. 16). In my opinion it would be better to avoid using the same term ('paraconsistency') for both a *formal property* that a logical system can have and a *philosophical position* that one may advocate—especially as there is already a well-established term for the latter, namely 'dialethism'. (Bremer *does* use the term 'dialethism'; he equates it, or rather its denotation, with strong paraconsistency.) What Bremer refers to as 'strong paraconsistency' and 'weak paraconsistency' might be more perspicuously dubbed 'metaphysical dialethism' and 'doxastic dialethism', respectively. Metaphysical dialethism is the rather radical view that the *world* is inconsistent (i.e., some statement and its negation are *absolutely* true—not merely "true" according to some formal interpretation); doxastic dialethism, in contrast, is the rather conservative view that our *beliefs* (theories, etc.) are sometimes inconsistent (and yet still need to be reasoned with in a sensible way). (Incidentally, it is clear that Bremer, like Priest, is a *metaphysical*, and not merely doxastic, dialethist.)

History. Bremer dedicates four paragraphs to the history of paraconsistent logic in his Introduction. In those paragraphs he very briefly covers the main figures in said history, namely Vasil'év, Orlov, Jaśkowski, Asenjo, da Costa, and Routley/Sylvan. He also dedicates a few short paragraphs to mentioning some of the prominent figures in the field today, such as Carnielli, Batens, Brady, Meyer, and Priest. This seems a bit meager for a book-length introduction to the field—though it is remedied somewhat by various historical asides appearing in passing in subsequent chapters. It is particularly unfortunate that Bremer neglects to discuss Aristotle, who is arguably the *real* forefather of paraconsistent logic in that his syllogistic does not license inferring arbitrary propositions from contradictory premise sets such as {Some S are P, No S are P}.

Motivation. "The main motivation for strong paraconsistency (dialethism) is *universality* as a feature of language and cognition," writes Bremer. "Universality means that we are aiming at—and supposedly capable of—a theory of, say, language *in general*, that is not just of this or that language or languages of this or that formal structure" (p. 19). He goes on to discuss the liar paradox, Curry's paradox (similar to the liar but with no use of negation), convention T, and some commonsense arguments for the view that natural languages are—and certain artificial languages ought to be—*semantically closed*, i.e., able to talk about their own semantics. In light of the liar and its kin, semantic closure seems to require a paraconsistent consequence relation (assuming we don't want everything to turn out true or provable). Bremer considers and rejects the two most common alternatives to the paraconsistent approach: the *truth value gap* approach (associated with van Fraassen, Kripke, and others), which holds that sentences like the liar are neither true nor false, and the *cumulative hierarchy* approach (associated primarily with Tarski), which postulates (or stipulates) an infinite hierarchy of languages, each of which can talk about the semantics only of the languages that occur below it in the hierarchy. The truth value gap approach is rejected on the ground that it is vulnerable to revised versions of the semantic antinomies. The hierarchy approach is rejected on the ground that it "result[s] in an outrageous pragmatic self-contradiction" (p. 27); to wit, it "says we are always talking from some level in the hierarchy, and *at the same time* makes a general statement to the effect that [in] constructing the semantics of a language (level) we just go one level up" (p. 30). Bremer's arguments (though, of course, not entirely original) are fairly convincing; at least, they make it difficult for the skeptical reader to simply dismiss paraconsistent logics out of hand as misguided or undermotivated.

Formal embodiments. It is not enough to advocate paraconsistency as a philosophical abstraction; if the paraconsistentist is to be taken seriously, he or she must develop a plausible formal system which *manifests* paraconsistency. Bremer devotes a chapter titled "Methodological Considerations" to setting out the criteria that such a system should be expected to satisfy. Roughly, a paraconsistent logic should: (i) invalidate explosion, $A, \sim A \therefore B$ (of course, it must do so by definition); (ii) have a conditional connective that validates (at least) modus ponens (and doesn't validate anything that clearly conflicts with our use of natural language conditionals, e.g., $(A \rightarrow B) \rightarrow A$); (iii) not redefine the connectives in a way that is more counterintuitive than explosion and related paradoxes such as $\vdash A \rightarrow (B \rightarrow A)$; and (iv) be able to provide a non-trivial basis for naive semantics and naive set theory.

In the next six chapters, Bremer provides brief introductions to some of the main families of paraconsistent logics in the literature: LP ('logic of paradox') and relatives (Asenjo, Priest et al.), relevant/relevance logics (Anderson, Belnap, Routley/Sylvan, Meyer, Brady, Dunn et al.), adaptive logics (Batens, Meheus et al.), and logics of formal inconsistency (da Costa, Carnielli, Marcos et al.). He also discusses a sequential calculus, SKP, to which he seems especially partial (despite the fact that, as he acknowledges, its completeness with respect to its semantics has not been proven), as well as some systems such as RM3 and BN4 that don't fit neatly into any of the aforementioned categories. Each of these families of systems is introduced formally (both semantically and proof-theoretically via axioms, natural deduction, tableaux, etc.) and then assessed in terms of the criteria set out earlier. Bremer's presentations of these systems is, in general, admirably lucid and succinct. His philosophical assessments of the systems are in some cases a bit facile, however. For example, logics of formal inconsistency (LFIs) incorporate an internal consistency operator, $\circ$, which, when applied to a formula, expresses the idea that that formula is consistent, i.e., not both true and false. While rejecting explosion $(A, \sim A \therefore B)$, LFIs nevertheless validate "gentle explosion," i.e., $A, \sim A, \circ A \therefore B$. Bremer criticizes LFIs on the ground that "introducing 'consistent contradictions' [...] awaits epistemic elucidation: If we have A and $\sim A$, then we should take $\circ A$ as false, shouldn't we? And how can we *take A* to be consistent and have A and $\sim A$ at the same time?" (p. 117). A fairly obvious response, which Bremer does not consider, is that $\{A, \sim A, \circ A\}$ explodes (i.e., entails everything) *precisely because* its elements can't be jointly true, even when inconsistent situations are taken into account.

Applications. In Part II ("Applications") Bremer discusses six applications of paraconsistent logics (dedicating a chapter to each): semantics, set theory, inconsistent mathematics, metalogic, deontic logic and metaethics, and epistemology and philosophy of science. The chapters on semantics, set theory, and inconsistent mathematics are largely based on work in those areas by Priest (paraconsistent formal semantics), Routley/Sylvan and Ross Brady (paraconsistent set theory), and Chris Mortensen (inconsistent mathematics).

The chapter on metalogic is particularly interesting. In it, Bremer considers some of the famous negative metalogical results of the twentieth century such as Church's undecidability theorem, Tarski's indefinability theorem, and Gödel's incompleteness theorems. "Given a dialethist background philosophy," Bremer writes, "one may re-interpret the philosophical readings given to some of these formal results" (p. 159). For example, Gödel's first incompleteness theorem is often interpreted as showing that *truth and provability diverge*, in the sense that there can be no sound and complete formalization of arithmetic; that is, for any attempted formalization of arithmetic, F, either some untruth will be provable in F or some truth of arithmetic will be unprovable in F. What Gödel actually showed, however, is that there can be no *consistent* and complete formalization of arithmetic; that is, for any attempted formalization of arithmetic, F, either some *inconsistency* will be provable in F or some truth of arithmetic will be unprovable in F. The first interpretation presupposes that inconsistency entails non-truth, i.e., that no true theory can be inconsistent. But this is precisely what the dialethist rejects. Thus, writes Bremer: "Truth and provability do not have to come apart. What comes apart is rather *completeness* and *consistency*" (p. 160). It remains to be seen, however, whether arithmetic can be adequately formalized within a semantically closed paraconsistent framework (as will be illustrated shortly, when we consider Bremer's discussion of "hypercontradictions").

In the chapter on deontic logic and metaethics Bremer discusses one very promising application of paraconsistent logic, namely to the formalization of norms and normative systems. In a paraconsistent deontic logic, unlike "standard" deontic logics, conflicting obligations do not render everything obligatory: deontic explosion, $OA, O{\sim}A \therefore OB$, fails.

Philosophical implications. Part III ("Discussions, Alternatives, Perspectives") contains chapters on "hypercontradictions," inconsistent ontology, alternative solutions to the paradoxes, paraconsistency and programming, and "universal logic."

A *hypercontradiction*, according to Bremer, is a sentence that is "as disastrous to a paraconsistent logical system as contradictions are for a system of standard logic" (p. 185). More to the point, it is "a paradox that can arise within [a] paraconsistent system and threatens it with triviality (e.g., by proving '$1 = 0$', so that *all* statements will be both true and false, taking the numbers to represent truth values in interpretations v)" (p. 192). Bremer discusses some alleged hypercontradictions proposed (independently) by Anthony Everett and Joachim Bromand, and proposes solutions for them. Everett's hypercontradiction is evaded by replacing the usual semantic *function* from formulas to truth values with a *relation* between formulas and truth values—a solution first proposed by Priest (see Smiley and Priest, *Can contradictions be true?*, ***Proceedings of the Aristotelian Society, supplementary volume***, 65, 1993, p. 49ff.). Bremer's response to Bromand's hypercontradiction (which applies even to semantics using an evaluation relation) is much more radical: he proposes to restrict the principle of substitutivity of identicals, so that one can't infer e.g., $v(A, 0)$ from $\{v(A, 1), 1 = 0\}$, where v is an evaluation relation. (This may prevent triviality, but we are still faced with the not insignificant (!) problem of being able to "prove" that $1 = 0$.) Wisely, Bremer admits that his proposed solution "is not optimal," and that dialethists "should go on looking for a better solution" (p. 197).

The term 'universal logic' has gained popularity of late (see e.g. Jean-Yves Béziau, ed., ***Logica universalis***, Birkhäuser Verlag, 2005; Ross Brady, ***Universal logic***, CSLI, forthcoming). There has been a persistent ambiguity in the use of this term: sometimes it is used to denote a particular *research program* concerned with developing a general theory of logics, considered as mathematical structures (cf. "universal algebra"); this is Béziau's sense of the term. On the other hand, it is sometimes used to denote a *type of logical system* that is "universal" in the sense that it is supposed to be applicable in *every* context, including contradictory and impossible contexts; this is Routley/Sylvan's and Brady's sense of the term. Clearly these are two very different concepts. Note that while paraconsistent logic (the research program) can be construed as the study and development of paraconsistent logics, universal logic (the research program) clearly *cannot* be construed as the study and development of universal logics. Unfortunately, Bremer's chapter titled "Universal Logic" does little to discourage the conflation of the concepts, presenting Béziau's notion of universal logic as if it were somehow an alternative to or competitor with Routley/Sylvan's and Brady's notions of universal logic (pp. 222–3).

Now for some relatively minor complaints. The book comes across as having been hastily published, with inadequate editing and proofreading. There are many misspellings and grammatical errors. The typesetting, particularly the awkwardly formatted symbols and lack of paragraph indentation, is rather ugly. Also, disappointingly, there is no index.

On the positive side, the book *does* offer broad, democratic coverage of nearly every existing approach to paraconsistent logic. (Bremer can't be accused of being underfamiliar with the literature in the field.) Also, each of the chapters concludes with some—though in some cases too few—thought-provoking questions, exercises ranging from easy to very challenging, and helpful suggestions for further reading.

Anyone with a significant interest in paraconsistent logic is likely to benefit from having this (incidentally, very affordable) book as a reference. However, for reasons I hope I have made clear, I cannot recommend it highly for pedagogical purposes, e.g., teaching a graduate or upper-level undergraduate course in non-classical logic or philosophy of logic; I would suggest instead the outstanding articles by Brown and Priest cited above, supplemented perhaps with Carnielli, Coniglio, and Marcos, *Logics of formal inconsistency*, in D. M. Gabbay and F. Guenthner, eds., ***Handbook of philosophical logic***, 2nd ed., vol. 12, Kluwer, forthcoming.

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S. SHELAH. *On cardinal invariants of the continuum*. ***Axiomatic Set Theory,*** Translated and edited by D. A. Martin, J. Baumgartner, and S. Shelah, Contemporary Mathematics, vol. 31. American Mathematical Society, Providence, 1984, pp. 183–207.

Since the paper under review is more than twenty years old, it might be useful for some readers to put it in the context of its day. Mostly due to the fact that new techniques were rapidly being added to the already amply supplied toolbox for dealing with cardinal invariants of the continuum, the study of these invariants was considerably more fashionable than it is today. Of course, the attractiveness of any area of mathematics is enhanced by the perception that there might still be tractable problems available for solving. While it is true that by the start of the eighties the easy problems on cardinal invariants of the continuum had all been dispensed with, optimism was still being fuelled by the exploration of novel non-ccc partial orders for adding reals as well as the development of preservation theorems for the countable support iteration of proper partial orders.

In spite of these developments, there were problems that would not yield to the available methods; among these was the question of the consistency of $\mathfrak{b} < \mathfrak{s}$. The cardinal $\mathfrak{b}$ is the least cardinal of an unbounded family of functions from the integers to themselves under eventual domination while $\mathfrak{s}$ is the least cardinal of a splitting family, a family of subsets of the integers $\mathcal{S}$ such that for every infinite $X \subseteq \mathbb{N}$ there is some $S \in \mathcal{S}$ such that $|S \cap X| = |X \setminus S|$. So the question tackled by Shelah in *On cardinal invariants of the continuum* was to find a model containing an unbounded set of functions from $\mathbb{N}$ to $\mathbb{N}$ of size $\aleph_1$ as well as having the property that, for any set $\mathcal{S}$ of $\aleph_1$ subsets of $\mathbb{N}$ there is a single infinite subset $X \subseteq \mathbb{N}$ such that for every $S \in \mathcal{S}$ either $S \setminus X$ or $X \cap S$ is finite. Not only does *On cardinal invariants of the continuum* succeed indoing this, but the method is sufficiently flexible that the size of $\mathfrak{a}$—the least cardinal of a maximal almost disjoint family of subsets of $\mathbb{N}$—can be controlled so as to make it either $\aleph_1$ or $2^{\aleph_0}$.

The natural strategy which presents itself is to iterate ω_2 times the process of diagonalizing some ultrafilter over the ground model. The Mathias partial order does precisely this and is proper as well; however, it also adds a dominating function from $\mathbb{N}$ to $\mathbb{N}$. Shelah's major advance was to see how to modify the Mathias partial order so that it still diagonalizes an ultrafilter, but does not add a dominating function. The proof provided the starting point for a vast theory now known as "creature forcings" or, more prosaically, as "norms on possibilities".

The difficulty with Mathias forcing is that it adds a generic rapid ultrafilter and then diagonalizes it, thus adding a dominating function. In order to prevent this from happening, Shelah arranged that his forcing diagonalized an ultrafilter that is very far from rapid. This is done by grouping together certain sets of integers into "creatures" and insisting that all sets in the ultrafilter contain large parts of many of these creatures; this results in the enumerating functions of sets in the ultrafilter having long stretches where they are slow growing. Of course, it is not possible to insist that creatures be kept intact since there will always be sets that split all of an infinite set of disjoint creatures and the ultrafilter must make a decision about this set. The key innovation introduced by Shelah was that of attaching norms—these might actually be better described as measures—to the creatures. With the help of these norms it is easy to imagine what is meant by adding to the ultrafilter only sets which intersect creatures on increasingly large subsets. In other words, given an infinite set which splits many creatures it is possible to choose the large half of a creature infinitely often.

Indeed, for many purposes precisely such a strategy can be employed; the model theoretically motivated paper: Saharon Shelah, *Vive la différence. I. Nonisomorphism of ultrapowers of countable models*, in ***Set theory of the continuum*** (***Berkeley, CA, 1989***), Math. Sci. Res. Inst. Publ., vol. 26, Springer, 1992 pp. 357–405, is a good example of how much can be accomplished with such a strategy. However, the development of mathematics does not follow textbook principles of organization, and it happens that the first example of a creature forcing actually employed techniques whose sophistication one would have expected to see only much later in the development of the subject. The reason for this is that any attempt to construct a complete proof based on the outlined strategy of simply choosing the large half of a creature will not guarantee that the ultrafilter diagonalized is non-rapid. Along with having large creatures, it is also necessary to be able to amalgamate creatures together into a single larger one. Of course, to understand exactly what this means, the reader will have to consult the paper itself, but the point to emphasize here is that the choice of this strategy was guided by a preservation theorem for iterations.

The problem to overcome was that simply preserving the unboundedness of the ground model family of functions from $\mathbb{N}$ to $\mathbb{N}$ is not sufficient to guarantee that there will be an unbounded family of size $\aleph_1$ after an ω_2 length iteration. The goal should be to keep the functions from the initial ground model, a model of CH, unbounded. However, if one only knows that at each stage of the iteration the family of functions from the intermediate model are unbounded, this is not enough to conclude what one would like. The solution provided in *On cardinal invariants of the continuum* is to isolate the key property of the Cohen partial order which allows the stronger conclusion: Shelah defines a partial order to be *almost* ${}^{\omega}\omega$ *bounding* if for every name g for a function from $\mathbb{N}$ to $\mathbb{N}$ there is ground model function f and a condition which forces that g does not dominate f on any infinite set of integers from the ground model. It is easy to see that this is enough to guarantee that any unbounded set of functions will stay unbounded after forcing with such a partial order. Moreover, it also a property whose combinatorial properties lend themselves to being incorporated into the partial order constructed in *On cardinal invariants of the continuum*. It is exactly at the point that it is necessary to verify the almost ${}^{\omega}\omega$ bounding property that having the freedom to amalgamate creatures is used.

The way in which the combinatorial structure of finite creatures reflects a property of the uncountable—such as almost ${}^{\omega}\omega$ bounding—explains the significance of the paper beyond the results established there, as well as the continued use in forcing arguments of the methods it pioneered. It is typical that the creatures used in arguments involving norms on possibilities are finite structures whose combinatorial properties control the behaviour of the uncountable objects constructed with their help. A good illustration of this is provided by an example in: M. Goldstern, S. Geschke, and M. Kojman, *Continuous ramsey theory on polish spaces and covering the plane by functions*, ***Journal of Mathematical Logic***, vol. 4 (2004), 2, pp. 109–145, where the authors are concerned about minimal coverings of the reals by sets which are homogeneous with respect to certain continuous colourings of pairs of reals, which they refer to as c_{max} and c_{min}. They describe a forcing which adds a real which does not belong to any ground model set which is homogeneous with respect c_{max} while at the same time ensuring that the every real is covered by a ground model set which is homogeneous for c_{min}. The proof uses a norms on possibilities argument in which the size of a creature is determined by how large a random graph can be embedded into it. As is typical, from the definition it is straightforward to see that the generic real will avoid ground model homogeneous sets for the colouring c_{max}. The hard part is the preservation of a small family of sets homogeneous for c_{min} and here the authors use a hard theorem from extremal (finite) graph theory to conclude what they want about the uncountable. It must be emphasized that this is not an isolated case, but one of several examples of arguments using creatures which result in an elegant interplay between the finite and the uncountable.

The reason for this is that the theory of norms on possibilities, developed by Rosłanowski and Shelah (Andrzej Rosłanowski and Saharon Shelah *Norms on possibilities. I. Forcing with trees and creatures*, ***Memoirs of the American Mathematical Society***) over the years since *On cardinal invariants of the continuum* appeared, provides a framework which makes it almost routine to analyze the uncountable using the finite. Because of this, even though the questions that inspired the original paper may not be as much in vogue as they once were, the techniques developed in *On cardinal invariants of the continuum* are certain to continue to yield interesting and important results.

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