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A NEW CENTURY IN THE LIFE OF A PARADOX

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When receiving word from Bertrand Russell about Russell's paradox and the resulting inconsistency of his logical system, Gottlob Frege, although "thunderstruck," had the prescience to see the silver lining. In his response to Russell, he wrote, "your discovery is at any rate a very remarkable one, and it may perhaps lead to a great advance in logic, undesirable as it may seem at first sight" ([5], 132).

Frege's prediction was an understatement. Directly or indirectly, it brought about *many* great advances. The logical and set theoretic paradoxes, of which Russell's is the most famous, brought about the "foundational crisis" in mathematics during the first few decades of the 20th century, ushering in a wave of new interest in the philosophical and logical foundations of mathematics and set theory. It is fair to say that every major approach to set theory prevalent today has its roots in work that emerged during this time, much of it explicitly framed as a response, or part of response, to Russell's paradox concerning the set of all sets not members of themselves. Moreover, Russell's own prolonged consideration of the paradox lead him to develop refined views in the nature of meaning, logical form, truth, and knowledge, which have shaped analytic philosophy to such an extent it is difficult to imagine how it might have developed otherwise. Russell's paradox and its variants continue to be a source of inspiration for theorists interested in the foundations or philosophy of mathematics, metamathematics, set theory, the history of logic, and the history of early 20th century analytic philosophy.

A new anthology, edited by Godehard Link, presents ample evidence of this. In June 2001, one hundred years to the month (or almost) after Russell's own discovery of the paradox in May or June 1901, an international conference was held at the University of Münich commemorating this momentous event. This sizable book collects together over 30 contributions from this conference. The topics addressed and the backgrounds of the authors are diverse and far ranging. It covers everything from advanced research in contemporary set theory, to the history of Russell's thought and the set-theoretic paradoxes generally, to topics in the philosophy of mathematics and logic.

I shall divide my discussion of the contributions in four broad and overlapping categories: (1) those dealing primarily with the history of the logical paradoxes and/or Bertrand Russell's work, (2) those presenting new research in set theory or the philosophy of sets, (3) those dealing substantially with the philosophy of mathematics or nature of infinity, and (4) works that use Russell's work or the paradoxes he discovered as a starting place in arguing in favor of a new solution, logical method or philosophical thesis. Unfortunately, however, I shall not be able to give every contribution equal attention.

1. Historical and Expository Contributions

The introduction and frontpiece of the volume, penned by the editor, presents a summary of the early history of Russell's paradox, Russell's own unique interests and approach to logic and the philosophy of mathematics, the themes that emerged in Russell's philosophical work at least in part as a result of his struggle with the paradox, and the historical significance and influence of Russell's work in these areas. The paradox of the set of all sets not members of themselves, or at least something very much like it, was known to mathematicians such as Zermelo and Hilbert possibly even a few years before Russell's discovery. Link argues convincingly, however, that it was Russell's publication of the paradox and his portrayal of it as not only a logical puzzle and roadblock to axiomatizing the foundations of mathematics, but a topic for philosophical scrutiny, that was most responsible for bringing about a "Grand Conjunction," or unification of work being done in the fields of philosophy, logic and mathematics, which had much less contact with one another in centuries prior.

There are places, however, where Link's interpretation of Russell is questionable, and in my own opinion, mistaken. He describes, for example, Russell's theory of types in *Principia Mathematica* (hereafter *PM*), as "a platonist ontology of individuals and properties (concepts, propositional functions), regimented into levels" (p. 11). This reading identifies Russell's hierarchy of propositional functions with a hierarchy of Platonic intensional entities, a reading which there is no textual support for, and indeed, which Russell himself denied in several later works ([13], [14]). The quasi-Platonic "universals" Russell believed in *circa* 1910 were understood by him as individuals, like all other genuine entities. Russell did not accept anything like higher-order logic's comprehension principle for them. Propositional functions, on the other hand, were just open sentences. The satisfication relation between an entity and a propositional function is not, as Link suggests, the same as the relation of predication between a thing and a property. I have argued this at length elsewhere (see [9]).

Link also moves beyond exposition at times into evaluation. He pronounces Russell's logicism a "failure" (p. 14), chiefly due to its assumption of an axiom of infinity. Unfortunately, the issue is not so simple. First, it does not do justice to the work of contemporary proponents of logicism, which Link only dismisses in a footnote as having introduced 'ideology' into logic, which is hardly decisive. Secondly, it makes no note of at least a half dozen or so responses Russell, or a contemporary neo-Russellian, might give to the challenge that a principle of infinity is not a principle of logic. Indeed, Landini's contribution later in the volume contains just such a response (and one is arguably implicit in Cantini's). Unfortunately, this review is not the proper place for a full discussion of this issue either, but one can't help but think that Link's introduction would have been more fitting had it left such evaluations for another occasion.

Russell's own discovery of the paradox, and the development of his work in foundational issues leading up it, is addressed in detail in Nicholas Griffin's erudite addition to the volume, "The Prehistory of Russell's Paradox." Like Link, Griffin stresses the importance of Russell's having been the first to "make a fuss" about the paradox, contrasting his attitude with Burali-Forti, who at first utilized the reasoning dealing with the ordinal number of the well-ordered series of ordinals itself mainly as a *reductio* argument against Cantor's assumption of a trichotomy principle for ordinals, as well as that of Cantor himself, who seemed to take the inconsistency of certain collections (the collection of all cardinals or of all classes) not as a puzzle in need of a solution, but merely as evidence for a realm of the absolute infinite defying rational understanding. Griffin proceeds to describe Russell's interest in paradoxes concerning infinity growing out of his attempts to give an account of continuous quantity from a broadly Hegelian standpoint in the mid-to-late 1890s. Russell's coming to appreciate the full importance of Cantor's work seems to have been a gradual process, but accelerated into high gear by his discovery and adoption of Peano-style symbolic logic beginning in August 1900. While accepting the importance and force of most of Cantorian infinite mathematics, however, Russell was at the time convinced that Cantor had been guilty of a "subtle fallacy" in his argument that there was no greatest cardinal, since nothing could apparently be larger than the class of all terms (or of all classes or of all propositions). Griffin describes a passage found in a manuscript of Russell's (an early draft of [15]) which seems to have been written in November 1900, in which Russell tries to diagnose this flaw in Cantor's reasoning by defining a function from the class of classes not just into but onto its powerclass. The function defined comes very close to inviting one to think of the class of non-self-membered classes when Cantor's diagonal argument is applied. Griffin then portrays it as a puzzle that Russell did not yet state the paradox, nor seem to have been explicitly aware of any version of it until mid-1901, and, when it does appear, it appears in his manuscripts first in the form involving non-self-predicable predicates, not clearly having anything to do with Cantor's work. (A fact that is all the more perplexing given that Russell himself claimed to have discovered the paradox while considering Cantor's powerclass theorem.)

Personally, I think this is not quite so puzzling as it may appear. Logicians in Peano's school did not often make a clear distinction between a class and its defining "class-concept", and, indeed, Russell likely would have regarded his own work as offerring a necessary corrective by distinguishing them at all (cf. [15]: §76). Still, the school continued to have its influence over Russell's way of thinking, and one finds him in his manuscripts at the time, and even in [15] at times, using the same style of variables for both classes and class-concepts, and the sign "ϵ" at times for class membership and at times for the copula "is-a" used before an expression for a class-concept. As Russell was explicit, he regarded the difference between class-concepts and predicates as "only verbal" ([15]: 58) and that the version of the paradox involving non-self-applicable predicates and that involving class-concepts not members of their defined class as in essence the same paradox ([15]: §101). Finally, I note in passing that Griffin portrays Russell's argument in §85 for supposing that "the ϕ in ϕx is not a separate and distinguishable entity" as yet another statement of the predicates version of the paradox, which is most likely not the case, as I have argued elsewhere ([8]).

A number of the historical pieces discuss the work of Hilbert and his associates in Germany. Volker Peckhaus, in his piece, "Paradoxes in Göttingen," outlines the discovery and impact of the set theoretic paradoxes in Germany beginning with correspondence between Cantor and Hilbert making note of problems stemming from the assumption of a greatest (cardinal) number. Hilbert was aware that these paradoxes showed difficulties for the set-theoretic principles he had assumed at the time. Ernest Zermelo, either in 1901 or 1902, discovered a paradox involving the assumption of the existence of any set containing all its subsets, taking a form at least very similar to Russell's paradox. Although Russell's discussion of his paradox in [15], and Frege's discussion of the resulting inconsistency in his logical system in [3], were increasingly taken notice of in Göttingen, Hilbert's and Zermelo's versions remained the best known for some years. Through Zermelo, several other scholars became interested in the paradoxes and Russell's work, principally Heinrich Goesch, Leonard Nelson, Alexander Röstow, and Kurt Grelling, whose paradox involving "heterological" words was a direct result of their consideration of possible solutions to Russell's paradox.

In "David Hilbert and Paul du Bois-Reymond: Limits and Ideals," D. C. McCarthy suggests that the roots of Hilbert's metamathematical views suggesting the solvability of all foundational questions in mathematics may have evolved in part in contradistinction to the attitudes of then-prominent mathematician Paul du Bois-Reymond, who had alleged instead that certain questions in mathematics (*e.g.*, the existence of infinitesimals) would always remain unsolvable. McCarthy notes also that du Bois-Reymond can be credited with the first use of an explicitly diagonal argument in 1875, in an argument to show that infinite orders (taken in a special sense germaine to his work on geometrical continua) do not have a least upper bound, thereby anticipating Cantor's procedure (despite applying it to a very different conception of infinite number).

In a piece entitled "Propositional Ontology and Logical Atomism," Francisco Rodríguez-Consuegra discusses Russell's theories about the make-up of complexes and facts, the nature of belief and logical form, in roughly the years from the 1910 first edition of *PM* through the *Theory of Knowledge* manuscript of 1913. Rodríguez-Consuegra alleges that Russell's views at this time were a failure, mainly because his theory of belief was out of sorts with his own theory of logical types, and because of a failure to give an adequate response to worries revolving around Bradley's paradox of relations. While Rodríguez-Consuegra does an admirable job identifying some of the chief issues Russell puzzled over during this period, his criticisms of Russell seem to be based mainly on misunderstandings. For example, at multiple places, Rodríguez-Consuegra charges Russell with forgetting that the Multiple Relations Theory of Judgment he espoused at the time was committed to relations being able to occur as subjects or terms of a relation, when, in

[16], he described particulars as entities that enter in a proposition "as subjects of predicates or the terms of a relation," and universals those entities that enter in "as predicates or relations." However, there is no inconsistency here. Russell's position was essentially the same as he had held since [15]. A particular, or in the parlance of [15], a "thing", is an entity that can *only* enter into a complex as subject or term of a relation. A universal, or concept, is something that *may* occur in a predicative or relational way, but may also occur as subject. Nor is this position, as Rodríguez-Consuegra alleges (as does Irvine in a separate contribution), at all at odds with Russell's theory of types in *PM*. Russell did not claim in *PM* that universals cannot occur as subject in elementary propositions, or that they are not individuals. Rodríguez-Consuegra appears to make the same mistake as Link in thinking that the "propositional functions" of *PM* were to be understood at all similarly to universals or other intensional entities making up atomic complexes. Indeed, Russell is quite explicit in *PM* that the values of a propositional function in no way presuppose the function itself ([17], p. 39–40, 54–55), whereas, of course, a proposition or belief involving the quality of Redness or the relation of Loving certainly presupposes these universals (cf. my [9]).

In "Classes of Classes and Classes of Functions," Bernard Linsky addresses a problem (identified by Tony Martin) involving the contextual definitions Whitehead and Russell gave of apparent class terms in their "no classes" reconstruction of class theory within their higher-order logic of propositional functions. These definitions do not, in an unambiguous way, make it clear how to fully eliminate incomplete symbols for classes of classes of the form $\hat{\alpha}\Phi\alpha$. The first phase of such an elimination would seem to involve the use of both bound and free variables for classes of individuals, which were apparently themselves to be eliminable in favor of bound and free propositional function variables. Certain natural proposals about the precise method of elimination had such untoward consequences as that $\hat{\alpha}\Phi\alpha$ and $\hat{\alpha}\Psi\alpha$, two would-be classes of classes, could be shown to have the same classes of individuals as members but nevertheless be distinct, since the claim that each has a given propositional function of individuals (*not* a class) as a member could be both well-formed and true in one case but not in the other. Linsky outlines a revised reading of *PM*'s *20 that avoids this result.

Allen Hazen's contribution, "A 'Constructive' Proper Extension of Ramified Type Theory (The Logic of *Principia Mathematica*, Second Edition, Appendix B)" presents a reconstruction both of the Ramified Theory of Types of the 1910 First Edition of *PM* as well as the

Theory of Types presented in Appendix B of the 1925 Second Edition of *PM*. Although noting that this is likely not what Russell himself intended, Hazen points out that both theories can be coherently understood according to a substitutional interpretation of their higher-order variables. Hazen goes on to note that the 1925 theory can be understood as having a strength in between that of classical Ramified and classical Simple Type theory, and that despite not including an axiom of reducibility, captures an interesting fragment of arithmetic. However, Hazen claims it falls short of full Peano arithmetic primarily in not including an unrestricted principle of mathematical induction. Hazen notes that Russell's own attempted proof of mathematical induction in the Appendix was flawed, as shown by Gödel. Hazen also faults an attempted proof later given by Gregory Landini for relying on a stronger principle of extensionality than is warranted according to the substitutional semantics Hazen offers for his reconstruction. While Hazen is no doubt right that the reconstructed system he presents does not support full unrestricted mathematical induction, whether or not the same is true of what Russell actually intended in that appendix is not so clear. Unfortunately, Hazen's presentation of Ramified Type Theory is unrecognizable when compared to Russell's own presentation, and seems to be based more on the work of later theorists like Church and Myhill. There has been increasing evidence, however, that what Russell had in mind was something rather different (see, *e.g.*, [10], chap. 10). Indeed, it is not clear from Russell's description of his language that he even intended for it to include impredicative function variables, making it hard to know what significance, if any, Hazen's reconstruction has for evaluating the work of the historical Russell.

Andrew D. Irvine describes what he takes to be a tension between Russell's epistemological views and his methodological views in "Russell on Method." First, he describes Russell as avowing a form of foundationalism in holding that a certain set of our beliefs about the world, those based on immediate awareness (perception, sensation or introspection) rather than inference, could be taken as certain and infallible, and in advocating an analysis of all discourse about the observable world in terms of what at various times he called "sensations", "sense-data" or "percepts" (*i.e.*, those things about which we could have such knowledge). However, Russell at other times stresses that our primary reasons for accepting even fundamental principles in mathematical logic, or in another special science, are based on less-than-certain inductive or abductive grounds. Russell also describes the value of philosophy as consisting largely in its ability to get us to question either or both of our knowledge of our pre-philosophical beliefs, or their univocality. This tension, as Irvine sees it, was only finally resolved in 1940's *Inquiry Into Meaning and Truth*, where Russell gave up the suggestion that perception yields direct, infallible knowledge.

It seems to me, however, that the tension was never so great as Irvine imagines. The point of analyzing claims about ordinary objects into statements about sense-data was not to bring such claims into the realm of the infallibly known. It must be remembered that even a statement such as "the book is on the table," would not be analyzed as an atomic proposition directly recording an element of my experience, or even a conjunction of such propositions (of however length). Russell understood books and tables as series of classes of sense data, and *per* his no-classes theory, discourse about such classes would be analyzed in terms of their defining propositional functions and quantification over predicative functions of the same type. The defining propositional functions for the collection of sense data would involve relations of continuity and similarity that would no doubt be complicated to specify. In the end, the analyzed propositions would likely involve many quantifiers, of many logical types, and a very complicated logical form. While the semantics of such quantified propositions may make their truth or falsity depend ultimately on many (and quite possibly infinitely many) atomic facts, these would not all be facts present to one's own perceptual faculties at any given time. Hence, even if we would be able to have indubitable knowledge of atomic propositions regarding immediate experience, Russell would never have held that this would automatically translate into knowledge of any substantive truths, even those as simple as the one regarding my book and my desk. The purpose of analysis was to shed light on what, metaphysically, it would be for the book to be on the desk. The purpose was not to justify our prephilosophical beliefs. The method shows exactly how rich our evidence would need to be in order to have reason to believe the precise, complicated statement as opposed to the vague, shadow of this truth that was the unanalyzed belief. Ideally, at best, the process would vindicate our assumption that it is *possible* for us to obtain knowledge of these richer facts, as well clarify the still mostly inductive and abductive means by which we could come to it.

2. Philosophy and Foundations of Mathematics

In "Inconsistency in the Real World," Tobias Hürter aims to bring into focus how substantive our assumption of even the smallest kind of infinite set (such as an unending set of natural numbers) really is, by attempting to imagine what it would be for this assumption to fail. He does this by outlining a theory T_0, describing an irreflexive transitive order, and containing the recursive definitions of addition and multiplication, but of which it is provable that some sufficiently large numeral cannot receive its standard interpretation. Hürter, I think misleadingly, portrays himself as having sketched a "hypothetical situation" in which "a number cannot receive its intended interpretation." This reading confuses a theory with a situation, and numerals with numbers. Numbers do not receive interpretations. It seems to me that Hürter has at most described a language that appears to use standard notation, but in which the class of things the *numerals* must be interpreted as referring to is finite, not a situation or even a theory whereupon there are only finitely many *numbers*.

Peter Schuster and Helmut Schwichtenberg's contribution, "Constructive Solutions of Continuous Equations," presents a modification of Errett Bishop's approach to constructive analysis, in which by taking real numbers as modulated Cauchy sequences of rationals, one can obtain proofs of the order completeness of the real numbers, the approximate intermediate-value theorem and a reconstructed version of the Kneser proof for the Fundamental Theorem of algebra, without appeal to the countable choice principle.

In "Consistent Fragments of Frege's *Grundgesetze der Arithmetik*," Kai Wehmeier surveys a number of consistent fragments of Frege's notoriously inconsistent foundational system in which, while the notoriously Basic Law V is retained, paradox is avoided by weakening the second-order comprehension scheme in certain ways. Wehmeier notes that even the strongest known system of this type, which adopts Δ^1_1-comprehension, is at once too weak to provide a full reconstruction of arithmetic or real analysis, and also has the odd result of proving the existence of any finite number of non-logical objects (or objects that are not value-ranges or extensions of concepts, in the Fregean sense).

In his contribution, "The Significance of the Largest and Smallest Numbers for the Oldest Paradoxes," Ulrich Blau describes a realist and Platonist conception of the universe $\mathbf{V}$ of all sets. Blau also postulates the reality of both transdefinitely large ordinals, each larger *by more than by any amount* than those ordinals whose collection of predecessors form a set, as well as transdefinitely small numbers including $\frac{1}{\Omega}$ ("one Ω-th"), $\frac{1}{\Omega+1}$ ("one over $\Omega+1$"), etc., characterized by a halving process going beyond the greatest ordinal length Ω without limit. Blau brings these views to bear on such philosophical topics as the semantic paradoxes, the notion of truth, and paradoxes of vagueness and motion. I have to confess, however, that I was not able to understand fully

Blau's proposed viewpoint on these issues, in part because it was often unclear what was meant by certain of his claims. For example, Blau describes several aspects of his views as being "formally inexpressible" and elements of his ontology as being "inconceivable". It was unclear whether or not these were meant as technical terms, and if these labels were meant literally and seriously. If so, I am tempted to echo Russell's own words ([11]: 22) regarding the so-called "inexpressible" of Wittgenstein's *Tractatus Logico-Philosophicus*:

> What causes hesitation is the fact that, after all, Mr Wittgenstein manages to say a good deal about what cannot be said, thus suggesting to the sceptical reader that the possibility there may be some loophole through a hierarchy of languages, or by some other exit. ... His defence would be that what he calls the mystical can be shown, although it cannot be said. It may be that this defence is adequate, but, for my part, I confess that it leaves me with a certain sense of intellectual discomfort.

In a paper entitled, "Russell's Paradox and Hilbert's (Much Forgotten) View of Set Theory"—a paper which, depite its title, is not much about either Russell's paradox or Hilbert's view of set theory—Jan Mycielski explores why it is that people believe in the consistency of ZFC and similar systems despite not having rigorous proofs. Mycielski postulates that we each have access to a mental representation in which we can understand the sets of ZFC as akin to "boxes" possibly containing other boxes, and proper classes as places in which boxes may appear but not themselves boxes. Mycielski also advocates a theory he calls "rationalism", whereupon mathematical entities are to be understood as having their existence within the minds and thoughts of mathematicians, and ultimately, within the physical phenomena of their brains. Mycielski's ideas are difficult to assess, since, to be blunt, they read as if they have been written by someone without formal training in philosophy. It is not clear, for example, whether Mycielski is meaning to give an informal argument in favor of the consistency of ZFC, or merely a psychological explanation of why people come to have these beliefs. Neither is compelling. Examination of the psychological processes of belief formation cannot properly be done without empirical research, and certainly cannot be done introspectively. Mycielski's boxes metaphor seems not much help in providing even an informal argument for the consistency (apparently via the conceivability) of theories like ZFC. While I can only speak based on my introspection, I do not think any imaginative mental representations I can attempt of this sort comes anywhere near to representing the full iterative hierarchy of sets; I can at most represent a large finite number of iterative steps, and the rest seems to exist only in a kind of conceptual understanding expressed in my language of thought (if not in my internalized tokening of the language of set theory). It seems blatantly question begging to think that these representations provide any evidence for true consistency. Mycielski's form of "rationalism"—a misleading title given its naturalistic bent—seems to me to be conclusively disproven by dozens of arguments well known to philosophers of mathematics, many of which are explicit in Frege's attacks on psychologism and formalism in works such as [4]. It is startling that Mycielski does not address such worries.

Another more sophisticated perspective from a working mathematician on issues in the philosophy of mathematics is given by Robert S. D. Thomas in "Mathematicians and Mathematical Objects." Thomas offers an apologetic for the usual lack of concern shown by mathematicians over the ontological status of the objects of mathematics, arguing that it does not matter for the standpoint of mathematics whether or not the objects it studies exist or not, and that instead, it is only the relations between them that is important mathematically. Here I am reminded of F. H. Bradley's remark that "the man who is ready to prove that metaphysical knowledge is wholly impossible"—or we may add, unnecessary—"has unknowingly entered the arena. ... [H]e is a brother metaphysician with a rival theory" ([2]: 1). If Thomas's position is not simply a form of structuralism (which has its own metaphysical presuppositions), then it seems to locate the metaphysical ground of mathematical truth in mathematical relations and properties of and relations between these relations, the reality and nature of which would still matter if mathematical theses are to be fully meaningful and truth-apt, as Thomas seems to admit. While it is no doubt true that working mathematicians need not belabor themselves over the metaphysical status of these relations or how it is to be explained semantically that so much of mathematical discourse seems to speak of "objects" when it is the relations that are basic, these are issues that a philosopher or metaphysician of mathematics still has every impetus to investigate.

In "Objectivity: The Justification for Extrapolation," Shaughan Lavine explores the rationale and justification for infinitary set theory given that finitary set theories of indefinitely large size can be made to accommodate the theories of measurement strictly needed to explain the phenomena of the physical world. Lavine claims that the infinitary theories can be extrapolated (in a technical sense) from the finitary theories, and that the justification for doing so is to avoid the context relativity of the indefinitely large, and accommodate arbitrary values for field quantities within a region studied within physics, and for the application of a theory of functions integrating differential equations.

3. Set Theory and its Philosophy

Harvey Friedman presents a new and powerful set theory in his contribution, "A Way Out," based on a single axiom schema regarding set existence, which we could state informally as follows (for any formula φ in the language of first-order set theory):

> **(Newcomp):** There is a set y such that for all x, $x \in y$ iff φ, *or*, for any set y, there are two distinct sets z and w such that $\varphi[x/z]$ and $\varphi[x/w]$ and $w \notin y$ and $z \notin y$, it holds that for every $v \in z$ such that $\varphi[x/v]$, $v \in w$.

The resulting theory, despite its simple formulation, not only interprets ZFC, but is mutually interpretable with the theory ZFC + the scheme of subtlety (the thesis that if φ defines a closed and unbounded class of ordinals C and ψ defines a subset A_α of α for every ordinal α, then there exist $\alpha, \beta \in C$ where $\alpha < \beta$ and $A_\alpha = A_\beta \cap \alpha$). Missing from Friedman's paper is any discussion of the philosophy of sets and their existence conditions to provide any intuitive rationale for favoring this formulation of set theory over the existing dominant theories. This in no way points to a flaw with Friedman's work, but as of the present, it leaves the system with nothing to recommend it except its considerable power and possible convenience as a mathematical tool.

In his "Set Theory after Russell: The Journey Back to Eden," W. Hugh Woodin argues against the widespread belief that the formal undecidability of the *Continuum Hypothesis* (CH) in ZFC entails that there is no intelligible answer to the question of its truth short of disambiguating different notions of "set" or "class". Woodin argues instead for a kind of conditional Platonism according to which such questions must be seen as having determinate answers at least if the axioms of Second Order Number Theory are to be regarded as true (*simpliciter*). Drawing upon his work in Ω-logic, Woodin formulates a conjecture which is most likely not unsolvable in the same sense that the CH is in ZFC, which entails an determinate answer to CH, and most likely its falsity given certain plausible assumptions regarding complete Boolean algebras.

Kai Hauser's contribution "Was sind und was sollen (neue) Axiome?" begins more or less where Woodin's contribution ends. Citing Woodin's work, as well as earlier interest in the subject going back to Gödel, Hauser discusses the question of whether or not set theory is in need of additional axioms beyond those of ZFC in order to settle such undecidable questions as CH. Hauser addresses the philosophical question regarding what the justification for such axioms could be, especially when the plausible candidates lack any sort of self-evidence, and the sense in which they are necessitated by the "intended meaning" of set-theoretic discourse is obscure at best if they are not demanded by mathematical practice. Hauser argues that key tools towards answering this question might be found in the theory of meaning and intentionality found within Edmund Husserl's phenomenology, citing Gödel as previously having identified Husserl's work as relevant. I have to confess, however, that I did not find Hauser's description of Husserl's relevant views to shed much light on the question in the philosophy of set theory and mathematics, and if anything, simply compounded the already baffling metamathematical issues by adding all the doubts and uncertainties of Husserlian phenomenology. It was not clear, moreover, why Husserl's work would be more likely to shed light on the issue than rival theories of meaning, such as Frege's theory of sense and reference or possible worlds semantics.

In Sy D. Friedman's paper, "Completeness and Iteration in Modern Set Theory," we find an argument in favor of the existence of inner models (similar to Gödel's interpretation of ZFC within the universe of constructible sets) satisfying large cardinal axioms, and in particular, that there is an inner model with a Woodin cardinal assuming NBG set theory plus certain assumptions regarding the completeness and iteration of closed and unbounded subclasses of the ordinals.

In their "Iterating Σ Operations in Admissible Set Theory without Foundation: A Further Aspect of Metapredicative Mahlo," Gerhard Jäger and Dieter Probst describe a set theory $\mathsf{KPi}^0 + (\Sigma\text{-}\mathsf{TR})$, which adds to the Admissible Set Theory KPi^0 an axiom permitting the iteration of Σ operators along the ordinals. They show that the proof-theoretic ordinal of this theory is the metapredicative Mahlo ordinal $\varphi\omega00$.

In his contribution, Karl-Georg Niebergall addresses the startling-sounding question, "Is ZF Finitisitcally Reducible?" Drawing upon and extending prior work done by Jan Mycielski and Shaughan Lavine, Niebergall notes that all consistent recursively enumerable first-order theories are proof-theoretically reducible to a locally finite theory, one not presupposing the existence of an actual infinity. Niebergall argues that this calls into question some otherwise plausible-seeming analyses of what it is for a theory to be finitary or a finitistic theory, as those that entail that a theory is finitistic iff each of its theorems can be finitistically justified have the undesirable result that even ZF (despite its axiom of infinity and lack of finite models) is a finitistic theory, and, in effect, make it so that the label "finitistic" does not carve out an interesting subclass of theories.

Michael Rathjen's contribution, "Predicativity, Circularity and Anti-Foundation," explores several features of a predicative, constructive but non-well founded set theory CZFA, based on Aczel's Constructive ZF (CZF), but dropping its axiom of $\in$-induction, and adopting the Anti-Foundation Axiom of Hyperset theory. Rathjen shows that the key features of coinduction and corecursion important for modeling the kinds of circularity in computer science are demonstrable in CZFA, thereby providing evidence that these sorts of circularity are distinct from those involved in the kind of self-reference ruled out by barring impredicative definitions.

Another paper dealing with constructive theories, "Diagonalization in a Constructive Setting," by J. L. Bell, returns to the main theme of the conference in examining the relative significance of Russell's paradox and Cantor's diagonalization method in constructive reasoning. Since the argument of Russell's paradox actually produces an explicit set R, the Russell set, as a counterexample to the hypothesis that a surjection exists from a given set X and its powerset, it retains its force even when we limit ourselves to intuitionistic or constructive reasoning, whereas not all uses of Cantorian diagonalization retain such force when operating under such restrictions.

4. Intermediate Works

Some other pieces in the volume take their point of depature from aspects of Russell's own work, and use this work as the basis for new and interesting research. By and large, I found these to be the most interesting and unique contributions in Link's collection.

In "Typical Ambiguity: Trying to Have Your Cake and Eat It Too," Solomon Feferman explores the notion of typical ambiguity notoriously found in Whitehead and Russell's own reconstruction of the logic of classes (or sets) in *PM*. Although the theory of types of *PM* requires that in a statement of the form $a \in b$, a be of type one lower than b, one can still find occurrences of formulae therein such as "$Cls \in Cls$". Here, one is forced to interpret the two occurrences of "Cls" as expressions of differing types, so that the formula can be taken to mean that the class of all classes of level n is a member of the class of all classes of level $n + 1$. In general, Whitehead and Russell adopted the practice of leaving off any explicit type indices, giving their set theoretical theorems the appearance of being type-free, but with the *caveat* that they could be interpreted as representing any of a denumerable number of different type-specific formulae where the relative types adhere to the language's restrictions. Fererman goes on to sketch a similar device for use within a conservative extension of ZFC containing a sequence of constants $U_1, U_2, U_3, \ldots$, each standing for the set of entities within a certain reflective universe within the iterative set-theoretic hierarchy. The use of this device allows for the consistent reconstruction of what appears (owing to typical ambiguity) as a formulation of naive category theory.

The volume also contains a reprinting of Hartry Field's "The Consistency of the Naive Theory of Properties," which first appeared in *The Philosophical Quarterly*. Field's starting place is with the lesser discussed, but historically first, version of Russell's paradox involing the property of being a non-self-instantiating property. This version of the paradox is usually thought to completely vitiate the naive theory of properties, *i.e.*, one according to which for every open sentence $\varphi(x)$, there is a property y that any x instantiates iff $\varphi(x)$. Field argues out that a consistent theory of properties of this sort can be retained if the underlying logic is changed from a classical logic to a non-monotonic multi-valued logic without the law of excluded middle. Nevetheless, the resulting theory allows for the embedding of consistent classical theories not making reference to properties, effectively retaining classical logic for those sentences with only restricted quantifiers for non-properties. The system Field suggests is ingenious, but one can't help but wonder if "naive theory of properties" is the right label for the result, given the deviation from classical logic (the validity of which, I believe, is the default naive assumption.)

Property theory is addressed in again in Holger Sturm's contribution, "Russell's Paradox and Our Conception of Properties, or Why Semantics is No Proper Guide to the Nature of Properties." As the title implies, Sturm argues that our theoretical understandings of properties should not be guided by the theoretical work to which they would be put in semantical theory. This is in part because of methodological considerations Sturm advances according to which, in *a priori* sciences, entities playing explanatory roles better satisfy this purpose when a full and independent account of their nature can be given (as with possible worlds with regard to the semantics for modal logic). It is also because Sturm believes there is nothing in the uses of properties for semantics making them indispensable; fine grained set-theoretic models could be used just as well. This leaves natural ontology—*e.g.*, the role properties can play in explaining objective similarities or causal features of the world—as our only guide. There is no hint there of any need for self-instantiating properties, and thus Sturm concludes that the preoccupation with solving the properties version of Russell's paradox is misplaced.

Andrea Cantini contributes a fascinating piece entitled, "On a Russellian Paradox about Propositions and Truth," which addresses another interesting Cantorian paradox discovered and discussed by Russell ([15], §500), concerning propositions understood as abstract intensional entities. If there are propositions, there can be sets of propositions. However, it seems that for each set of propositions m, we can generate a distinct proposition, *e.g.*, the proposition that *all members of m are true* (as Russell put it, the proposition stating the logical product of m). Diagonalization invites us to consider the set w of all those propositions that state the logical product of some set in which they are not included, as well as the proposition stating the logical product of w. We then ask whether this proposition is a member of w, and from either answer we can derive the opposite, at least assuming certain plausible principles of propositional identity. Although Cantini does not discuss it, this paradox is also of historical importance, as there is evidence that it, and similar Cantorian paradoxes of propositions, figured heavily into Russell's eventual abandonment of his ontology of propositions in later works (as is clear from other contributions in the volume).

Cantini goes on to describe two logical systems in which terms for propositions (considered as intensional objects) can be utilized, and a predicate for truth for propositions introduced, even allowing for quasi-Tarskian truth-schemata for them to be derived and a limited amount of self-reference. One system is based on Aczel's work on "Frege structures" in [1] (a form of combinatory logic), the other on a system of stratification based on Quine's NF. Both systems avoid the paradox of propositions by ruling out the existence of the set w described above (for example, in the latter case, because its defining condition is unstratified when properly analyzed).

I have stressed the importance of consideration of paradoxes such as this one for a variety of theories of meaning in my [6] and [7]. It seems to me that the reverse is true as well: consideration of the proper "solution" to this paradox cannot be fully investigated without consideration of issues in metaphysics and the philosophy of language. The theories of propositions and truth formalized by Cantini seem lacking in philosophical motivation (apart from their solution to the paradox) without a fuller examination of the nature of truth, the conceptual role propositions are to play in our theories of meaning and language, and some concrete reason to think that restrictions on the existence of propositions or collections of propositions present in Cantini's theories are exactly what we would expect given a proper understanding of meaning and truth. This of course is not to say that further examination into this area might not validate theories akin to Cantini's.

Two papers in the collection, one by Gregory Landini and one by Philippe de Rouilhan, address Russell's so-called substitutional theory of classes and relations explored during the years 1905–1907, and eventually abandoned in favor of the ramified theory of types of *PM*. Russell had argued in [15] that everything that can be named, mentioned or counted (including propositions themselves) must be an "individual", capable of occurring as logical subject in a proposition, and hence, concluded that a proper logic must employ only one style of variable. The substitutional theory represented an attempt to reconcile this attitude with the need for finding a solution to the logical paradoxes. The theories center around a four-place relation, written $p/a; b!q$, which means that q (typically, a proposition) results from the substitution of the entity b for a wherever a occurs as logical subject in the proposition p. On this theory, both classes and propositional functions are excluded as entities, but one can in effect do the work of higher-order quantification by quantifying over two entities: a proposition and an entity in it to be replaced by other entities. For example, rather than considering a function $\hat{x}$ *is human*, one can consider the "matrix" consisting of the proposition $\{$*Socrates is human*$\}$ and Socrates. The theory yields results very similar to a simple type-theory, and Russell's paradox is excluded because there is no way to represent a matrix taking "itself" as argument, as something such as $p/a; p/a!q$ is ungrammatical. Philosophically, it provides an explanation for what goes wrong with the paradoxes without positing different ontological types of entities about which the same things cannot meaningfully be asserted.

The chief difficulty plaguing the theory was that it necessitated a logic involving the existence and nature of propositions as structured intensional entities, and thereby lead to Cantorian paradoxes of propositions such as that discussed in Cantini's contribution. A version of such a paradox particular to the substitutional sytem has been dubbed by Landini, the "p_o/a_o paradox". It involves correlating each matrix p/a with the proposition $\{p \supset a\}$ where "$\supset$" is understood not as a statement connective, but as a dyadic relation sign for the relation of (material) implication that holds between propositions. This yields a distinct entity for each matrix, and thereby, by diagonalization, to the consideration of a matrix p_o/a_o where p_o is the proposition:

$$\{(\forall p, q, r)[(a_o = \{p \supset a\} \ \& \ p/a; a_o!r) \supset {\sim}r\}$$

and a_o is any entity you like. When we consider the result of substituting $\{p_o \supset a_o\}$ for a_o in p_o, we seem to get a proposition that is true iff it is not, at least according to the principles governing propositional structure assumed in Russell's original formulation of the theory.

In one of the most fascinating pieces of the collection, "Logicism's 'Insolubilia' and Their Solution by Russell's Substitutional Theory," Gregory Landini takes as his starting place Russell's attempt in his 1906 work (and outlined in his paper, "On 'Insolubilia' and their Solution by Symbolic Logic" published in French in 1906) to solve the p_o/a_o and related paradoxes by abandoning his commitment to quantified propositions. This means there is no such proposition as p_o, as described above. However, this left Russell without the means to capture impredicatively defined "matrixes", and thereby left him unable to reconstruct enough class theory to provide the desired foundation for arithmetic. Russell's response to this was to adopt an assumption, according to which it still holds that for every open sentence A there existed a non-quantified proposition p and entity in it a (providing a matrix p/a) where the result of substituting any x in for a in p was true iff A, *i.e.*:

$$(\exists p, a)(\forall x)(\exists q)(p/a; x!q \ \& \ (q \equiv A))$$

for any A in which p, a and q are not free. Unfortunately, however, this assumption is strong enough to generate, in a slightly more indirect way, a version of the p_o/a_o paradox. Russell was sufficiently discouraged to abandon the approach. However, Landini argues that Russell gave up too soon, and sketches a proposed amendment to Russell's 1906 theory. It involves noting that propositions, unlike sentences of the formal language, can be infinite in complexity. Landini suggests adding a series of predicates, C^0, C^1, C^2 ..., where $C^0(p)$ would mean that p has finitely many constituents, $C^1(p)$ that p has $\aleph_0$ constituents, $C^2(p)$ that p has $2^{\aleph_0}$, and so on. One might then revise the above assumption as follows:

$$(\exists p, a)((C^1(p) \ \& \ C^1(a)) \ \& \ (\forall x)[C^0(x) \supset (\exists q)(p/a; x!q \ \& \ (q \equiv A))])$$

The result is a logical system powerful enough to capture Peano arithmetic and real analysis, but avoiding the p_o/a_o paradox and its variants. (The argument behind the paradox does not yield contradiction, but only a *reductio* of the assumption that the class of entities with finitely many constituents can be correlated one-to-one with those with denumerably many.) Moreover, the presence of infinitely many propositions with finitely many constituents can be proven outright in the system, and hence need not be taken as a "non-logical" axiom.

Landini's suggestion is that this approach has the potential to resuscitate logicism in a form very close to that originally conceived by Russell, while at the same time providing a philosophically robust solution to, and explanation of what goes wrong with, Russell's paradox and others. While his suggestions certainly deserve further scrutiny, it was not entirely obvious to me, however, that Landini's revised schema can be regarded as a principle of pure logic. It assumes that for every open sentence, $A(x)$, there is a non-quantified proposition p of denumerable complexity containing a constituent a, which becomes a true proposition when and only when that constituent is replaced by a finitely complex entity satisfying $A(x)$. Landini's argument in its favor points to an infinite disjunctive proposition of the form:

$$\{a = b \vee a = b' \vee a = b'' \vee a = b''' \vee \ldots\}$$

where $b, b', b'', \ldots$ are all the entities satisfying $A(x)$. However, this reasoning does not seem to cover the case in which there may be more than denumerably many finitely complex entities satisfying $A(x)$, and while Landini admits that it is an assumption that there are only $\aleph_0$ many finitely complex entities, I was not able to follow his reasoning that the above should hold even if this assumption were mistaken.

The historical issues in Landini's paper segue nicely into de Rouilhan's contribution, "Substitution and Types: Russell's Intermediate Theory," which considers the viewpoint Russell adopted immediately afterwards, whereupon he retained the notion of substitution to give a philosophical explanation of types, but readmitted quantified propositions into his ontology, only dividing them into a hierarchy of orders, beginning with individuals, then elementary propositions, then propositions quantifying over entities of the next order down, and so on. Traces of this theory are evident in [12], but gone by the time of *PM*, where Russell denies propositions as entities altogether. De Rouilhan ties the development and demise of this theory to Russell's changing philosophical views on the nature of truth and belief, and concludes that Russell's primary motives for abandoning it in favor of the mature ramified theory of types were those of ontological simplicity and convenience. A throughgoing answer to the question regarding Russell's motives could only be answered by a more careful examination of the manuscripts of the period, but this seems to me to be a tad oversimplistic. Russell had given philosophical arguments as early as [15] (*e.g.*,

p. 43) for rejecting theories that postulate different logical "types" or "orders" for genuine individual entities. By portraying propositions (and by extension, propositional functions) in *PM* as logical constructions rather than genuine entities, Russell was simply adhering to the conclusions of his own long-held arguments.

Geoffrey Hellman's addition to the volume, "Russell's Absolutism vs. (?) Structuralism," involves a comparison of various forms of structuralism in the philosophy of mathematics: set-theoretic structuralisms, "*ante rem*" structualisms, modal structuralisms and category theoretic approaches, coming to the conclusion that the best prospects remain for a theory combining the final two approaches. Despite early arguments against the structuralist elements in Dedekind's work, Hellman claims that Russell ended up advocating a form of structuralism himself. While Hellman's discussion of structuralism is interesting in its own right, the tenuous connection he attempts to draw to Russell's work does not withstand scrutiny. The evidence seems to be based on Russell's claims that arithmetic can be expressed in the language of pure higher-order logic containing no non-logical constants. Hellman takes this to mean that Russell's approach is tantamount to one in which one one simply takes the conjunction of the second order Dedekind-Peano axioms, replaces the constants "0", "successor", "number", *etc.*, with variables of the appropriate types, and asserts the logical truth of the universally quantified conditional from the resulting open sentence to an open sentence similarly derived from a truth of number theory. It takes only a cursory familiarity with Russell's technical writings in mathematical logic (*e.g.*, [12], [15]) to see that this isn't even close to the approach he actually takes. In these works, Russell maintains the core of the Frege-Russell conception of numbers as equivalence classes of like-cardinality classes, but insists that "classes" are "logical fictions", and hence that class-abstracts are "incomplete symbols" that can be defined away in context in terms of higher-order quantification. This applies to numerals such "0" and "1" too. Russell's view is that these signs do not stand for entities at all, but nevertheless, the contributions they make to the meaning of statements in which they appear are always the same. They are therefore closer to constants than variables; in the sense in which there are numbers 0 and 1 at all in Russell's view, they are unique (at least within their type); they are not placeholders for whatever might occupy a certain position in a structure, or satisfy a given role for applying a mathematical theory. Similar remarks could be said with regard to

Russell's analysis of the successor relation-in-extension as an "incomplete symbol," as well as the class of natural numbers. There is no trace of structuralism there.

Vann McGee's interesting contribution, "The Many Lives of Ebenezer Wilkes Smith," takes it start from Russell's 1923 paper "Vagueness." There, Russell considers a person, *viz.*, Ebenezer Wilkes Smith, whose life does not have a determinate beginning or ending (since neither process of coming alive nor dying is instanteous process). Russell argues that this seems to make the name "Smith" vague as there is no determinate thing (determinate series of temporal stages) which it names. McGee reads this as a precursor to Peter Ungar's "Problem of the Many," and argues that Russell's way of posing the problem has a generality that Ungar's does not. After surveying some possible responses and finding them wanting, McGee concludes that these sorts of cases constitute another argument establishing the inscrutability of reference.

The final contribution to the volume, a piece by Albert Visser entitled, "What Makes Expressions Meaningful? A Reflection on Contexts and Actions," is difficult to categorize, since it is not clearly related to Russell's work nor to Russell's paradox, but it is still a worthwhile contribution. Visser argues against the widespread view (often read into Frege's "context principle" of [4], but more explicit in Dummett's work on Frege) that the sentence is the primary vehicle of linguistic meaning. Visser does this in two ways. One is to point out many instances of seemingly meaningful language that is not explicitly or even clearly elliptically sentential (addresses on letters, book and chapter titles, calling someone's name, and so on). The other is to point out there are many places where a sentence on its own cannot be regarded as fully meaningful, largely because certain expressions within it require the full discourse to provide enough context for them to be meaningful, or the sentence as a whole in isolation makes no real "move" in a language game. Visser's arguments seem entirely compelling to me, although I have to confess I've never found the sentence-priorty semantic thesis at all attractive. Indeed, I don't think Frege himself meant his context principle to be read in such a strong way (cf. [6], chap. 3).

5. Conclusion

This is an important collection. The new research presented in this one book is roughly equivalent to a year's worth of articles in an important journal. The overall quality is impressive. Only two or three of the contributions would likely not have passed muster at a refereed journal, a low percentage for conference proceedings of this kind. Among the remainder, there is more than enough to provoke substantially the thoughts of an active scholar working in any one of several different intellectual disciplines. Historians of logic will especially find the pieces by Griffin, Peckhaus, Landini and Hazen useful. Working mathematicians and set theorists will no doubt find the results in the contributions by Schuster and Schwichtenberg, Woodin, Friedman, Niebergall, Rathjen and Bell too important to ignore. The papers by Wehmeier, Lavine, Thomas and Hellman provide stimulation for those interested in the foundations and philosophy of mathematics. Even those interested primarily in the philosophy of language and metaphysics will find something worthwhile in the contributions of McGee, Visser, Sturm, Feferman and Cantini. Those of us interested in more than one of these topics are sure to return to this collection again and again.

Perhaps the greatest praise I could offer for Link's collection, however, is that it presents the 2001 conference in Münich in such a strong light that it makes those of us who were not in attendance kick ourselves for missing out. Those who were in attendance surely saw the second century in the life of a paradox kicked off with a bang.

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