A GLOBAL VIEWPOINT ON RUSSELL'S PHILOSOPHY*

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This book continues a well established tradition: that consisting of studying Russell's philosophy through a group of different contributions. The more or less regular appearance of this kind of book (or special issues of a journal) is a sign that this philosophy preserves its interest, i.e. that it is still a live philosophy, and the characteristics of some of the contributions indicate, besides, that this interest is not only historical but closely related to full contemporary problems. The tradition started with the already classical Schilpp 1944 (reedited in 1951, 1963 and 1971), continued in the seventies through Schoenman 1967, Klemke 1970, Pears 1972, Nakhnikian 1974, Thomas and Blackwell 1976 and Roberts 1979,[1] and has included (to my knowledge) special issues of *Rivista Critica di. Storia della Filosofia*. (1953), *Revista de Occidente* (1971), *Revista Portuguesa de Filosofia* (1972), *Revue Internationale de Phiosophie* (1972), *Synthese* (1980–81), *Russell* (1984), *Mathesis* (1988) and *Russell* (1988).[2]

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* Review-essay on C. Wade-Savage and C. Anthony Anderson (eds.), *Rereading Russell: Essays in Bertrand Russell's Metaphysics and Epistemology*. Minneapolis: University of Minnesota Press, 1989. (Minnesota Studies in the Philosophy of Science, vol. 12.)

[1] P.A. Schilpp (ed.), *The Philosophy of Bertrand Russell*, La Salle, Ill.: Open Court, 1944. R. Schoenman (ed.), *Bertrand Russell, Philosopher of the Century*. London: Allen & Unwin, 1967. E.D. Klemke (ed.), *Essays on Bertrand Russell*, Urbana, Ill.: University of Illinois Press, 1970. D. Pears (ed.), *Bertrand Russell: A Collection of Critical Essays,* New York: Doubleday, 1972. G. Nakhnikian (ed.), *Bertrand Russell's Philosophy*, London: Duckworth, 1974. J. E. Thomas and K. Blackwell (eds.), *Russell in Review,* Toronto: Hakkert, 1976. G. W. Roberts (ed.), *Bertrand Russell Memorial Volume,* London: Allen & Unwin, 1979.

[2] My review-essay on this last issue of Russell ("Russell's first technical philosophy") is forthcoming in *History and Philosophy of Logic*, **11/2**. A problem common to this issue and the present book is that both were published some years after the respective conferences had taken place. In particular, the Minnesota conference took place in 1982, although this date is not mentioned by the editors in the preface or the introduction, and the reader realizes it only through a contributor's note on p. 218. The consequences are

The main problem of this kind of collective works is that they usually lack any common point of view, although in some of them a particular period of study is selected. Fortunately, our book belongs to this last category (together with the mentioned issues of *Synthèse* and *Russell,* which are devoted to Russell's early philosophy) and its precise intention was to study Russell's later philosophy (especially the metaphysics and epistemology from 1927 to 1959). This was a very valuable goal, for this important period has been almost neglected by the scholars; however, the actual result concerns also Russell's early logical writings and to some extent the epistemology from 1912 to 1921, with which the volume embraces almost all of Russell's technical philosophy.

The editors' introduction tries to organize the material into five groups, but the result is not very convincing,[3] so I shall divide this essay according to two criteria: first, I shall select a group of contributions according to the importance of the topics they consider; second, my comments will be ruled by a global and mainly methodological approach to Russell's whole philosophy,[4] which will make the chronological order advisable. Accordingly, Section 2 will be devoted to Russell's theory of descriptions, Section 3 to his theory of logical types, Section 4 to his epistemology (and related metaphysics), and Section 5 to say something brief on the rest of the contributions.

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especially unpleasant for some articles (I am thinking of the contributions by Goldfarb, Cocchiarella, Hylton and Pears; see below), given that in the meanwhile new very relevant literature has appeared. A sign of the delay is that two contributions (Demopoulos/Friedman and Earman) were published elsewhere in 1985.

[3] However, the introduction in itself is quite useful as it provides very good summaries of all articles in the book. Unfortunately, the editors also include a misleading historical account of Russell's philosophy according to five "phases": preanalytic (1893–99); logical (1900–10); early analytic (1911–18); middle analytic (1919–27); late analytic (1928–59). If we regard the "content" of Russell's doctrines, this division is as artificial as many others, but when we read that the underlying criterion has been "from phenomenalism to neutral monism to structural realism", according to the standard "changes of mind" view (p. 4), then we realize that this part of the introduction might have been written 50 years ago, for it does not consider any kind of deep methodological unity in Russell's evolution (see especially Section 4 below).

[4] I tried to provide this global viewpoint in my Ph.D. thesis (*El método en la filosofía de Bertrand Russell. Un estudio sobre los orígenes de la filosofía analítica a través de la obra de Russell, sus manuscritos inéditos y los autores que más le influenciaron,* University of Barcelona, 1987, x + 800 pp.) as well as in other subsequent writings which I shall mention as I develop my comments.

P. Hylton's article tries to find out the significance of "On denoting" (OD) in the development of Russell's philosophy, in order to defend the thesis that it was the origin of important philosophical and methodological changes, both as the result and the starting point of a historical process. The main results of the discussion are the following: (i) Russell did not compose OD to give an account of sentences containing descriptive phrases involving impossible entities (or "some puzzles that he just happens to come across", p. 92), for this was already possible through the theory of denoting concepts contained in *Principles;* rather, it was destined to explain "the variable" (i.e. the nature of generality), essential for deriving mathematics from logic, as well as the role of definitions of particular terms; (ii) the involved elimination of the descriptor was not especially concerned with reasons of ontological economy, but rather with the internal difficulties present in the celebrated passages of OD on "Gray's *Elegy*" and in the pre-OD unpublished manuscripts; (iii) the philosophical consequences of OD were not especially related to the disappearance of denoting concepts, but rather to: the development of a conception of logical form; the introduction of a new concern about the importance of language and the context of words for philosophy (which led Russell to "incomplete symbols"); and the introduction of a new conception of philosophical analysis; (iv) this new method of analysis incorporated an eliminative feature through the definition of classes (a "second" kind of incomplete symbol) in terms of propositional functions, and led Russell to the "multiple-relation" theory of judgment (and its corollary that there are no propositions) and to the view that physical objects are "logical constructions" (through the corresponding descriptive analysis of sentences where they appear).

I am afraid that almost all of these results must be rejected as mistaken in the way they are stated.[5] (i) The theory of denoting concepts from *Principles* did not have a satisfactory explanation on how it is possible that certain denoting concepts denote nothing at all: to merely say that in some sentences the involved description does not make the supposed entity present in the proposition is not a solution, but just a statement of the problem. However, it is true that Russell had available other means of dispensing with those annoying pseudo-entities (e.g. the Fregean distinction between sense and reference), although after trying with some of them he found the total elimination of the descriptor more convincing. In any case, it is hardly satisfactory

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[5] The detailed arguments of my alternative account are contained mainly in my "The origins of Russell's theory of descriptions according to the unpublished manuscripts", *Russell*, 9: 99–132 (1989).

to consider this matter by eluding Russell's difficult zig-zags concerning the meaning-denotation problem from 1903 onwards (see below). Besides, Russell himself pointed out in OD a set of puzzles which were later shown to be solved with the new theory. As for the explanation of the generality, Russell already knew the two Peanian quantifiers from 1900 (*Principles* was mainly a pre-Fregean work), but as the treatment of 1903 was very complex, he was trying to simplify it by reducing the number of (denoting) concepts involved. In the course of this process, he discovered that the descriptor can be reduced in terms of propositional functions,[6] but OD involved no special improvement in the former explanation of "the variable". Finally, the importance of the definitions of particular terms was already considered in 1903, although Hylton is right in pointing out that it was necessary to explain the informative power of statements of identity (this was precisely one of the puzzles involved).

(ii) Ontological economy was an essential part of Russell's methods from 1899 onwards, and it is pretty clear that the idea of dispensing with the descriptor was the main step towards enabling the logicist construction to take place with only Peano's two quantifiers, i.e. by also dispensing with the rest of the denoting concepts appearing in 1903. Therefore, to deny the weight of Ockhamian reasons here needs stronger arguments than the (admittedly "dogmatic"; p. 95) thesis by Hylton. As for the passages on "Gray's *Elegy*", Hylton does not offer any study of the meaning-denotation puzzle; even worse, he points out the relevance of the unpublished manuscripts concerned, but he provides no exegesis at all,[7] so that it is difficult to seriously take his new claim into consideration.

(iii) The logical form, as opposed to grammatical form, did not depend on the devices which appeared in OD. It is an important concept (in fact a primitive idea: the "constancy of form") in *Principles,* and can be easily traced back to Bradley,[8] as for instance can be seen in the very example given by Hylton (p. 97), according to which universal judgments are really hypothetical ones (in Bradley's terminology). Thus, the idea that the philosopher must try to overcome the misleading linguistic appearances is previous to OD. Besides, Hylton does not consider Russell's important

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[6] Apparently by following the conceptual apparatus from Frege and some particular previous symbolical eliminations by Peano; see my forthcoming "A comparison of the theories of descriptions by Frege, Peano and Russell".

[7] See my forthcoming "Some new light on Russell's 'inextricable tangle' about meaning and denotation and the appearance of his first theory of descriptions".

[8] See my "Bertrand Russell 1900–1913: los principios de la matemática (parte 1ª)", *Mathesis,* 4: 355–92 (1988), and chs. 1 and 4 of my *The Mathematical Philosophy of Bertrand Russell: Origins and Significance,* Naples: Bibliopolis, forthcoming.

changes concerning this notion from 1913 onwards as he makes a quotation from 1914 (pp. 99–100) without the needed exegesis, so that he seems not to realize that by then Russell had admitted several acceptable "forms" for his constructions (see also Section 4).

Concerning the importance of language, it is true that there is some intended parallelism between natural language and philosophical categories in *Principles* which became weaker in OD, but as pointed out in the above paragraph Russell had already available in 1903 a Bradleyan theory on the difference between grammatical and logical (true) form, which can be detected in some places in this work. It is also true that Russell's first known reference to the importance of the context for the actual meaning appears in OD, but we must not forget that Frege maintained a similar doctrine (his well know principle of the context)[9] in a work Russell had studied. Likewise, it is very inaccurate to suggest that Russell said that denoting concepts were "incomplete symbols" *in OD* (p. 96), when this expression appeared first five years later in *Principia*, in a more global context (see below for more about incomplete symbols). Finally, to say that OD supposed a new conception of philosophical analysis is not exact if by this expression we understand a reductive (or constructive) analysis, which Russell inherited from Moore from 1898–99 onwards,[10] and practised abundantly up to 1905. It is not true even that the distinction between acquaintance and description was a consequence of OD;[11] the precise thing added in OD was a new (more or less Fregean) paraphrastical device.

(iv) The eliminative element of Russell's constructive method was explicitly present in his first attempts of setting up a no-classes theory (in his correspondence with Frege),[12] and of course in the elimination of the descriptor in terms of propositional functions; therefore, it can hardly be located, as Hylton claims, in the later elimination of classes following similar methods to the ones applied to descriptions. However, it is of course true that the elimination of classes supposes the introduction of a new kind of incomplete symbol (in the terminology of *Principia*), although the list has to be completed with a third kind of them: propositions (explicitly in 1910 but under-

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[9] See my article mentioned in note 5.

[10] See my: "Bertrand Russell 1898–1900: una filosofía de la matemática inédita", *Mathesis* 4: 3–76 (1988); "La primera filosofía de Moore", forthcoming in *Agora*; and ch. 1 of the book mentioned in note 8.

[11] As I have shown in the article mentioned in note 5, the distinction appears in previous unpublished manuscripts, and proceeds from an older similar distinction.

[12] The elimination was implicitly present even in Russell's constructive logicist definitions of *Principles* (although to accept that, for instance, numbers are nothingbut classes was a later decision).

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lying Russell's publications from 1906 onwards).[13] Thus, the actual origin of the "multiple-relation" theory of judgment was not the kind of elimination pointed out by Hylton, but simply the need for providing a philosophical basis to the necessary dispensing with propositions caused by the facing of semantic paradoxes (therefore, the abandonment of propositions could hardly be a corollary of the mentioned theory). To conclude, the elimination (the logical construction) of physical objects (as well as the dozens of similar constructions) is a descriptive device, but not a paraphrastical one, as Hylton seems to suggest. It is a consequence of the eliminative element *only insofar as physical objects are classes* (of appearances, etc.), with which they are also incomplete symbols for the same reason (thus, they belong to the second kind of these symbols).

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W. Goldfarb's main claim is that Russell adopted a ramification of propositions and propositional functions from 1906 onwards exactly for the reasons he adduced: the need for avoiding the semantic paradoxes (which are to be regarded as genuine paradoxes), but not because he was a constructivist in the sense that he used the vicious circle principle as a consequence of his supposed belief that propositional functions (and the classes they give arise to) do not exist until they are actually specified or constructed. In general, I agree with the spirit of this claim, but I think it contains two things that have to be rejected. First, the ontological sense of the ramification concerned only propositional functions; propositions were no genuine entities at all for Russell from the unsolvable difficulties he found out in their very "nature", after the discarded attempts of setting up a substitutional theory dispensing with classes and propositional functions in terms of propositions (already in 1906–7 according to the unpublished manuscripts). However (this is the second thing), this does not mean that the abstract entities (classes) which are supposed to arise from propositional functions were acceptable to Russell, as it would seem from a non-constructivist viewpoint; from 1903 onwards he tried to build up a no-classes theory, which underlay all his attempts up to 1908 and 1910. I think the only sense in which Russell can be regarded as a constructivist lies in describing his definitions as constructive, but at this point the main differences with standard constructivism appear immediately: in general, Russell's definitions are also reductive and

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[13] I have compared the three kinds of incomplete symbols, in the context of a wider study which includes the whole development from 1901 to 1910, in my "Russell's theory of types, 1901–1910: its complex origins in the unpublished manuscripts", *History and Philosophy of Logic*, **10**: 131–64 (1989).

then eliminative, and they are stated from a Cantorian ground on which the intensional construction of transfinites is possible.[14]

The rest of Goldfarb's article shows some consequences of this unsatisfactory starting point, especially his attempt at reconstructing the period involved "in somewhat speculative way" (p. 34) instead of resorting to a study of the unpublished manuscripts. For instance, he says that the "multiple-relation" theory of judgment "seems to play no real role in Russell's explanations of his logical system" (*ibid.*), which is very misleading for, although one could master the machinery of the system with no idea of this theory, only by resorting to it can the obscure role of propositions in 1906, 1906 and 1910, and therefore the corresponding structure of primitive ideas and propositions (which are the basis of the logical system!) be understood. This is the main problem making the comparison of Russell's several published theories with each other (pp. 36–38) unnecessarily tentative. Finally, this is also one reason for the lack of an explanation of the relationship between Russell's conception of the universality of logic (p. 27) and his need for regarding its main laws as only mere statements (and therefore not genuine propositions), for they contain apparent variables (pp. 35–36).

N. Cocchiarella's contribution comes to complement (and improve) his former efforts in disentangling the difficulties of this important period in Russell's evolution (as well as of the subsequent ontological evolution), although he continues without resorting to the relevant unpublished manuscripts. This has the disadvantage of filling in the gaps with rather speculative historical arguments, given that Russell usually only presented in his publications the *results* obtained in the former unpublished detailed work. He begins by improving his former comparison between Russell's theory of types of 1908 and 1910, and then he tries to explain the difficulties in the evolution of the ontological commitments of propositional functions in *Principia* and later works, to the point of describing Russell's later presentations of them as mere "linguistic conveniences". I shall say something about both these things.

To my knowledge, Cocchiarella was the first in pointing out some important differences between Russell's published theories of types of 1908 and 1910.[15] But here he insists that one of these differences consists in the status of propositional functions; so, while in 1908 they were "nonentities"

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[14] The arguments justifying my view of Russell's evolution in the development of the final theory of types can be found in the article mentioned in note 13, and a detailed account of evolution of his thought concerning Cantor's theories in chs. 2 and 4 of the book mentioned in note 8.

[15] See his "The development of the theory of logical types and the notion of a logical subject in Russell's early philosophy", *Synthese* **45**: 71–115 (1980).

(p. 43), in 1910 they are single logical subjects (Cocchiarella also claims that the converse situation takes place with propositions). He correctly uses the strong ontological status of propositional functions in 1910 against Church claims that the fragmenting that the "multiple" theory of judgment causes on these entities means their elimination as entities. However, he offers no convincing arguments that they have not the same status in 1908. He shows the eliminative definition of classes in terms of propositional functions to be the sign of their strong status in 1910 (p. 46), but we find the same situation in 1908. He adds that Russell's pre-1910 rejection of propositional functions can be found in *Principles,* but the exegesis he offers is not enough and can easily be balanced by resorting to other passages. Finally, he points out that the paradox of classes was also applicable to propositional functions, so that they could hardly be the basis of the system; however, this is obviously not applicable to 1908, once classes were explicitly eliminated.[16]

Cocchiarella's study of the subsequent evolution of the status of abstract entities in Russell's philosophy is very interesting and original, particularly when he shows that after Russell's conversion to nominalistic philosophy, the implicit rejection of propositional functions makes logicism impossible (for defining numbers as classes of classes requires the defining of classes in terms of propositional functions as "logical subjects"). However, I would like to discuss some of the points involved. First of all, it seems at least doubtful that Russell had been very worried by Cocchiarella's conclusion about logicism as he himself explicitly renounced (under the influence of logical positivists) any strong sense of mathematical truth not being purely linguistic, and some intuitive sense of absolute truth was the main basis of his old logicism.[17] Besides, the passage from 1918, which we are told as a sign of Russell's regarding propositional functions as mere logical conveniences, says only that they are expressions becoming propositions when an undetermined constituent is determined, which is exactly the same intuitive explanation given by the time of *Principia,*[18] i.e. before the problems with the multiple theory of judgment (1913) were published. Of course, Russell's introduction of the second edition of *Principia* (1925) introduced an "atomistic hierarchy of sentences" (p. 57), once the influence of Wittgenstein became consolidated in an extensional sense, and here the exegesis offered by Cocchiarella is very illuminating. But I think that in considering differences

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[16] I have criticized these and similar arguments in a detailed way in the article mentioned in note 13 (especially on pp. 156–59). Likewise, there I pointed out and attempted to explain the strange status of propositions in 1908 by resorting to the unpublished manuscripts (see Section 2 above on the "multiple" theory).

[17] See my "El logicismo russelliano: su significado filosófico" *Crítica,* forthcoming.

[18] Ref. 13, p. 161 (note 27).

between *Principia* and 1918 and 1925 one has to supply some general context of explanation to the two main changes which took place in the meanwhile: the reconsideration of the multiple theory (1913–1918) and the new theory of proposition (1919 onwards). As Cocchiarella offers neither of them, he ignores the implications of the new psychologistic theory of proposition (p. 56), and more important, he seems not to see the link between the difficulties with the new theory of judgment and the theory of types (pp. 53–54).[19]

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D. Pears' discussion of Russell's 1913 manuscript, *Theory of knowledge,* is rather limited in its scope. The main goal here is to study Russell's treatment of acquaintance with predicates and relations and then "the most difficult problem" (p. 171): his account of our understanding of propositions through the multiple theory of judgment, including the force and significance of Wittgenstein's criticisms. The fulfilment of these goals would require at least the providing of a global context of the abandoned book, along with a full explanation of the difficulties of the application of the theory to propositions, and enough exegesis of Wittgenstein's cryptic objections. I think that Pears provides us only with the first one. He is right in pointing out that the kernel of the problem lies in Russell's intended acquaintance with relations, and also that his identifying the involved form with the fact that something has some relation to something involves an infinite regress, for it always requires another proposition just of that form (pp. 175–76), but it is also necessary to see that Russell's celebrated scheme for relational propositions violates *at the same time* the theory of types (by locating at the same ontological level individuals, properties and relations), Bradley's objection against relations (in fact the infinite regress of Pears), and the need for avoiding nonsense (which is possible by changing the order of the elements). As for Wittgenstein, the only explanation for his famous letter of June 1913 is that "acquaintance must be intensional" (p. 179) as it has to allow

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[19] The main link is of course Wittgenstein's criticisms of regarding "forms" as genuine constituents. As I try to explain in my forthcoming "El impacto de Wittgenstein sobre Russell: últimos datos y visión global", the abandonment of forms as constituents, which proceeds ultimately from the old Bradleyan objection against relations, already made any theory of types (or any other linguistic hierarchy) impossible, which Russell apparently recognized only in 1924 (but to my knowledge without introducing any other "new" version of this theory, as Cocchiarella suggests). See also Section 4.

the subject to put the elements in a correct order, which is on the right lines, but is obviously insufficient.[20]

The article by C. Wade Savage is a long attempt at putting order in Russell's complex evolution concerning sense-data from 1912 to 1959. The author starts by correctly summarizing the standard sense-data introduced in 1912, as based on a subject-object relation and the judgment involved as a multiple relation between the subject and the constituents of the corresponding sense-datum (p. 142), although it had been better to recall that this schema was also valid for "conceptual judgments". The problems begin when Savage tries to explain the later evolution, i.e. the actual history of Russell's abandonment of sense-data. Then, he does not see that passages extracted from Russell's great epistemological works of 1921, 1927, 1940 and 1948 cannot be adequately compared among each other without some previous general explanation of the deep philosophical *sense* of this evolution. Savage seems to think that the sense of Russell's evolution is only a matter of degree according to which "the original sense-datum becomes the ideal, practically unachievable limit of the actual data of sensation and perception" (p. 139). From this viewpoint he undertakes a hopeless search of the isolated passages where Russell more or less forgot the need for not mentioning the expression "sense-datum" or similars (pp. 149–50), and then he proposes the above interpretation as a general explanation (pp. 151 ff). I think that from his new psychologistic theory of propositions in 1919 and his reconstruction of mental events in 1921 and onwards, Russell completely abandoned all attempt at maintaining pure data, after having understood that they cannot be the *foundation* of empirical knowledge, but the *result* of explicit or implicit theories, which he accurately applied to linguistic isolated terms. That is why I think that the best thing about the paper is its pointing out the compromise implicit in Russell's evolution from a open acceptance of pure data to "a pure coherence theory" (p. 139) based on a closed system of rational beliefs (pp. 159, 161). However, the real compromise can be better described by resorting to what I have called the progressive evolution from atomism to holism.[21]

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[20] However, Pears correctly points out that this was the origin of Wittgenstein's picture theory of meaning. In my article mentioned in note 19 I propose a global explanation of these criticisms (together with the respective "ways out" of Russell and Wittgenstein) by taking into consideration the recent contributions of Sommerville, Blackwell, Iglesias, Griffin and others.

[21] In my "Bertrand Russell 1920–1948: una filosofía de la ciencia entre el holismo y el atomismo" (forthcoming in this journal) I try to present Russell's apparently chaotic evolution as a progress towards a greater weight of the theoretical context and a preeminence of relations (theories, structures, contextual or implicit definitions, a certain

The situation as to the paper by W. Demopoulos and M. Friedman is very different concerning this point. In fact it seems to me one of the most interesting and important articles of the book (and even of the recent literature on Russell), for the authors perfectly understand the role of implicit or structural definitions in Russell's later philosophy and succeed in pointing out the close links of Russell's constructions in *The analysis of matter* (1927) with the completely contemporary problem of the model-theoretic approach to the empirical theories (which, incidentally, can be extended to Russell's later works). The kernel of the paper is as follows. Russell said in 1927 that all we know of the world are its structural (mathematical) properties, which led him to use in practise implicit definitions (e.g. matter is what fulfils physical laws). This approach (which is somewhat similar to Ramsey's, who interpreted theoretical terms as quantified variables) was already criticized by M. H. A. Newman in an almost unknown article (1928) under the argument that since structures depend only on cardinality (for they are defined as relation-numbers), to say that the physical world has a particular structure is only to talk about a cardinal number, which is rather trivial and of course not very empirical, unless we admit some additional knowledge about certain "important" structures, which would mean to violate the main claim. The authors finally explore the analogies of the difficulty with Putnam's objection to Carnap's view that empirical theories are formal systems only partially interpreted . I suspect that this analogy has a long future, especially if related with the recent semantic approach to physical theories by Sneed and others.

Another good point of the paper is that it makes sense of a until now rather cryptic letter from Russell (in his autobiography) to Newman, which contains an open recognition of the objection and the modification of the original claim: "I had not really intended to say what in fact I did say", for some other things, besides pure structure, are also known, e.g. spacio-temporal continuity among percepts, and similar examples (p. 192). It is a pity that the authors do not complement the paper with some account of Russell's early and later evolution from atomism to holism and with a study of the analogies with Eddington's similar views at that time. That might have made it possible to avoid the common mistake with which they start: that the work of 1927 supposed Russell's rejection of phenomenalism and the conversion to a Lockean causal theory of perception.[22]

idealism, etc.) over terms (data, epistemic atoms, nominal or explicit definitions, a certain realism, etc.).

[22] As I explain in detail in the article mentioned in note 21, Russell never was a phenomenalist and always defended a causal theory of perception, although both notions received a new explanation according to Russell's neutral monism and quantum and relativity theories.

I think that the same mistake also pervades Sainsbury's paper on Russell's postulates (1948) ruling scientific knowledge (e.g. pp. 210–11), which, however, comes to complete the former book by the same author, where Russell's later philosophy was almost completely eluded.[23] Sainsbury's main claim here is that Russell's principles of evidence (general and conditional statements determining the relation between data and hypotheses), which are true in virtue of the postulates of scientific inference (contingent facts about the actual world), cannot be said to be *known* at all without giving rise to skepticism. Thus, if these principles are contingent their truth is open to doubt, and if they are necessary it is their "credibility" which is open to doubt (p. 216). I am not sure whether Sainsbury's point is worth the while because I am not able to satisfactorily understand the difference he holds to exist between the principles of evidence and the postulates. In fact he describes some postulates as empirical generalizations and others as principles of evidence by pointing out passages where Russell introduced them in terms of frequency (p. 206), but Russell's postulates can hardly be described as empirical generalizations since they have to justify in some way precisely that kind of generalization (of course Sainsbury says that the supposed knowledge of the principles is only a priori). I suspect the point to some extent depends on the mistake I pointed out above, for when Sainsbury quotes Russell as defending the need for some "synthetic" property of the actual world (p. 204) he seems not to realize that this kind of property has to be regarded from the viewpoint of neutral monism, i.e. affecting in the same way both subject and object, since he relates the claimed skepticism to Russell's "realist perspective", i.e. the belief that "the course of nature is independent of our knowledge" (p. 217). However, I think that Russell's view by this time was rather more similar to Berkeley's and Hume's than to Locke's, although of course already within the framework of modern physics.[24]

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To finish, I shall include a few lines on each one of the rest of the articles as they appear in the book. H. Hochberg has tried to resolve Russell's paradox of impredicability without resorting to the theory of types, by transforming the supposed property into a certain relation and then avoiding Wittgenstein's objections and Bradley's paradox against relations. I find

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[23] R. M. Sainsbury, *Russell*, London: Routledge, 1979.

[24] Russell himself explicitly admitted this similarity, as I have shown in the article cited in note 21 (see also ref. 22).

especially interesting his comments on the notion of form, though I doubt that his approach contributes to a better understanding of Russell's actual philosophy.

R. Fumerton's paper is devoted to defend Russell's theory of proper names as disguised descriptions from different objections, even to the point of giving to some of these objections a Russellian form, so that they result incorporated into Russell's main idea. I have nothing to say, apart from denying that the regarding of names as disguised descriptions is an original doctrine of Russell (it proceeds from Bradley), and pointing out that, therefore, it is previous to the theory of descriptions, so that it can hardly be a consequence or an extension of this theory.

J. Farrell Smith's contribution considers Bar-Hillel's objections to Russell's later claim that some egocentric particular (indexical name) is indispensable, given that even Carnap's manoeuvre of replacing them with a spatiotemporal description ultimately requires some origin to any system of coordinates, which has to be known independently. She recognizes, however, that these names are to some extent necessary to describe our awareness of sensory experience. Some discussion of Russell's linear epistemology, depending on his general reductive method which always *needed* primitive (undefined) ideas, had illuminated the arguments.

J. Earman has proposed some criticisms of Russell's 1948 account of probability, after having constructed a very technical general classification of six different concepts of projectability on a Goodmanian basis. The main criticism is that Russell failed to distinguish future-moving induction (which depends on future evidence) from past-reaching induction (which depends on past evidence), so that he did not realize that Goodman's objection (which Russell independently discovered) applies only to the second one.

J. Hawthorne's paper is a comparative study of Russell's and Maxwell's treatments of induction, both built up on the ground of a Bayesian epistemology. The view common to both philosophers was the rejection of "judgment empiricism" (that all contingent knowledge can be empirically justified), and the main difference concerns the role of Russell's postulates of scientific knowledge. I find original the effort to trace Russell's view of induction back to Bayes' theorem, but I cannot agree with Hawthorne's idea that Russell's later philosophy (which never gave up neutral monism) defended the world as composed of mind-independent events (see above).

C. Anthony Anderson has studied Russell's little known paper "On order in time" (1936) and has compared it with similar less technical version from 1914 to 1927. One of the results is that in 1927 Russell proved the existence of instants by using the axiom of choice, while in 1936 he adopted a more ex-

tensional view by dropping the axiom. Besides, Anderson defends constructionalism against the well known criticisms by Benacerraf.

E. R. Eames's paper analyzes Russell's evolution concerning cause. The general aim seems only to be expositive and apologetic, but even so she resorts too much to the mistaken practise of dividing Russell's evolution into artificial "phases", which is especially misleading when she describes the 1927–1948 period as "realist" without enough exegesis, and does not realize that the corresponding notion of cause, according to modern physics, is completely different to the one of 1914.[25]

Finally, K. Blackwell (the Russell Archivist in McMaster University, Hamilton, Canada) offers us a vivid and interesting survey of Russell's evolution, always depending on an almost mystically close dependence on science. His precise information about the unpublished material in the Russell Archives concerning *Human Knowledge* (1948) is to be expected to encourage scholars to undertake a deep, and probably fruitful, study of this rather neglected great work of a great philosopher.

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[25] See notes 21 and 22.